REPORT OF A VISIT TO FOREIGN LABORATORIES

FEB. - JUNE
1913

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CARNEGIE INSTITUTION
OF WASHINGTON

NUTRITION LABORATORY
BOSTON

REPORT OF A VISIT TO FOREIGN LABORATORIES

FEBRUARY TO JUNE, 1913

By

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Boston, Massachusetts.

1916

LIST OF INSTITUTIONS VISITED.

Place	Name	Directors of laboratories and associates whose work was personally viewed.	
Florence	General Medical Clinic	Marchetti Capezzuoli Frugoni	94
	Physiological Laboratory of the Imperial Institute	Fano	108
Naples	University of Naples	Galeotti	112
Rome	University of Rome	Luciani	125
Budapest	Agricultural Institute, Buda University Medical School, Pest (Institute of Pathology)	Tangl	135
Vienna	Hochschule für Bodenkultur	Durig	166
	University of Vienna	Meyer Kassowitz von Noorden	168
Munich	University of Munich (II Medical Clinic)	Müller Neubauer	172
	(Physiological Laboratory)	Frank	175
	Hygienic Institute	Gruber	179
Berlin		Friedenthal	181
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	University of Berlin (Physiological Laboratory)	Rubner	193
	Kaiserin Auguste-Victoria Haus	Langstein	195
	Institut für Gärungsgewerbe (Physiological Laboratory)	Völtz	196
	Königliche Landwirtschaftliche Hochschule (Tierphysiologisches Institut)	Zuntz Caspari Müller	197

Place	Name	Directors of laboratories and associates whose work was personally viewed.	
Moscow	Frauen-Hochschule	Schaternikoff	229
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	University of Moscow (Physical Institute--Laboratory of Professor Louguinine)	Swientoslawski	24
(Sofia)	(University of Sofia)	Bachmetjew	253
St. Petersburg	Institute for Experimental Medicine	Pawlow	255
	Women's Medical College	Likhatscheff Albitsky Kartaschefsky Sskolow	260
Helsingfors	University of Helsingfors (Physiological Laboratory)	Tigerstedt	266
Stockholm	Karolinska Institute (Physiological Laboratory)	Johansson	270
	Laboratory of the Stockholm Board of Health	Sondên	276
	Karolinska Institute (Pharmacological Laboratory)	Santesson	280
Copenhagen	University of Copenhagen (Physiological Laboratory)	Henriques	281
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Utrecht	University of Utrecht (Institute of Physiology and Physiological Chemistry)	Pekelharing Zwaardemaker	301
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Place	Name	Directors of laboratories and associates whose work was personally viewed	
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	Guy's Hospital Medical School (Physiological Laboratory)	Pembrey Haldane	319
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INTRODUCTION

Objects of European trips.

It has frequently been asked me, both in America and Europe, how I, as Director of the Nutrition Laboratory, can afford to take four months' time, in addition to a sum for travelling expenses and my salary, for a foreign trip. It is difficult for many to understand the policies which would justify this expenditure of time and money, but I feel myself in an especially advantageous position to estimate the value of such a trip.

Bio-calorimetry, gaseous exchange, and body-temperature measurement have all received relatively little attention on the part of physiological laboratories. By virtue of its equipment, the Nutrition Laboratory is particularly fitted to undertake researches in these lines. While a large sum of money has been expended upon the building, its equipment, and the maintenance of its staff in order to have it in the best possible condition for studying such problems, I have also considered it desirable to keep closely in touch with the foreign workers in these fields of research to avoid the duplication of results, to repeat important experiments whose experimental evidence may possibly be too fragmentary for general acceptance, and to make myself familiar with the methods and apparatus in use in the foreign laboratories. I have pursued the same policy in regard to American workers, visiting such laboratories as that of Professor Armsby in State College, Pennsylvania, the U. S. Department of Agriculture, and particularly the laboratory of Professor Graham Lusk in the Cornell University Medical College. These men or representatives of these laboratories frequently visit the Nutrition Laboratory and hence we are kept more or less in touch with their researches. On the other hand, many foreigners find it impossible to come to America on account of the distance and the expense, and to familiarize myself with their work, I find it desirable to make a tour of the European laboratories about every three years.

The main object of the European trip, therefore, is to keep in touch with the different workers in the lines of research in which we are interested and to seek new ideas and methods for use in our own investigations. A second point of almost equal value is to disseminate information regarding our own researches. This is particularly necessary as many of the results of our own work are published in the form of monographs which are not in any way comparable with the papers in scientific journals used by most research workers to present their results to the scientific world; it has accordingly been difficult for foreigners to secure an adequate understanding of the character of our work, particularly when the results are published in the Carnegie Institution publications. It is true that the Carnegie Institution publications are deposited in practically all of the large libraries of Europe, but this is not generally known by the European physiologists; it is a matter of personal knowledge, however, that when attention has been directed to the publications, use has been made of them. Other research institutions, not connected with universities, have had the same experience, notably the Solvay Institute in Brussels, and more recently the Kaiser Wilhelms Institutes in Berlin, which at first were but slowly known and are now being looked to by investigators for scientific results.

Still another advantage gained by these periodical trips to Europe is the advance information secured regarding methods and researches which are in progress in the laboratories visited and are freely shown to me. As a result, when I return from Europe, I am at least one year in advance of the scientific literature on these subjects. By this system of European trips, the laboratory is thus kept strictly up to date.

Such intercourse is of mutual benefit and renders possible a entente cordiale between ourselves and the laboratories visited. The entire independence of the Nutrition Laboratory from all other existing institutions, the broad principles of administration adopted by the Board of Trustees and the President, and the select field of research have raised this laboratory above many others and hence entrée can be secured to many laboratories which are not usually freely opened to visitors. I am therefore brought into contact with the greatest men in physiology, hygiene, and medicine, not superficially, but in frequent and intimate intercourse for periods of days or weeks. The Physiological Congress held triennially in different European cities gives an opportunity for making known to each other the workers in the various scientific fields of research. It has not been necessary for me to attend these congresses for I have had the opportunity to visit these men in their laboratories and frequently at their homes, and thus see them at work, tell them of our own work, and exchange ideas and criticisms.

Through these trips, also, it is possible to find candidates among the younger members of the laboratory staffs for Research Associates in the Nutrition Laboratory. Furthermore, as I meet many men who are compiling the results of others investigations for use in textbooks, handbooks, and encyclopaedia articles, it is possible to put them in touch with the sources of information regarding the work of the Nutrition Laboratory and make sure of a proper representation of our own results.

The opportunity to study the most recently devised apparatus, become familiar with its technique, and if thought desirable, order direct from the mechanician or the manufacturer a duplicate for use in the Nutrition Laboratory is a valuable one. It has likewise been possible to introduce into European laboratories apparatus which has been devised in this laboratory by supplying sketches, blue prints, and occasionally sending a model for reproduction. Other members of the staff--Mr. Carpenter and Mr. Higgins--have also been able to introduce various apparatus and technique into some of the European laboratories they have visited.

Finally, my experience has shown that through my wide acquaintance and pleasant relations with European investigators, I have been able to be of service in promoting better feeling among the various laboratories and in correcting misunderstandings. It is easy to see the potentialities of such friendship in smoothing out difficulties between individuals.

The special objects of the tour of 1913.

My first European tour was made during the construction of the Nutrition Laboratory and the primary object of that trip was the study of laboratory construction and equipment. A second trip was made about three years later to study special respiratory technique and the metabolism investigations on diabetics. Following these trips, other members of the Nutrition Laboratory staff were sent to Europe to study certain details in technique and laboratory facilities which I personally had not time to investigate. During my trip of 1913, several important points were specially studied.

Administration.

Since this laboratory is somewhat unlike other laboratories, its method of administration has caused me not a little difficulty. Accordingly I made a special study in the laboratories visited of the methods of administration, the relations between the Director and the staff, the working space allotted to the members of the staff, the locker accommodations, the freedom of the supply room, the times of coming and going, and similar matters of administration.

Library.

Considerable attention was given to the best methods of making the library accessible and useful to the staff and of the care and disposition of the incoming reprints. I also took pains to find out where the publications of the Carnegie Institution were located, whether in the departmental libraries or in the personal library of any one of the professors or in both. This has an important bearing upon the method of distributing reprints, since the primary object of such distribution is not to enlarge the library of a particular professor but to make the material as accessible as possible to the largest number of assistants working in the laboratories and of men in the particular line of research treated of in the publication. Many suggestions of value were obtained.

Program for the investigation on the influence of ethyl alcohol.

Shortly before I went to Europe, an extensive program for the study of the influence of the ingestion of alcohol on man was prepared and sent for criticism and suggestion to a large number of foreign investigators. Typewritten letters were also sent to many of these, stating that I should subsequently visit their laboratories and should be glad of the opportunity to discuss with them the plan and general character of the proposed investigations with the hope that they would freely make suggestions and criticisms. As a result, I had personal conversations with a large number of competent critics who gave me many valuable suggestions.

Metabolism studies at high altitudes.

It is becoming of increasing importance to study the influence of high altitude upon metabolism. Notwithstanding the extensive investigations of Zuntz and his co-workers and the more recent investigations of Professor Haldane and his associates on Pike's Peak, it seems desirable if possible to study the existing methods of research so as later to establish a station or a temporary station upon Pike's Peak for carrying on studies with a better technique, as the many investigations which have previously been carried out were made with apparatus which were necessarily transportable and the methods were somewhat inaccurate. After considerable diplomatic correspondence with Professor Galeotti, arrangements were made for a representative of the Nutrition Laboratory, Mr. H. L. Higgins, to spend a portion of the summer at the Mosso Laboratory on Monte Rosa. The details were arranged in a personal interview with Professor Galeotti and by cable and letter with Mr. Higgins. The results of this trip are given in Mr. Higgins' report.

Such intimate connection with the more prominent investigators in studies at high altitudes should give us a thorough knowledge of their technique and an opportunity to improve upon the methods used. Mr. Higgins' experience with Professors Barcroft and Haldane made him especially well fitted for this work.

HAVRE, FRANCE

Professor Jules Lefèvre

In order to meet Professor Lefèvre personally in Havre, I took the French line from New York. Although I had written Professor Lefèvre previously as to the date of my arrival, it was only after a series of mishaps that I finally met him at his apartment and had a conversation of about an hour and a quarter. Lefèvre is a man of about 55 years of age, typically French in his volubility and enthusiasm, but probably no deeper than most of his countrymen. He is a professor or teacher in the High School in Havre and has had no laboratory for a number of years. For the last eight years he has been working upon his large volume "Chaleur animale et bioenèrgetique" and has accordingly done no experimental work, but expects to begin again shortly. He evidently desires to become associated with the new Societé Scientifique d'Hygiène Alimentaire in Paris, whose new laboratory is in process of construction. In fact, he spoke to me of moving to Paris in two years. I should judge from his conversation that he expected an appointment there and wishes very much to visit the Nutrition Laboratory, Rubner's laboratory, and other foreign laboratories. He is very enthusiastic about the <u>ècole Americaine</u>, as is evidenced from the large place given to it in his book, and was full of the usual Gallic expressions of appreciation.

He had only just received the alcohol program but told me he would read it carefully and write me regarding it. Apparently he had confined himself exclusively to literary work for the past decade and when reading his large book, one could easily see how that time was occupied. Personally I have found it very accurate. The relatively few references I have had occasion to check have always been correct.

Lefèvre is much interested in the liver as a heat centre. Although I had difficulty in understanding all of the details of his conversation, as near as I could make out, his impression is that if an animal is half frozen so that it is nearly motionless, dead, and cold, and it is then wrapped in cloths but without artificial heat, it will revive. During the process of revival, if a thermo-element is thrust into the liver, it will be found that the temperature of the liver will rise before the temperature of the muscles; in other words, the liver begins the heat-production and from the liver are carried substances which stimulate the heart and finally the muscles to action. This process inside the liver is a process of heat-production without oxidation in that the substances are formed in the liver and are carried to the muscles. On the other hand, the heat-production in the heart and muscles must be accompanied by an oxidation process. He therefore considers that the liver is a thermogenic centre. In this connection, Lefèvre cited experiments that he had reported in the Brown-Sequard journal a number of years ago.

One of the most interesting points in connection with my conversation with Lefèvre was his attitude towards Chauveau. I had always supposed that Chauveau held the highest place among French physiologists and that all Frenchmen from piété supported his views as much as they could. Lefèvre says, however, that this is not true.

Lefèvre said that Chauveau is a physician and not a physiologist; in fact, he is a veterinarian, and while he is a genius, he is not a man who studies into things deeply. All of his research on muscular work has been done from the standpoint of a physician and not from that of a physiologist. Finally, he emphasized very strongly the fact that he thought that Chauveau was entirely wrong on the question of static work. He emphasized this by drawing up his arm and showing that the muscle under tension was very hard, and said that the physician would say, "There! The muscle is hard and work is being done." This he gave as an illustration that Chauveau always considered everything from the standpoint of a physician.

Subsequently I talked with others both outside and inside of France and found that while the French are inclined to look upon Chauveau more or less as a veterinary and not as an abstract scientific man, still he certainly holds an extraordinarily high place in the minds of practically all physiologists. When I spoke of the French attitude towards Chauveau, many of the foreign investigators have said that they wished they could have accomplished for physiology a fraction of what Chauveau has done.

Lefèvre was very enthusiastic about the possibilities of the new respiration apparatus and maintained that it is well named "universal". He thought that Rubner was entirely wrong about there being two kinds of temperature regulation and believed, as we do, that it is all muscular work, such as shivering, etc. In support of this he cited an experiment that he had made a number of years ago. He was quite opposed to the modern idea of physiologists experimenting with electricity on muscles and similar substances and thought that the only experimenting on physiology worth doing was on man.

It is easy to see from a conversation with Lefèvre that his absence from the laboratory for the last ten years has given him a point of view which is essentially that of the investigators whose writings he has read in preparing his large book of compilations. Naturally, the preparation of this book has given him a very wide knowledge and understanding of the literature. On the other hand, one is instantly struck with the difference in the understanding of the literature by Lefèvre and by Tigerstedt. Tigerstedt has a wonderfully sound head and can certainly value literature as no one else that I know of can. Lefèvre did not give me the impression of being able to value understandingly contributions in physiology.

Evidently his large book, perhaps by its great size, has made a profound impression upon some French physiologists, particularly Dastre of Paris. On the other hand, many of the Frenchmen whom I met while in Paris are quite inclined to belittle Lefèvre's ability. Certainly his earlier experiments on bath calorimetry or radiation calorimetry as reported in his short papers were not carried out with any idea of fundamental exactness and seem more or less like playthings at the present day. In fact, there is very little, if anything, of definite value in his publications on calorimetry.

After leaving Havre, I was of course again much interested in Lefèvre, owing to his connection with the projected researches and construction of the new laboratory of the Société Scientifique d'Hygiène Alimentaire in Paris. Consequently he will be referred to subsequently in this report in connection with my notes on that society.

PARIS, FRANCE

Societé Scientifique d'Hygiène Alimentaire.

Although my visit to Paris in 1907 had not been especially productive in the acquisition of new material or new ideas for the laboratory, I thought it desirable to inlcude it in this trip, as I felt that more than six years was too long a period to elapse before making another visit and getting into touch with the physiologists who were working there. This desire was strengthened by a visit of Professor Gabriel Bertrand to the Nutrition Laboratory in the fall of 1912, when he told me that the Societé Scientifique d'Hygiène Alimentaire contemplated the construction of a laboratory for studying the problem of nutrition. This society had commissioned Professor Bertrand to visit the Nutrition Laboratory with a view to inspecting our respiration calorimeter and judging of its practicability for their new laboratory. Professor Bertrand spent a day here, making many inquiries in regard to the cost of building such a calorimeter and the scientific staff necessary for the proper conduct of calorimeter investigations. He took away with him many sketches and subsequently purchased a number of photographs of different parts of the apparatus.

According to Professor Bertrand, the leading spirits in the society were Professor Armand Gautier, Dr. Roux of the Pasteur Institute, M. Alquier, the secretary, and several others whose names I do not now recall. Before going to Paris, I had received letters from several of these gentlemen, stating that they would like to see me to discuss the problems and plans which had been suggested for the society. The first formal meeting with these gentlemen was at a luncheon given by Dr. Roux of the Pasteur Institute, the guests including the members of the Commission appointed to have in charge the construction of the respiration calorimeter. Among those at the luncheon were Dr. Roux and Professor Bertrand of the Pasteur Institute, Professor Dastre of the Sorbonne, Professor Gautier, Professor d'Arsonval, M. Alquier, Dr. Letulle of the Boucicault Hospital, Mademoiselle Pompilian, and two or three others whose names I now forget. At this luncheon general plans were discussed and I was requested to make some remarks giving my views. Unfortunately my limited command of French and the rather formidable nature of the meeting made it difficult for me to express myself suitably, but arrangements were made by which I could get into touch with each individual member of the Commission at some later date.

After the luncheon, Professor Dastre took me to his laboratory at the Sorbonne and gave me some of his views in regard to the projects then on hand. Among other things he told me that "the" society had raised 2,000,000 francs by means of a lottery and that a lot of land was given to them or had been purchased from the Government near the Pantheon. The foundations of the building had already been laid. When I went to see them subsequently, I found that they were level with the ground and that the concrete base had been put in. It was thought very necessary for the society to make a beginning as in France wealthy men will not give money to a contemplated project, but after the project has been launched and there is tangible evidence of a beginning, money begins to pour in without any effort to raise it. For example, the Pasteur Institute was started with great difficulty and had at first hardly enough money to keep it going, but at the present day it is very prosperous and money is contributed at the slightest request. It is expected that the experience with this society will be similar to that of the Pasteur Institute.

Professor Gautier afterwards told me that the laboratory would not be completed for two years. He has retired from his professorship and expects to devote his entire time to the new laboratory during its construction. In fact, in Paris the inclination is to look upon the society as Gautier's and the journal that it publishes is called Gautier's journal. I also heard that there is more or less of a tendency to consider the society and its laboratory as playthings of Gautier's, for but few seem to have confidence in his ability to accomplish much with the whole project. M. Tissot told me that the whole thing is a dream of Gautier's and is not practical and he does not think that anything will ever be done in the new building for French science. Tissot is extremely pessimistic and maintains that there is no interest in pure science in France. I think that Tissot is somewhat the exception in his views, however, and having more or less lost his own hold on pure science, he is inclined to depreciate the efforts of all others.

Apparently the formation of the Sociètè Scientifique d'Hygiéne Alimentaire stimulated the building and the equipment of the respiration calorimeter in the Hospital Boucicault. This calorimeter was visited both by myself and by Mr. Carpenter six and five ~~three~~ years ago, respectively. A description has been published of it but no control tests have been made of the apparatus and one may say that it has never yielded one fact of scientific importance to the world. During the flood in Paris, the building in which the calorimeter was located was flooded, so that the apparatus was completely spoiled; it was then taken apart and temporarily stored in the Pasteur Institute. After this experience, the Socièté Scientifique d'Hygiène Alimentaire decided to secure higher land for its laboratory and accordingly the land near the Pantheon was purchased, at the corner of the rue Estrapade and rue Clothilde. The plans for the laboratory were published in a brochure, a copy of which was given me. These plans show a very elaborate calorimeter laboratory, this being the dominant feature of the building.

The particular question to be decided is which calorimeter shall be installed in the new building. The society sent Professor Bertrand to Boston after his New York lectures to inspect the Nutrition Laboratory. Meanwhile Mademoiselle Pompilian, who had constructed the apparatus in the Boucicault Hospital, went at her own expense to Budapest to see Tangl and to Berlin to see Rubner and Zuntz. She also saw Professor Armsby of State College while in Berlin and finally went to Bonn to see Hagemann.

The committee appointed to have in charge the construction of the calorimeter is somewhat unwieldy, being 10 in number; apparently each member has a different view from the others as to what should be done. Professor Bertrand has told the committee that I was coming to Paris, and in consequence a number of dinners and conferences were planned so that I might meet the different members of the commission.

It was the intention of the commission to consider two propositions for the construction of a calorimeter, one to come from Mademoiselle Pompilian, the designer of the calorimeter at the Hospital Boucicault, and the other to come from Professor Lefèvre at Havre. Mademoiselle Pompilian's plan had already been prepared and presented to the commission but when I left Paris the latter part of February, Lefèvre's plan had not appeared. Since that time it has been published in the journal of the society. Lefèvre's work in bath calorimetry some years ago and the publication of his large book had favorably impressed some of the members of the commission. Professor Dastre was very strongly in favor of Lefèvre's plan and spoke very freely and very strongly to me regarding it. Drs. Roux and Gautier were preferably for Mademoiselle Pompilian's plan. Many of them said very pleasantly that our apparatus was the only suitable one for their laboratory, but possibly they thought otherwise, their expression being due to

French politeness. One feature of the situation which always struck me as being somewhat unfair was the fact that Mademoiselle Pompilian, the author of one of the plans, was always present at all of the conferences, while Professor Lefèvre, being in Havre, naturally could not be present.

It was obvious to many of the members that the commission was too large. Professor Bertrand, who impressed me as being the most level-headed of them all and to have a clearer grasp of the situation than any one else, was very frank in stating his belief that the commission was too large and that very little could be accomplished until some one individual was responsible for the whole matter; in other words, while the society as organized had a president (Gautier) and the laboratory was being constructed, no director had been appointed for the laboratory and no one individual was responsible for its general plan, its construction, and its equipment. This seemed to me, and especially to Professor Bertrand, a very grave error. Professor Bertrand was sure that the only thing to do was to copy the respiration calorimeter in the Nutrition Laboratory, sending some to to America to study it and learn the technique.

When M. Alquier gave me a copy of the brochure describing the new laboratory, I pointed out several features of the building which seemed to me to be defective. I also spoke of them to Professor Gautier at the luncheon given by Dr. Roux. He then told me that he had invited the architect to meet me at dinner at his house and we could at that time go over the whole plan.

The guests at Professor Gautier's house included the members of the commission, one of whom was M. P. Regnard, also the architect and some other friends. In discussing the plans with the architect, I found that the arrangement of the whole building had been determined by the design of Mademoiselle Pompilian's calorimeter, which required a peculiar series of rooms, one above the other, which practically modified the whole architectural design. For example, a sub-basement was necessary, about six feet high. M. Alquier told me that her first plan called for four large rooms on one floor, which would occupy the whole area to be covered by the building but she finally changed the plan to three rooms over each other. The architect told me that while the foundations were laid up to the level of the land, the whole upper structure could be changed in plan without change in the foundations.

I told them that I thought they ought to have a large museum, perhaps larger than the one which had been planned for. Although the present room might do for a simple historical collection, nevertheless they ought to have a museum of sufficient size to put in it a large number of foodstuffs showing the progress of science in this line. The architect showed me that he could extend the building at one side if necessary but I was content with simply having raised the question.

The calorimeter laboratory, which is now planned to accommodate Mademoiselle Pompilian's apparatus, is about 7 by 10 meters. I considered with them the possibility of installing our bed calorimeter and our No. 3 calorimeter in this room side by side. The extensive use of soda lime also called for the construction of a well-ventilated room in the basement with a good concrete floor for washing. I found the architect most interested and willing to accept suggestions. He said that he realized as never before the difficulty of trying to meet the demands of ten different members of the commission and bemoaned the fact that no one man had been appointed director who should be responsible for the whole construction. As an illustration of the defect in the plans due to this divided responsibility, I might call attention to the fact that the corridor leading from one wing of the building to the other passed directly through the library or reading room with no other interconnection.

The architect told me that he had been unable to bring together the various interests and to satisfy the different members of the commission. He said that the plan I outlined was the first logical plan that he had seen. Each member of the commission would look at the plans for a moment or two and then promptly forget about them and be of no service until called upon again; then some snap judgment would be made which would have no scientific bearing.

At this meeting at Professor Gautier's house, I was also disturbed by the lack of coordination shown among the French scientists. Each was enthusiastic in regard to his own particular line and everything redounded to the glory of France, etc., but there seemed to be no key word in connection with this new society. At present, Gautier, the president, and Alquier, the secretary, work together somewhat, but Alquier had very little confidence in Gautier's judgment and the architect distrusted them both. I was unable to find out why no director had been appointed for the laboratory, but was told that the funds had been slow in accumulating and they felt that they ought to make a good showing first and then get more funds so as to be able to call a man with a greater degree of certainty.

After having seen so many of the members of the commission having in charge the building of the calorimeters and hearing their diverse opinions on the subject, I felt as if I should not leave Paris without making a definite statement, first, as to my understanding of the situation and second, as to my recommendation for action. It seemed to me in this way there would be less liability to confusion and misunderstanding of my point of view due to my limited knowledge of French. Consequently I wrote in English a report to the members of the Council of the Society and gave it to M. Alquier, who after reading it said there was nothing in it to change. He subsequently had it translated, assisting in the translation himself, had copies made for each member of the Council, and sent one to me. This recommendation is inserted here.

Report submitted to the Council of Administration of the Société Scientifique Hygiène Alimentaire, February 26, 1913.

Gentlemen:

Since my conference with Professor G. Bertrand in Boston last October, I have thought seriously on the question of your new laboratory. Never having met M. Lefèvre personally, I planned especially to sail from New York for Havre in order to see him. On my arrival in Havre, I had a short discussion with him on questions of a general nature. Since reaching Paris, I have frequently seen M. Alquier and Mademoiselle Pompilian and have become very well informed as to the general plans and the state of the whole project in conferences with MM. Bertrand, Roux, Gautier, Alquier, Dastre, Weiss, and others. It is unnecessary to state that I am vitally interested in the success of this institution for I see first of all the most intimate and friendly relations between this laboratory and mine.

According to my understanding of your work, two plans have been proposed, one by Mademoiselle Pompilian, based upon her extensive experiments with her apparatus in the Hospital Boucicault, and the other the plan of M. Lefèvre, the details of which are not yet known, but which is based first on his experiments on bath calorimetry of a dozen years ago, and second upon his admirable treatise "Chaleur animale et bioénergétique".

In talking with different members of your commission, I have heard a number of opinions expressed; certain of these favor the plan of Mademoiselle Pompilian and others the ability of M. Lefèvre for meeting the required conditions of such research.

I have the deepest admiration for the remarkable treatise of M. Lefèvre and for his experiments which, in the age in which they were written, had a fundamental interest although science in its progress has left them behind like useless landmarks rather than as sources of scientific information. M. Lefèvre says frankly that his extensive editorial work has prevented him during the last ten years from engaging in any experimental research. As a mathematician he is eminently qualified for calculating with great exactness the theoretical conditions necessary for the complete success of the apparatus, but his long-continued absence from experimental research and particularly his lack of practice in the experimental technique of the modern calorimeter would make his task, if he were to succeed, extremely difficult. I do not mean that M. Lefèvre is incapable of constructing a kind of calorimeter but it seems to me doubtful if he can construct a calorimeter with exactness until after many years of costly experimenting. This is why I recommend <u>not</u> adopting the plan of Lefèvre.

The second plan, that of Mademoiselle Pompilian, is much more advanced in that Mademoiselle Pompilian has already constructed an apparatus and has made use of it. She has furthermore had the advantage of having visited several European laboratories where researches of this nature are carried on and is consequently in touch with the different types of apparatus. While her plan is based upon her previous experience, it includes numerous proposed modifications which are as yet only theoretical. These modifications will necessarily call for a considerable number of experiments before they can be accepted with any certainty as justifying their adoption.

My opinion of the project of Mademoiselle Pompilian is based upon the following:

1. In 1907 I made a trip to Paris and spent about an hour examining the apparatus in the Hospital Boucicault in company with a young physician belonging to the staff of the hospital. Mademoiselle Pompilian was at the time in Budapest but being familiar with such apparatus, I consider myself competent to judge of its merits or demerits. As a consequence of this inspection, I asked my first assistant, Mr. Carpenter, on his next trip abroad, to inspect the apparatus also.

2. Mr. Carpenter visited the Hospital Boucicault in 1908. He examined the apparatus minutely and took 20 photographs from different points of view. He consulted MM. Dorian and Richards who devised the methods for the automatic regulation, and finally wrote out a detailed report, informing me that, in his opinion, there were no characteristic points in the new apparatus of Mademoiselle Pompilian which would justify their introduction in our apparatus. He was by no means satisfied with the apparatus. Mademoiselle Pompilian was not at this time in Paris, but Mr. Carpenter has been for several years in my laboratory and is particularly well qualified to examine and criticise such an apparatus.

3. The description of Mademoiselle Pompilian's apparatus in the Bulletin de la Société Scientifique d'Hygiène Alimentaire and M. Lefèvre's treatise have been carefully read and analyzed.

4. On the 24th of February, I had a long conversation with Mademoiselle Pompilian at the Pasteur Institute in the presence of Dr. Roux in the course of which Mademoiselle Pompilian described certain points of the apparatus in detail and gave me her ideas relative to its installation and the kind of experiments for which it could be used.

To sum up the results of my observations, in the first place the apparatus did not appeal to me favorably, but I decided not to judge it prematurely and sent Mr. Carpenter to inspect it. His report was also unfavorable. After the critical study of the description published concerning the apparatus and my talk with Mademoiselle Pompilian, it remains for me to say that although I feel sure she would succeed in constructing some kind of an apparatus if sufficient time and money were given her, I consider some of her ideas more or less fantastic. If your society desires to install an accurate apparatus, it will secure results more quickly by building a calorimeter which has had much use and long trial than by undertaking the construction of an apparatus which might necessitate several years of preliminary experimenting before actual experimenting can be begun, not to speak of the great expense which its construction would necessitate.

Although I recognize that it is truly presumptuous to suggest to France the idea of going to America for a respiration calorimeter, I desire to acknowledge here that were it not for the fundamental researches of Professor d'Arsonval, America would not have been able to construct her apparatus. It will be a great pleasure, then, for America to repay in a small measure the great debt which she will always owe to France for the fundamental principles so admirably developed by Professor d'Arsonval.

As I explained in detail to your architect, M. Bauhain, it is unnecessary to change the architectural plans of your building to obtain the conditions of construction required by the American apparatus. The room designated for a calorimeter laboratory is large enough for two independent chambers of different dimensions of such sort as to permit of conducting two distinct categories of experiments. This I have explained more fully to M. Gautier and M. Roux. I suggest that you construct a chamber corresponding to the bed calorimeter which I have used in my laboratory for four years and which is especially suitable for all experiments in which the subjects are lying down. In this chamber the male subjects can remain as long as they are not obliged to defecate, since they can generally urinate while lying on the side. Female subjects can remain only until they are obliged to urinate.

Oftentimes 12-hour experiments are made at night, but for the exact determination of the consumption of oxygen in any respiration chamber, it is important that the volume of air in the chamber be minimum. If small quantities are to be measured by the hour, that is to say, during bed calorimeter experiments, the volume should be less than when a man works strenuously on a bicycle. Consequently the chamber of the bed calorimeter has a volume of only about 800 liters.

A second chamber can be easily constructed in your calorimeter laboratory, long enough to permit of lying down and sleeping, high enough to stand erect in and likewise sufficiently large to permit the use of a bicycle ergometer, permitting very strenuous muscular action. This chamber has a capacity of 3,500 liters.

My recommendations relative to the best method to be adopted to assure the perfect installation of these chambers would be to send two persons to Boston for a minimum period of six months. I shall be very happy to put the services of the laboratory at the command of these gentlemen and to offer them all the facilities for instructing themselves not only on the details of construction but also of manipulation. One of these gentlemen should be an expert in instruments and the other a competent assistant. Personally I recommend that M. Bull of the Marey Institute, as an expert in instruments, be intrusted with the commission. He should be admirably able to study the apparatus, assure its faithful reproduction, and in case of accident or disorder, could always be called upon thereafter to help in its repair or in giving directions.

M. Bull's assistant should be carefully chosen with the understanding that he shall be a member of the staff of the new laboratory, that he shall assume the responsibility of the respiration chamber, take all the care of it, train the young assistants who are to make the small repairs, and direct the mechanical part of an experiment. He must be a physiologist and should see that no important repair and no attempt at improvement be made without the consent and the advice of M. Bull. Once the apparatus has been installed, he should operate it perfectly with few adjustments. It is dangerous to attempt improvements in this apparatus, such attempts being likely to lead to the loss of a whole series of experiments.

I do not know whom to suggest as an assistant for M. Bull. At the time of my visit to Paris in 1907, there was an excellent assistant in the laboratory of M. Chauveau,--M. Jules Mansion, but I think he has since left Paris. If M. Bull consents to go to America as a representative of the society, his advice should be taken on the choice of his permanent assistant, for it is essential that they work in perfect harmony.

In my opinion M. Bull is extraordinarily well qualified to accept the commission. The advantage of being able to rely upon him for authoritative advice instead of being obliged to depend wholly upon the regular staff of the laboratory is valuable. His services would be necessary only in case of accident.

Finally, gentlemen, permit me to give you one last suggestion, based upon my own experience. I feel that the administrative questions which have frequently been raised and which have disturbed me in Boston would be still more serious for your institution in Paris, directed as it is by a Council and not by one man assuming entire responsibility. After the apparatus has been completed and operates perfectly, there will be a tendency to misuse it. The whole clinic at Paris will have "interesting cases" and will come to beg you to make such or such an experiment. The temptation to study certain particularly interesting cases will be, I admit, very strong, but I can assure you that it is only by adhering to a carefully thought out plan that you will be certain to secure results worthy of the magnificent institution which your public spirit has rendered possible.

All the resources of my laboratory, my advice, and my help are always at your disposal.

Very sincerely yours,

(Signed) F. G. Benedict

Director.

Upon the basis of this recommendation M. Bull of the Marey Institute was commissioned by the Society to come to the Nutrition Laboratory to study the calorimeter. The original plan which I proposed was not adopted inasmuch as M. Bull was sent to America without an assistant, and his stay was cut down to three months. I expressly stated that I assumed no responsibility for the successful construction, maintenance, and use of a calorimeter which was built under such conditions, and I wished the Committee and the Society to understand clearly that if M. Bull went to America under these conditions, he did so at the desire of the Society and with no guarantee on my part that he would have sufficient experience with the apparatus to enable him to return to Paris, construct a calorimeter, test it and successfully keep it in operation. M. Bull wrote me later that he had presented my views to the Society, that they exonerated me from any responsibility in the matter, and expressed their appreciation of my willingness to allow M. Bull to study in the laboratory for three months.

Professor Armand Gautier.

Professor Gautier, who is president of the Sociéte Scientifique d'Hygiène Alimentaire, told me that there is a great interest in Paris in regard to the food and nutrition of man. During my stay in Paris I was invited to a lecture on the food of the Parisian given at the Sorbonne by Professor Gautier. The room was crowded, the audience being very appreciative of what he said to them. Professor Gautier speaks very rapidly and my command of French is very imperfect, so that I understood but little of the lecture. His complimentary references to American work brought forth the only applause of the evening. This applause, which was prolonged, was evidently stimulated by some personal remark. He spoke particularly of the Nutrition Laboratory, showing slides of the calorimeter on the screen. As a matter of fact, Professor Gautier told me afterwards that he knew or hoped that I was somewhere in the audience, although as he had not then met me, he did not know where I was. His lecture will be printed in full in the Revue Scientifique together with a colored plate which he showed.

My personal conversation with Gautier was much interfered with by the fact that he spoke very rapidly and not very distinctly, so that with my poor command of French, I understood him but imperfectly. He is very outspoken in his discussion of German research and maintained that it partook of the nature of commercial enterprise, citing particularly Abderhalden's numerous papers. He maintained that the Germans published a great many papers, all upon the same point, with but slight variance and relatively few new ideas. On the other hand, Gautier spoke in the highest terms of the English physiologists.

In discussing the alcohol question, he maintained that alcoholic liquors are used in too concentrated a form but if taken as wine or diluted with water, they do little harm. Mademoiselle Pompilian also held the same view, i. e., that wine was a good thing of itself if the alcohol was not too strong but diluted. I noticed at the luncheon given by Dr. Roux that many of the men took very little alcohol, chiefly in the form of champagne and at the time of the toasts. There was no smoking at all, possibly due to the presence of Mademoiselle Pompilian. At Gautier's house there was much more wine served and much more wine taken. There was no smoking as I recollect until we entered Gautier's private study.

Professor Gautier is a man who has completed his university life and now finds himself unoccupied. He must do something and so has busied himself with the Société Scientifique d'Hygiène Alimentaire. The popular impression in Paris is that it is more or less a plaything of his. He has been a popular lecturer and writer but has not contributed extensively to physiology or to chemistry in France. There are very few who think he will accomplish much with the new laboratory. My personal intercourse with Professor Gautier led me to believe that he was a man who was most interested in the Society but more from personal pride than from an expectation of making fundamental contributions to science. I judged he was a man of considerable means. He has a local reputation of being interested in the wine trade and in furthering the production and use of wine. The experiments which he made not long ago in which he gave alcohol to animals have been quoted to me frequently as an indication of his propaganda for the use of alcohol. Personally he was most cordial and kind as were the members of his family and every attention was shown me by him that one could possibly expect. He seemed to be very appreciative of my interest in the building and my criticisms of the plans and method of construction. The fact that Alquier, Bertrand and practically all of the French scientific men have very little confidence in his ability to make much of the Society is, to my mind, very unfortunate as it tends to give the Society a handicap at the start.

Monsieur J. Alquier.

M. Alquier, who is the General Secretary of the Société Scientifique d'Hygiène Alimentarie, called at my hotel repeatedly while I was in Paris. He evidently put himself entirely at my disposal and intended to give me all the information possible in regard to the Society and its plans. On the way to the dinner given by Dr. Roux, he showed me many of Mademoiselle Pompilian's plans. He also told me that the Society, by the terms of one of the gifts received, is practically obligated to build some form of respiration chamber, but they found a great deal of difficulty in deciding what kind of chamber to build. M. Alquier had no confidence in Mademoiselle Pompilian's plan and when I explained our Americanism "make good", he said it applied in her case. In other words, she has built a calorimeter, so called, at the Hôspital Boucicault and it had never yet "made good". M. Alquier also told me that Dastre was very much in favor of Lefèvre's plan, for being a mathematician, Lefèvre has computed all of the dimensions, volumes, resistances, insulations, coefficients, etc., but as a matter of fact, Lefèvre's plan is entirely on paper, and his practical experience has been confined to bath calorimetry.

He was much surprised to find that in the Nutrition Laboratory we could make our analyses with women assistants and with so few assistants. Personally he is much interested in the chemical side of agriculture and has had much experience in using petroleum in the place of ether for fat extraction. He told me that Dr. Roux has no stenographer, writes out everything in long hand and does all of his own calculation. On the other hand, Alquier is modern in his methods, has a machine for calculation, and wants to see the results immediately. He remarked that most men who become the heads of research institutions stop work and sit in their offices, wearing out their chair seats, but it was necessary for a man to keep about the laboratory in close touch with the work. He said that Dr. Roux is about the laboratory all of the time.

When I discussed the alcohol question with him, Alquier said that he thought the stimulation was not due to alcohol but to the extracts and that one can get the same results with alcohol-free liquids, at least with small amounts, by using solutions of the extracts. He had done some work in this connection but the results had never been published.

M. Alquier impressed me as being a very ambitious, hard working, intense sort of an individual, and certainly has the welfare of the Society and science at heart. He has evidently a great many things to do and is continually on the run from this place to that. Whether he will accomplish very much remains to be seen. Just what his relationship to the new society and to the new institute will be I could not find out but apparently he will fill an important place. He was very kind in his criticism of others and in offering his own ideas. Altogether I liked him exceedingly and found him one of the most stimulating men I met in Paris.

Mademoiselle Pompilian.

For a number of years past Mademoiselle Pompilian has played an important rôle in Paris in the scientific study of nutrition. She either has money of her own or influential friends and was able to raise the money to build the respiration calorimeter formerly at the Boucicault Hospital. M. Alquier told me that it cost 150,000 francs. I do not know his authority for this statement and the estimate may be exaggerated, but it is sufficient to show that the cost was very large. This apparatus, which was described in the journal of the Société Scientifique d'Hygiène

Alimentaire, was inspected by myself and Mr. Carpenter some six years ago, both of us finding it impractical. The chief value of the apparatus seems to be the extensive employment of self-registering thermometers, chiefly those constructed on the principle of the expansion of two metals, the Dorian and Richardsrecording apparatus being especially prominent.

Mademoiselle Pompilian was extremely interested in the development of the new society for the study of nutrition in Paris and has evidently the ear of a number of the commission. She is formally installed in the Pasteur Institute where her apparatus is now stored, and Dr. Roux lays great emphasis upon her opinion. I had several opportunities to meet her, both at the dinners and also at the Pasteur Institute. I met Dr. Roux and Mademoiselle Pompilian one afternoon at the Pasteur Institute by appointment and had a long discussion with them. Inasmuch as Dr. Roux is able to secure a large amount of money, the different members of the commission are anxious to meet his views and Mademoiselle Pompilian has tried to impress upon him ~~his views and Mademoiselle Pompilian has tried to impress upon him~~ the features of her plan as she sees more of him than any of the others.

Mademoiselle Pompilian had very definite ideas and emphasized very strongly the importance of having long experiments so as to study the progress of metabolism. She maintained that human beings are not machines or physiological subjects, and that we should be studied in the open air under ordinary living conditions. She had a firm belief in the closed-circuit principle, but wished to have glass walls. Dr. Roux also emphasized the importance of having plenty of sunlight and recommended the building of a calcrimeter according to Mademoiselle Pompilian's plan, with glass walls and plenty of light and air, so that one could live as if outdoors. I told him this was absolutely incompatible with the accurate, scientific measurement of the heat output. Mademoiselle Pompilian's attitude was due, I think, to her desire to meet the conditions that Dr. Roux had in mind. It seems that Dr. Roux has for many years suffered from tuberculosis and he of course believes in fresh air and sunlight.

I told them that we had had experience with the calorimeter since 1895, that we had tried all forms of tests, and were unable to find any material difference between the results obtained with the respiration calorimeter and those secured with the respiration apparatus, in other words, that the metabolism in the open room did not appear to differ from that in the dark chamber. In discussing the metabolism of different individuals with Mademoiselle Pompilian, I told her that when the height, weight, and sex, were known and the subject was without food in the stomach, we could ordinarily predict the metabolism to within 15 per cent. This statement seemed to startle and discourage her and she asked what was the use of a calorimeter. It may be that her idea of accuracy in results was not much closer than 15 per cent. She remarked that she had frequently found great differences in individuals, which is quite contrary to our experience. Just how she found these differences I do not know. Certainly the respiration calorimeter formerly located in the Boucicault Hospital would not justify measurements of this kind. Personally I do not think that she knows what an accurate experiment is.

After several hours' discussion, I was unable to find that any of her ideas were worth following up in order to modify or better our present respiration calorimeter. Mademoiselle Pompilian thoroughly believed that her plan would be successful and had confidence that all of the apparatus that she had worked out at the Boucicault Hospital could be counted upon as being of proved value. She had not the slightest idea, however, as to the expense of the numerous modifications which she wished to incorporate in the apparatus or the time required for working out the numerous plans.

Mademoiselle Pompilian had not read Carnegie Institution Publication No. 123, had made no effort to obtain it, and apparently did not know of its existence. When I told her that it was in several libraries in Paris, she said that it takes three years to get a new book at the Bibliothéque Nationale, for it requires this time to catalogue it and put it on the shelves. Subsequently M. Alquier told me that she could not have tried to get the book, as he personally knew of the existence of a number of the books in Paris. In all probability, there was a copy in the Pasteur Institute as an assistant there was making some translations for the journal of the Société Scientifique d'Hygiène Alimentaire.

Mademoiselle Pompilian told me of an interesting experience with Rubner in Berlin, saying that he took her about the laboratory and finally came to a room marked "Respiration Calorimeter". Taking out a key, he opened the door with an air of great secrecy and impressiveness and allowed her to look in for a moment or two, saying that it was his own private room and no one else was allowed in it.

I found it difficult to discuss the plans freely with different members of the commission, owing to the fact that Mademoiselle Pompilian, the author of one of the plans, was almost invariably present, while M. Lefèvre was not. Under the circumstances I could not criticise both plans to advantage. I called this fact to Dr. Roux's attention, and he said that I should feel no hesitancy in criticising her plan very severely if necessary as I was rendering a service to French science.

Mademoiselle Pompilian is a very fluent talker, speaks excellent English, and is a very pleasant woman, but I consider her not at all practical. She told me her plans in a general way but she was so diffuse in her descriptions and they were so mixed up with other features of the building and with the general plans that I did not get a good idea of them.

Consequently I gained a much better knowledge of the general scheme of her plans from M. Alquier who had her proposition in writing.

Pasteur Institute.

Dr. Roux and Professor Bertrand.

Dr. Roux is the director of the Pasteur Institute, Professor Gabriel Bertrand is director of the chemical laboratory, and Dr. Metchnikoff is the director of the bacteriological department. Professor Bertrand had visited the Nutrition Laboratory a few months previous to my going to Paris. When I called upon Professor Bertrand in his laboratory, I found the alcohol program lying upon his desk. He immediately suggested that we should study the effect of the intermediary oxidation products, such as very dilute ethyl aldehyde and acetic acid, as he assumes that the path of the oxidation of the alcohol in the body should be the same as outside the body. This is really a toxicological problem but nevertheless interesting.

He also suggested another very interesting method of studying the effect of alcohol, i. e., by producing the alcohol <u>inside</u> the body, and in this connection, referred to the "grape cure". There is in certain parts of France a method in vogue of treating bacterial diseases by giving the patients large amounts of grapes, the patients consuming sometimes several kilograms of grapes in a day. The yeast cells on the outside of the grapes produce a new growth of bacteria inside the stomach, if not, indeed, the intestines, which kill the pathogenic bacteria. Professor Bertrand thought that in such cases the alcohol might not be so quickly absorbed as usual. Alcohol is usually absorbed rapidly in the stomach and it may never get so far as the intestines, but under the conditions of the "grape cure", the alcohol produced might have an interesting effect on the nerves of the intestines. Of course the simultaneous ingestion of large amounts of grape sugar would affect the study of the respiratory exchange, but results having a psychological value might be obtained.

Professor Bertrand showed me about his laboratory, which is wonderfully complete in its equipment. His large machinery hall is especially fine. In this room he has a machine for getting a high vacuum and drying large masses of organic material at room temperature which is a marvel of accuracy, but very cumbersome and has very large stopcocks, a complicated pump, etc. It had four large chambers, each with a heavy plate glass window.. Tight closure was secured by using a waxed thread. When I asked Professor Bertrand why he used such large stopcocks, he said that it was to reduce the friction of the gases as they were pumped out. This struck me as being very peculiar for with a vacuum I do not see why there should be such a friction of gas. Professor Bertrand estimated that today the installation of one of these machines would cost not far from 8,000 francs. I saw a small form of this machine in Professor Hamburger's laboratory in Groningen. While the apparatus is very cumbersome, it gives very accurate results. Professor Bertrand used it for diastase and says that he gets altogether different results by drying in this way from those obtained when drying by heat, as diastase loses much of its activity when dried by the latter method. For further comments see notes regarding my visit to Professor Kossel in Heidelberg, who dries after freezing in carbon dioxide.

My personal impressions of Professor Bertrand were fully substantiated by conversation with many chemists and physiologists in Europe. He is, I think, without doubt looked up to as the most eminent physiological chemist, if not regular chemist, in France. He seems to have a much better balanced judgment, a broader view, and is less volatile than most of the French scientists. On the whole, Professor Bertrand impresses one as an extremely accurate technician, a worker, and a man of excellent judgment. He is intensely interested in oxidases as evidenced by the fact that nearly all of the students in his laboratory are working on this subject. He is evidently a man who is perfectly capable of carrying on satisfactory research work in such an institution.

Physiological Laboratory of the Sorbonne.

Professor Dastre

I met Professor Dastre at the dinner given by Dr. Roux on Washington's Birthday at the Hôtel Avenue. He is a member of the Commission of the Société Scientifique d'Hygiène Alimentaire and is particularly interested in this society. After the dinner he took me directly to his laboratory at the Sorbonne, where I met Dr. V. Henri and many other workers, including particularly Dr. Bierry. The laboratory had the appearance of being very busy, with a large number of workers and a very good equipment.

Work was being done on ultra-violet rays, on hydrogen-ion concentration, on metabolism experiments of various kinds, and the usual physiological experimentation of the university laboratory. Dr. Henri is working particularly with the mercury vapor lamp but also uses the Ruhmkorff coil and with aluminium arcing points or cadmium arcing wires he lets the discharge pass between two very heavy wires, about No. 4 size, and therefore gets a much larger amount of ultra-violet light. He uses only quartz vessels and finds that the light completely decomposes the sugar solutions. If he uses a solution of 5 per cent strength, the time of the decomposition depends upon the amount of the solution and the intensity of the light. He finds that the products of decomposition of the ultra-violet light absorb the light itself, so that the solution must be continually stirred. He has many interesting devices for doing this. He claims that the light reduces the sugar to carbon dioxide and water and says that he can make an "elementary analysis of sugar at room temperature" by the light method. Obviously this is only of theoretical interest but nevertheless of great importance.

He is also working on the effect of light on bacteria, the lower organisms and mice, and has carried out an especially interesting series of experiments on mice during pregnancy. Rubner's law of growth interests them very much and they are trying the effect of ultra-violet light on growth, using the weight of the food and excrement and the weight of the animal as an index. Rubner says that the animal doubles its weight with certain food in a certain time and increases at a regular rate. I pointed out to Dastre and Henri that the mice in these experiments might have been stimulated by the light to a much greater muscular activity than those without this stimulation; consequently to make the experiment complete, there should also be a record of the muscular activity, particularly as no records of the carbon-dioxide production per day are secured. I explained to Dastre the principle of our suspended cage whereby our records of the muscular activity are obtained and he was much interested in it.

In discussing the alcohol program, Dastre spoke of the effect of alcohol on growth, evidently having in mind the ultra-violet light experiments. I told him that I hoped he would make an experiment with alcohol, but if he is to use the body-weight alone as an index, he must make sure that the muscular activity is constant or comparable.

A number of researches were in progress in Dastre's laboratory in regard to the sugar content of the blood and the sugar in diabetes.

They find two sugars in the blood, so called "combined" sugar and "free" sugar. With man they find approximately an even quantity of each, but with other animals it varies. The amount of sugar in the blood is less with those species of animals having a lower body-temperature, consequently with birds, which have a high body-temperature, the amount of sugar is very great. The method used for determining the sugar is to precipitate the albumin by mercuric nitrate and determine the "free" sugar in the filtrate; the precipitate is hydrolyzed in an autoclave 45 minutes at 120° C and the hydrolyzed sugar is then determined. As in many European laboratories, the Bertrand method is used. They heat the solution exactly three minutes after boiling begins, thus establishing a constancy in the amount of time necessitated by the reduction. As they always adhere very strictly to this rule, they are very much pleased with the method. The one great difficulty in carrying out the determination of the two kinds of sugars with diabetics is that about 25 c. c. of blood are necessary. Since the influence of "combined" and "free" sugars in the blood has not been satisfactorily studied in diabetes, I was very much interested in their work.

Dr. Bierry very generously inscribed a copy of his book to me and gave me several reprints for our laboratory library. He was much interested in the differences we find in our experiments on the ingestion of levulose and glucose, particularly in those found with a normal person. He is evidently a man whom we should keep in touch with our diabetic work as he is a skilful and careful worker.

Dr. Henri also impressed me as being a careful and enthusiastic worker, and I heard good reports of him in the different European laboratories. Dastre has all the enthusiasm and some of the superficiality characteristic of French savants. He was very enthusiastic about Lefèvre and his book. I learned subsequently that Dastre practically made it a prerequisite for certain of his courses that students should have Lefevre's book. This seemed to me specializing to a considerable extent on the question of bio-energetics in a course.

Marey Institute

Professor Richet and Dr. G. Weiss.

The Marey Institute at Boulogne-sur Seine well merits a visit, as I found on my earlier visit six years ago. Professor Kronecker, although much interested in the welfare of the Institute, is not now actively connected with it. I met the former director, Dr. G. Weiss, at the Institute and later dined with him at his house. Dr. Weiss is a mathematical, physical physiologist and thinks only in mathematical terms. His duties as a member of the faculty of medicine and his many offices, examinations, and administrative cares leave him very little time to attend to the work of the Marey Institute. Professor Richet is now the director, and Dr. Weiss the sub-director. Professor Richet was unfortunately ill all of the time that I was in Paris and although I had several notes from him, I did not see him personally.

At the time of my visit there were relatively few researches in active progress. The problems of muscular work were naturally uppermost in my mind and I was accordingly much interested in the work done with a kinematograph and in the photographs of muscular work. Dr. Weiss maintains that the law of the conservation of energy may not hold true and cited the Nernst theory. He is a mathematician, full of formulas, and I find that most of the men consider that he is inclined to interpret physiological facts too mathematically. Weiss cited Helmholz who speaks of 100 calories of potential energy or heat of combustion as a certain amount of energy "libre" and a certain amount of energy "gebundene". Only the first can be in part converted into external muscular work. For instance, if the energy were 100 calories, and the external work were 20 calories, the efficiency is not 20 to 100, but 20 to 80, which easily represents the

Fig. 1. Professor G. Weiss in the library of the Marey Institute.

energy that can be converted into work. Dr. Weiss was much interested in hearing of the Cathcart book and said that he wishes to continue the study of muscular work. He showed me the bicycle ergometer (fig.2) which Amar used in his work and told me the history of Amar's research. It seems that Amar had some money granted him for work in Algeria and came to Weiss for advice as to what line of research to take up, who suggested that he study muscular work. Dr. Weiss did not have much confidence in Amar's work, however, especially in his gas analyses, which were made by a micrometric method with a capillary tube.

At the time of my visit Dr. Weiss was interested in the machine in which the legs could work separately so that he was able to study the metabolism with each leg working independently. I obtained a photograph of this machine (see fig.3.) He drew curves showing the height to which the weight was lifted by the foot and the rapidity and the tremor of the leg, if any, when it was being lifted, etc. This was not unlike the work I found being carried on at the University of Helsingfors by Dr. Carl Tigerstedt. I pointed out to Dr. Weiss that there was a certain amount of negative work and that the weight of the leg which was lifted each time was unknown and told him that I preferred a method in which the legs were balanced, as on the pedals of a bicycle wheel. In this way, one leg practically balances the other, although as Berg, Dubois-Reymond and L. Zuntz have shown, the balance is not exact.

This suggested to me the possibility of a research in which all of the work would be done first with one leg, then with the other, and finally with both, doubling the load, etc. A considerable amount of work could be done on this. Such work might require an increase in the weight of the copper disc so that it could be carried past the dead point when working with one leg.

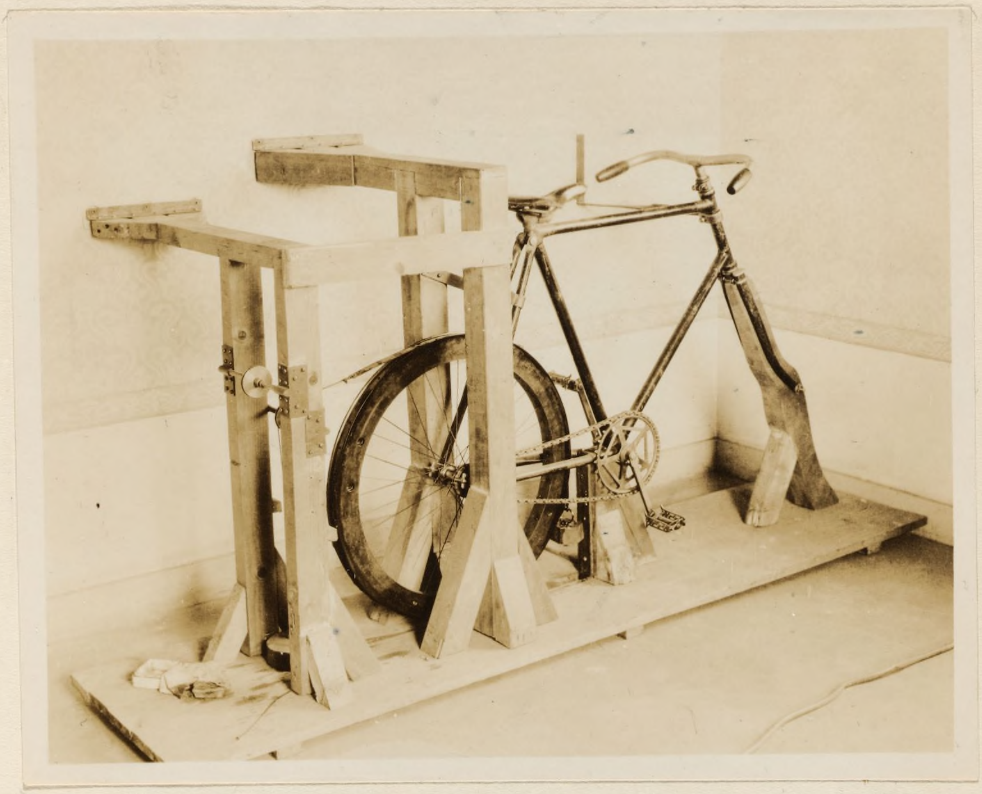


Fig. 2. Original Amar bicycle ergometer in the Marey Institute.

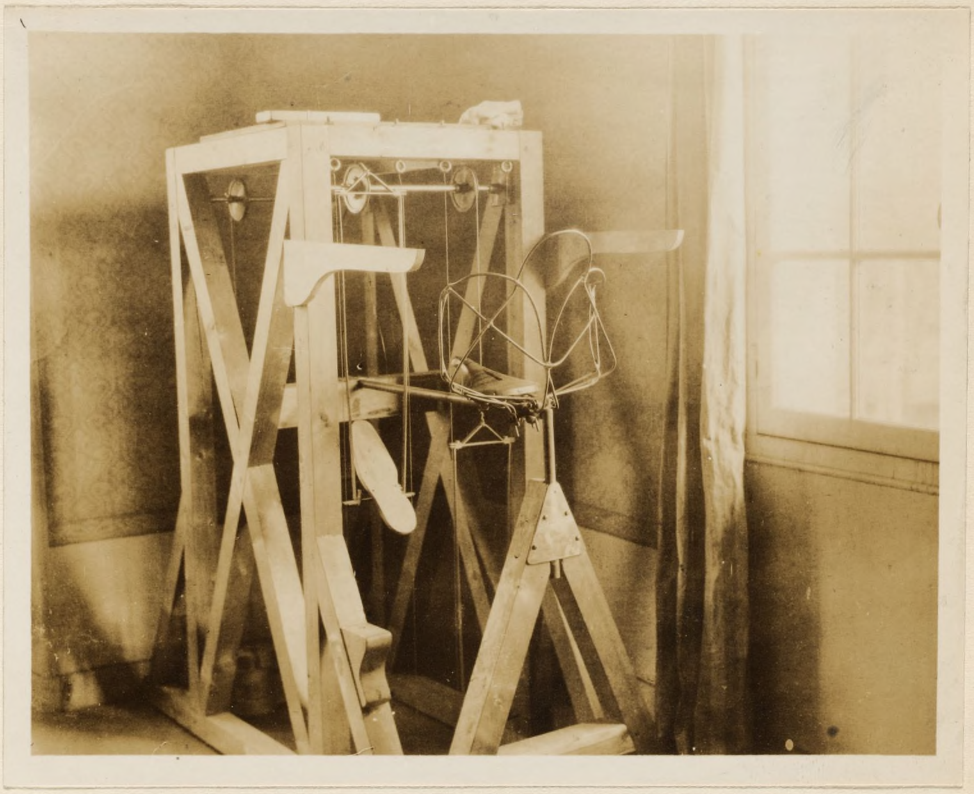


Fig. 3. Pedal apparatus used by Weiss at the Marey Institute for his experiments on muscular work.

Work is done first with one leg and then with the other, and each component is analysed.

In his study of the muscular work of the leg, Weiss used two 100-liter Tissot spirometers for the measurement of the respiratory gases, but without the writing attachment. (See fig. 4.) He also used Chauveau valves, drawing samples from the spirometer and analyzing directly on a Laulanié apparatus, reading to 0.05 c. c. (See fig. 5) He brought up the interesting point that if phosphorus is used in place of pyrogallic acid, you can get excellent results but it is not so important that the phosphorus be kept from the light as that the air over the phosphorus be freed beforehand from carbon dioxide. If the air containing the carbon dioxide is sent over the phosphorus first, for some unaccountable reason the phosphorus quickly loses its absorbing power and it is necessary to remove and remelt it before using it again. They will go no farther with this question in the Marey Institute.

In discussing the question of the conservation of energy, Weiss brought up the point that radium gives out energy continually.

Another interesting research just begun by Weiss was with frogs, in which the effect of a hydrogen atmosphere and the respiratory exchange in work were being studied. Dr. Weiss found that if the frogs were left in hydrogen for one and a half hours, then in ordinary air for one-half hour, and again in hydrogen for one and one-half hours, and this routine was continued for 10 hours, the frogs would die, the microscope showing a distinct alteration in the nerve cells. The experiments on the muscular work of frogs were carried out by means of a special chamber immersed in water, with a contrivance for having the frog do work by lifting a weight with the leg. The cord connecting the frog with the weight passed through a tube containing vaseline to prevent air from leaking out and water from leaking in. Dr. Weiss assumed that the work of pulling the cord through

Fig. 4. Two Tissot spirometers used by Weiss in his investigations at the Marey Institute on muscular work.

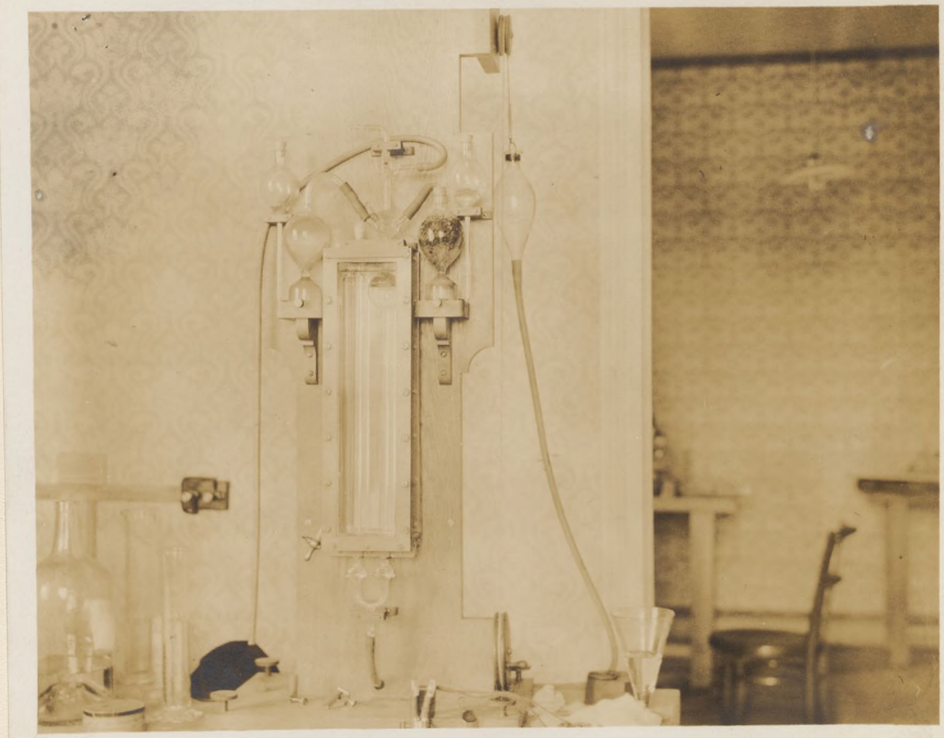


Fig. 5. Laulanié gas analysis apparatus used by Weiss in the Marey Institute.

Part of this is employed for absorbing oxygen.

the vaseline was not too great to affect the results. The respiratory gases were very carefully analyzed by the complicated apparatus of Regnault. The research on muscular work was still in process of development.

I noticed in this laboratory a Blix-Sandstrom kymograph, electrically driven, which ran with considerable noise. Dr. Weiss told me that when it first came it was noiseless, but their mechanician took it apart to repair it after a slight accident and never got it back into a noiseless condition. Subsequently I noticed in Phillipson's laboratory in Brussels that he also had a Blix-Sandstrom kymograph and that this, too, ran with noise.

Monsieur Lucien Bull.

The most active worker in the Marey Institute is M. Bull, who has been there for at least 18 years. As an expert technician, designer, and constructor, he probably has no equal. For example, he was at the time of my visit building a very large string galvanometer, (fig. 6). Since it appeared to be similar to the one I saw in the laboratory at Columbia University, New York City, which was being constructed by Dr. H. B. Williams, it is quite possible that Dr. Williams may have obtained his design from the Bull apparatus. Dr. Bull uses glass strings in preference to quartz, making and silvering them himself.

I examined very carefully his ingenious tuning fork plan for synchronizing a motor to open and close a slit of the photographic apparatus, also for rotating an aluminium disc in front of a lantern to cut off the light, and finally ordered the apparatus for our laboratory. A small motor is actuated by the tuning fork.

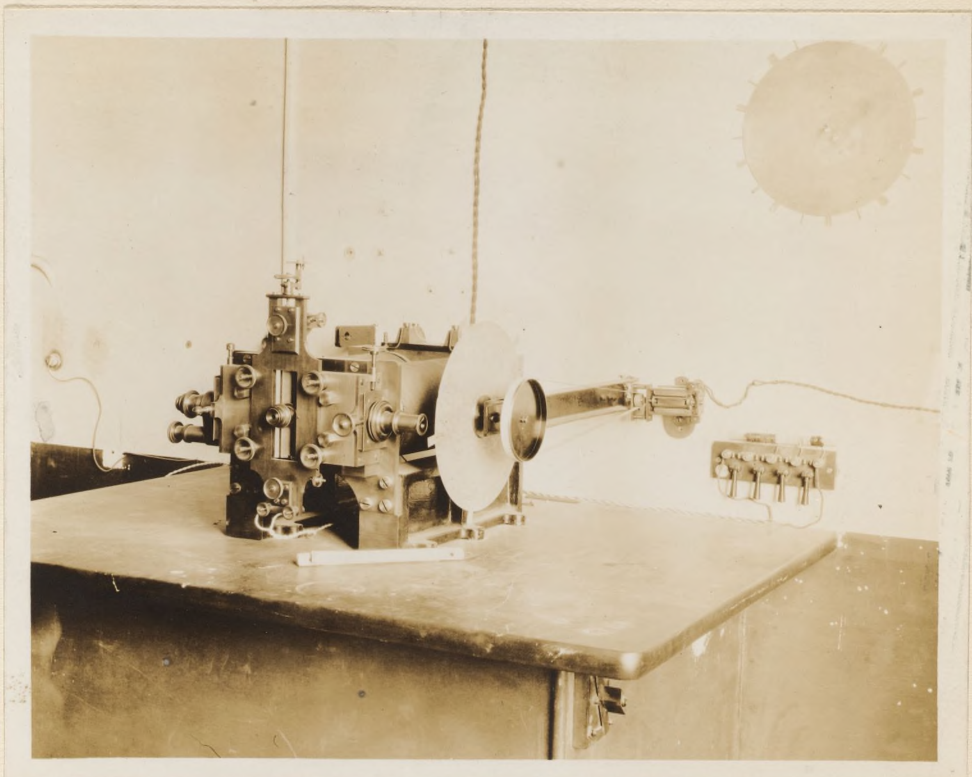


Fig. 6. String galvanometer constructed by M. Bull at the Marey Institute.

Bull's rotary time marker is shown at the right.

I discussed with Dr. Bull the possibility of his coming to Boston to inspect our calorimeter and make a report to the Société Scientifique d'Hygiène Alimentaire concerning it. He said that Richet had also suggested that the Society should send him to Boston to study our calorimeter and had even sent Dr. Bull with a note to Gautier to discuss the matter about four months previous to my visit. Dr. Bull said he did not think that Professor Gautier favored the plan. I told him that I would be very glad to recommend his being sent if I were sure that he himself would be willing to accept the commission. He finally told me that he would go if the Society wished him to. He thought that my plan of having him study the apparatus so that, while not on the staff, he would be at hand in an emergency, was a very good idea. The Society subsequently sent Dr. Bull to Boston after I had made the recommendation, but unfortunately his time was limited to three months instead of the five or six months which I had recommended.

Dr. Bull had a wonderful series of cinematograph films, showing a man walking, running, and jumping, birds flying, a bullet being shot through a soap bubble, and similar objects. I have ordered a set of these films for the laboratory, also a protection apparatus for them. Bull's tuning fork was obtained through Tainturier who makes it regularly. It is now put out as a regular part of the equipment of the Cambridge Scientific Instrument Company's string galvanometer.

Personal impressions of the Marey Institute.

Aside from the really excellent work being done by M. Bull, it struck me that the Marey Institute is a laboratory with an equipment not suitably used. One or two assistants were working in the laboratory but there did not seem to be a general air of activity, and it gave me the impression of being a large private laboratory for the especial use of M. Bull. That M. Bull is making good use of it is unquestionable but it would seem as though a more extensive use could be made of the equipment. I understand that several nations contribute towards its maintenance and that it is possible to send students or assistants to occupy certain tables, but there does not seem to be the general use made of it that is made of such laboratories as the Naples Experiment Station and the Woods Hole Station. This is all the more to be regretted since there is a great opportunity for doing good work, as along the line of muscular movement, photography, etc., there is no place quite equal to it in the world. It has nearly every type of apparatus for time registration and for photographing that can be found anywhere. There is also a very good library which is apparently not used to any great extent. It is possible that the location of the Institute at some distance from Paris may prevent its more general use but it would seem with such an equipment that it ought to attract a larger number.

Museum de l'Histoire Naturelle.

Dr. J. Tissot.

Some time before going to Paris, I wrote to Dr. Tissot but heard nothing from him and was not able to find him for some time. When I finally met him, he was very courteous and expressed great regret that he had not been able to connect with me by mail. At present he is in two laboratories, the Museum de l'Histoire Naturelle, where I saw him, and in another laboratory founded by a wealthy man for tuberculosis research. Dr. Tissot is the director of this latter laboratory and does but little work in the other.

The only new apparatus that I found in the laboratory was a large radiation calorimeter. This is located in a cold, dark room, the windows being covered with matting. The calorimeter is a wheel drum about 6 feet in diameter, 2 feet wide at the periphery, and a little wider at the hub. It is rotated by a motor. There is a belt running from the motor to the axis and the whole wheel rotates 100 times a minute. The door is a triangular opening closed with gasket and clamps, as shown in some of the photographs in the report of my trip in 1907. The animal, usually a dog, is suspended in a cage which hangs on the axis and does not turn when the calorimeter turns, but the cage can be adjusted so that the dog has to run and turn the calorimeter.

When the door of the calorimeter is open, the ventilation is sufficiently good as the composition of the air remains constant; in experiments when the respiratory exchange is being determined, the door must of course be closed. Under these conditions, the animal can be allowed in rest experiments to remain in the apparatus for 6 hours; in work experiments, the dog can be kept in the apparatus 2 hours.

A thermal junction of ferro-argentan inside the chamber is connected by an ingenious mercury device with a second thermal junction outside at the same level and about 5 feet away. Dr. Tissot is somewhat troubled by dirt collecting on the surface of the mercury and by the oxidation of the mercury as we have been in this laboratory. I suggested that he coat the mercury with a few drops of glycerine. The thermal junctions are connected with a d'Arsonval galvanometer in another room, the deflections being photographically registered. To rotate the sensitized paper, Dr. Tissot uses a Richards Frères thermometer as a drum. The paper is fastened to this drum and has to be rotated very slowly. It is a very crude, home-made, wooden box. There is a copper can placed over the paper with a longitudinal slit in it so that the light passes through the intersection of two slits, i. e., at a point. Every hour the circuit is interrupted for a moment and a photograph of the base line obtained. The curve for each 24 hours is about 500 mm. long. It is a very pretty curve, but one questions whether it is exact. It seemed to me that the principle of registering temperatures in this way was very good and worth looking into but I am not so confident of the accuracy of the radiation calorimeter as is Tissot. He considers it to be the best apparatus of the kind in the world, but admitted that it is necessary to rotate the calorimeter chamber very rapidly so as to equalize the temperature as otherwise his results were not good. He has published a note regarding the principle of the calorimeter and the photo-registration apparatus, but as yet has published no details.

The laboratory at the Museum de l'Histoire Naturelle looked forlorn and deserted. Although now 87 years old, Chauveau continues to be actively interested in the work and does some work himself occasionally, but comes to the laboratory but rarely. There is not the best of feeling between Chauveau and Tissot as the latter thinks that he has been kept down by Chauveau a great many years. On the contrary, M. Alquier says that Tissot is an acknowledged pessimist and is disagreeable to every one in Paris. He works all alone, never sees any one, and no one wants to see him.

Tissot impressed me as being a typical French scientist who is satisfied with a superficial working out of a general idea and has no conception of mathematically exact experimentation. From all appearances, I should judge that his activity in scientific work, particularly in the study of calorimetry and metabolism, had absolutely ceased. I think his interest in the new institute and his avowed lack of interest in the Chauveau laboratory prevents his doing any work of value in the latter laboratory. He is also disappointed that he was not given a professorship in the Museum for which he had recently competed and which had been given to a competitor.

BRUSSELS, BELGIUM.

Solvay Institute

Dr. Slosse and Professor Héger.

Although I knew that Professor Héger had been made honorary professor of the University and had retired from the directorship of the Solvay Institute, I was very glad to visit Brussels again as Professor Héger is a stimulating individual and presents still another aspect of French work. Since the death of his wife a year ago, he has devoted considerable time to scientific research in the Solvay Institute along lines not closely connected with our investigations. His son, who has gone into medical legal work, has been associated with him.

Professor Héger was especially interested in the alcohol program and had evidently not only given it considerable personal thought but had called it to the attention of Professor Slosse, Director of the Solvay Institute. He had also shown it to Dr. van Laer of the Institute. One afternoon, Professors Héger and Slosse spent several hours with me in discussing the plan. Certain of their criticisms were very well raised but I think they considered the ethical side of the question rather than the scientific side. I find these two points of view frequently confused by scientists in considering the program. It is quite evident that the program has been so drawn that no one at first sight will say that it is anti-alcohol and therefore dismiss the matter as a preconceived conclusion.

Professor Héger also wrote me a personal letter, but of an official nature, regarding the program. He fears that the first statement in it, to the effect that 75 grams of alcohol per day is completely oxidized in the body, will be used in a wrong way by advocates of the use of alcohol and

Fig. 7. Professor Paul Héger in his library at the Solvay Institute, Brussels.

cautioned me very strongly about it. There was a marked difference between the attitude of ProfessorsHéger and Slosse and that of Professor Kassowitz whom I saw later in Vienna. Professor Kassowitz was a rabid crank with no conception of a scientific judgment on questions of alcohol; Professors Héger and Slosse immediately recognized the scientific aspects of the question but feared that the statements would be misused by those in favor of alcohol, emphasizing the fact that in Belgium the alcohol problem is particularly difficult to handle.

While Professor Héger is not actively engaged in metabolism investigations along lines that interest us particularly, he has a very broad view of science as is evidenced by his interest in the alcohol program and by his friendly efforts to arrange discussions and meetings between myself and some of the eminent Belgium scientists. He had previously had a very sharp discussion with Dr. Slosse as to the composition of the feces, Dr. Slosse taking the ground that the feces were in large part undigested food while of course Héger maintained that the undigested food formed only a small part of the feces. The question of the appearance of cherry pits, bits of undigested husks, etc., seemed to play an important rôle in Dr. Slosse's argument. Subsequently I went to a lecture by Dr. Slosse at the Solvay Institute. I think I have never heard a lecture in which the subject was so well presented; the illustrative tabular matter placed on the blackboard was admirable. In this lecture his admission as to the true character of the feces was much more scienfitic.

At the time of my visit, Dr. Slosse was working on van Slyke's method for amino-acids but complained, as did Dr. Edgard Zunz of the University of Brussels, that van Slyke changed his method so often that it was difficult to get any standardization. Dr. Slosse told me that he was greatly hampered in his work at the University by the lack of assistants and was thus obliged to do much of the routine work himself, such as making up standard solutions. Apparently, the question of providing assistants for the professors is given but little attention at the University of Belgium.

Dr. Slosse is very much interested in nutrition matters and had made a metabolism experiment on a Belgian with Dr. Waxweiler, stimulated, he said, by the work of Professor Atwater. I was greatly impressed by Dr. Slosse's ability; he has a most critical mind and is very keen. I asked if he would accept an invitation to become a Research Associate in the Nutrition Laboratory but unfortunately his university ties would not permit him.

Dr. van Laer.

Dr. van Laer, who is the pure food crank of Belgium, and is interested in oxidases, is wholly subservient to the ideas of Dr. H. W. Wiley and considers him one of the greatest scientists in America. Dr. van Laer was kind enough to criticise the alcohol program carefully; a copy of the program bearing his comments is filed with the criticisms of other investigators.

Dr. Phillipson.

Dr. Phillipson is a young man of talent who has devoted much time to science. He has done some teaching in the university and has also carried out considerable research in the Solvay Institute. His research as a whole appears to me more like that of a dilettante rather than the work of a deep scientific thinker; nevertheless it is very creditable for the most part.

Either through private means or through the friends of the Solvay Institute, he has been able to purchase considerable expensive apparatus. At the present time he is working with a string galvanometer manufactured by Edelmann in Munich, and has made what he considers to be quite a discovery. He finds that when a person has his hands in the saline electrodes and is thinking of something disagreeable or there is a slight movement after a shock, such as would be caused by a noise, there is a wide movement of the string off the field and a slow return, i. e., a kind of psycho-galvanic reflex. He had used this for studying the work in connection with multiplication, the noise of a revolver shot, or the breaking of electric light bulbs, and various other stimuli. He found it very difficult to explain but I believed it to be due to muscular action and Professor Héger had somewhat the same view. I thought this reaction might be interesting in connection with our alcohol research. Several experiments were made upon me personally but when the pistol was fired in the room, there was no reaction and no evidence of change upon the plate, which disturbed Dr. Phillipson very much. He told me of an experiment made with their diener in which no stimulation was apparent when they cursed and scolded him very severely but if a stranger walked along the corridor, he instantly became aroused and produced the reflex, this being due to the stimulation of the instinct developed by training to look out for loiterers and possible thieving.

In examining the apparatus Dr. Phillipson was using, I found he had a very interesting resistance for compensating the body current. This has recently been devised by Samolikoff and does not appear in Edelmann's catalogue. For rotating the Frank photographic kymograph, he used a Blix-Sandstrom kymograph as a motor, which also ran with noise like the one seen in Dr. Weiss' laboratory. Dr. Phillipson uses paper in his apparatus, for the most part, and also films. When paper can be used, it is much cheaper and gives sufficiently good results. This kymograph took the standard Belgium film.

While in this laboratory I had a good opportunity to observe the serious effect of smoking during scientific observations. The same mornment that the experiments were made upon me, an experiment was made with a woman; out of the five tests, I think three or four were lost owing to the use of tobacco. For example, one assistant's sole duty was to concentrate the electric arc light upon the string of the string galvanometer. The lamp was not very good and the arc moved considerably so that hand regulation was necessary. Just at the moment when the signal was given from the testing room for one experiment to be made, the man in charge of the arc light was shaking his head to clear the smoke from his eyes and simultaneously the arc light moved out of the field of the string and the experiment was lost. In another instance Phillipson himself was picking up his cigar from the desk and had it half way to his mouth when the signal came from the operating room. He should have thrown the switch immediately to set the apparatus in motion but took the time to carry the cigar to his mouth and his hand down to the key again. During the time thus lost, the reaction took place and when the film was developed, there was nothing on it. Another assistant who had a duty in connection with these tests failed to respond promptly because at the moment he was lighting a cigarette.

In discussing this matter subsequently with Professor Héger, he asked me if I would tell Dr. Phillipson himself about it, so when I met him the next day, I ran the risk of making myself disagreeable by calling his attention to my observations. On the following day I dined with him at his country house and when we went into his study after dinner, he attempted to show me about some experiments he was making. He had drawn a somewhat elaborate diagram to illustrate them, and before the ink was dry, a large mass of cigar ashes fell upon the paper at just the psychological moment and spoiled the whole thing. He instantly saw the point, laughed, and drew the diagram over again.

BONN, GERMANY

Tier-physiologisches Institut

Professor Hagemann

After making arrangements with Professor Hagemann by telephone, I went to Bonn and spent the afternoon and evening with him. His laboratory looks essentially as it did years ago and the more I inspect it, the more it appeared that he was in a hopeless tangle, even worse than was the case a few years ago. He said he was very short of assistants if, indeed, he had any. There seems to be no scientific interest in the laboratory. He is as puttering as ever and does not appear to have the faintest conception of scientific accuracy. Since my visit there three years ago, he had published a large book describing his calorimeter and giving the results of some experiments with a horse. Although with his apparatus no oxygen determinations are made, he finds it necessary to make corrections of 4 to 6 per cent on his carbon-dioxide determinations alone, to say nothing of his heat determinations. His average correction of 4 per cent was determined by blanks running from 2 to 6 per cent. He has not the faintest idea of how to make an accurate alcohol check experiment and apparently all of his work is useless. He spent nearly an hour showing me how by means of an electric clamp he had fused one platinum wire to another for use in glowing marsh gas in the Sondên-Pettersson gas analysis apparatus. The mechanician Kauermann is still there and is evidently the brains of the laboratory. To a certain extent, the entire time spent for the visit was wasted as I secured nothing of value in going there. Unless a very great change is made in the character of the work, it will not be worth while to visit Bonn again on these foreign trips.

DÜSSELDORF, GERMANY

Klinik für Kinderheilkunde.

Drs. Schlossmann and Murschhauser.

Being familiar with the literature which has been published from the Klinik f. Kinderheilkunde in Düsseldorf, I was particularly anxious to visit it and see their Zuntz-Oppenheimer respiration apparatus. An appointment was made with Professor Schlossmann, who kindly arranged to have an experiment with an infant in progress during my stay, although for a few weeks previous to that time they had been experimenting with dogs. The hospital is some distance from the centre of the city, and beautifully situated, with large buildings and extensive grounds. From the standpoint of metabolism investigations, this clinic is better organized, better equipped, and has greater potentialities than any other place that I saw in the whole of my European tour. The system can be described by no other word than "wonderful".

Believing that the success of an infant clinic depends upon the procuring of good milk, Professor Schlossmann has organized an elaborate system for securing both the milk of wet nurses and cow's milk. A number of wet nurses with their children are kept at the hospital all of the time, a beautiful ward being provided for the children. The nurses are completely under Professor Schlossmann's control and his orders are adhered to in the slightest detail. I went into the children's ward and saw the nurses sitting around just prior to "milking time". They were carefully stroking the breasts from the outside towards the nipple to make the milk flow more regularly. Each wet nurse had a graduate held between the knees and from time to time after stroking the breast, took hold of the breast and forced the milk by a regular milking process into the graduate. The nurses are expected to supply 2 liters of milk each day, or sufficient for three children, i. e., two children besides their own. If they are unable to do this, they are not considered worth their room and board and are discharged. Dr. Schlossmann finds, as do all others, that the nurses sooner or later become troublesome. One of them will become discontented and want to leave; this disaffection soon spreads and they all become restless so that they have to be discharged.

The milk which the nurses supply is used to feed sick infants; all of the surplus milk is placed in cold storage. The milk thus stored freezes in about 36 hours and is kept until the supply of wet nurses is insufficient for the needs of their patients. Dr. Schlossmann says that the therapeutic properties of woman's milk remain unchanged for a year, but the milk loses its food value; I do not know just what he means by this loss of food value. He showed me in a cold closet a sufficient supply of milk on hand to last several months.

The clinic formerly ran their own farm but this is now managed by a business man under their direction. Great care is taken in every detail of handling the milk, cleaning the bottles, bottling and sterilizing the milk, etc.; the milk supplied to the clinic is almost sterile. By his system, Professor Schlossmann is able to obtain better woman's milk and cow's milk than almost any other clinic in Germany.

As an illustration of the effects of feeding good milk, Professor Schlossmann has had made a number of cinematograph films showing the whole system of collecting and using the milk. These films were first taken for the Dresden Hygienic Exhibition but have since been used generally throughout Germany as educational films. He very kindly ran off a number of these for me, showing the care used in handling the milk as illustrated by pictures of the cows, the stables, washing the cows, milking, etc. A particularly interesting film from an educational point of view was the one illustrating the story of a mother who had fed her infant store or milk-cart milk and who had been obliged to take it to the hospital for treatment. This showed the arrival of the mother at the hospital with her baby in her arms, the meeting with the porter, her visits to the different wards, her meeting with the Director, the reading of a letter from the doctor to the Director, her anxiety over the health of her child and happiness when shown the good condition of the infants in the hospital, with of course the grand finale when she comes to get the child and finds it perfectly well. It is a most impressive exhibition.

The respiration apparatus is in a special room in the basement, which is very well lighted and well equipped. It is exactly like one which I saw in Berlin and while very complicated is very good. It has one great advantage in that the respiration chamber and pump are in one tank. The chamber is removed from the water by a crane and set of pulleys. With this apparatus the carbon-dioxide production and oxygen consumption cannot be determined in short periods and the periods must be at least 2 to 3 hours long. For the determination of the oxygen a large gasometer is used and the water is weighed exactly as Oppenheimer first used it. One point which interested me was their statement that they could not always rely upon the factory analysis of the oxygen: they said that frequently they had received oxygen marked as 98 to 99 per cent but had found it as low as 92 per cent. The carbon dioxide absorbed by the caustic potash is determined by the Fresenius method of weighing. This takes several hours.

The pump is washed out and the volume of liquid made up to 400 c.c.; the carbon dioxide is then determined with a fraction of this.

When I was there, an infant was inside the apparatus and asleep. It was absolutely quiet for the greater part of the time that I was there, so that I was able to take a photograph of the apparatus with the infant inside which required a 5-minute exposure as it was taken through water. (See fig. 8.) An interesting feature of the experiment was the military precision and system shown throughout. There was a prodigality of assistance. Four men stood by like a corporal's guard to put the baby in or take him out of the apparatus. There were no records of the pulse-rate and no graphic records of the muscular activity, but one man was detailed to watch the infant continually and make notes of the activity every moment. One great criticism that I have to make of this system is that they are not able to sharply divide periods of complete repose. In a 5-hour experiment of which three and a half hours are complete repose and one and a half hours great excitement, they must attempt to compute the metabolism during the period of excitement and deduct it from the metabolism during the whole experiment to secure the correct value for the metabolism.

Most of their infants are trained to sleep in light rooms and not be affected by the noise in the ward. Professor Schlossmann thinks that he finds the lowest values for the metabolism after a 24-hour fast and maintains that even very young infants can stand a 24-hour fast very well. The experiments are frequently made in the evening. For instance, the infant gets no food after 6 p. m. one evening, is kept awake all night and the next day, without food, and then is put into the chamber for 4 or 5 hours at 6 p. m., or 24 hours after the last meal. If they have good fortune, he sleeps most of the time.

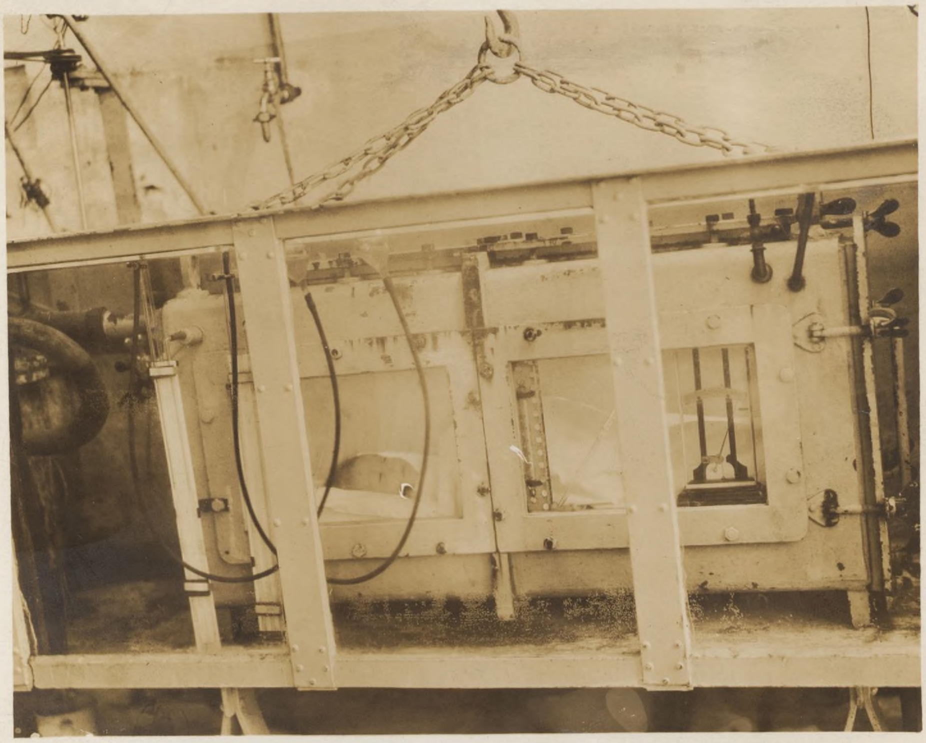


Fig. 8. The respiration chamber of Schlossmann and Murschhauser in Düsseldorf.

This shows the chamber completely immersed in water, with the sleeping infant. The photograph was taken through water and required a five-minute exposure, during which time the infant remained perfectly quiet. By means of a chain and hook shown in the upper part of the picture, the chamber can be instantly raised out of water and the door at the extreme right opened to remove the infant.

Professor Schlossmann impressed me as being a man of intense energy and remarkable executive ability. I consider him one of the greatest organizers that I have ever met in either business or professional lines. His wonderful system, the whole control of his clinic, and the interrelation of every individual part simply baffle description. He did not impress me, however, as the abstract scientist that I had expected to meet. He was quite "hail fellow, well met" and seemed to indulge in a great deal of "jollying" among the assistants and in the laboratory. He is what may be termed a "hustler", has complete command of the whole situation, can gather around him good, accurate, and painstaking workers, and knows how to make others work remarkably well. But those who know him, even his friends, say that he is very superficial and relies upon good assistants to keep up the standards. He was very kind to me both during my visit and in his subsequent correspondence with me. I was interested to find that he had personally carried out a research a number of years ago on the fuel value of milk in which he found 3.8 calories for the carbohydrate of milk instead of 4.1 calories as commonly used. Since that time he has used the value 3.8 calories although he said that almost every one in Germany still uses the old value (4.1 calories). Since my return to America I have had some correspondence with Professor Schlossmann, particularly in regard to a paper in which he criticises the priority with regard to the records of muscular movement inside the chamber. He wrote me a long, very friendly letter in which he said that he wished to cooperate in scientific investigations on a plan which should be devised by us jointly. He has also presented to the Laboratory a complete set of the large number of papers which he has published on infant metabolism.

In Dr. Murschhauser Professor Schlossmann has an admirable and very clever associate. Dr. Murschhauser formerly studied in Münich and has an intense accuracy in quantitative work. He is evidently a very keen man. As an example of his keenness, I might cite that Dr. Murschhauser called my attention to the error on page 181 of the paper describing the universal respiration apparatus in the Deutsches Archiv f. klin. Medizin, where it is stated that the balance weighs to 0.1 gram when we really meant to say 0.01 gram.

Professor Schlossmann's sagacity is shown by his arranging to have Dr. Murschhauser devote all of his attention to respiration experiments instead of calling upon him for routine or clinical work. As a matter of fact, I believe that Dr. Murschhauser is not a trained physician but is a skilled chemist. With Dr. Murschhauser, Professor Schlossmann is remarkably well equipped to carry out investigations on metabolism with a respiration apparatus. One has a feeling that without Dr. Murschhauser, the respiration experiments could not be made, for Professor Schlossmann does not give one the the impression of being an experimenter himself. I predict, however, that as long as he remains in Düsseldorf and has Dr. Murschhauser as an associate he will have one of the most prolific clinics for good, high grade metabolism experiments and research with infants that exists in Europe. It seems to me far better than either of the other children's clinics that I saw.

HEIDELBERG, GERMANY

In no other small city in Germany are there gathered together so many men who are so active in the particular lines of work closely associated with the Nutrition Laboratory as in Heidelberg. Professor von Krehl, Professor Cohnheim, Dr. Siebeck, Dr. Grafe, and Professor Kossel make a collection of men that it is difficult to duplicate in any large or small city. There is apparently no intimate cooperation between the different laboratories and particularly between Professor Cohnheim and the other laboratories. von Krehl, Siebeck, and Grafe work together in von Krehl's institute. Kossel and Cohnheim do not work together as they are men of quite diverse interests although I believe Cohnheim does do a little in connection with von Krehl's clinic.

Krebs Institute

Professor Czerny, Dr. von Dungern, and Dr. Werner.

At the request of Dr. John Collins Warren of the Huntington Cancer Research Hospital in Boston, I attempted to secure information regarding the work of the Krebs Institute which is under the direction of Professor Czerny. At a dinner at Professor Cohnheim's I met Dr. von Dungern who has charge of one of the divisions of the Institute. He told me that Dr. Werner, who has charge of the other division, was getting particularly good results in cancer by the injection of cholin. I understand that the beneficial use of cholin was a more or less accidental discovery of Werner's as a result of the fact that the X-ray acted upon the testes and the secretions, frequently making the men sterile so that they had to wear lead aprons to protect themselves. Werner reasoned from this that cholin, one of the active constituents of the testes, would prove beneficial in cancer. Dr. von Dungern also told me that with superficial cases they were getting good results by the injection of radium salts and mesathorium. They had also used selenium but not with very good success. He personally thought that the greatest prospect of success was in the use of cholin.

Dr. von Dungern has resigned his position in the Krebs Institute and is going to Hamburg as he has just accepted a call there to establish a cancer division of a new hospital. Other people in Heidelberg told me that Professor Czerny is soon to retire on account of old age and there was grave doubt as to where the money would be obtained to continue the Institute. At present it was very much pressed for funds and it was feared that when Professor Czerny retires, the work would be given up. It is quite possible that this financial uncertainty may have hastened Professor von Dungern's leaving.

University of Heidelberg (Physiological Laboratory.)

Professor Kossel.

Mrs. Kossel died very suddenly two weeks before we reached Heidelberg and Professor Kossel's condition was pitiable. Nevertheless I frequently had opportunity to talk with him and to speak of scientific subjects as he said that it took his mind from his troubles.

Professor Kossel was much interested in the alcohol program but said that he could offer no criticism as it seemed to be very complete. The latest subject that has particularly interested him is a new method for preparing very active desiccated preparations of organs and digestive juices. He removes the organ from the animal and freezes it with solid carbon dioxide. It is then placed on the movable table of a shaving machine and cut by rapidly rotating knives into very thin shavings. The rapid movement of the knives throws the shavings into a box with double walls, surrounded with solid carbon dioxide. These thin shavings are then quickly transferred while in the frozen state to a large vacuum desiccator over sulphuric acid, much like our Hempel desiccators. The heat of vaporization keeps the mass still frozen, and the substance finally becomes fully dried at the temperature of the laboratory,--at least it never gets above the laboratory temperature. Kossel finds that under these conditions the glands and secretions preserve their chemical and physiological activity much better, and cited particularly his experiments with pancreatic juice. This desiccated juice is so active that in order to get fresh juice he simply suspends the dried extract in water. This makes a colloid solution which is very active, as active, indeed, as fresh pancreatic juice. Personally I think there should be a great application of this principle. It has long been believed in our laboratory that physiological preparations should never be dried above room temperature. I wrote to Professor Harvey Cushing about the possibility of utilizing dried pituitary glands and remember that he told me of some man in New Haven who spent all of his salary in buying the desiccated glands from the beef factories in Chicago. These glands were of course very low in efficiency and activity compared with the glands when dried and prepared by the method used by Professor Kossel. This new method should revolutionize the preparation of these glands. The glands are very expensive and an increase of 50 per cent in activity would cut down the cost by one half.

Professor Kossel was particularly bitter in his criticism of Dr. Levene of the Rockefeller Institute who, he thinks, has acted in a very unkind and uncharitable way. He cited in particular a man in Tubingen who claims that Levene takes the things that he publishes and puts them out as his own. The Tubingen man now writes at the end of his papers "I particularly request of Mr. Levene not to take this work up as I have experiments in hand dealing further with this matter." In speaking of the Rockefeller Institute, Professor Kossel says that the whole income of Heidelberg University is only as much as the income of the Rockefeller Institute for one year.

Professor Cohnheim.

Since my visit to Heidelberg, Professor Cohnheim has removed to Hamburg where he is associated with a large hospital. While in Heidelberg I saw a great deal of Professor Cohnheim and my respect for him was greatly increased. He certainly is very fertile in ideas on all subjects and an interesting discussion is sure to arise over any matter brought up with him. As to the soundness of his views, that is quite another matter, but he certainly is ingenious and always lends a zest to any discussion. For example, I referred to Putter's work in Naples, having in mind Dr. Alfred G. Mayer's statement made to me a year ago that since Putter's work they had had to revise entirely their ideas of sea life. Putter worked at Naples when Cohnheim was there and Cohnheim stated that his ideas are all false. He determined the carbon in sea water by the Messinger process which uses sulphuric acid and potassium bichromate. Under these conditions and with the presence of sodium chloride in the salt water, there was naturally a large amount of chlorine present. Putter did not remove the salt and evidently the chlorine attacked the rubber stoppers which he used, so that he found some carbon. Later another man repeated the work, removing all of the salt and using no rubber stoppers, and found no carbon in the water. Cohnheim says that Putter puts out a great many papers but he (Cohnheim) no longer reads them for they are all wrong.

At a dinner one evening, Cohnheim became reminiscent and gave many anecdotes regarding the physiologists. Dr. Grafe of Heidelberg was also at this dinner. As these anecdotes are probably not recorded anywhere, I feel them of sufficient interest to put them down here.

Pflüger was intensely interested in the problems of muscular work and one evening in Bonn the student body were astonished to see this old gentleman with his whole family seated in one of the front rows of a vaudeville theatre watching the performance of an acrobatic family. Pflüger's theory was that muscular work was performed at the expense of protein ingestion. He conceived the plan of inviting the whole acrobatic family to his house and providing food so far as possible in the form of protein, fat, and carbohydrate to see what class of food they instinctively selected. Since he believed that protein was the source of muscular work, he naturally expected that they would select protein. The next day somebody asked Pfluger what they ate. He was very much disgusted and said: "Don't speak of it." He had furnished beer among other things and they had taken that in preference to everything else.

Pflüger is credited with being extremely egotistic. As an illustration of this, Cohnheim told of the experience that Zuntz had with him. When Zuntz published his book on the work in high mountains with Muller, Caspari, and Loewy, he dedicated it in a graceful manner to Edward Pflüger. Pflüger criticised this dedication on the ground that

Zuntz did not give him the title of Geheimrat and state that he was professor in Bonn, etc. Zuntz's reply was characteristic and graceful. He said: "My dear friend, I beg to remind you that on the Bismarck monument there only stands Otto von Bismarck."

For twenty-five years a strife was actively waged between the two pioneers in physiology, Voit and Pflüger. Finally Voit went to Bonn to make a courtly call in an attempt to settle the discussion. He sent in his card but the diener came out and said that Pflüger would not see him and consequently Voit went quietly back to Munich.

At the time of my visit to Heidelberg, I was much interested in the Pratt experiments on the feeding of meat to dogs with atrophied pancreas. We had found that meat when fed to these dogs did not produce a rise in the metabolism as with normal dogs, and we were inclined to attribute it to the fact that there was no mechanical work of digestion which played any rôle. Professor Cohnheim contended that any operation affecting the alimentary tract affects the stomach digestion and believes that the large stools are due to a disturbed stomach digestion rather than to the absence of the pancreatic secretion. He had made experiments on dogs with fistulas in all parts of the alimentary tract and his deductions were drawn from these experiments. When I told him that the dogs recovered on feeding pig pancreas, he thought it was due to the rich supply of trypsin in the pancreas assisting the stomach. This was subsequently the basis of some discussion with Dr. Pratt and criticisms written in letters by Professor Cohnheim.

In speaking of the relationship of calories to carbon in the urine, Cohnheim stated that some one had worked out the calories to carbon ratio for all physiological bodies and found it not far from 11 or 12 calories per gram of carbon. Acting upon this, I have had computations made for all of the physiological bodies that were burned by Mr. Emery and myself and we find the number of calories per gram of carbon ranges from 7.673 calories with uric acid to 13.646 calories with alcohol.

Cohnheim considered that the electrocardiogram was a plaything for clinicians and a scheme to get patients to pay for a new thing. He personally had not much use for it and thought it of very little value in clinical work. In this, so far as I can judge, he stands nearly alone in Europe for almost every one else with whom I discussed the matter felt quite certain that the electrocardiogram had many possibilities in diagnostic fields.

I think no one in Europe has read the alcohol program more carefully and has made more notes and comments on it than has Cohnheim. I spent nearly two hours in the laboratory one morning with him going over it in very great detail and he had many interesting suggestions. He told me that a few months before a man came to him and said that he wanted to found an institute to study the alcohol question something like the Solvay Institute but on a smaller scale. He asked Cohnheim if any of his students would make good directors, chemists, or research workers in the laboratory and also asked Cohnheim to draw up a plan of the work that should be done in such an institute. Professor Cohnheim said he would have nothing to do with the institute unless he, Cohnheim, was allowed the greatest freedom in selecting the problems to be studied, and would be perfectly free to publish the results as found. He told me subsequently that the man was the representative of a society to further total abstinence. After Cohnheim had sent him his program, he heard nothing from him.

Professor Cohnheim also told me of a young man who was doing some work with Magnus in Utrecht. This young man was working on the influence of alcohol, but I have forgotten the exact phase that he was studying. In the progress of the research he found that wine was worse than alcohol itself. As the father of the young man was a great wine merchant, he told his father of the results, who said that he would disinherit him if he published them. In consequence the results were never published.

Professor Cohnheim's comments on the alcohol program are too extensive to enumerate here and they will be found filed with the comments of other investigators.

Probably Groebbels? Mar. 1932

Professor Cohnheim told me that a man with considerable money came to his laboratory a short time before who had written a great deal about canaries, publishing a number of volumes. He wanted to work on the digestion of canaries with Cohnheim. Cohnheim said that fistulas were impossible but that he might work upon the respiratory exchange. So Cohnheim devised a closed circuit apparatus on the principle of our apparatus. He does not weigh the oxygen but as the experiments are very short, he uses an ingenious benzol manometer which, however, cannot be used with man. With this manometer he has a closed circuit of definite volume, and, after calibrating, measures the amount of oxygen consumed by noting the loss in pressure indicated by the manometer instead of weighing each time. He uses about 60 milligrams of oxygen at one time. Everything is immersed in water and a rubber ball foot pump is used, very much like that employed by Likhatcheff in St. Petersburg. He weighs the carbon dioxide by absorbing it in a soda lime tube. The whole thing is very simple. Professor Cohnheim says that the singing birds are particularly interesting as the heart forms the greatest percentage of the body-weight that is found in any living thing. The canary weighs about 17 grams. Cohnheim has been able to train the canaries so that they will sing in the bell jar while under water.

I Medical Clinic.

Professor von Krehl.

Professor von Krehl is very nervous and fidgety but a "hustler" and extremely keen. In physiological matters he is wholly dominated by his clinical experiences and can hardly see a "normal" thing. All experiments are, for him, finding the abnormal and the normal has little interest for him. He says that we must live today and not a hundred years from today and must work on the abnormal so that we may help those who are suffering. This is the clinical point of view, which does not recognize the fact that until the normal value has been established, the abnormal value cannot be rightly estimated. In fact, it is quite contrary to the belief in our laboratory that we cannot adequately study the abnormal until we have thoroughly established the base line for the normal metabolism. I told him of our experience in the research on diabetes when we found that we had no results with normal subjects with which to compare the results obtained in our experiments with diabetics. He contended, however, that abnormal metabolism is so different from the normal that it is better to study it without attempting to compare it with the normal metabolism.

He is much interested in heart cases and wants a respiration chamber to use in studying them. He said that too small a chamber would disturb respiration so that the subject would be afraid and have unpleasant feelings, etc. The unit apparatus interested him very much. I went over the article in the Deutsches Archiv f. klin. Medizin with him very carefully and at his request ordered a Crowell blower for his use. I also offered to lend him a spirometer which was subsequently sent to him and copied.

It was later sent to the Physiological Congress in Groningen. A letter received from Dr. Grafe, Dr. von Krehl's assistant, dated September 17, 1913, stated that the unit apparatus was nearing completion and that they expected to begin work with it early in the winter. They planned to build the unit apparatus first and subsequently to add some kind of a chamber to it.

In discussing the fasting experiment with Professor von Krehl, he said that Levanzin must have been crazy and that if we found any abnormal metabolism, the results would be of little value. I told him of Dr. Southard's examination and he emphasized the importance of his report, saying that it would be of great significance. I therefore wrote to Dr. Southard who has kindly prepared a written report which should meet all of the criticisms that Dr. von Krehl raised. Dr. von Krehl believes that the psychic condition plays a great rôle in the metabolism.

I remember very well discussing with Professor von Krehl three years ago the possibility of a "luxus" consumption. He then spoke of society women who led a sedentary life and lived high, saying that they must have a higher metabolism. Since that time Dr. Grafe has made experiments on a dog which were published and read by Dr. Morgulis and myself. This work was evidently done in support of von Krehl's belief that there was a "luxus" consumption, namely a higher plane of metabolism, with an exceptional amount of nutriment.

Professor von Krehl was likewise much interested in the rectal temperature apparatus. He had seen the prospectus of Siemens and Halske and was not surprised at my adverse report. Personally he does not care for self-registering thermometers and does not think they are necessary. I explained to him the possibility of using two thermal junctions with a d'Arsonval galvanometer.

Professor von Krehl emphasized the fact that Rubner does not like adverse criticism and if one contradicted him, trouble would surely follow.

I told Professor von Krehl of our experience with the question of the chemical regulation of the body temperature. I have become more and more convinced that it will be necessary for us to repeat Murschhauser's work on guinea pigs at different temperatures and also Rubner's experiments. The experiments made by Dr. Morgulis on the dog Bill, in which exactly the same degree of metabolism was found at temperatures 10° apart, but with no shivering indicated by the pneumograph, are most significant.

Dr. Grafe.

Dr. Grafe, who was first assistant in the laboratory of Professor von Krehl, is a very bright man, whose enthusiasm sometimes leads him to jump at conclusions too quickly, but who is still a really good man. He has published extensively--perhaps too extensively--and has entered into a polemic with Abderhalden. His trying to be somewhat polemical shows many signs of youth. On the other hand he is one of the brightest men that I met in Europe and I expect him to have a great future.

In discussing with him the Pratt-Morgulis experiments in which a respiratory quotient over 1 was found with the dogs with an atrophied pancreas after feeding excessive amounts of carbohydrate, Grafe said that he had found a respiratory quotient over 1 with dogs but that they were dogs with an Eck fistula which were obviously not normal. He knew of no experiment on a normal dog in which a respiratory quotient over 1 was found.

Dr. Grafe has made experiments with regard to rectal feeding and finds that the specific dynamic action, so called, still persists. He made control experiments with sodium chloride solution. Professor Cohnheim, thinking this might be attributable to intestinal work, repeated Grafe's experiments with dogs, using fistulas, but found no results, thus substantiating Grafe's findings in every detail.

Abderhalden, Grafe, and Henriques of Copenhagen carried on contemporaneous experiments on feeding swine with ammonium salts and studying the nitrogen balance. The question of priority has arisen and Grafe tells of some very unpleasant experiences with Abderhalden. After writing him a personal letter explaining the situation, Abderhalden has, according to Grafe, done some things which no gentleman would ever do, such as using a confidential letter in a public way and making improper use of the material. Grafe says that Abderhalden thinks that he is a great man and that Grafe is only a little fellow, so that people will believe Abderhalden rather than Grafe. Dr. Grafe also said that Abderhalden had a quarrel with Henriques, and had had a lot of trouble everywhere he went. He had trouble in Basel, and then went to Berlin, where he again got into trouble. He was always a hard man to get along with and has been in a controversy with nearly every one with whom he has come in contact.

The whole question of entering into a polemic was the subject of conversation at a dinner with Professor Cohnheim, Dr. Grafe, Dr. Siebeck and myself. Professor Cohnheim and I both agreed that polemics were utterly useless and Cohnheim summed it up very tersely by citing the old Pfluger-Voit controversy, saying "Never engage in a controversy. They may last twenty-five years and no one is better off."

At this same dinner we discussed the question of French versus German science. They all seemed to believe that the French were very much more productive of theories but that they never sustained them by practical experiments, and that the Germans' strong point was the proving of a theory by an extensive amount of experimental research, although they, too, are doubtless given to theorizing like the French.

Dr. Grafe has found a lower metabolism with catatonics than with normal individuals in spite of the fact that there was great muscular tension but no motion. He said that the catatonics would hang by the wrist from a cord for hours and then eat and hang themselves up again. This was in the old days when restraint measures were adopted in the hospital. This question of tension in the muscle brought up the point that some one says that a muscle works only when it is in motion, but that there is no work when it is absolutely tense. Of course this brought up the question of static work. Static work equals 0 and only when the muscle is active is work done. Grafe also found that with electrocardiograms the catatonics had much less heart muscle tonus than normal individuals.

Dr. Grafe said that his experience with Lusk had led him to think that he was very sensitive. von Noorden had told Grafe's brother that Lusk was working on luxus consumption and he was very anxious to know why and how Lusk was doing it. I told Dr. Grafe that probably von Noorden referred to the paper that Lusk gave in Washington in which he stated that Voit said that if you give plenty of metabolites under these conditions the cells metabolize more material.

Dr. Grafe is very desirous of studying the calorific equivalent of oxygen with swine when there is a respiratory quotient above 1. I told him the matter had been under consideration a long time in our laboratory but that we felt that Professor Lusk would do animal work and we would not begin yet on animal calorimetry.

GENEVA, SWITZERLAND.

University of Geneva

Professor Guye.

I was particularly interested in seeing Professor Guye again, inasmuch as I wished to discuss with him his opinion regarding the constancy in the composition of the atmosphere which we noted in Boston. He has reported the results of several researches, which were presumably accurately carried out, in which variations in the composition of the atmosphere were clearly shown.

On arriving in Geneva I went immediately to Professor Guye's laboratory and had a most profitable afternoon with him. The laboratory impresses one as being a maze of glass blowing. He has the idea, which I believe was first brought out by Professor Ramsey in London, that no connections other than glass tubing are allowable in accurate work. They use a small hand blast lamp or blow pipe which makes it possible to do all kinds of things. I have ordered some of these lamps for the Nutrition Laboratory. I also saw there an exceedingly small bomb for weighing minute quantities of gas, which had a reduction valve that Professor Guye said was made for him by the Société Genevoise. I have been unable as yet to secure either the bomb or the valve.

I also saw in this laboratory an interesting method for purifying mercury. Professor Guye says that the best way to purify mercury was by distillation in a slow current of air since under these conditions all the impurities were oxidized. The method was originated by Hewlett, an American, and published in the Physical Review about 1911. A very ingenious electric heater was used in which some modifications had been made by an assistant of Professor Guye. A rather complicated glass vessel was an integral part of the heater. When I was there, this vessel was broken, but they maintained that the principle was absolutely sound, and Guye told me that with this method he obtained the purest mercury he had ever seen.

One is impressed by the seriousness and intensity of Professor Guye. He is all over the building, has many different things going, is intensely interested in what everybody is doing, and is evidently a man capable of handling a great many things at once. I have one unfortunate standard for judging the character of his work. In a research published from his laboratory by Dr. Watson, the composition of the outdoor air was proved, at least to their satisfaction, to be variable. If, with all the degree of refinement that he uses, they find this variation in the composition of the outdoor air, which I think has been clearly shown is not variable, one wonders how much of their other work will stand keen criticism. In other words, the general impression is that while apparently there is an attempt to secure great accuracy, underneath something fundamental may still be lacking.

I regretted that I had ever read Watson's work, as it seems to me it invalidates practically the whole of the work that Professor Guye is doing. On the other hand the ostentatious efforts to secure the greatest degree of refinement must have a salutary effect on students, although I now recall the criticism of an American university professor who had been at the laboratory a year or so before which substantiates my belief that the striving for refinement is more apparent than real.

As a delightful gentleman and an inspiring teacher, Professor Guye probably has few equals. Had I not had the experience with air analysis, I should have been more deeply impressed by his work. The most impressive thing was the glass blowing and the fundamental belief of his laboratory that only fused glass joints are permissible.

BASEL, SWITZERLAND.

I Medical Clinic.

Professor Staehelin.

Professor Staehelin now has a large clinic with 300 beds, so that he is unable to give his attention to other work. I think he is permanently lost to the scientific world as an original investigator. He is busy editing a treatise of several volumes and has nothing of scientific research in view.

When I asked him about the Berlin fast he said that nothing had been published. In Heidelberg Dr. Grafe told me that Staehelin found that the young woman who made the air analyses had become unreliable and he had no confidence in the results. Professor Staehelin did not mention this to me but was noncommittal as to when the results would be published.

In discussing the alcohol program Professor Staehelin said we should look out for vasomotor changes as affected by psychic conditions. He told me about some plethysmograph experiments with a diener in which he tried to excite the man by firing a pistol. This noise produced no effect, but when the diener heard a strange step in the corridor, he was at once aroused, as his whole training made him instinctively on the alert to see that the man meant no harm and that he had business there. Staehelin claims that the psychical effect must always be kept in mind.

He also criticizes the use of a stomach tube as it is uncertain, and the psychic effect is very harmful. He says when using it one gets all kinds of results, due to excitement and sometimes an inhibited flow. He was doubtful as to the wisdom of its use except on a trained subject.

Dr. Gigon.

Dr. Gigon is now in the I Medical Clinic and is not doing so much scientific work as formerly. I investigated carefully some of the researches he published a short time ago. He uses Müller valves with two large Woulff bottles of about 2 liters each. These are filled about two thirds full with water, a glass tube as large as would go through the tubulatures just dipping under the water. He considered them very satisfactory.

I expected to find an elaborate system for diluting expired air to make it about one per cent carbon dioxide. Instead of that I found that he simply expires the air into a spirometer, and after fifty liters of expired air has collected, he pumps in 100 or more liters of external air, and analyzes on this basis. He does not mix it at all and thinks an ordinary diffusion sufficient. He uses a mouthpiece and, like Loewy, has a band to tie it to the face; he believes that all leaks are thus prevented, and the best results obtained. He makes all of his experiments with the subject asleep, if possible, usually in the evening after fasting all day, and considers the sleeping condition more favorable for such measurements. His analyses are made on a very imperfect Pettersson gas-analysis apparatus. I was much impressed by his carelessness in ordering a burette for this apparatus. He attempted to secure a burette that would allow him to measure at the same time both the carbon-dioxide increment and oxygen deficit in expired air, and had therefore planned to get from 0 to 5 per cent of carbon dioxide, but had provided for a measurement of the oxygen only between 19.5 per cent and 21.0 per cent, in other words 1.5 per cent instead of 4 or 5 per cent. This would imply a lack of fundamental physiological knowledge. I pointed this defect out to him and he seemed to be very much chagrined over it.

There is nothing to indicate that Gigon will not soon follow in Staehelin's footsteps and no longer carry on scientific work. In my opinion he will not be the loss that Staehelin is. Gigon seemed to have a very good opinion of himself, and a very poor opinion of Grafe.

I found that his impressions of accurate chemical processes were very vague, and I was most disappointed in this man as I had read so much of him. While talking with him I was constantly reminded of the "affair" with poor Landergren, and I saw whence came Gigon's inspiration. Almost the next day I received word that Landergren had died. Gigon is, I consider, very much overrated, although apparently those who know him intimately are not so much impressed with his ability. He is very sharp but he lets things go that are not accurate. His analyses, tests, etc., are not at all of the Stockholm school. Evidently he is not so good a worker as I thought.

Physiological Institute, University of Basel.

Dr. R. Metzner.

I met Dr. Metzner for the first time on this occasion, and found him most agreeable. He is in pharmacology and is much interested in the possibilities of utilizing the unit respiration apparatus for small animals, such as rabbits. I gave him many sketches and points regarding it together with photographs of the apparatus. He is altogether one of the most charming personalities that I met in my European tour.

Professor Jaquet.

This is another good man gone wrong. Formerly one of the most brilliant technicians, he is now interested only in practice. I had two hours with him and while he is as interesting as ever, he is saturated with philosophy, writes on the philosophy of love, and thinks that the whole of life is wrong. He is distinctly "off". It appeared to me that he is as erratic as was Ostwald, and apparently believes he is a philosopher. The more I think of it, the more I regret that this man has, speaking scientifically, gone wrong. That a man with his gifts for construction and design should so run after "false gods" is deplorable. He will never be more than a second-rate clinician and a very poor philosopher, but he was formerly one of the leading designers of intricate and good physiological instruments. In fact, I told him that he had no successor, that there were a great many good clinicians and good physicians, but that there was no one who stood as high as he in the mechanical line. He finally admitted that he might be tempted into mechanical work again, but did not care much for it.

Jaquet had a great deal to say in regard to the alcohol program. He thinks such an investigation is almost impossible, and asked me how many years I thought it would take to complete it. I said "Probably 20 years." He wonders at the Americans for undertaking it. The great difficulty, according to him, is to separate the metabolism due to the combustion of alcohol from its poisonous effect, but admitted that this same criticism applies to most of the work in pharmacology. Personally I told him I did not see how the metabolism of strychnine, for example, could have much importance. Jaquet maintained that alcohol poisons the system all the time, and when I told him that with me 100 grams of cane sugar would produce a digestive disturbance, with canker sores in the mouth, etc., and that I did not think that 100 grams of alcohol would produce so bad an effect, Jaquet said that alcohol always poisons the system and that cane sugar never does.

FLORENCE, ITALY.

General Medical Clinic.

Drs. Marchetti, Capezzuoli, and Frugoni.

On my two previous foreign tours I did not include Italy in my itinerary, but it seemed to me advisable to see at least once the Italian laboratories of physiology, particularly as there had been some recent investigations on technique that I thought should be carefully looked into. My first stop was at Florence, where I went to the laboratory of Dr. Marchetti who has associated with him Dr. Capezzuoli. I went all over their clinic and saw the possibilities for research in pathological cases. They have an old Zuntz-Geppert apparatus purchased at the Vereinigte Fabriken seven years ago. (See figs. 9 and 10.) As a matter of fact, neither of these gentlemen had ever seen Zuntz, and they had studied out the method of use from the first article published by Magnus-Levy. Dr. Marchetti told me that he had spent seven months in learning to use the apparatus and keep it in condition, and that at present he and Dr. Capezzuoli were the only ones in Florence who could make analyses with it, although it was used in part by assistants.

Marchetti had done some work with high-oxygen atmospheres which interested me especially. He also told me that he had had made some special burettes so that he could determine 65 per cent of oxygen. The oxygen-rich mixture was fed in from a spirometer which is no longer in existence. He collects the gases over a saturated solution of common salt, colored with rosolic acid to prevent the absorption of oxygen by acidulated, distilled water.

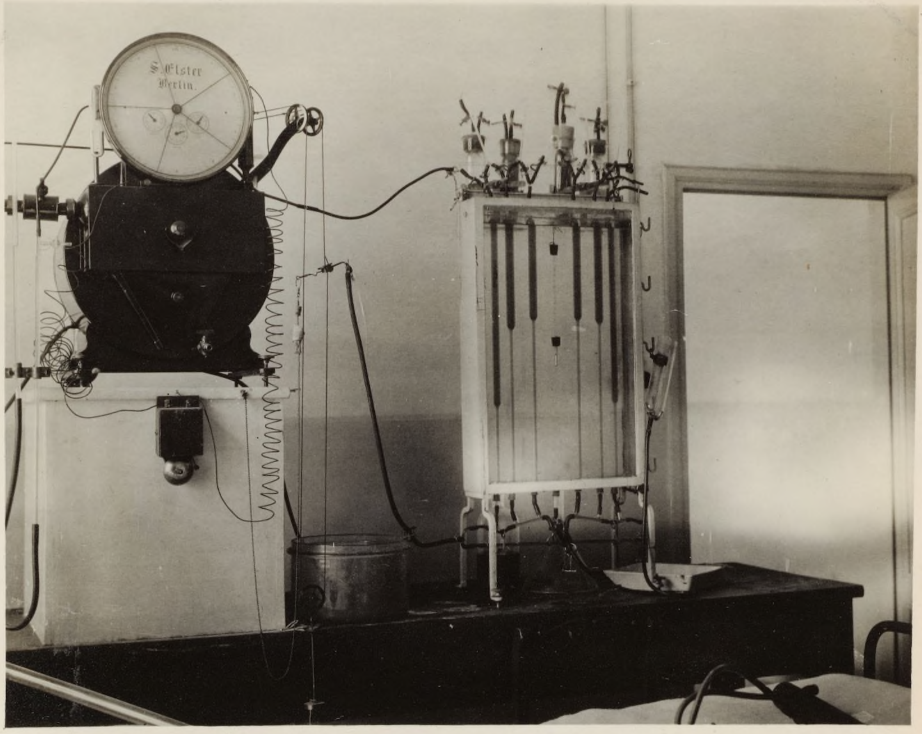


Fig. 9. Zuntz-Geppert gas-analysis apparatus in Marchetti's laboratory in Florence.

(One of the older models.)

Fig. 10. Details of the Elster meter used with the Zuntz gas-analysis apparatus in Marchetti's laboratory in Florence.

In discussing the Zuntz-Geppert apparatus, I found that Marchetti uses copper gauze and ammonia to absorb the oxygen, but has used phosphorus; he considers the latter as dangerous with the 60 per cent of oxygen which he uses. He also dislikes extremely the small rubber connections and screw pinchcocks between the burettes and the absorption pipettes of the Zuntz-Geppert apparatus and maintains that ground glass stopcocks should be used. I was much interested in the fact that this apparatus was described as a modified Zuntz respiration apparatus. As a matter of fact, the nosepieces were much inferior to those of Zuntz, and the mouthpiece came from the Vereinigte Fabriken. The only new point was the Marchetti valve.

I made a number of photographs of the Marchetti valve (see figures 11, 12, and 13) and Marchetti was good enough to give me the original drawing from which the illustration was made for his article. The valves were extremely interesting but not every one was satisfied with them. For example, Dr. Frugoni told me that he was working with the apparatus now and did not like the Marchetti valves, saying that they were too cumbersome, that there was too much resistance, and that they produced dypsnoeic breathing in ten minutes. On the other hand, he considered the Morelli valves from Pavia very satisfactory. (See figs. 12,13 and 14.)

The ideal respiratory valve secures tight closure with minimum weight and maximum flexibility. Of all the valves I have ever seen, Morelli's seems the best. Morelli is an assistant to Professor Forlanini in Pavia, and is evidently a very clever fellow and a very good worker. He has devised a glass valve with a small rubber balloon which, when inflated, acts as a flap to seal the opening. The flexible rubber neck of the balloon serves as a hinge so that the ball moves readily. The valve is admittedly complicated but singularly enough very, very good.

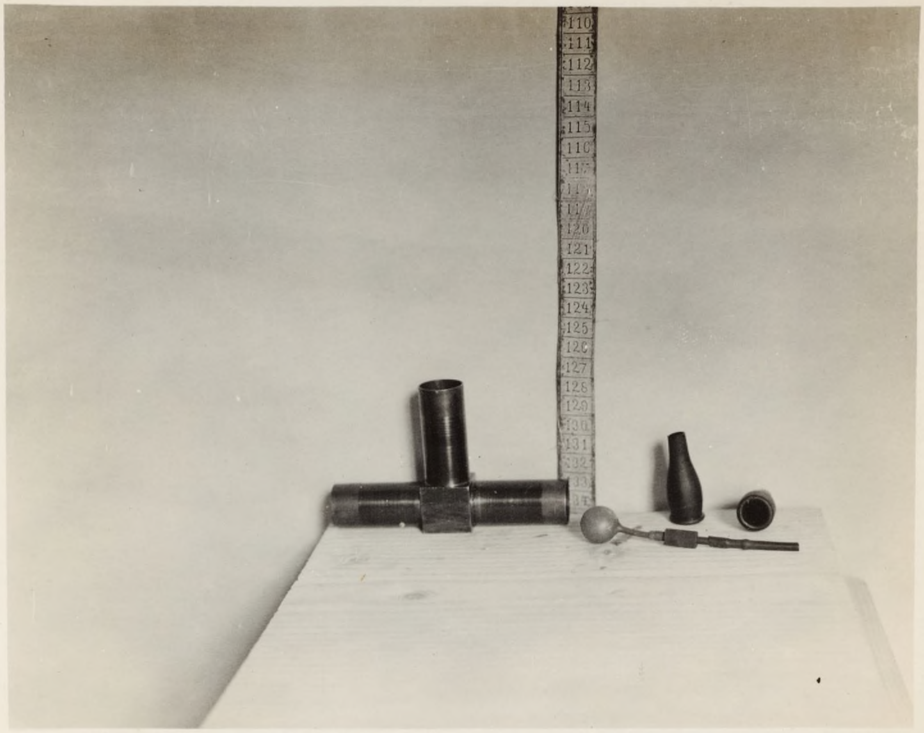


Fig. 11. Part of the Marchetti valve, the rubber balloon for the Morelli valve, and rubber nosepieces for the Morelli combination mouth- and nosepiece.

Fig. 12. Glass Morelli valve and brass Marchetti valve.

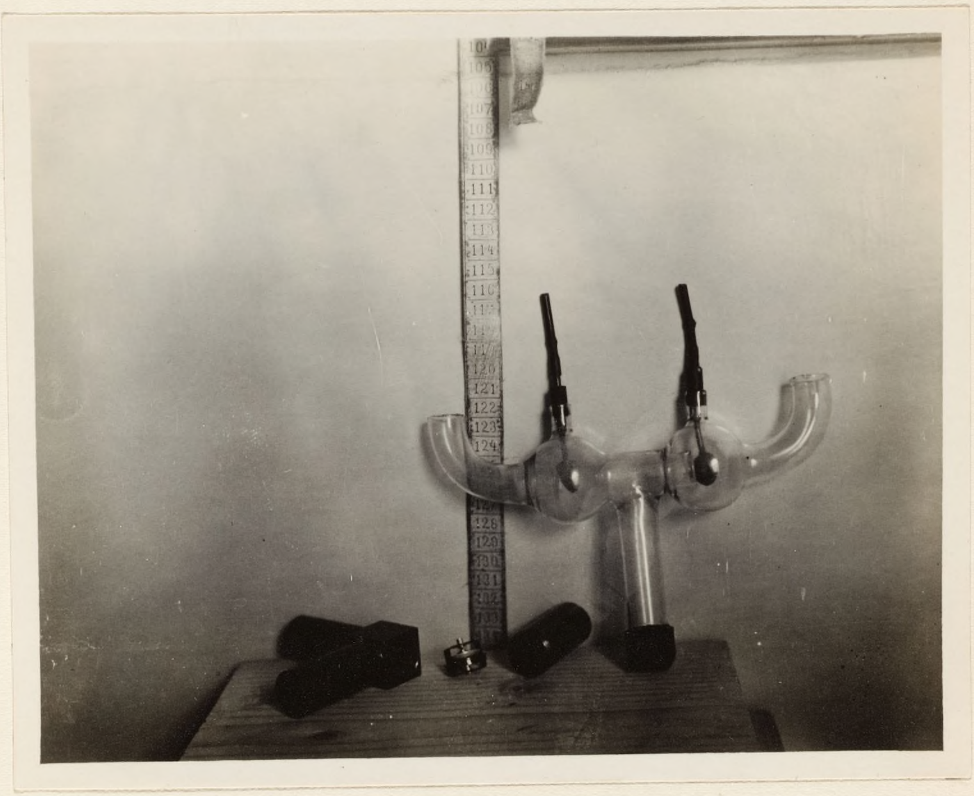


Fig. 13. <u>The Morelli valve used by Marchetti in Florence.</u>

In the foreground are also parts of the Marchetti valve.

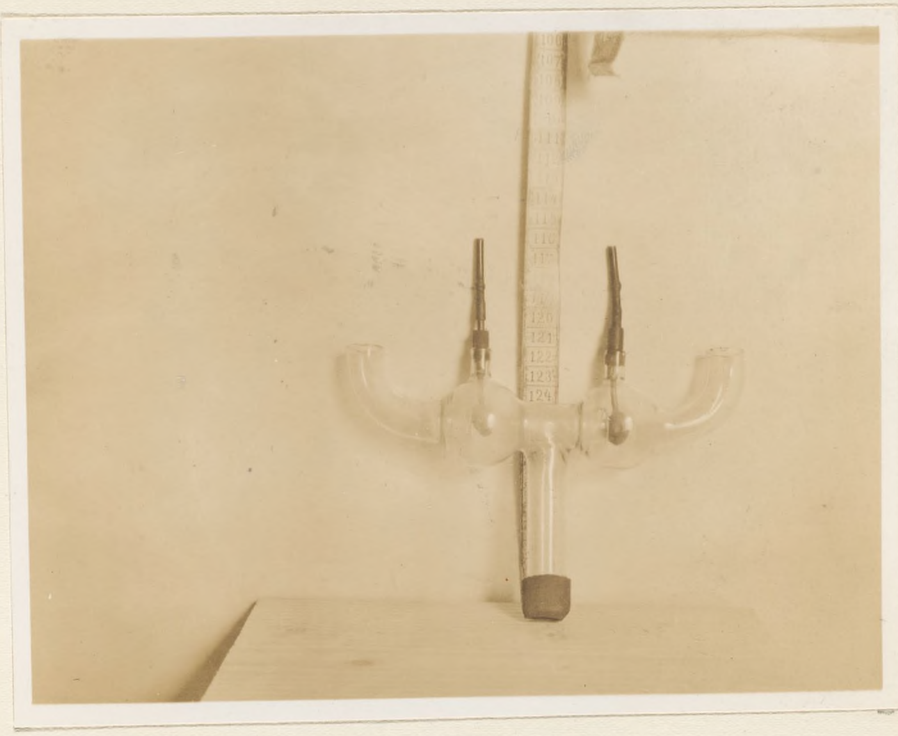


Fig. 14. Details of the Morelli valve used in Marchetti's clinic in Florence.

They had two of these valves in Marchetti's clinic. One of these had been used by Marchetti but he discarded it for his own metallic valves which he said were better because they had less dead space. Frugoni also had a new set of Morelli valves. I tried these new valves and found they moved very easily; they seemed, without exception, the lightest valve that I ever breathed through. The little rubber balloon, with its rubber stem looked very much like a small plum on a stem and vibrated back and forth as the valve opened and closed, producing a "chattering". It is a very interesting sight as it is so perfectly visible.

Marchetti says that the Morelli valves are very light and give less resistance than his metal valves but are difficult to keep in condition. The little rubber bags must of necessity deteriorate quickly but when once adjusted and new, it is a very good valve. The renewal of these bags must be very difficult. According to Frugoni and Marchetti, Zambelli of Turin, who makes and sells these valves, has these rubber bags made especially for him, and they cannot be obtained except through him. A spherical form is made for these rubber bags, but it is not a true sphere as one side is flattened, and this side is used to press against the open glass tubing. Marchetti told me that he thought that if the rubber bags were kept in a 10 per cent glycerin solution, and deflated when not in use, they would last a long time. I ordered a set immediately.

The combination nose- and mouthpiece used with Morelli's valves interested me very much. (See figs. 15, 16, and 17.) In talking with Dr. Marchetti about it, he immediately raised the obvious objection that the dead space was very large, and, indeed, he said this was the reason why he devised his metal valves which have but a small dead space. He was also of the opinion that the Zuntz glass valves

Fig. 15. The Morelli combination mouth- and nosepiece used in Marchetti's clinic in Florence.

The sliding metal joint holds the lips firmly in place. The two rubber tubes are placed in the nostrils.

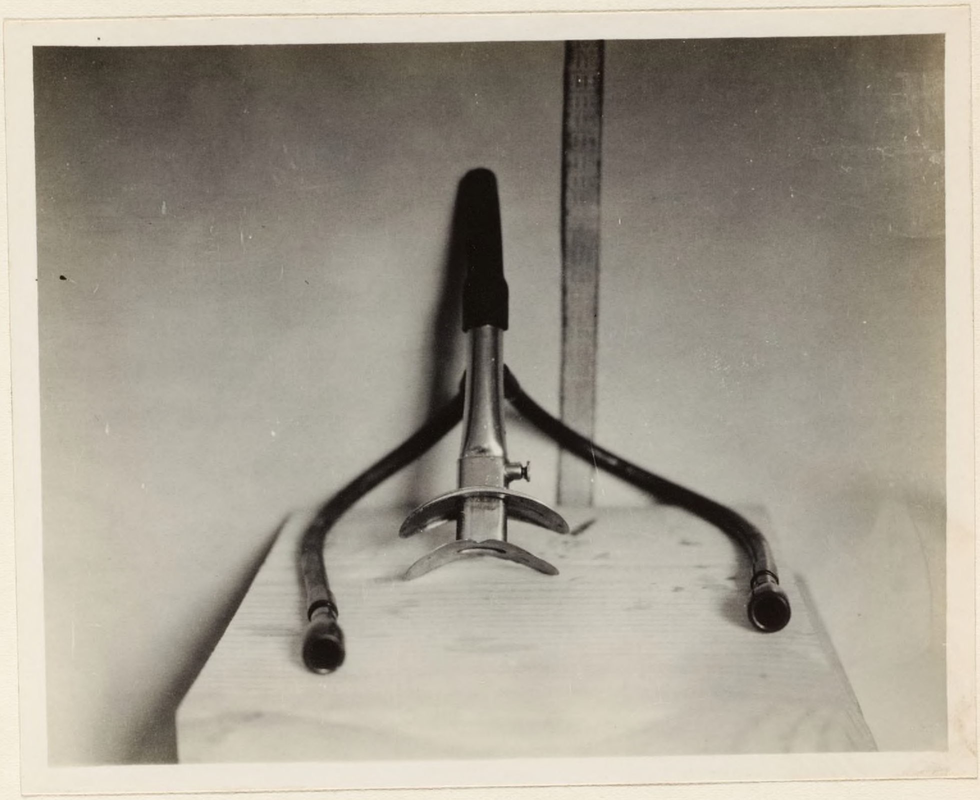


Fig. 16. Another view of the Morelli combination mouth- and nose-piece used in Marchetti's clinic in Florence.

Fig. 17. Dr. Capezzuoli in Florence demonstrating the use of the Morelli combination mouth- and nosepiece.

had too much dead space. The method of inserting the nosepiece of this combination into the nose did not appeal to me very much, although both Marchetti and Capezzuoli believed that the closure was perfectly tight. The double metal flap to clasp the lips is certainly ingenious.

I tried the nosepiece used with the Morelli valves but found it impossible for me to fit the rubber nipples closely into the nose. Later Frugoni showed me another size of nipple, but we could not find a person in the laboratory whose nose either set would fit and remain tight. All said that the mouthpiece was the best, and Marchetti said that they preferred <u>his</u> nosepiece and <u>his</u> mouth clamp. As a matter of fact, so far as I could see, Marchetti's mouthpiece was nothing but the handy rubber mouthpiece so long used by Zuntz.

I was particularly interested in the fact that in Forlanini's laboratory in Pavia, Morelli and others are evidently much interested in studying the respiratory exchange; Marchetti seemed to think that there would be considerable work in the future in that laboratory along these lines. Doubtless Morelli intends to make many respiration experiments on the pneumothorax cases of Forlanini.

Marchetti says that two of his assistants are working with his apparatus, studying the influence of day and night on the respiratory exchange, without food, and also the toxicity of the urine, and of expired air, but they find the work very difficult. We must watch for the published reports of this work.

The library of Professor Grocce in Marchetti's clinic was an excellent one. I saw many American medical journals but little of other physiological science. They told me that there are eighty

Italian medical journals. Among the best are the Gazzetta Chimica Italiana, which is published in Rome (Via Panisperma 89), costing 34 francs a year; Professor Fano's journal, Archivio di Fisiologia, Dr. Lo Monaco's journal, which is the only pharmaceutical journal published in Italy; Rivista Medica; and Policlinico. Lo Monaco has a very extensive list and might possibly exchange with some English or Americal journals. It seems advisable to subscribe for one or more of these journals for the Laboratory so as to keep in touch with Italian work and thereby acquire a slight knowledge of Italian.

My general impression of Marchetti's clinic was that it is extremely provincial. They are familiar only with Italian work but know that, of course, very well indeed. Inasmuch as the Italian work in metabolism is slight there is evidently not much interest in the subject or in respiration experiments.

Physiological Laboratory of the Imperial Institute.

Professor Fano.

Professor Fano is evidently the gentleman philosopher and physiologist of Italy. He is a senator, a man of affairs, editor of a journal, a man of considerable wealth, and, in addition, a man of very charming personality. (See figs. 18 and 19.) He has an excellent library and while his laboratory is very old, it is well equipped, including a string galvanometer. He has spent much time working upon a tortoise heart, studying especially a combined electrocardiogram and photograph of the muscular excitement by using a rubber bag placed upon the heart. For a slow moving manometer, he considers the Nernst light to be sufficient, but it is necessary to use the arc light for the string galvanometer. Professor Fano showed me his whole laboratory although it was during the vacation period and there were but few active workers. A magnificent garden was attached to the laboratory in which animals are kept and places provided for fish used in the research work. An interesting research was being carried on by an assistant upon the use of an ultramicroscope.

Professor Fano told me the history of Luciani and the fasting man, Levanzin. Luciani, he says, is extremely guileless and consequently took Levanzin to the reception room of the senate, but Fano said that most of the men were surprised that Luciani had such a man about. Fano said that when Levanzin offered to shake handshe found he could not bring himself to touch his hand. He was not surprised at our experience, but emphasized the fact that Luciani is a very ingenuous man, very much like a child,--not a man of affairs, although a senator. As a matter of fact, he almost never goes out of his laboratory, and

Fig. 18. Professor Fano in his study in Florence.

Fig. 19. Professor Fano in the garden behind his laboratory in Florence.

knows nothing outside of it, being wrapped up in his writing.

I showed Professor Fano the data of the Levanzin experiment. He said immediately that he believed that all of the tables should be printed with the greatest detail, maintained that there was never an experiment like it, and that it should be given the fullest publication possible.

One of the most interesting comments made by Professor Fano was the fact that he was much impressed by the expenditure of the funds of the Carnegie Institution of Washington. He evidently has a pretty good knowledge of the financial condition of the Institution, and of the amount of work done, and expressed himself as surprised that we accomplished so much in our laboratory on so limited an appropriation. This is quite contrary to the usual impression among Italian scientists, who believe that we have unlimited funds.

Professor Fano impressed me as being one of the most delightful gentlemen it has been my privilege to meet, but it is evident that he is no longer as actively engaged in research or in editorial work as he formerly was. His chief interest is in Italian state affairs, as he is an active senator; hence he is no longer looked upon as a source of serious scientific research.

NAPLES, ITALY

University of Naples.

Professor Galeotti

As Professor Galeotti has recently published work on the amount of moisture in expired air, I wanted particularly to see his apparatus, his valves, and his method of making the tests. So I made arrangements to visit his laboratory in Naples. I found him a remarkably genial and pleasant man, who impressed me most strongly as a very intense worker and a man who was capable of covering a great many fields. His laboratory is modern and very well equipped, although in a very old part of the town.

In connection with his apparatus for determining the moisture in the expired air (see figs. 20, 21, 22, 23, and 24) I found that he was using a Verdin spirometer, or gas meter, which he had secured from Paris. He stated that he had found this very good indeed. It costs about 130 francs and at the end of two years it must be discarded, as the valves wear out. I tested it myself and found it gave no great resistance to breathing. It weighs about 6 kilograms and can be carried on the back. With this spirometer he uses the Zuntz gas-analysis apparatus.

To absorb the moisture from the outgoing air he uses two calcium chloride tubes in parallel. The tubes struck me as being very, very small, and yet on careful examination the calibre of the entrance tube did not seem to be very much smaller than the calibre of the nosepieces ordinarily used in connection with our unit apparatus. The two currents of expired air from the calcium chloride tubes join in a Y-tube and go to the spirometer. There was cotton wool in the outgoing end of the calcium chloride U-tube. In trying the apparatus, I found it very

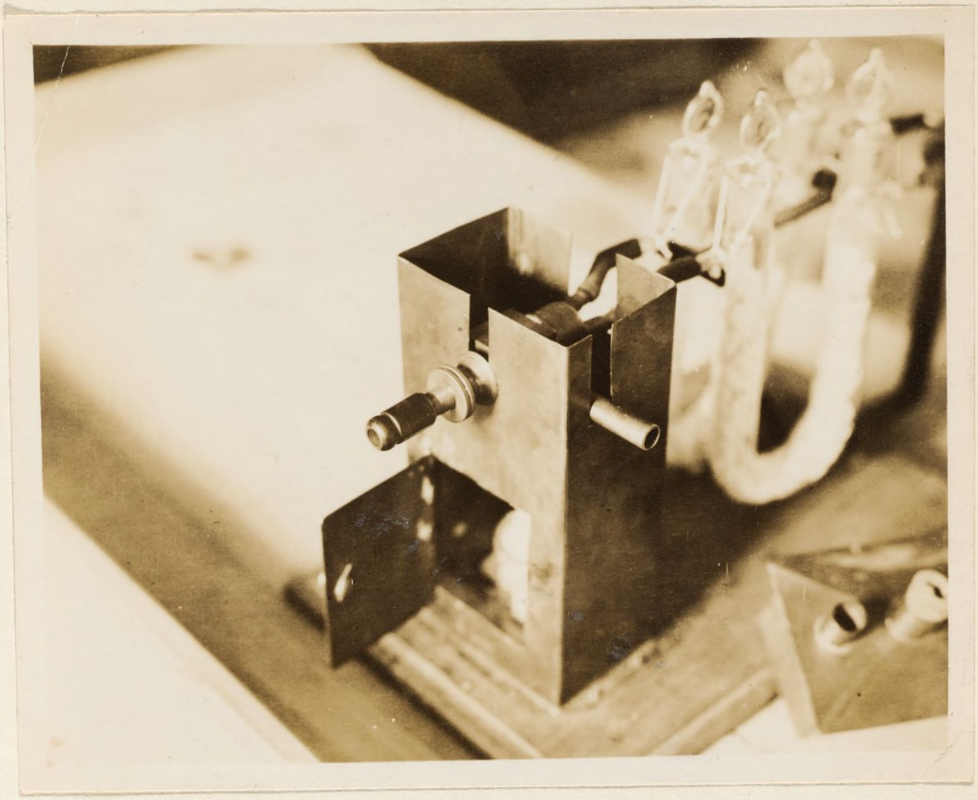


Fig. 20. The Galeotti apparatus for studying the water-vapor in the expired air.

In the chamber beneath is seen a white candle used to keep the valve warm and to prevent condensation. A certain kind of parchment paper is used for the valves as this paper endures heat without disintegrating. The calcium chloride U-tubes are at the right.

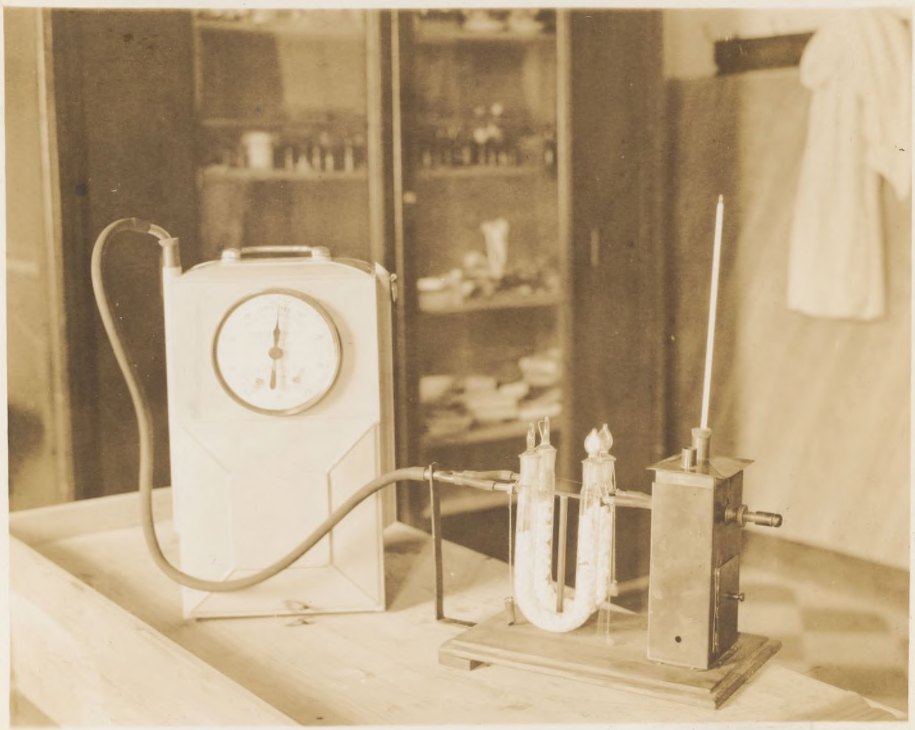


Fig. 21. The complete apparatus used by Galeotti for studying the moisture in the expired air.

The valve chamber with door closed is at the right. Next are the calcium chloride tubes, then the spirometer to measure the total amount of expired air.

Fig. 22. Subject expiring through the Galeotti apparatus for determining the amount of moisture in the expired air.

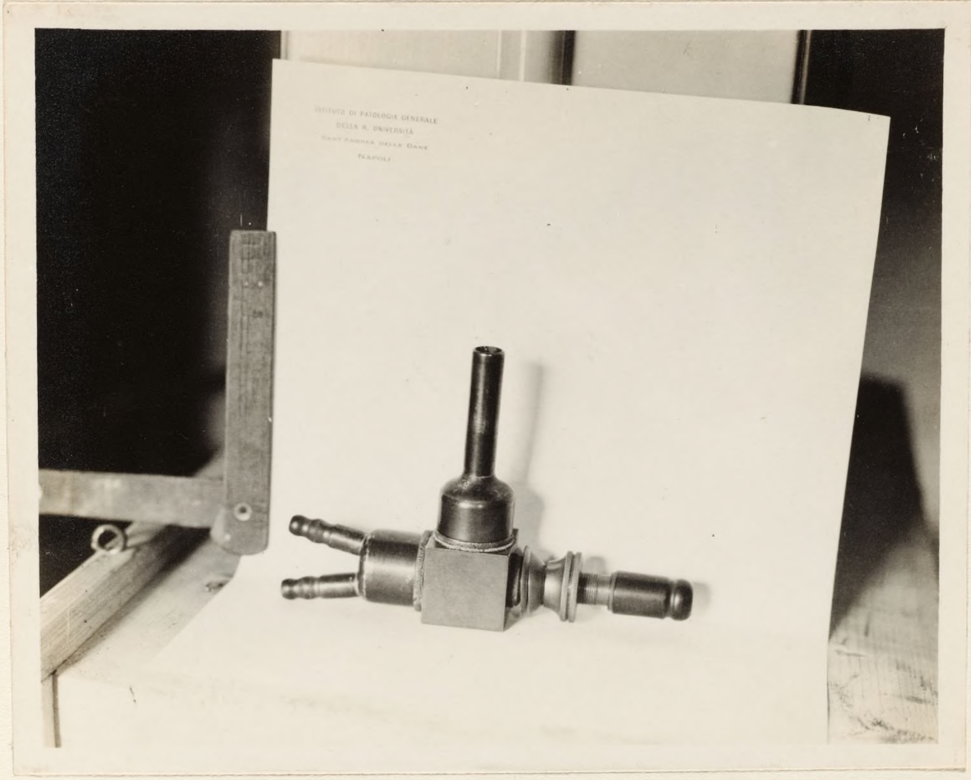


Fig. 23. Galeotti valves for the determination of moisture in the expired air.

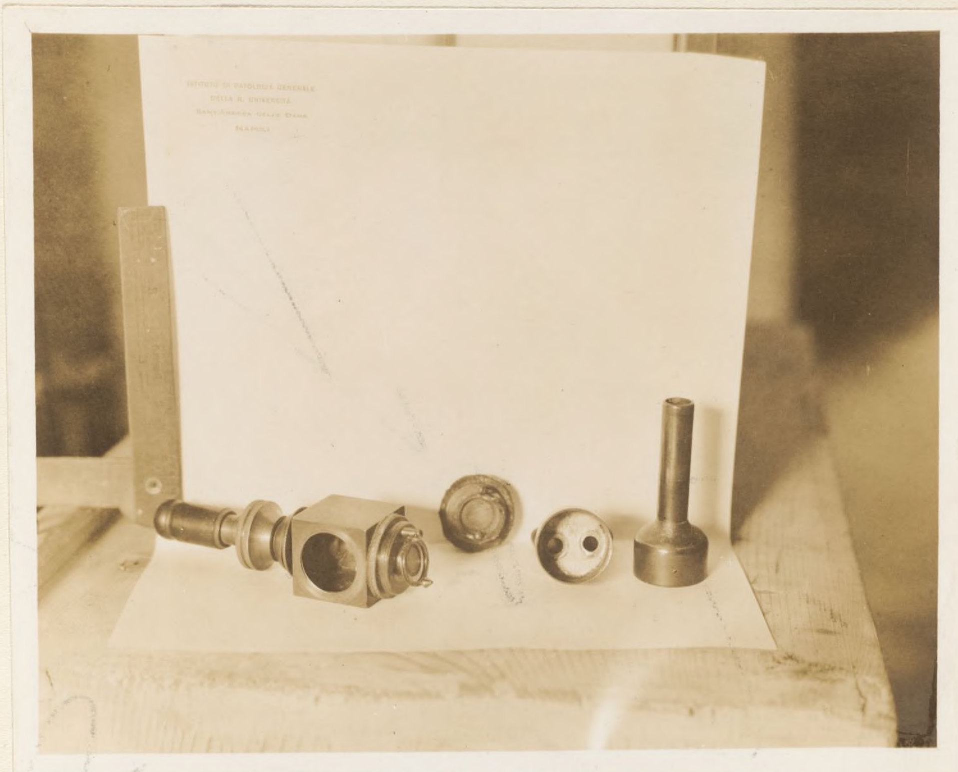


Fig. 24. Details of Galeotti valve for studying the moisture in expired air.

difficult to breathe and it seemed to me that there was considerable resistance. I pointed this out to Galeotti, who tried it himself, and then ordered that some of the cotton wool should be taken out. This was done, but I still found a considerable hindrance to respiration. The experiment lasts but four minutes, there being no preliminary breathing to adjust the respiration conditions.

The valves seemed very small, with very small openings. Their construction was ingenious as they were made of small metal rings, covered over tightly with thin pieces of parchment paper. Galeotti stated that he used parchment paper as he found it would stand heat, and the valve was warmed up to 150°C. Even at this temperature, the paper does not show any deterioration. In the bottom part of the valve chamber is a small candle, which he uses to heat the air of the valves, thus giving no opportunity for water to condense in the valves, as the temperature is always kept about 37°C. A thermometer is provided and a copper box, which is covered loosely so as to allow free access of air. The general impression that I obtained of this apparatus was that the whole thing was on a very small scale.

Galeotti is on some commission to study Italian fevers, and therefore has devised a very crude calorimeter for studying the respiration of rabbits in fever. (See figs. 25, 26 and 27.) It was hardly more than a calorimeter on the method of mixtures, and it had never been tested except by pouring hot water into it. I told him this would not suffice and he should use an electrical check test, which he planned to do. In the same room was a rotating treadmill for exercising dogs (fig. 28), which was very crude and very inexpensive. The arrangement of the dog stalls in the basement was excellent, although they had not yet been put into actual use. All of these apparatus show the versatility of Galeotti's mind, and his ability to do a great deal with

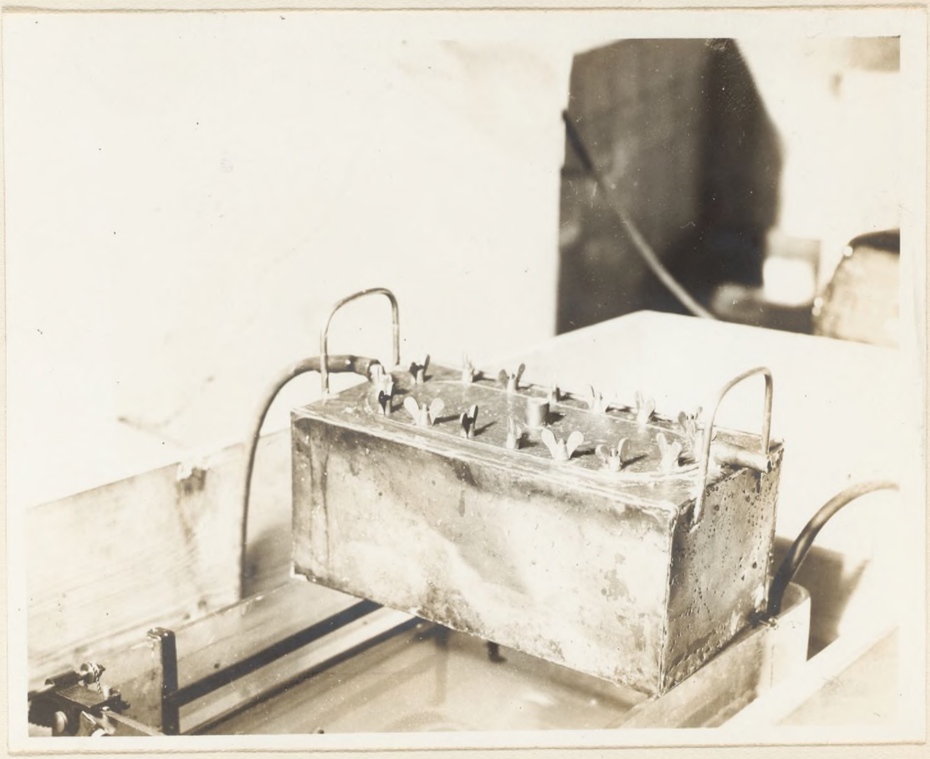


Fig. 25. Galeotti's respiration calorimeter.

This photograph shows the chamber outside of the water with the thumb screws to keep the lid in place and make it air- and water-tight.

Fig. 26. Part of the respiration chamber devised by Galeotti in Naples.

This shows the tank and the small calorimeter which is immersed in water, the handles alone showing above the water. The apparatus has recently been used for an experiment on fever in rabbits; the report of the research appeared in the Biochemische Zeitschrift about the middle of February 1914.

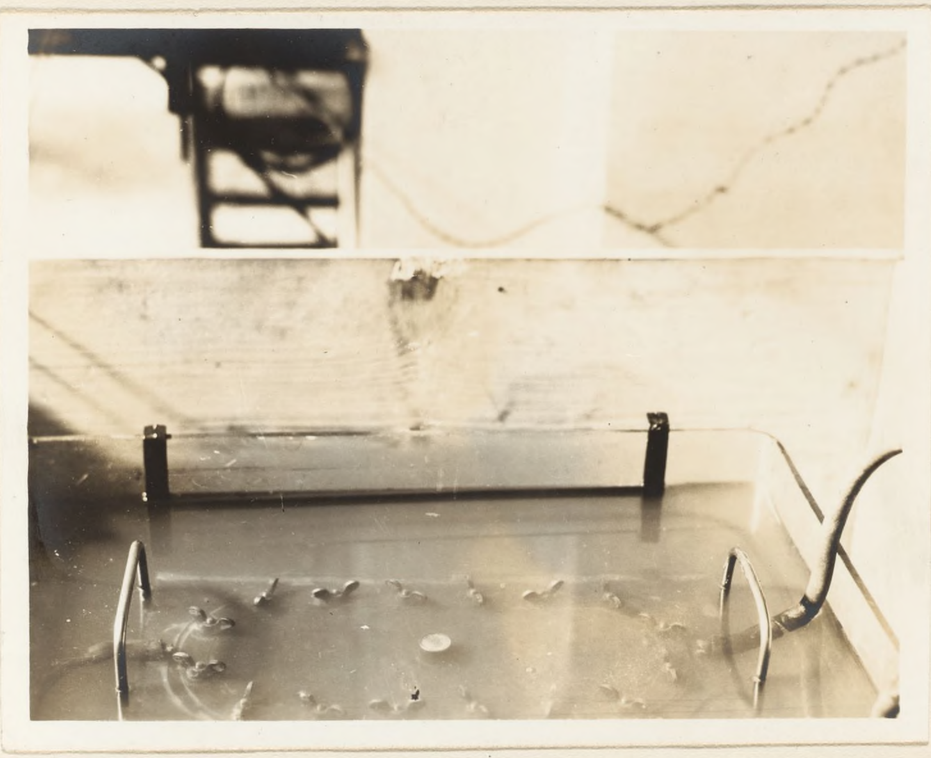


Fig. 27. Bath with Galeotti's respiration calorimeter immersed in the water tank.

Note the wing nuts holding the calorimeter in place.

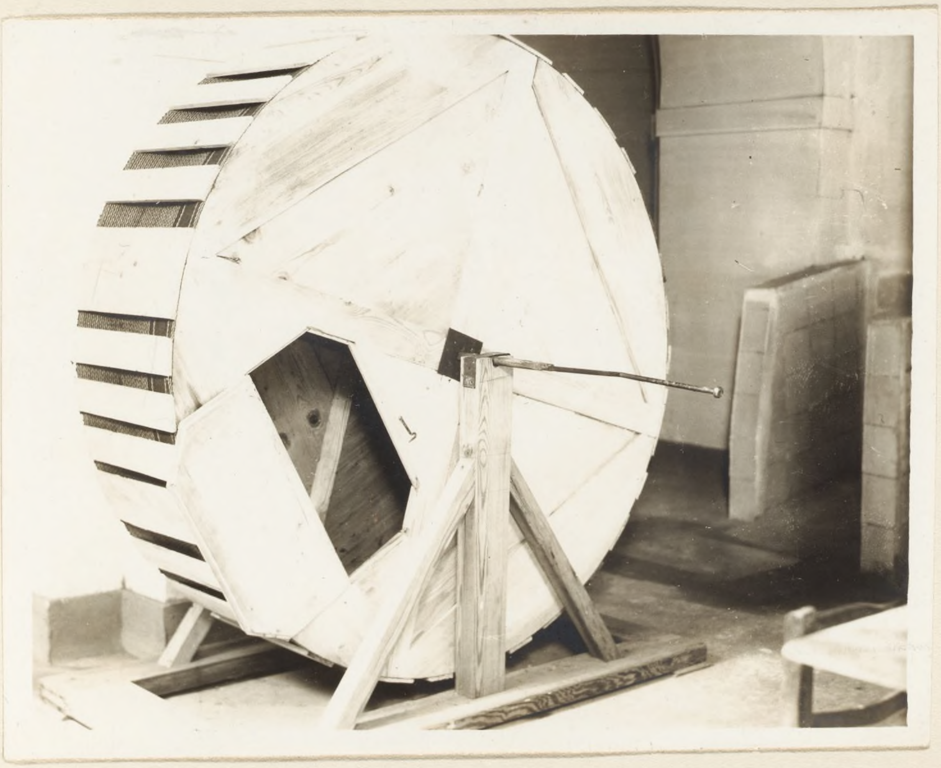


Fig. 28. Galeotti's dog treadmill.

The dog is placed inside; the handle is then turned and the dog walks round.

small resources.

What interested me particularly in Professor Galeotti was his proposed expedition to Monte Rosa in the summer of 1913. They expected to study the alcohol problem in considerable detail, considering the effect of alcohol upon work, etc. All of the men in the Monte Rosa party were total abstainers. As we were to study the alcohol problem the following winter in the Nutrition Laboratory, I arranged to have Mr. Higgins go to Europe and work under Galeotti's direction, so as to acquire considerable information regarding the technique of the alcohol experiments and obtain a number of suggestions. In this plan the potentialities of the work in America were not overlooked.

Galeotti was much interested in a method for getting blood pressure, using an apparatus called the oscillometer sphygmometrique of Professor Pachon, constructed by Ch. Verdin, No. 7, rue Linne, Paris. Although this apparatus interested me, I did not feel it advisable to order it for the Laboratory without further testing.

Galeotti speaks English very well, is a polished gentleman, and has really a most attractive personality. He is, of course, very well acquainted with all the Italian physiologists. He told me that Professor Fano is now such a public man and is so occupied with things of state that he is no longer working much in physiology. He has worked very hard and is anxious to rest. Galeotti pointed out that in Italy it is very difficult to work hard all of the time on account of the climate which is very enervating. They find themselves tired out by spring, and believe that a man must relax then.

Professor Galeotti and his laboratory impressed me very favorably. On the other hand, I saw there an experiment with a dog in which a study was being made of the volume of the respiration after a removal of part of the lungs. This experiment gave me a very bad impression; I cannot see how a man of Professor Galeotti's scientific attainments could have allowed a research to be carried out in so slovenly and careless a manner. Otherwise my impression of Galeotti and his work was very favorable. He is a man of keen thought and wide interests, and evidently has a good command of literature outside of the Italian. He is young and enthusiastic and should be the leading physiologist in Italy.

ROME.

University of Rome.

Professor Luciani.

The main object of my visit to Rome was to discuss in detail with Professor Luciani the experiments in the Nutrition Laboratory on the fasting man, Levanzin. Professor Luciani (see figs. 29, 30, 31 and 32) made the historic research on the fasting Italian, Succi, and his report of it has been translated into German, and widely read. I did not expect much in the way of apparatus, general equipment, or active work in metabolism along our line. As a matter of fact, there is nothing. The work in Rome rests upon its past laurels, although they are soon to have a new physiological institute. Professor Luciani tells me they have raised 2,000,000 francs for the building alone, not including equipment, and it will be begun in a short time. Professor Luciani is, however, somewhat pessimistic on the subject and thinks he will not live to see it finished, as he is now seventy-one years old. The present laboratory is an absolute disgrace to a large university. One can hardly imagine worse working accomodations. It is quite easy to understand why Professor Luciani has had no interest in active experimental work for years past. In connection with the new institute which is now projected, and for which the funds have been raised, Professor Luciani and his associate, Dr. Baglioni, studied the plans of all the other physiological institutes now in existence and decided to make theirs one-half larger than any existing institution for physiology in Europe.

Many things of interest are in the present laboratory. For

Fig. 29. Professor Luciani at his desk in his study in Rome.

Fig. 30. Professor Luciani looking over the last edition of his "Human Physiology" in his study in Rome.

Fig. 31. Professor Fano and Professor Luciani in Professor Luciani's library in Rome.

Fig. 32. Another view of Professor Fano and Professor Luciani in Professor Luciani's library in Rome.

example, one finds there the old collection of embryos, first started by Moleschott, very unsuitably presented and unsuitably housed. There is also an excellent collection of framed photographs of the various physiologists. I obtained an interesting view from the window of Professor Luciani's study, showing the excavations now in progress for a large government building. In these excavations they are continually finding old Roman houses, and one sees here the treasures covered up in the centuries of dust and dirt of Rome. Inasmuch as this section was the residence section, little interest is taken in these daily excavations, and the material is carted off without reference to its historic interest.

One noticeable feature in Luciani's laboratory is that over every door is a Latin inscription, which has been written by Professor Luciani. Baglioni says that these are much copied by visitors. For example, over the door of Luciani's study is placed the inscription "In hoc studiorum deversorio loci praeses libenter acquiescit."

The fact that Professor Luciani spoke very little French and German made it extremely difficult for me to carry on a conversation with him. On the other hand, Professor Baglioni, who was a very kind and willing interpreter, spoke excellent German, and in that way I was able to convey my meaning to Professor Luciani through him.

Professor Luciani has an excellent library but apparently, in spite of the fact that he is writing continuously, it is not used. I had a distinct impression that much material outside of Italian literature had escaped him, although I found a whole set of the American Journal of Physiology there. For example, in discussing the Chittenden low-protein ration, I told him that I had written a criticism in the American Journal of Physiology. He stepped into the library and brought in the volume.

He was quite chagrined to think that he had not seen this criticism of Chittenden before finishing his last edition of the Physiology of Man, although it was almost within arm's length of his desk.

Luciani told me that not long ago a young Italian, a Dr. Fichera, aroused considerable interest by his theories and experiments. This investigator argued that possibly cancer arose from the foetal condition, i.e., was inborn, and reasoned therefrom that there was in the foetus probably an anti-body. All cancer cells being embryonical, he accordingly made extracts of the human foetus on the autolytic idea and injected this extract with, for a time, very promising results. Recently, however, he had had remissions with return of the cancer, so that he was obliged to renounce this theory as being false or not proved. As palliative treatment, he used radium and Sonnentherapie Finsen for cutaneous lupus, etc., cutaneous tuberculosis, also fulcurations. Dr. Ghilarducci likewise uses this in skin and vagina cases. Pancreatin "einspritzung" was also used but no promising results were obtained, only a "Verbesserung".

I asked Professor Luciani as to his opinion regarding the authenticity of the 50-day fast of Merlatti. He was very certain that the fast was genuine, for the man was nearly dead at the end of fifty days and a commission watched him continously. While the public report was a diary rather than a scientific exposition, certain data looked satisfactory to Professor Luciani, particularly the loss of body weight. The book was published under the title "La Mort du Faim" and is the book referred to in the German translation of Luciani's book on fasting. Since I know that Professor Luciani is such an ingenuous gentleman, I am wondering just how much reliance to place upon the genuineness of this fast. I fear his judgment is not very final.

Inasmuch as I wished to report accurately in the new fasting book the several fasts made by Succi, I asked Professor Luciani to write to him and get for me the list of his fasts, particularly those subsequent to his own experiment with Succi. For some reason or other Luciani was disinclined. He said it would be much better for me to write to Succi directly, and I wrote. As a matter of fact, the letter came back, although I addressed it, as Luciani told me, to "Succi, the famous faster". In discussing the Levanzin experiment Luciani was very much distressed about the matter and wished me to let him write to Mrs. Levanzin, saying that I had been in Rome and told him how Levanzin acted. He wished to tell Mrs. Levanzin that I was sorry that Levanzin felt as he did, and that for the benefit of science he ought to get in touch with us before he left Boston. I refused absolutely to let Professor Luciani do this, for I thought it unwise, if not, indeed, dangerous to have anything more to do with this irrational individual.

Both Luciani and Baglioni told me that no metabolism work was being done in Rome by anybody. There was no apparatus for doing such work. Formerly Dr. Aggazzotti was working with a Zuntz apparatus, but he had recently been called to Pavia and took the apparatus with him. Professor Albertoni in Bologna was also interested in the statistical study of the nitrogen of food, and studied all of his problems from the analyses of food and ingestion, but did nothing with alcohol. He has added nothing new.

In discussing the alcohol program Baglioni says that there is an informal movement on foot among the younger psychologists in Rome against alcoholism. This was not led by the regular anti-alcohol propagandists (they are ever present) but by a group of psychologists, Tamburini, Mondesano and Giamelli, students of Tamburini, also Sergi and Mingazzini of Rome, the latter being the head of the insane hospital. This movement really comes under neuropathology. It is directed especially against excessive alcoholism and not against moderate drinking. In Italy one speaks of alcohol and wine. In this sense alcohol means strong spirits, wine the lighter. Baglioni, the physiologist, drinks a half liter of wine per day but never in the daytime. He always takes it with the last meal of the day.

General impression of Italian laboratories.

I had hoped in this visit to Italy to see Mosso's former laboratory in Turin, but was unable to make connections with either Foa or Aggazzotti in that city so did not go there. Neither was I able to go to Pavia, for I learned too late that considerable work had been done there that I should see. In Florence and Rome, and, indeed, in Naples there is hardly enough in our particular line to justify our going there again soon. Galeotti in Naples is unquestionably a very bright and coming man and some of his work will bear most careful attention.

In general, the work in the Italian laboratories did not impress me as being serious or earnest. I feel that I must have missed a great deal by not going to Pavia or to Turin, particularly so after what Mr. Higgins tells me of his experience on Monte Rosa. It was particularly fortunate that I was able to put Mr. Higgins in touch with the most active Italian workers at Monte Rosa for in that way I think we now have a fair estimate of all of the Italian investigators. Certainly, through Galeotti and by correspondence with Fano and Aggazzotti, we should be able to keep much more intimately in touch with the Italian work than ever before. Already I have been able to secure many helpful suggestions and reprints through correspondence with Dr. Fano.

BUDAPEST, AUSTRIA-HUNGARY

Professor Tangl

Perhaps no one individual in Europe has made so profound an impression upon me for his capacity to do an enormous amount of work, and good work, as has Professor Tangl of Budapest. As the director of two separate institutes, one in pathology in the University Medical School and the other in agriculture, Tangl seems to be utterly tireless. He has collected about him a group of very keen, active men, also some very conscientious and hard-working assistants, so that he has been able to accomplish marvels. Furthermore he apparently has the confidence of the authorities and financial backers of research in Austria-Hungary, as his institute in Buda, the Agricultural Institute, is one of the best equipped in Europe, and his laboratory in the Medical School on the other side of the river, while not in a modern, first-class building, is very well equipped with all sorts of apparatus.

Tangl's extensive contributions, which frequently fill entire volumes of the Biochemische Zeitschrift, have certainly attracted a great deal of attention and for the most part have been very well received. Some of the contributions from his laboratory, however, particularly the research by Alexander on the brain, have not been especially well thought of. Indeed, I have had certain doubts with regard to his experiments on curare. It therefore seemed to me important to get in active touch with Tangl. Furthermore I wondered whether it would not be possible for us to obtain through his laboratory some man well trained and versed in his technique to come to the Nutrition Laboratory, either as Research Associate for a year or possibly for permanent appointment. Having all these things in mind, I was particularly interested in making this tour to Budapest.

The Agricultural Institute in Buda.

My time in Budapest was about evenly divided between the Agricultural Institute and the Medical School. In the Agricultural Institute is the large respiration chamber (not calorimeter) with Blakeslee meter pump, recently designed for use with horses, which is an exact duplicate of the one possessed by Professor Armsby in the Pennsylvania State College. This apparatus apparently had not been in use for some time, and I doubt if any respiration experiments of a satisfactory nature have ever been made with it. Certainly determinations of oxygen are out of the question.

At the Agricultural Institute I also saw a small modification of the Regnault-Reiset apparatus, including the sulphuric-acid absorbers and soda-lime cans used by us. These are shown in figures 33 and 34. The photographs are rather indistinct as they had to be taken very late in the afternoon on a dull day and the room was poorly lighted. Nevertheless they show clearly the Crowell blower, the motor, the soda-lime cans, and the sulphuric-acid cans.

Professor Tangl has already described the apparatus, which was built for experiments with swine. I found it in use while I was there, the experiments being in charge of Dr. Weiser and an assistant whose name I have unfortunately forgotten.

Dr. Weiser had been working on rachitic pigs. They were fed corn and dry blood, but no calcium salts. As controls Dr. Weiser had other animals fed with calcium salts. For his experiments he was able to secure animals 7 months old weighing 7 kilograms, and control animals 7 months old weighing 50 kilograms. An analysis of the bones of the animals fed on a diet free from calcium salts showed plenty of magnesium but little calcium. All of the bones were soft and broke up easily in the hand.

Fig. 33. The absorbing train used with Tangl's respiration apparatus for swine.

The respiration chamber is shown very indistinctly in the rear, but the white porcelain, sulphuric-acid vessels may be seen clearly. The soda-lime cans are set on end and covered with glass plates. The electric motor with the Crowell blower is in the foreground.

Fig. 34. Another view of the absorbing train used with Tangl's respiration apparatus showing the sulphuric-acid containers and the soda-lime cans.

It should be noted that glass connections and rubber stoppers are used with the sulphuric-acid bottles instead of our usual metal couplings.

It made me feel quite at home to see the Crowell blower and the porcelain sulphuric-acid absorbers so well known to us in our laboratory. The soda-lime cans had been extensively modified by Professor Tangl, for although he uses a can of about the same size as ours, the intake tube enters near the bottom and the outlet tube leaves near the top, the top being closed by a glass plate held in place by a rubber gasket and clamp. Personally I see no advantage in these changes.

Instead of using metal tubes for connecting the porcelain sulphuric-acid containers, as we do in our laboratory, Tangl has substituted large rubber stoppers and glass connectors. The absorbers were connected to the rest of the system by means of rubber tubes which were carefully wired into place each time they were used. At the end of each period it was necessary to deflect the air-current and unwire all these connections in order to weigh the absorbers. The weighings were made on a very expensive agate-bearing balance. Each time the weights were taken, the absorbers or the oxygen cylinder had to be lifted and placed upon the balance stand.

Samples of air were drawn from the chamber through a long lead pipe by means of a lowered mercury pipette, the principle being exactly the same as that used by Staehelin in the large apparatus in Berlin. The apparatus in Tangl's laboratory, which was devised by Junkunc, was apparently made either <u>simultaneously with</u> or <u>prior to</u> the description of the Staehlin apparatus and is an independent discovery. It embodies the principle of lowering on a worm or threaded screw a bath of mercury in which a bell-jar is immersed. As the mercury is lowered, air is drawn into the bell-jar. It was very obvious to me that the tube leading from the point where the samples are taken to the lowered mercury bell is altogether too long, as there must be a very large amount of air residual in the tube to affect each experiment.

No spirometer is attached to the apparatus, but there is a dial manometer and there must be a slightly diminished pressure all the time. When there is a leak, of course nitrogen leaks into the apparatus, the amount being determined by air analysis. The Crowell blower is used on the principle of suction rather than as a blower. If there should be outward pressure on the glass plate used on top of the soda-lime can it would have to be clamped down. As a matter of fact, the glass plate is held in place with wax, assisted in part by the suction. Tangl finds the same difficulty that we do with the automatic reduction valves on the oxygen cylinders.

A ventilating fan inside the chamber is started 5 minutes before the end of each period to give equal temperatures every hour. The average temperature of the chamber is determined with a resistance thermometer, the records being used in calculating the oxygen. It is rather interesting to note that while we are inclined to measure the carbon-dioxide production in short periods and the oxygen consumption in long periods, Tangl reverses this principle and determines the carbon dioxide in 12-hour periods and the oxygen in 1-hour periods.

The animals were allowed to move around in the chamber, graphic records of the major movements being obtained by means of a very ingenious arrangement devised by Professor Tangl in which a tambour pointer actuates the delicate clockwork of a dismantled anemometer. At the time I was there, the apparatus was not functioning perfectly although the principle was very simple and plain.

A smaller Regnault-Reiset closed-circuit apparatus, constructed mostly of glass parts, was used in connection with a metabolism experiment on a duck. The bird was placed inside of a small bell-jar which was completely immersed in a tank of water (figs. 35 and 36). As a matter of fact, the

Fig. 35. Closed-circuit respiration apparatus used in Tangl's laboratory in Budapest for studying the metabolism of ducks.

The duck is placed in a bell-jar which is immersed in a tank of water, formerly a part of a Zuntz apparatus used for studying the respiratory exchange of fish. The bell-jar is connected with a series of absorbing vessels and a blower at the left and with a large pipette and an oxygen bomb on the right. The pipette has two chambers, each with a capacity of 500 c.c., and is also immersed in water.

Fig. 36. Another view of the apparatus used in Tangl's laboratory at Budapest for studying the respiratory exchange of ducks.

In this photograph may be seen the soda-lime vessels, the sulphuric-acid bottles, the small rotary pump on the wall above them, and at the right the bell-jar immersed in water.

bell-jar and tank were the remains of an old Zuntz apparatus for determining the respiratory exchange of fishes. By connecting a blower with a series of caustic-potash and sulphuric- acid bottles, Tangl has succeeded in constructing a closed-circuit apparatus with which he can determine both carbon dioxide and oxygen. The air in the bell-jar is analyzed at the beginning and end of the experiment. If there is no change in the nitrogen content of the air, it is assumed that the apparatus is tight.

A Russian, who had been working at the Agricultural Institute, had devised a scheme for separating the feces and urine of a duck very easily. Large rubber finger-cots were attached by means of a wire triangle over the anus and over the surgically made opening for the urine, and the duck was apparently not in any way incommoded by wearing this arrangement. (See figures 37 and 38.)

In speaking of the possibility of studying the effect of fattening upon animals, Tangl said that a hen could be fattened in 14 days. The importance of studying the respiratory exchange during fattening, and particularly the calorific equivalent of oxygen during fattening, has frequently been brought to my attention. I remember years ago, especially after the earlier fasting experiments, I felt that in addition to the experiments on fasting in which there was no formation of fat, we ought, if possible, to secure experimental periods when there was formation of fat with a minimum amount of destruction. Professor Tangl has frequently spoken of this, as have others, and it is evidently a point that the Nutrition Laboratory should take up shortly, as we have the calorimeters for this work at our disposal. At present, however, it seems to me that we can do only one of two things,--experiment with a large number of geese or hens inside the bed calorimeter, or wait until a smaller form of calorimeter has been successfully constructed

Fig. 37. Scheme for separating the urine and feces of a duck.

This photograph was taken at Tangl's laboratory in Budapest and shows clearly the rubber containers attached to the duck to separate urine and feces.

Fig. 38. Another view showing the scheme used in Tangl's laboratory in Budapest for separating the urine and feces of a duck.

and tested, such as, for example, a calorimeter for infants or dogs. It seems highly probable that before the winter of 1914 is over we shall be in a position to study the interesting problem of the calorific equivalent of oxygen during the period when carbohydrate is being actively converted into fat.

University Medical School (The Institute of Pathology) in Pest.

The laboratories of the Institute of Pathology, although in a building by no means modern, are completely equipped with apparatus for research. A relatively large number of men were working there, all of whom seemed keen and enthusiastic about their work. With such men as Dr. Hàri and Dr. Verzàr at one's right hand, a large amount of work can be accomplished. In this institute Professor Tangl has recently been developing two forms of calorimeters, a micro-calorimeter for very small animals and a respiration calorimeter for dogs. On the afternoon of the first day that I was at the laboratory Professor Tangl gave a lecture to the members of his staff, and for my benefit spoke in German. In this lecture he explained the principle of his two new calorimeters.

The calorimeter for very small animals is a differential calorimeter, closed at the front end by a glass plate, and fitted with two small Dewar flasks, each very carefully insulated by eiderdown. The inner sides of the Dewar flasks are blackened and the outsides are silvered. An air-tight cylindrical vessel, made of copper 0.1 or 0.2 millimeters thick, is fitted into each Dewar flask, thus forming two chambers. Constantan-copper junctions are used, being very large wires with small resistance.

The animal is placed in one of the copper chambers, confined in a wire cage to avoid contact with the walls. The bottom of this chamber is made of sheet copper to hold the urine and feces, and contains a small amount of paraffine oil which floats on top of the urine collected, thus preventing evaporation. The second chamber is provided with a control cage of the same size as that holding the animal, i.e., 120 grams, and also with a resistance. In this duplicate chamber heat is developed electrically. The heat capacity of the two chambers is not exactly the same, but the difference can be established by calibration. With frogs, leeches, and a crocodile the experiments were ideal, because a compensation could be made very easily, but with mice or rats it was very difficult owing to their activity.

Very small Williams bottles are used with this calorimeter, 12 centimeters high over all and 50 millimeters in diameter. They were made in Tangl's laboratory and are shown in figure 39. Pocket flash lights are employed for reading the thermometers, a battery being attached to the flash light which is placed near that part of the thermometer stem on which the mercury is to be read.

The galvanometer used with both of Tangl's calorimeters is a so-called Brocasch galvanometer from Cambridge. To prevent extraneous currents from affecting the instrument it is placed inside of three iron pipes, the smallest of which is 24 centimeters in outside diameter, the next 28 centimeters, and the largest 38 centimeters. The walls of the pipes are each 4 millimeters thick and there is a slit cut in one side to allow the galvanometer deflections to show through. It may thus be called an iron-clad or armor-clad galvanometer. It was said to be very satisfactory. A similar galvanometer was observed and discussed in connection with the laboratory of Professor Cremer in Berlin.

A detailed description of the micro-calorimeter has already been published. (See Biochemische Zeitschrift, 1913, 53, p..21.) Although A. V. Hill has recently described a calorimeter with Dewar flasks, Tangl's was evidently constructed some time before and doubtless

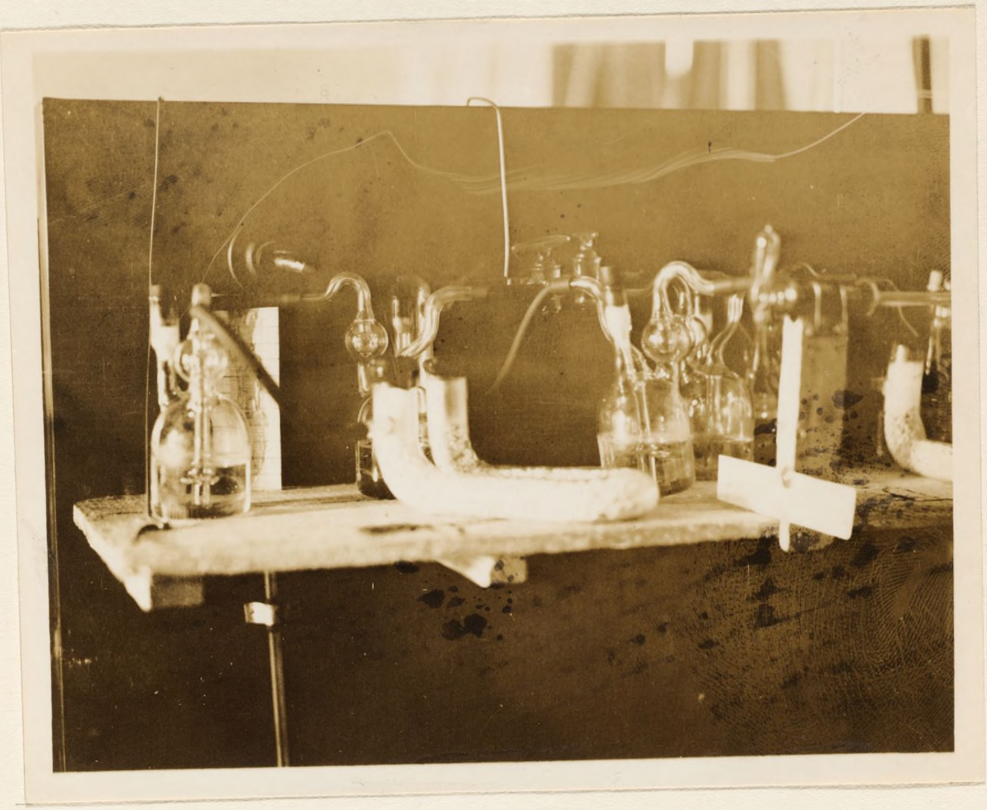


Fig. 39. Very small soda-lime tubes and Williams bottles used by Tangl in Budapest with his micro-calorimeter for small alligators.

has the priority. Furthermore Hill uses a mathematical formula for computing the heat.

With this micro-calorimeter Tangl has made interesting experiments on leeches, both during fasting and after they had taken blood. He found that there was a 200 per cent increase in the heat production after they had taken blood, which we may call either "Verdauungsarbeit" or "Stoffwechselsarbeit". He concluded that the work of the kidneys amounted to about 10 per cent of the "nüchtern" work of the animal.

In discussing the insulation of this calorimeter he said that eiderdown was a better insulator than the Dewar flask. He pointed out the fact that ducks have to stand temperatures of minus 40° C., and the difference between -40° C., the environmental temperature, and the body temperature of the duck, which is +40° C., calls for very perfect insulation.

A second electrical compensation calorimeter large enough for dogs was in process of development. I took several photographs of it. (See figures 40 and 41.) It appeared to be very large for the size of the dog that was inside the chamber as I photographed it. The apparatus has not yet been described (January 15, 1916).

One night at my hotel I drew up notes regarding Tangl's micro-calorimeter as follows:

1. A calorimeter with constant radiation conditions. This calorimeter is essentially the emission calorimeter of Chauveau, and is also somewhat similar to that of Hasselbalch and Bohr and the radiation calorimeter of Tissot, which I saw recently when in Paris. In all of these radiation calorimeters, or emission calorimeters, there was always a large radiation surface.

2. With Professor Tangl's calorimeter the insulation is perfect; in fact, it is too perfect. Practically all the radiation, or at

Fig. 40. Front end of Tangl's respiration calorimeter for dogs in Budapest.

This photograph shows the front of the apparatus removed. The dog is in one chamber and the electric control in the second chamber.

Fig. 41. Another view of Tangl's new calorimeter for dogs in Budapest, showing the front opening.

least the greater part of the radiation, is through a glass plate in the front of the calorimeter, or through metal pipes through which the air-current passes.

3. There is no temperature rise inside the chamber. This is controlled by the rate of ventilation, which affects (a) the heat brought out by the warm air and (b) the heat of vaporization of water.

4. This is really a radiation calorimeter with a very poor radiation surface, that is, the glass plate, which is admittedly poorly insulated and removable, presents the greatest possibility for a change in the radiation constant.

5. I showed Professor Tangl a design for a calorimeter for the Nutrition Laboratory which has not been worked out as yet, but which is based upon the Bohr-Hasselbalch principle in which an electric fan blows air over two calorimeters, side by side, connected by a thermal-junction.

6, I pointed out the difficulty of developing a normal current in the compensating part of the calorimeter, using a formula A x E x t x 0.2378. As I told Professor Tangl, you can get a recording voltmeter, a recording ammeter, and a recording wattmeter, but the errors will all be greater than 1 per cent. As an alternative both instruments could be read every 4 minutes. I then reminded him of the silver voltmeter designed by Lehmann, which is automatic; but this is not yet completed. I also spoke to him of the scheme of Mr. Lange of the Nutrition Laboratory who has proposed developing under adiabatic conditions a small part of the heat by developing it in a Dewar flask by an electric current. In this flask one can read the temperature to 0.001° C. By using a non-volatile liquid and determining its specific heat, it is possible to measure any amount of heat. Thus one could develop 1 per cent of the total heat admitted to the compensating chamber by a subject, or

50 per cent of the heat or any fraction. We can also adjust the external temperature automatically by raising it to correspond to the internal temperature.

Mr. Lange's scheme has not as yet been tried out, but it interested Professor Tangl very much indeed. Since this discussion with Professor Tangl I have succeeded in securing two integrating wattmeters which we are hoping will serve our purpose admirably in the calorimeter.

In connection with both of Tangl's calorimeters I found an electrical control apparatus which was designed for controlling fluctuations in external voltage. I secured a model of this apparatus for the Nutrition Laboratory, but it has not as yet been tested. (See figure 42.)

In the Institute for Pathology I found a number of minor researches in progress. I was shown, for example, the interesting method of using suction stomach pumps provided with a valve. This method has also been described in the Biochemische Zeitschrift ~~also~~. Tangl was likewise doing a lot of work on the stomach with X-rays, studying the effect of narcotics. It was found that narcotics retarded the passage of food from the stomach; on the other hand, the activity of the stomach was not much decreased; it was affected to the greatest extent by ether, less by morphine, and still less by magnesium sulphate. Professor Durig of Vienna greatly doubts the accuracy of this work.

The amount of blood leaving a certain artery was measured by its passage along the tube in somewhat the way that Starling measures it but I was unfavorably impressed by the work. It seemed to me quite careless and not up to the standard of the rest of Tangl's research.

Dr. Verzár showed me the work they had been doing with micro-titration, using the apparatus of Emmich of Graz, who has written a book on micro-chemical methods. For example, they are studying the

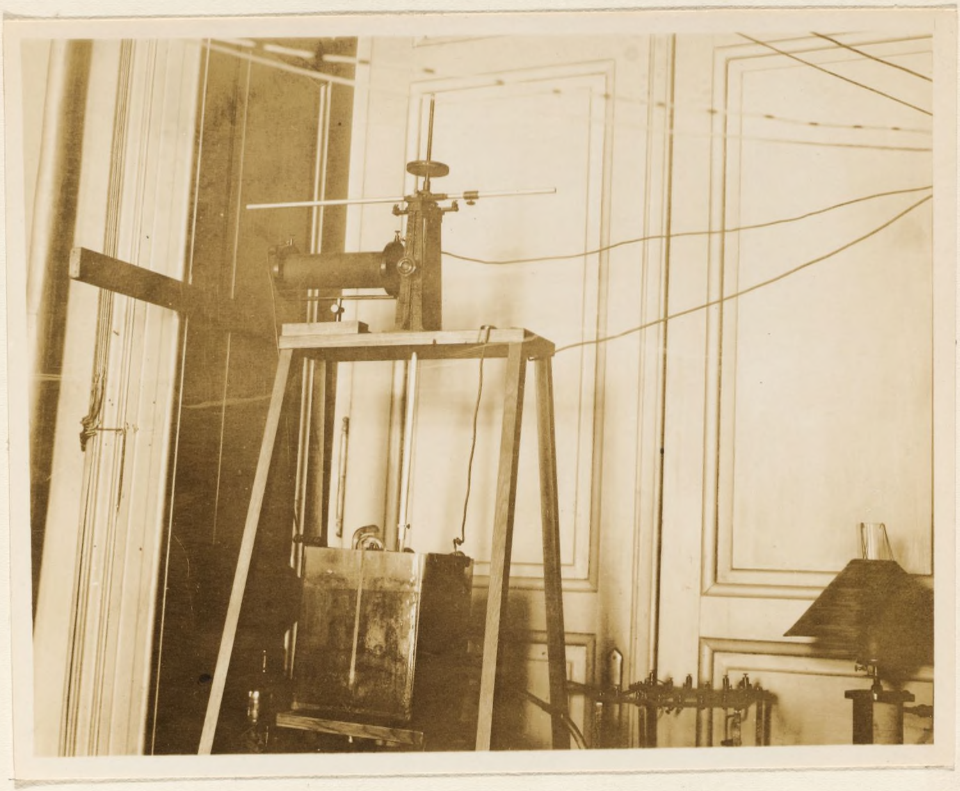


Fig. 42. Electrical control apparatus used in connection with Tangl's calorimeters in Budapest.

The apparatus regulates the city current and the result is most satisfactory.

amount of chlorine in blood serum, and found they could work with an accuracy of 0.1 per cent, using 1 c.c. of the solution. I ordered one of these titration bulbs for use in the Nutrition Laboratory.

I also saw in this laboratory a dog treadmill (see figures 43 and 44) one of which I secured for our own work with animals.

Curare experiments.

One of the most important objects of my visit to Budapest was to obtain more definite information regarding the long series of experiments with dogs that Tangl and his co-workers have been making on the effect of curare. The artificial ventilation apparatus of Hans Meyer was used (the intake of the pump being from outdoors) and the air expired by the curarized dog was carried through an Elster meter to a Zuntz-Geppert gas analysis apparatus. The dog they were working upon had had the pancreas completely extirpated 13 days before and had severe diabetes with sugar in the urine. No narcotic was used but the animal was tied to the board, his neck opened, and a Y-shaped tracheal fistula inserted for artificial respiration. One carotid artery was then connected with a blood pressure apparatus (a Hürthle manometer) and the two jugulars used for the injection of a sugar solution of hormone in one and of curarine in the other. A cannula was arranged for introducing the sugar solution, the curarine being injected with a syringe after the dog had been placed in the warm chamber designed by Tangl. (See figures 45 and 46.) Graphic records of the blood pressure were secured on the kymograph, relative values being thus directly shown. Tangl uses phosphorus for absorbing the oxygen and says it works very well indeed. The carbon dioxide is absorbed in 5 minutes, and the oxygen in 10 or 15 minutes, but he still uses the thermo-barometer of Zuntz.

After the dog is placed in the warm chamber, one half hour is allowed to elapse before beginning the first experiment. The rectal

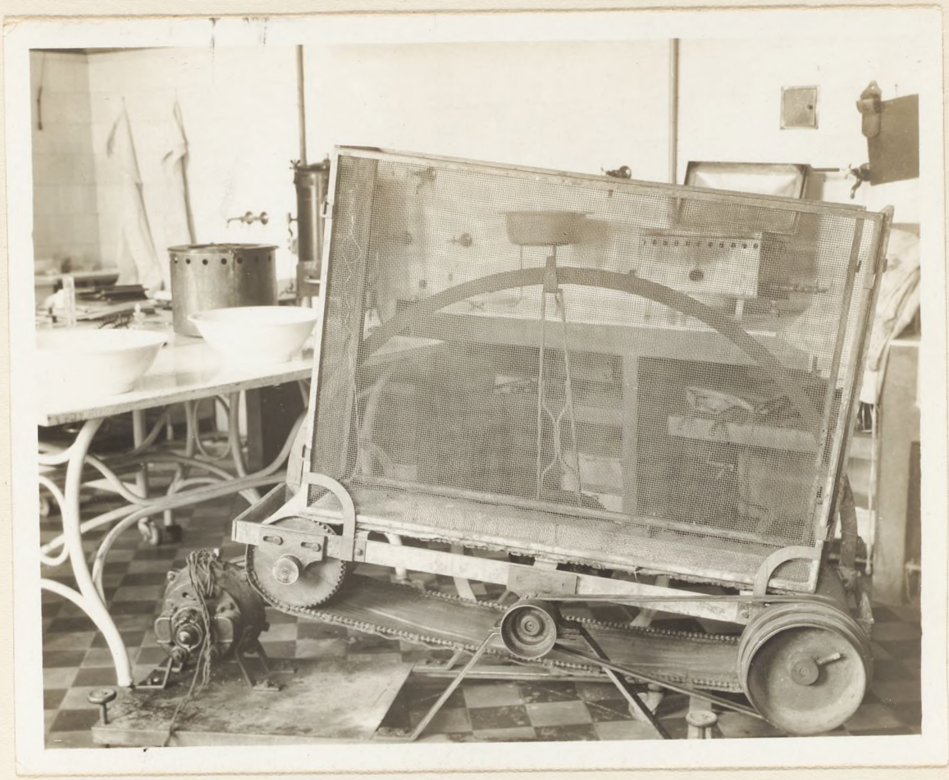


Fig. 43. Treadmill for dogs used in Tangl's laboratory, Budapest.

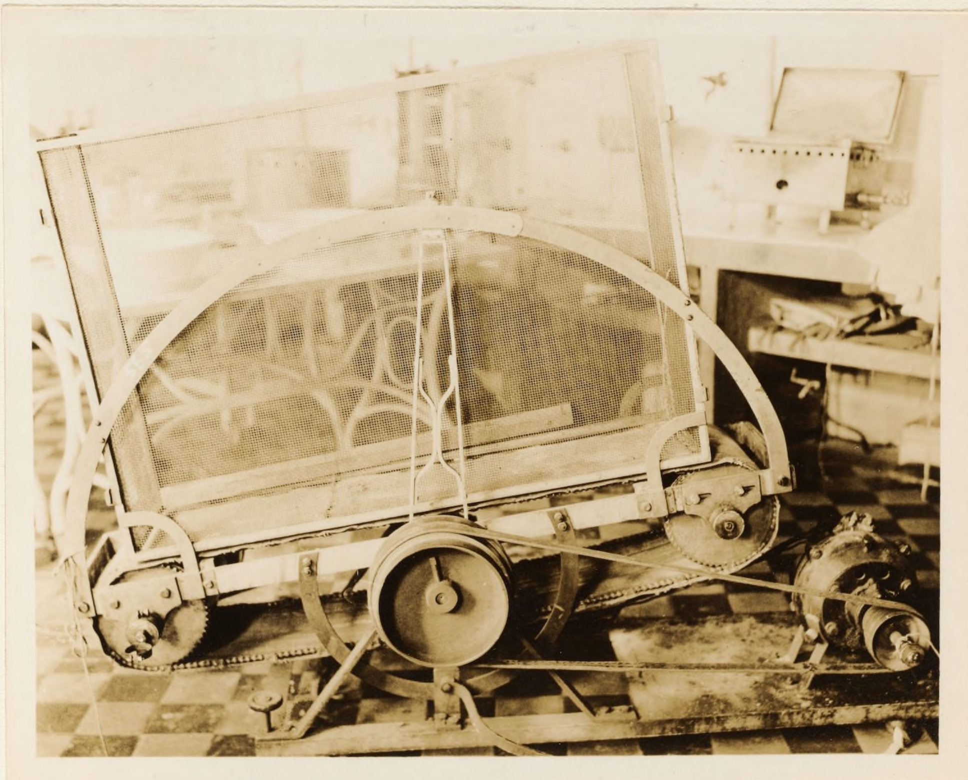


Fig. 44. Another view of the treadmill for dogs in Tangl's laboratory.

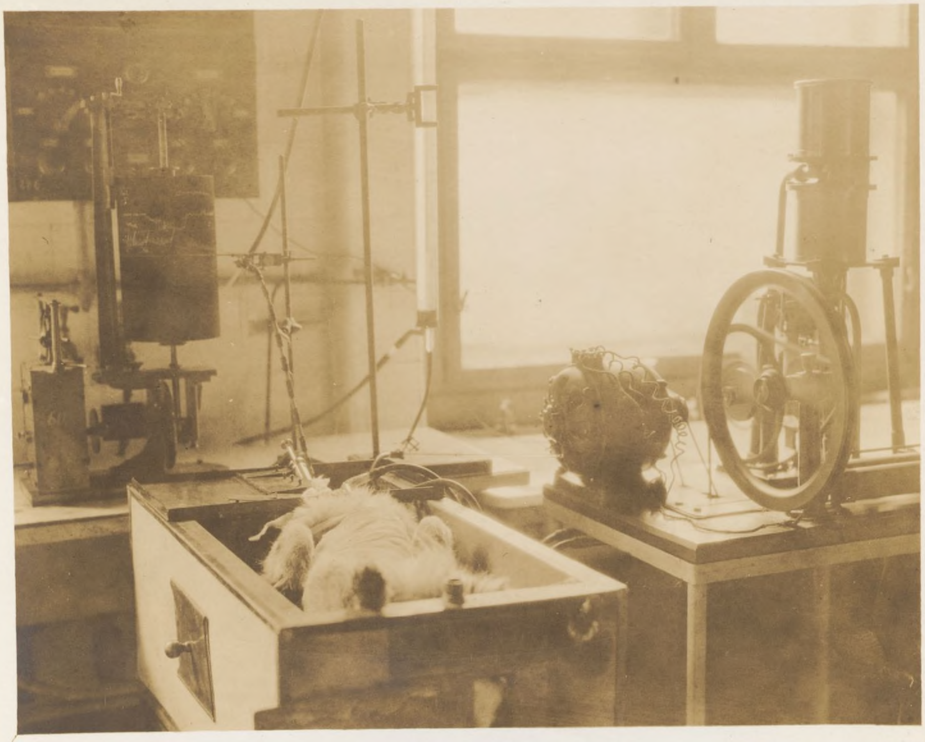


Fig. 45. Artificial respiration apparatus for curare experiments in Tangl's laboratory, Budapest.

The dog in the chamber is under the influence of curare and artificial respiration is brought about by means of a tracheal fistula. At the right in the photograph is the Hans Meyer artificial ventilation pump. The chamber is kept warm automatically in order to prevent loss of heat.

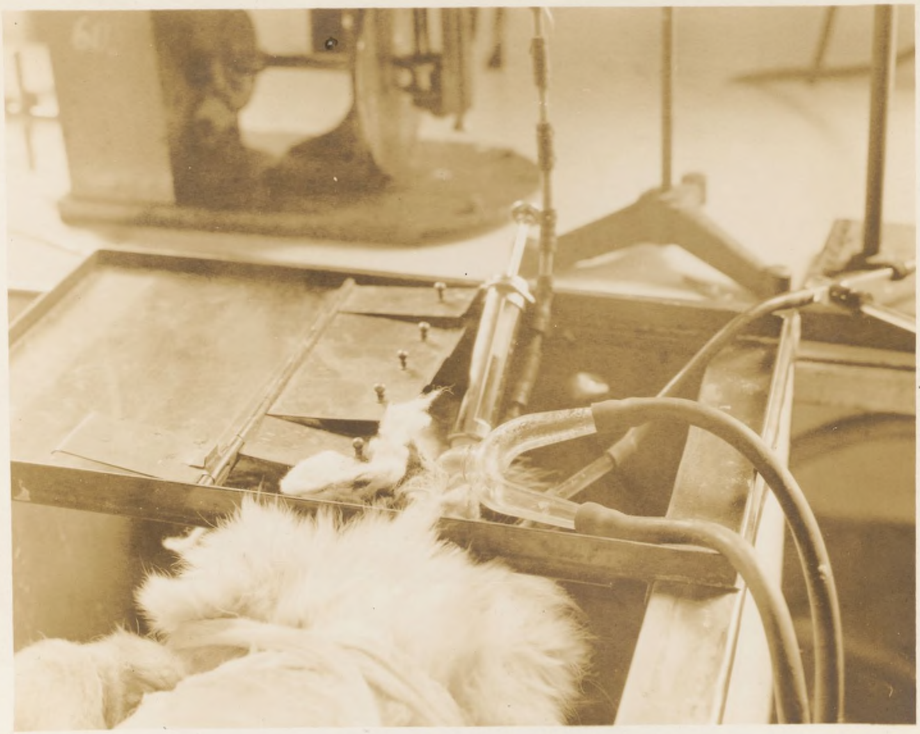


Fig. 46. Another view of the artificial respiration apparatus for curare experiments in Tangl's laboratory, Budapest.

This photograph gives the details of the tracheal fistula. (Experiment of Dr. Verzár.)

temperature, the temperature of the air, and the temperature of the warm bath are all taken. With male dogs a catheter is used, and the urine drops constantly into a small graduate resting on the bottom of the warm chamber. If the nitrogen metabolism is being studied, the bladder is occasionally washed out with boric acid. Dr. Verzár emphasized the fact that under these conditions the animal was not an animal but a preparation. The urine can be analyzed for sugar and in separate periods as often as desired. The curarine solution is injected at the rate of 1 c.c. every 10 to 20 minutes, but to make sure that there will be no movement, 1 to 1½ c.c. are injected about 8 minutes before the experiment begins. The dog can hear and feel pain but is powerless to move. The room should be absolutely quiet.

The analysis was made by Dr. Verzár's co-worker, a student in his eighth semester, who smoked a cigarette continually. I did not think much of this method of making an analysis, although Dr. Verzár assured me that it was most accurate. They used 20.88 per cent oxygen for outdoor air, as determined with their regular apparatus, and considered that this was a constant for the apparatus, but also emphasized the fact that all the values are relative.

As a matter of fact they had never made this particular type of experiment before that morning. They usually gave the hormone and sugar together, but on this day they gave the sugar first, and three hours later the hormone from the fresh pancreas of a dog. I personally saw Dr. Verzár remove the pancreas and was astonished at his skill. The wound is re-dressed every two days, as diabetic dogs are especially subject to infection.

The main object of curarizing the dog is to prevent muscular activity. On the other hand, in discussing with Dr. Hári the absence of muscular activity in respiration experiments inside the respiration chamber, I find he is thoroughly convinced that any activity of the dog is equalized within 24 hours.

The question occurred to me whether in the curare experiments the carbon-dioxide centre of stimulation does not affect the carbon-dioxide exchange and whether it is not a question solely of blood flow plus regular respiratory volume. With the same pressure may there not be a change in the rate of blood flow and consequently an alteration in the amount, if not indeed the quality of the respiratory exchange?

The hormone was prepared by rubbing up the pancreas of the dog with sand and with salt solution. When this is injected the pulse rate increases from 69 to 74. This increase may be due to an increase in the protein combustion. I inquired whether the nitrogen was increased as a result of this but they could not tell me. If the glycogen is gone, one would expect that the dog would lay on glycogen before much was burned, although in this instance when the sugar was burned, there was no change in the respiratory quotient. I asked Dr. Verzár if they had tried repeating the injection, but apparently they had not.

I have a memorandum in my notebook to the effect that I spoke about artificial ventilation. It is a serious question whether or no with artificial ventilation there would not be some disturbance of the respiratory exchange with these dogs. It might possibly be justifiable to assume that the oxygen consumption measured in these experiments represents that actually burned by the dog, but the difficulties incidental to determining the respiratory quotient exactly lead one to question seriously whether one can use such an "animal preparation" to indicate the <u>character</u> of the respiratory exchange. On the other hand I was profoundly impressed by the care and accuracy with which these experiments were made and came away convinced that they were done with masterly technique. It occurred to me also that the blood morphology should be studied during narcosis and during curarization. If the rate of blood flow does not change, one may have changes in the oxygen carrying power due to other than metabolic causes.

In discussing the influence of curare upon dogs, Tangl assured me that normal dogs show no change in metabolism as time goes on. Of course, this must be taken to mean over relatively short periods, because if the dogs were fasting continuously there would be an increase in nitrogen and therefore a change in the respiratory quotient.

The object of the hormone experiment was to see if, after a dog had been depancreatized and could not burn sugar, the injection of hormone or the extract made from the fresh pancreas of a dog would stimulate the burning of sugar. As a matter of fact this particular research has been published recently and I believe the results indicate that there was no increase in burning sugar following the introduction of hormone.

As a small matter of technique I saw again the use of a ground glass plate with the Zuntz-Geppert apparatus, to help in illuminating the burette. (See figure 47.) This glass plate has been used in all of Tangl's laboratories and apparently is very satisfactory.

<u>Professor Tangl's personality</u>.

Professor Tangl is very magnetic, enthusiastic, and versatile. He speaks many languages and has an especially good command of English. His laboratory is therefore an admirable place for Americans or English-speaking people to visit. One can easily see why the authorities, both academic and financial, have so much confidence in him. He is a man who can control a large number of various interests at one and the same time, and who evidently makes a wise expenditure of money.

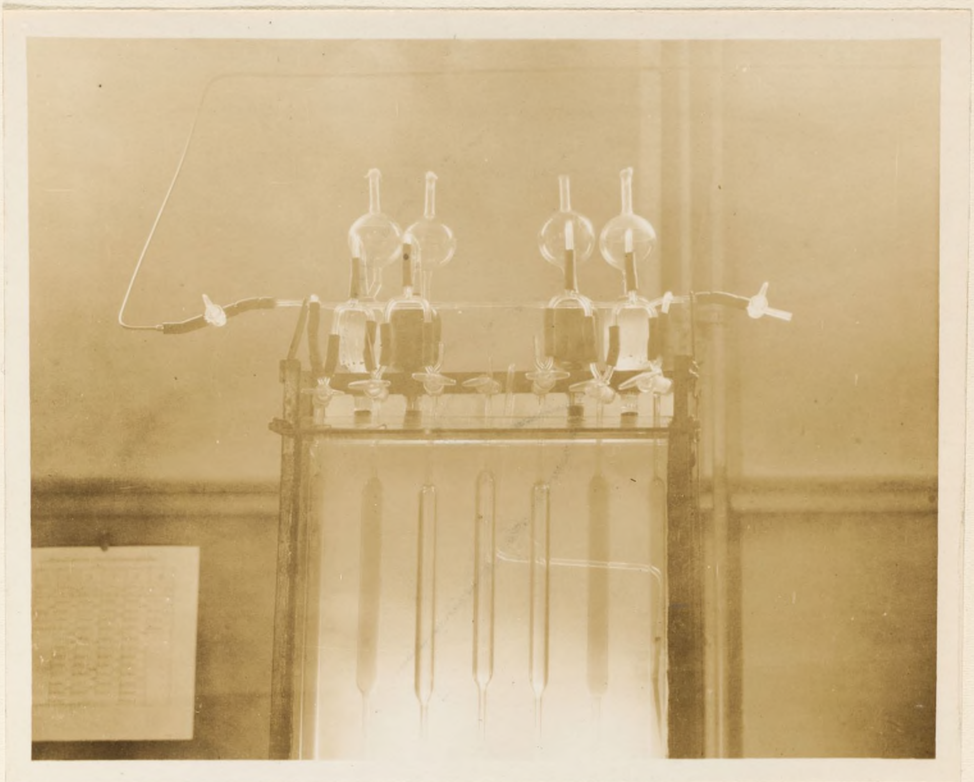


Fig. 47. Details of the glass stop-cock arrangement at the top of the Zuntz gas-analysis apparatus in Tangl's laboratory in Budapest.

Behind the pipettes may be seen the ground-glass screen with electric illumination, so commonly used by Tangl. This is one of the few apparatus in Europe in which ground-glass connections join the top of the burette with the different pipettes.

General impressions of Professor Tangl's laboratory.

The Institute of Pathology is a remarkable institution and by far the best laboratory in Europe for metabolism investigations. In very few laboratories have I noted such complete harmony and such a charming atmosphere as in the laboratory at Budapest. The men are very enthusiastic and, what is more important, are very loyal. For example, Dr. Hári has been with Tangl 10 years and Dr. Verzár I believe even longer. Dr. Hári told me that if a man only shows the "lust" or passion for work Tangl makes every possible opportunity for him, giving him apparatus, assistants and funds. The men all worked together, there were apparently no bitter rivalries, no secrets, and none of the little unpleasantnesses that one sees so frequently in foreign laboratories. Furthermore I heard a minimum amount of adverse criticism of other laboratories here. For example, both Professor Tangl and Dr. Verzár spoke most enthusiastically of Professor Galeotti in Italy. Altogether there is quite a charming atmosphere about the laboratory and one can easily understand how so much work can be done.

Professor Tangl's career has been a varied one. He began his work at the Veterinary School or "Hochschule", then became interested in physiology, and now is working in the field of general pathology. Tangl deplored the fact that visitors very seldom get to Budapest. Many get as far as Vienna, but think that Budapest is a very long distance away when, as a matter of fact, it is only four hours' ride from Vienna. Tangl told me that Professor Armsby of the Pennsylvania State College had been there for two days and that Zuntz had been there two or three times but that no other men visit the laboratory. I personally think that men should go there, as he has a remarkable laboratrry.

Tangl's own men travel considerably, but while they acquire the technique of others they do not give others a chance to acquire theirs. One of the great criticisms of Tangl's laboratory is in regard to the technique, particularly in connection with the curare experiments. Personally I feel that there is too much inbreeding there. The men are practically all Hungarians and naturally loyal to their own country, but there should be more workers from outside and they should receive more ideas from outside. There was one outsider there, an American named Glazer, but he is the only outsider whom I know of who has worked in the laboratory and he has not been particularly successful, I believe.

It was very clear to me that while formerly Berlin, where Zuntz, Rubner, and others are working, might have been considered the centre for metabolism workers in Europe, the work of Tangl and his co-workers has unquestionably given Budapest a prominent place, and if Tangl's activities continue it is certain that Budapest will become the greatest centre in Europe for work on metabolism. It is furthermore a well known fact that agricultural investigations receive much greater consideration in European countries than elsewhere and anything which will further the European agricultural interests is looked upon as a great blessing. This is particularly true in Hungary.

I devoted only four days to this place but on any subsequent tour made by either myself or any of my associates as the representatives of the Nutrition Laboratory it will be necessary and, indeed, profitable to spend more time here.

VIENNA, AUSTRIA.

Hochschule für Bodenkultur.

Professor Arnold Durig.

Since both Professor von Noorden and Professor Durig were supposedly away from Vienna, I planned to make but a short visit to that city, allowing only one Sunday there. On going out to the Hochschule für Bodenkultur to see Professor Durig's laboratory I was very much pleased to learn from one of his assistants that he had come back suddenly from Italy and was in town. I therefore looked him up and spent the afternoon with him.

My conversation with Professor Durig was most illuminating. He told me, for example, that at that time he was carrying out alcohol experiments on a man. The man was saturated, so to speak, with glycogen beforehand, so as to obtain a respiratory quotient of 1.00. He was then given alcohol and in 5 minutes his respiratory quotient became 0.82 and remained there. This work, I think, has subsequently been published.

In discussing the question of the mechanical efficiency of man Durig told me that in computing and correcting for the horizontal component in their experiments they get results approximating those found by Carpenter and myself. At the same time he told me that he had been very much dissatisfied with the gas meter method for measuring the expired air, as he believed that the meters they used actually alowed the air to go by and thereby recorded too small an amount. He was having the work repeated in the laboratory on a treadmill by an assistant who, he thought, was doing very well indeed. As a matter of fact, the data obtained with the treadmill showed that their results should have been lower, and therefore much nearer the values obtained by Carpenter and myself.

Durig had not seen the report by Douglas, Haldane, Henderson, and Schneider on the Pike's Peak expedition, but disapproved of the Haldane method of getting the blood from under the finger-nail, criticizing this procedure very severely. He remarked that he thought the English physiologists were not up to the standard of six years ago.

Durig also told me that he did not believe the experiments made by A. Müller with regard to the nitrogen addition of 210 grams were correct and that Leimdörfer had come to him, Durig, for help in interpreting the figures for the diabetic experiment.

It was a source of satisfaction to me to have Durig speak in the highest terms of the publications from the Nutrition Laboratory. He said that he always felt when he read reports from our laboratory that every point taken up was thoroughly investigated and supplemented by so many experiments as to be firmly established and that he did not recall any instance when they had been overthrown. He emphasized especially the diabetic work.

University of Vienna.

Professor H. H. Meyer.

Professor Meyer had no comments to make on the alcohol program other than to say that he thought it very complete indeed. He warned me that no one in Vienna took Professor Kassowitz's statements about alcohol very seriously. He showed me his artificial respiration apparatus, which is widely used. Indeed I saw this apparatus in Professor Tangl's laboratory and subsequently in London in Professor Starling's laboratory; in London, however, it had not proved very satisfactory as it leaked. Professor Meyer also showed me his operating table made of magnalium. He spoke in the highest terms of the Castagna kymographion which is employed extensively.

A point of special interest to me was a string galvanometer room in the basement. It was distinctly a cellar with no windows of any kind. The walls were painted white and the room was finished off comfortably for experiments with the string galvanometer. Of course, the galvanometer was installed directly upon the ground.

Professor Kassowitz.

By appointment I spent the afternoon in the library of the Gesellschaft der Ärzte with Professor Kassowitz. His first remark was that he was an abstainer and this was the last remark and every third remark throughout the whole discussion. He was possessed with the idea that it is wrong to drink alcohol, and he cited all sorts of evidence to prove his point. He was very critical of Gruber of Munich, whose address given before a scientific Gesellschaft a year or so before was very unfortunate. Kassowitz maintained that alcohol must combine with protoplasm. He compared the human body to a steamship crossing the ocean with several barrels of alcohol as cargo. If the alcohol were burned there would be a lot more heat given out but the steamship would have to be slowed down so that the drafts from the flames would not set it afire. The boat would go slower but would ultimately reach its destination.

Kassowitz was himself no experimenter, but was continually citing the old experiments of Chauveau and also the experiments on isolated hearts, in which work curves were obtained showing a retardation of the heart action with alcohol and an acceleration with sugar. He spoke about experiments which he had made in Durig's laboratory. Durig subsequently told me that Kassowitz came to his laboratory and made experiments with a dog on his treadmill. He gave the dog a great deal of alcohol, in fact, so much that the dog could not stand up, but he put him on the treadmill and the dog lay down and went to sleep. Kassowitz then pointed this out as an instance in which alcohol was unfavorable to work and could not be used to produce energy. This anecdote illustrates Kassowitz's method of examining experiments on the influence of alcohol critically.

On the other hand, I am bitterly disappointed that I did not realize at the time I was in Vienna that Kassowitz was so much interested in infant metabolism, for recently I have seen writings of his that are astonishingly accurate, keen, and acute, particularly his discussion with Schlossmann concerning the metabolism of atrophic infants. I had occasion, in connection with the first book on the gaseous metabolism of infants, written in conjunction with Dr. Talbot (Publication No. 201) to discuss in considerable detail the views of Kassowitz and his reply to Schlossmann. As a matter of fact I find that all of our latest experimental work completely verifies the belief of Kassowitz, who contended strongly against the idea that the heat output is proportional to the square meter of body surface, maintaining that it is a function of the active mass of protoplasmic tissue. Unfortunately shortly after I left Vienna Professor Kassowitz died, and I have never been able to communicate to him what would probably have been very gratifying information.

Professor von Noorden.

At the time I was in Vienna Professor von Noorden was at the Congress in Wiesbaden, but I met him in Munich. I called his attention to the fact that Falta had not at all appreciated that our second book on diabetes was in preparation and I could not understand how Professor von Noorden could have known of its existence and not called Falta's attention to it. Professor von Noorden said he knew that Falta was writing a book on inner secretion, but did not realize that he was laying so much stress upon diabetes, and that he personally had not seen Falta's book and the particular section on diabetes.

Dr. Falta's book has subsequently been published. In it he criticizes very severely the first book published by Dr. Joslin and myself (Publication No. 136) but apparently he did not receive information with regard to our second book (Publication No. 176) or at least did not look it up or try to get information from us about it until his proof sheets were all in. His complete disregard of the second diabetic book is accentuated by the fact that later, when he did mention it, he merely said that it bears out his original contention and did not enter into any lengthy criticism of it.

I had considerable conversation with Professor von Noorden in regard to his leaving Vienna and establishing himself in Frankfurt. There seemed to be much conjecture as to why he was leaving Vienna.

Evidently he was not happy there. He said he was going to Frankfurt to establish an entirely new clinic. As he was taking no assistants with him from Vienna, it would be necessary to develop an entirely new staff. He hopes that ultimately he will have more time to devote to scientific research.

MUNICH, GERMANY.

University of Munich (II Medical Clinic)

Professor Friedrich Müller

I met Professor Müller at his house and had a long talk with him regarding various matters. He is quite inclined to think that the research carried out by A. Müller in von Noorden's laboratory on the addition of 210 grams of nitrogen is correct and that there was an actual addition of nitrogen. He says that he himself has seen this point tested, and 50 to 70 grams added in four weeks in nephritic cases. He wonders if there is ammonia or loss through the skin, etc. He does not believe that the nitrogen is retained as protein in the body. The theory that the nitrogen balance may be at any level is not true, he thinks, but considers a nitrogen balance at the minimum level as of value. He says that Müller, who is really no relation to him, is a very good man.

In discussing the controversy between Lusk and Grafe with regard to the dextrose-nitrogen ratio, Müller told me that Lusk had written an article in which he stated that he did not believe Grafe was right. Müller said that he himself did not believe that Grafe was right but that one must not say so. Professor Müller thought that Lusk had treated Grafe very badly but Müller revised the Lusk article himself. Müller says that there is a man in Lusk's laboratory now writing but that Lusk is back of it.

Professor Müller attached very little importance to the work of Rolly, Grafe, Plesch, and Brugsch, but thought that Bergmann was very good.

Professor Neubauer.

After my conversation with Professor Müller I met Professor Neubauer and had a long talk with him on alcohol research problems. Professor Neubauer told me that he had studied the effect of alcohol on acidosis and found it had no effect on the acidosis of a normal man but did have an effect on the acidosis of a diabetic. He also found that alcohol lessened the sugar output of a diabetic and hence its action may have been, after all, indirect.

One of his assistants, a Dr. Schweisheimer, who studied the method of Nicloux in Paris, had made an investigation on alcohol in the blood. He studied the influence of alcohol on the cerebral spinal fluid and had written a publication on the subject. The investigation was made in an attempt to show whether the people arrested for drunkenness were really intoxicated or had met with an accident, were suffering from a fainting spell, or something of a similar nature. Blood was distilled at 50° C. in a vacuum immediately after the taking of the alcohol. Schweisheimer found that men who were abstainers did not burn the alcohol as well as non-abstainers.

In studying the effect of uric acid, Neubauer compared the Folin method with the Wiechowski method, and found that the former was a very good method except in cases of fever. In fever he used the Ludwig method. The Folin method and the Wiechowski method checked each other excellently, but sometimes Neubauer found five times the amount by the Folin method. Some other substance evidently gives a reaction. This is the great difficulty with the colorimetric method.

In discussing this subject Neubauer said that a representative of the Liebig Extract Company from Amsterdam told him that there were many substances which would give the uric acid reaction, and that many men do not believe in the Folin method for determining creatine. There was no substitute for it, however, and their objection was purely a commercial one.

Neubauer also told me that the uric acid theories are being upset, that Weintraub says that there is no endogenous uric acid, that when we eat meat it irritates the lining of the alimentary tract and we get synthesized uric acid. Weintraub's views are sustained by one other investigator. According to Neubauer, the uric acid question may be a passing fad; nevertheless some very interesting things have come from it.

I was interested to learn that Neubauer does not believe in the great retention of nitrogen claimed by Müller of Vienna.

Professor His of Berlin says that he recommended Neubauer strongly to Simon Flexner of the Rockefeller Institute but Neubauer did not want to go to America. He thought he ought to go for he would never get a clinic in Germany. His says that he himself came back from the United States with many different ideas.

Physiological Laboratory.

Professor Otto Frank.

As nearly as I can make out from conversation with various men in Munich one of the rare occasions when Professor Frank seems pleased and glad and happy and not absolutely pessimistic is when I call upon him for, contrary to the experience of others, I have invariably found him very pleasant and agreeable.

One of the first topics that he brought up was the alcohol program. He was particularly interested in the psychological side. Like many others, he gave me his own personal experience with alcohol. He is accustomed now to take one glass of beer daily with his evening meal, but is unable to note that it helps him to solve abstract mathematical problems. One evening, contrary to his usual practice, he drank two liters of beer. He had a very hard formula to work out that had bothered him for a long time. As a result of drinking so much beer, he could not sleep; suddenly an idea came to him. He got out of bed and wrote it down, but when he attempted to work out the details, he found that he could do nothing at all. He also told me that he understood Nernst does his most brilliant things while under the influence of alcohol and all of his writings seem to Frank to show spasmodic work with brilliant ideas rather than regular, planned, concentrated research.

In discussing psychological technique Frank said that it was very unsatisfactory, as an intelligent man gets very much bored in making the inane mental tests or playing with blocks. The first experimental period is without and the second with alcohol; the subject loses interest in the last test. The same thing applies to routine work in the laboratory, and Frank finds that women do this better. He also finds that he can get a more complete return of his lectures from women students.

Frank was unusually merry and talkative and inasmuch as the impressions made by different writers on a great physiologist like Frank are worthy of record, I made a particular point of noting these, as throwing an interesting side-light upon the character and the habit of thought of different investigators.

Frank said that he knew Tangl in Ludwig's laboratory, but finds that all over Germany Tangl is looked upon as not reliable, although Frank does not know why. He, like many others, had the impression that von Noorden was a "commercial". He also maintained that Friedrich Müller did very little himself, as he was too busy in his practice to do much original work but that he had a lot of brilliant men about him.

Frank discussed in detail Kraepelin and his influence on Munich, saying that Kraepelin had done much to stop the drinking habit in Munich. Frank told me that a number of years ago when the physiologists met in Munich of an evening, it was common for all to drink more or less beer, and now they rarely do; in fact, they had practically all become total abstainers, which Frank attributes to Kraepelin's influence. On the other hand, he finds that Kraepelin is very objectionable in his manner in many ways. One evening Frank was invited to dine at Kraepelin's house; after the dinner Mrs. Kraepelin poured out some wine for Frank who drank it. Kraepelin immediately launched a tirade at him and severely arraigned him for drinking, saying that he was a physiologist and ought to know better. I can imagine that Frank's sharp, caustic remarks must have been interesting in dinner table conversation.

A short time previous Gruber had given a lecture before some people in Hamburg, and had thrown in a bomb there with regard to the alcohol question as he had previously told Frank he intended to do. Frank maintained that Kassowitz was absolutely unreliable and that he was particularly bitter against Gruber because of the latter's statement regarding the nutritive value of alcohol in his Hamburg lecture.

Frank maintained that many temperance advocates used beer and lighter drinks and referred to the fact that Tigerstedt took the wine cure in Paris. He also said that Kraepelin told him that a prominent German psychiatrist was using alcohol to combat sexual perversion. Frank argues that there must be some outlet to man's passions and that alcohol is one of them.

Professor Frank promised me to write out in detail his views on one or two phases of the alcohol problem. He thinks most of the old alcohol work is very poor.

Frank was very anxious to get Weinland the appointment in Erlangen but it was a very difficult matter to put through. He criticized Heilner of his own laboratory very sharply as belonging to the Ehrlich school for which Frank had no use as he thought it had no technique. He also commented very freely upon Lusk's piety for Voit and pointed out the fact that Rubner was not so generous.

Frank tried to get the Deutsches Museum to take the Pettenkofer-Voit apparatus. He maintained that if he kept it in his building it would take up several large rooms, that the apparatus could not be used for work on man by either himself, Weinland, or Cremer in thirty years' time, and the new addition to his laboratory proved that the room was needed. He therefore decided to remove it.

I was interested to note that as a severe critic of the Porter apparatus manufactured by the Harvard Apparatus Company Frank found the inductorium and the pendulum satisfactory but was very much dissatisfied with the rest of the material. He deplored the fact that Professor Porter, whom he once considered to be at the head of American physiologists, no longer does any work.

I discussed at considerable length with Professor Frank his kymograph and our difficulties in getting one made, although we had written to Schmidt in Giessen about it. He told me that Schmidt had failed and that one of his workmen had taken over the shop and the work. Finally he agreed to see to it personally that our work was put through and placed an order. He had changed the kymograph somewhat which made it more complete, putting in a maltese cross arrangement so that the contacts could not shoot by. He found he was able to obtain 4 meters per second with this apparatus but uses an electric motor for high speeds. He also has a new device to "get up speed" before exposure is made.

As a confirmed pessimist and gossip I think no physiologist in Europe is quite equal to Professor Frank. He is very keen and very bright and, of course, has done beautiful work, particularly on membrane manometers, but impresses one as having quite a narrow point of view. He is by no means so liberal minded as one would expect in a follower of Voit.

From the standpoint of technique and from experimental work one need look for very little in the line of metabolism in the Munich laboratory at the present time, aside from the really very excellent work done by Professor Weinland. Unfortunately I could not see the latter as he was away.

Hygienic Institute.

Professor Gruber.

I was particularly anxious to meet Professor Gruber as I had never before met him. I found him a most genial and pleasant gentleman, reminding me strikingly in his personal appearance of Professor Metzner in Basel.

We immediately began discussing the alcohol program and I found he was a very strong believer in Kraepelin's work. He thought that Kraepelin was very objective but as an alienist he saw the consequences of the misuse of alcohol by men and was naturally very much against alcohol. Unlike Professor Frank, Professor Gruber did not believe that the alcohol experimental period occurred at a time when the subject was very much bored for Gruber says that Kraepelin's alcohol periods were very perfectly controlled.

Personally Professor Gruber had been a total abstainer for ten years. Before that he took about one-half liter of beer a day. He thinks he feels better for not taking it and is less irritable.

He emphasized the importance of muscular work in experiments with alcohol and laid much stress on Durig's work on the mountains and with alcohol. He thinks, as does Durig himself, that the experiments should be repeated.

In discussing some recent experiments made by a Swede on the effect of alcohol upon the shooting ability of soldiers, Gruber told me that the efficiency was 30 per cent less during the alcohol period. He also said that Kraepelin had found almost no effect with alcohol in his earlier experiments. I asked him why Kraepelin did not publish these in all fairness. Gruber maintained that Kraepelin did not publish them, not because there was no effect from alcohol, but because he thought in this instance that the reason why there was no effect was due to the fact that all of the men were excellent shots. He wishes Kraepelin to repeat the work. If Kraepelin had obtained the same results as the Swede, he certainly would have published them on the ground that they confirmed the Swede's work.

Gruber speaks most highly of Kraepelin's assistant, Weiler, and also of Professor Neubauer, but laments the fact that the latter will not be able to get a clinic inasmuch as he is a Hebrew. Gruber thinks Neubauer ought to go to America.

It was rather late in the afternoon and I did not have an opportunity to see Professor Gruber's laboratory, but he impressed me as being a clear thinker and a most courteous gentleman.

BERLIN, GERMANY.

Dr. Hans Friedenthal of Nicolassee.

Having heard so much of Dr. Friedenthal and in so many ways, particularly with regard to his high speed centrifuge, I made a special trip to Nicolassee to see him and found the visit well worth while. Dr. Friedenthal, whose home is in Nicolassee, has also a private laboratory there which is very ingeniously and completely fitted up; he is evidently a man with many lines of thought. He has given much attention to the measurement of the body, collecting records of the different diameters of the body, and the general body dimensions. He has an interesting method for photographing the body at different angles, his study being full of pictures of different individuals showing the length of leg, shoulders, head, etc. He was much interested in the laws of growth and particularly in the length of the fetus at different periods of development.

His centrifuge was built upon a new plan, the alternating current motor being directly coupled to it, with very high alternations of current. The outside movable part of the centrifuge had a diameter of 30 centimeters and 36,000 revolutions per minute could be obtained. An interesting scheme for determining the rapidity of the machine was his use of a small motor with a disk rotating somewhat as a Fizeau wheel on the end of the armature shaft.

Dr. Friedenthal suggested a practical use for the centrifuge in preparing selenium-colloidal substances for injection in cancer. He maintained that ordinarily part of the vein into which the substances were injected sloughed off and he thought that this was due to some sediment in the material which was not really colloidal. By centrifuging the substance and thus driving out all material other than the purely colloidal, he found that much better results were obtained. With this centrifuge, also, he was able to secure albumen-free milk. He likewise found that if he took vegetable material and centrifuged it, both the water and soluble material could be thrown out, the residue being perfectly dry. Dr. Friedenthal has centrifuged finely powdered vegetable material for use in making a vegetable milk or vegetable solution, which he substitutes for animal milk in feeding infants in an infant asylum in which he is interested.

In discussing with him the question of the active mass of protoplasmic tissue, I found that he believes the body is in part a machine and in part living protoplasm. He thinks that all contractile substances form a part of the machine and that if he could only determine the metabolism of the living matter he would find the same metabolism per kilogram regardless of whether the measurements were made on an elephant, a man, or a rabbit. If curare is used it would prevent muscular activity. It occurred to me at the time that it might be interesting to see what the metabolism per kilogram is of the eggs of the snake, the turtle, and the ostrich.

Dr. Friedenthal is one of the most interesting men I ever met. He is peculiar in his ways and contentious. In fact, he is considered as altogether too argumentative and contentious in the meetings of the Physiological Society. He has had several clashes, I understand, with Rubner. On the other hand, he certainly is a most original thinker and well worth visiting.

I Medical Clinic.

Professor His.

While in Berlin we were fortunately present at the opening of this new clinic, which is certainly well-equipped and well-ordered. I was, of course, interested in the chemical laboratory and particularly in the Jaquet respiration apparatus (see figures 48 to 51) which had been transferred from the old clinic and was being installed in the new clinic, although of course it was not yet in running order. I noted particularly that the walls of the Jaquet respiration apparatus in this case were made of heavy glass, set into an iron framework with wax cement, and that even the floor was of heavy glass with a number of seams. It seemed as if there would be many opportunities for a leak. As the space under the bed was not used, it was cut out of the respiration chamber and therefore did not form a part of the extraneous volume of air. The hygrometer thermometers were graduated in tenths from 0 to 44 degrees. They were made by Bleckmann and Burger and are listed in their catalogue as Nos. 23 and 24 respectively. I noted particularly the manner of wetting the bulb of the wet-bulb thermometer. Apparently considerable care was taken but the method did not look particularly good. The bulb was round and not long and drawn out. Heavy strings or cords hanging from the bulb fell into a small glass tube 6 millimeters in outside diameter and were wet by capiallary attraction. It would seem very improbable that the capillary attraction could be sufficient to cover the bulb and keep it thoroughly wet.

I had a most delightful evening with Professor His at his house,

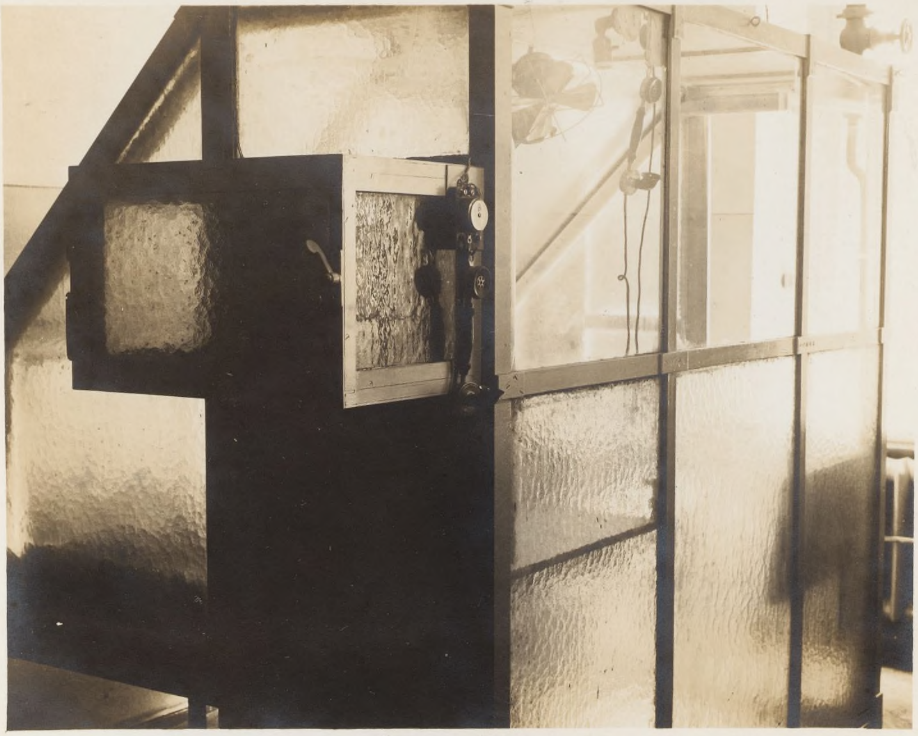


Fig. 48. General view of the Jaquet respiration apparatus in the His Clinic.

This photograph shows the telephone outside and inside the chamber, the electric ventilating fan inside, and the door in the back. The box in the front of the picture is for introducing food. At the left the couch can be seen through the glass. The construction of the chamber whereby the space under the couch is not included in the chamber is clearly shown.

Fig. 49. Interior of the Jaquet respiration chamber in the His Clinic.

Fig. 50. F.G.B. lying on a couch in the Jaquet respiration chamber in the His Clinic in Berlin.

This shows well the glass walls and the iron framework into which the glass is cemented.

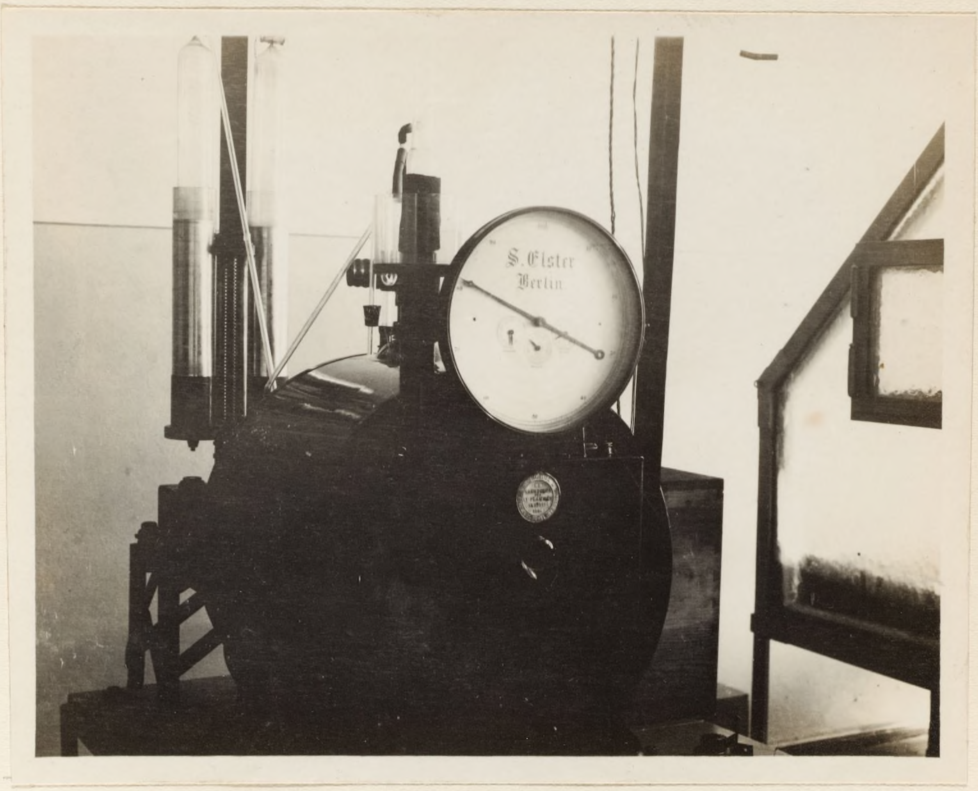


Fig. 51. Elster gas meter used in connection with the Jaquet respiration apparatus in the His Clinic in Berlin.

In the rear at the left is shown the modified method of taking samples devised by Staehelin and Kessner, with the spiral lowering the mercury reservoir.

and of course talked over his new clinic with him. He was not now particularly interested in Bang's method of determining sugar in urine nor in work on diabetes. He said that every clinic in Europe is doing work on diabetes and why should he?

Professor His discussed many different scientists in the course of our conversation. His opinion regarding Abderhalden was interesting. He said that Abderhalden had had trouble with Bunge and again with Fischer. He worked with both of these men and did good work but always used somebody's else ideas. He used to be a very hard worker, averaging 20 hours a day, but now he is continually engaged in discussions of a polemic nature and is writing considerably for the newspapers.

Professor His expressed the very highest regard for Chauveau as would be expected for having been in the Basel clinic for many years, His is decidedly French in his tendencies. Mention was made of Robin and Professor His said that Robin's appointment in Paris was due to his wealth, the authorities arguing that Robin would finally die but his money would not. Robin is distinctly an elegant physician and is known in Paris as the "theatre ladies' physician".

With regard to the clinic in Basel His said he always has a longing for it and even Friedrich Müller to-day has a longing for it. His feels that he would like to be back in Basel.

I spoke to him about Friedmann, his first assistant, of whom I had previously formed a very poor opinion. He said that Friedmann was a very good worker, but he could not explain the crazy respiratory quotients which he had obtained in some recent work.

In discussing the alcohol question Professor His maintained that alcohol cannot harm a man who occasionally uses a little, and perhaps two times a year a little in excess. Personally he finds no difference in mental work after a little alcohol.

He says that Friedrich Müller always estimates very highly a man whose technique he knows nothing about, which explains his estimate of the work done by Müller of Vienna on the nitrogen balance. His personally thinks that the nitrogen balance is difficult to obtain.

One rarely meets a man who is so brilliant, stimulating, and suggestive as is Professor His in discussion. Unfortunately one gets the impression that he is quite superficial and that his ideas are more brilliant than sound. Nevertheless he is very stimulating and well worth visiting. I remember distinctly his visit to us in Boston in the fall of 1912. In going about our laboratory at that time he made most suggestive statements and asked most interesting questions. One can easily see why he holds the position he does in Berlin, although as a matter of fact among his colleagues he is not estimated as highly perhaps as he is among the laity.

Professor Friedmann.

Professor Friedmann has been working on curare, not curarine, and obtained some perfectly crazy respiratory quotients of 0.33 and 0.46. The fact that the quotients are all very regular shows that his method is constant but it is doubtful if the figures are true. Friedmann said Cohnheim had told him that he had lost all faith in respiratory quotients, as he (Cohnheim) had found all sorts of respiratory quotients with snails and with Wirbeltiere. Personally I do not believe that the researches of Friedmann are accurate. He has not yet published them. He thought that possibly his results might be due to an anaerobic combustion and that under his conditions of experimentation the oxygen consumption was absolutely independent of the carbon-dioxide output.

Later, at a meeting of the Berlin Physiological Society, I heard a man who had worked with Friedmann give a talk on a certain piece of work. When somebody asked him what kind of albumin-free substances he had used, he said he was shortly to publish the results with Friedmann and was not at liberty to give the composition of the substances used. This answer caused considerable astonishment, not only on my part but on the part of Zuntz and others who were present.

Subsequently in talking with Wolf in Cambridge, I found that he had worked with Friedmann and he felt perfectly certain that the young lady who made the air analyses in the experiments with curare had obtained entirely false results and that therefore the work was utterly worthless. Wolf personally had written to Friedmann that he would advise him not to publish the work. Of course all this goes against His's views of Friedmann as an exceptionally good man.

Königliche Tierärztliche Hochschule (Physiological Laboratory)

Professor Max Cremer.

Professor Cremer has fallen heir to the laboratory left by Professor Abderhalden when he went to Halle. This laboratory is fitted up most elegantly and each room is so arranged that it can ultimately be used for either a physical or a chemical laboratory, as Cremer argues that his successor might be interested mainly in chemistry whereas he is apparently interested in physics. The equipment is nothing more nor less than wonderful and certainly justifies a visit to the laboratory.

All of the private offices had double doors which seemed to me rather practical. A special room is arranged for distilling and heating dangerous vapors. The lecture table is constructed on the principle of an elevator so that a section of it can be raised or lowered as desired. The table is lead-covered, with a trough running outside from about half-way from the floor to the top. I noticed that the toluol drying oven was commonly in use in Professor Cremer's laboratory and, indeed, all over Germany.

Abderhalden had left a special photographic spectroscopic apparatus with a Nernst lamp and a spectroscopic analyzer to get a monochromatic light, which was said to be very good indeed. It was a very expensive apparatus but I understood that Cremer had not used it.

I also noticed that in this laboratory, as in Tangl's, the "Panser" galvanometer was very popular. The galvanometer was enclosed in a series of metal cases to dampen the external effect. These dampenings were cut down by 25 per cent for each case so that if they had three cases it would be $\frac{1}{4} \times \frac{1}{4} \times \frac{1}{4}$ = about 1 per cent of the disturbance.

Each laboratory room had provisions for hanging a galvanometer, also free I-beams overhead. Professor Cremer took me all over the heating, refrigerating, and ventilating plants and likewise the compressed air plant, all of which are very ingeniously arranged. As Professor Cremer had but just moved into the building, of course no work was in progress and I met none of his assistants.

University of Berlin (Physiological Institute).

Professor Rubner.

Professor Rubner was, as usual, quite disinclined to talk of scientific matters, although we had a good deal of social conversation. He had very little use for the ideas of Professor Chittenden and thought the fact that Chittenden's men lived on less than what would normally be demanded by hunger proved but very little. He was also rather sceptical regarding the French society presided over by President Gautier in Paris and thought very little would come from it. He said the work of Mendel and Osborne made a fine impression upon him.

In discussing the question of the storage of nitrogen in the body he maintained that there are 9000 square meters of surface in the body cells if the area of the cells is calculated and he thought that excess nitrogen would simply be absorbed and that the circulating protein of Voit was simply absorbed protein. He cited one experiment in which a man ate 71 grams in excess in three days and added it all to the body. On going back to the normal amount of ingested nitrogen, the excess nitrogen was given out again immediately.

Rubner was particularly interested at that time in experiments on yeast, and showed me his micro-calorimeter with which he thought much work could be carried out. He said many things could be done with yeast cells that could not be accomplished with human cells. Indeed he had been able to determine the oxygen consumption of yeast cells.

In speaking of the new institute, Rubner told me that instead of its being out in Dahlem where the other institutes are, it is to be near his laboratory. The building will be about two stories high.

It will be entirely separate from the Physiological Institute, being used only for scientific experiments.

It is always a source of great regret to me that Professor Rubner is so disinclined to discuss scientific matters. I have never been able to draw him out to any length in scientific discussion, either in his visit to America or later in my visit to Berlin. I am sure he must have a great many ideas and if one does not agree with him it would be most interesting to have his views on such subjects, for he has had a wide experience.

Kaiserin Auguste-Victoria Haus, Charlottenburg

Professor Langstein

I was particularly anxious to see Professor Langstein's laboratory, after having seen Schlossmann's laboratory in Düsseldorf, but I was much disappointed in the equipment so far as metabolism experiments are concerned. They use the old Pettenkofer type of chamber, watch the baby intermittently, and take it out for about four hours a day to feed it. The results are calculated on an approximately 24-hour basis. They determine only carbon dioxide but lay great stress upon the fact that they determine the water content. Just what earthly use the water determination is I am not sure. They personally do not like the Düsseldorf apparatus, as they always fear that the oxygen bomb may not work and the baby will die.

The animal house was very good indeed, particularly the operating room and its equipment, although it was very difficult to keep the room sterile. One very practical point was the use of colored bars on the different animal cages, such as red, blue, etc., and the dishes colored to match. The result is that there is no confusion in the meals given the different animals, for a blue dish would go into a cage with blue bars, etc.

On the whole my impression of the Langstein laboratory was not particularly favorable. Unfortunately my personal impression of Professor Langstein himself was not of the best; I saw him but a very few moments, however.

Institut für Gärungsgewerbe (Physiological Laboratory).

Professor Völtz

The Institut für Gärungsgewerbe, although some distance out from the center, is well worth a visit on account of its new location and the many interesting things to be found there. Professor Völtz has been particularly interested in the alcohol remaining in animal tissue after the ingestion of varying amounts of alcohol, and indeed has devised a plan for distilling or destroying the entire body tissue, collecting the alcohol in potassium bichromate and sulphuric acid, and then determining the amount by a modified form of the Nicloux method. He showed me the delicacy of this method and we have since attempted to use it here in the laboratory. Völtz thinks that an animal at rest can cover his needs to within 80 per cent by alcohol, but that during muscular work it cannot burn so much alcohol. I saw many of the dogs and other animals upon which he is making such experiments, and his ingenious devices for collecting samples of urine and feces of the various animals. There were several dogs that he said were surely "potators".

Unfortunately Professor Völtz reads practically nothing but German and knows very, very few words of English; it is therefore very difficult for him to get in touch with the literature from our laboratory. He evidently is left rather free in his work, and is certainly one of the most objective workers that I have met in Europe.

Königliche Landwirtschaftliche Hochschule (Tierphysiologisches Institut).

Professor Zuntz.

A visit to Berlin for me means chiefly an opportunity to see and talk with Professor Zuntz. (See fig. 52.) I never lose an opportunity to see him, either at his house or at the laboratory or on some walk. One of the first opportunities I had during this last tour was on the first of May, when we all went out to Potsdam and had a most delightful walk together in the afternoon. On this occasion Professor Zuntz gave me a great many interesting points and hints regarding Mr. Higgins's contemplated journey to Monte Rosa in Italy.

I felt after every talk with Zuntz that it would be utterly impossible to get down in my notebook any considerable portion of what he had said. My notes must necessarily be very fragmentary and are by no means so complete as I could wish to have them, for Professor Zuntz is always entering into all sorts of interesting discussions and throws interesting side-lights on practically all phases of physiology and physiological chemistry.

One of my first opportunities of talking with him at his house was on Sunday, May 5th. He commented upon Siebeck's work and called it extremely good, laying particular stress upon the value of his alveolar air method, which he considered excellent. He also considered Siebeck's small spirometer good. Zuntz said he had a small spirometer that he would show me but this he never did.

In discussing other scientists in Germany he did not estimate Rolly of Leipsic very highly but spoke very well of Krogh and Völtz.

Fig. 52. Professor Zuntz in his study in Berlin.

In speaking of the radium emanation experiments made formerly by Staehelin and his associates in the His clinic Professor Zuntz said he felt that they were useless. In commenting upon the article presented by Fleischmann at the Berlin Physiological Society, in which Friedmann spoke about fever and pituitrin, Zuntz remarked that he was astonished, as was I, to have him refuse to report how the solution was made albumin-free.

Zuntz considered Haldane very clever and made many interesting comments as to his absent-mindedness and his devotion to his pipe. He said that Haldane always lighted his pipe as soon as he left his room to come downstairs.

Zuntz saw Douglas on Teneriffe and got a very good impression of him. He was by no means satisfied with the Haldane carmine titration, although he saw Douglas make the titration a hundred times on Teneriffe and not get a red color. In discussing the Haldane titration method he said he disliked the way in which Haldane pricked his finger and then shook it. He asked Haldane if he had ever seen a needle and as he said he had not, Zuntz sent him one. In his method of titration Haldane uses but 6 to 7 drops of blood and yet finds carbon monoxide and oxygen in the blood to 20.73 per cent, i.e., four figures, but really can determine only one part in six or seven.

We spoke of the question of the secretion of oxygen and Zuntz thought it was not true that there is such a secretion. He believed that during work carbon monoxide is not in equilibrium, for some of the blood must be almost oxygen-free during severe work and only in work does Haldane find that a secretion of oxygen takes place. Zuntz also believed that carbon monoxide may actually be absorbed by the lung tissue.

In discussing the Douglas bag method Zuntz said that it was simply the old Luciani method. He considered 50 seconds an extremely short time for an experiment; 5 minutes, he said, is very short for a rest experiment with Cheyne-Stokes breathing, and although during work the respiration is much more regular and the error is thus less, nevertheless he thought 50 seconds too short.

With regard to the Haldane alveolar air method, he maintained that in breathing into a tube held before the face, it is difficult to get normal respirations and there must be some abnormal respiration beforehand. This is quite different from breathing regularly into a mouthpiece as with this appliance the breathing shortly becomes very regular. Zuntz said that among 30 men he had never seen but one man, Neuberg, who could not breathe with the mouthpiece. This is quite contrary to the opinion which prevailed several years ago when I was in Europe that hardly five men could be counted who had normal respiration with the Zuntz mouthpiece. Of course our experience in the Nutrition Laboratory has since that time been very great, and we could hardly take that view now, although how long a time is required to train one to the Zuntz mouthpiece may still be a matter of discussion.

In speaking of Durig's new experiments involving calculations of results obtained with a gas meter Zuntz maintained that Durig walks at the rapid rate of 120 meters per minute and has a big ventilation. An error in the records of the meter may accordingly play a great rôle. In Zuntz's experiments, on the other hand, the men walk slowly,--about 70 meters per minute,--and large volumes of air were not passed through the meter. Zuntz thought that Durig's gas meter might let air pass by, but that in his (Zuntz's) experiments on the treadmill and in the experiments with Heinemann he frequently used a wet gas meter and not the dry one. With a large gas meter, such as had been used in experiments with horses, a speed of 500 meters per minute could be employed with no danger of the air passing through without being recorded.

Zuntz criticized our work experiments, saying that he thought that water may have escaped absorption in the sulphuric acid containers. This criticism was the basis of considerable correspondence with the Laboratory in my absence and an extensive series of experiments was made by Mr. Carpenter which proved that we were right in assuming that the water had been completely absorbed.

In discussing alcohol experiments Zuntz said he had made trial experiments with alcohol and corn, to find if the loss by fermentation made it possible to absorb the starch better by the alcohol method. The alcohol was wholly absorbed but the starch fermented in the intestine and there was accordingly some loss. He found that from a total amount of starch more energy could be absorbed and utilized by the animal when the starch was first converted into alcohol than when the equivalent of starch was given directly; he also found that alcohol was used rapidly with large doses. He advocated giving the alcohol to the animal at the end of the fattening period, just before it was to be slaughtered. As a matter of fact he thought if there was to be any fat in the liver this would be just as good a time to have it formed. Zuntz said that he was much abused for his paper describing these experiments.

With regard to the alcohol question Zuntz pointed out the fact that certain kinds of wine were exciting in their action, such as Moselle, and inclined to make one feel happy. He said he thought that the drowsiness was due to a substance other than ethyl alcohol. He also told me about the interesting experiments which Völtz reported at the Berlin Physiological Society, in which he demonstrated that alcohol could be absorbed from the bladder and that after being absorbed, it was excreted in very small part by the breath, but enough to change the color of the green potassium-bichromate solution.

In commenting upon the alcohol work he said that in cooperation with six other men he had carried out a lot of work with fusel oil in capsules; that there was a law against the use of fusel oil in Germany but that people would not drink low fusel oil liquors. The report of this work was published in Pflüger's Archiv. Zuntz used 20 dogs in his experiments, giving some of them pure wine and others fusel oil. He found that 1 gram of fusel oil was equal to 4 or 5 grams of alcohol. In both cases the animals became drunk and later died, although occasionally they found an adaptation to fusel oil. Zuntz also cited some work done by Fritz Strassmann which he thought we ought to look up. (Strassmann, Pflüger's Archiv, 1891, volume 49, p. 315; also, Deutsch. Viertelj. f. offentl. Gesundheitspflege, 1890, Heft III.)

I spoke of the difficulties of securing a good reduction valve and Zuntz said he had never been able to find one in Germany.

In discussing the nitrogen balance experiments so frequently used Zuntz maintained that a minus nitrogen balance is an indication that the food is not right. He found, for example, in his walking experiments a continual gain or loss of nitrogen, and a loss of 1.5 grams of nitrogen per day through the skin.

I was much interested in what Professor Zuntz told me regarding Professor Atwater's visit to Col d'Olen. When Professor Atwater arrived he was in fair shape. He took two very careful training walks but returned from each walk somewhat distressed. He was advised by all his associates that it was best not to go to the top of Monte Rosa as they thought he was inclined to arterio-sclerosis, but he was very determined. Zuntz said that he himself was now too old to do much mountain climbing, that he would willingly ascend 5000 meters in a balloon or go on the railroad to the top of Pike's Peak, but would not do the work of climbing, because he considered the muscular work of ascent a much greater strain than the mere living at a high altitude.

Zuntz thought experiments on the fattening of geese ought to be carried out to determine the calorific value of oxygen with high respiratory quotients. He maintained that geese are much better for study than hens as they are quieter and can be fattened in four weeks. He pointed out the fact that Bleibtreu got a respiratory quotient of 1.38. Zuntz himself with a ruminant has obtained a value of 1.12 but this experiment was complicated by the fact that methane was produced.

He spoke of the large chamber which he had planned to place in a calorimeter and said he had decided to build a chamber inside of the present chamber, using the cold air outside as the means of bringing away the heat. He could get the air at 15° and bring away all the heat by the air current. I asked him why he did not build the calorimeter first. He maintained that after the failure of Hagemann's expensive apparatus he could not get any money for further work, so decided to build first something that was sure to be successful. His only interest in calorimetry was to show the calorific value of oxygen during fattening experiments with animals. Personally he thinks this is now the great problem in calorimetry before us.

In discussing Professor Lusk he maintained that Lusk was too much absorbed in one subject, i.e., diabetes, and that he has set him right frequently on other things.

The old contention between Zuntz and Voit in regard to the 24-hour experimental period versus short experimental periods is still on, and Zuntz was glad to see that we are inclining to the short period plan.

Personally Zuntz prefers the word "Erhaltungsumsatz" to "Grundumsatz". I think that "Grundumsatz" is better. The "Erhaltungsumsatz" depends upon the state or condition of the nutritive plane. The "Grundumsatz" conveys the idea of minimum and is well expressed. I also think it would be undesirable to add to the terms we already have.

At present Zuntz does not use the Spritzpumpe on the Oppenheimer respiration apparatus, but a paddle which is turned about by the same motor which runs the ventilation. This paddle stirs up the sodium hydroxide and gives very good absorption without leaks. In his apparatus Zuntz has the ventilating system in one tank and the respiration chamber in another. He says he prefers these two tanks because he can change the temperature and alter the relative humidity at will. He can cool his motor, etc., with ice or in experiments with the temperature as high as 30° can have air with a relatively satisfactory humidity by passing it through the cool KOH and letting it deposit water.

Zuntz had devised a scheme for feeding the pigs in the small Oppenheimer respiration chamber. By using a rubber bag with a clamp at each end he could open one clamp, squeeze food into the chamber and then reclamp the bag and in this way introduce food without letting air into the chamber. He made 24-hour experiments. When the pigs grew too large for the small respiration chamber he put three of them at once into the large chamber.

In discussing our unit apparatus Zuntz thought that we should make water tests by evaporating a definite amount of water, but this is a very difficult procedure owing to the diffusion of water through the rubber. I cited the "houses" over the "pans" of the Middletown respiration apparatus. He cited Pflüger's lecture experiment with a rubber tube filled with carbon dioxide. Both end of the tube are pinched off; the following morning the tube will be found completely collapsed, the carbon dioxide having been diffused out and no air diffused in. But, as Zuntz pointed out, Pflüger was dealing with about 100 per cent of carbon dioxide while we are dealing with 2 per cent.

In many of his alcohol check tests of the respiration chamber he finds unburned alcohol in the air of the chamber. Indeed, at times the potassium hydroxide smells of it, but the respiratory quotient is 0.667, which is very good.

In discussing the total ventilation of the lungs during severe work Zuntz said that when there is a production of 2,000 c.c. of carbon dioxide per minute the total ventilation would be about 70 liters per minute. With regard to muscular work he finds that when the body is overloaded the mechanical efficiency is less perfect. As a matter of fact we have found an efficiency as high as 47.9 per cent, which Zuntz thought was very remarkable.

I had an opportunity to ask him what he thought of the Friedmann tuberculosis cure, as Friedmann had recently been in America. He said that in Berlin there were two views regarding Friedmann. Most scientific men were against him, considering him commercial.
Regarding cancer he said that the injection of heavy metals was now the great thing and that Emil Fischer was working on the problem to find a substance that will act as a complement or combination material

or as a medium for the heavy metal.

We had many discussions in regard to laboratory management and smoking in the laboratory. Zuntz personally did not approve of smoking but many of his assistants smoke. Formerly in managing the laboratory he preferred to have conferences in which the men discussed their work but said that lately there seemed to be a tendency for each man to work by himself and not tell any one else what he was doing. He said that Robert Koch, who is interested in tuberculosis, carried on his laboratory this way and actually preferred it.

Zuntz showed me his blood gas pump which was a modified form of the Pflüger pump. Zuntz had tried out the Bohr method of using water over mercury, but he was sure that water vapor passed through the mercury. Now he personally uses compressed air with about three atmospheres to pump the mercury over. This blood gas pump was ordered for the Nutrition Laboratory.

I had a good opportunity to examine his gas analysis apparatus which is based upon the differential principle, the analyses being made at the beginning and end of an experiment. The apparatus (see figs. 53 to 56) was extremely delicate and elaborate, for not only was it unaffected by changes in the temperature or barometric pressure during the actual time of the analysis but the apparatus could also be left over night. Zuntz was always making differential analyses. I could not see the value of them but Zuntz said they are valuable in the methane determinations. He said in determining the methane the difference in solubility plays a great rôle. He can estimate the carbon dioxide to within 0.0004 per cent.

We had a long discussion in regard to gas analysis methods and I was astonished to find that Dr. Markoff, who was working with Professor Zuntz on the Sondén apparatus, had never even seen our book on the

Fig. 53. Indistinct details of the Zuntz gas-analysis apparatus for differential gas analysis in the Tierphysiologisches Institut in Berlin.

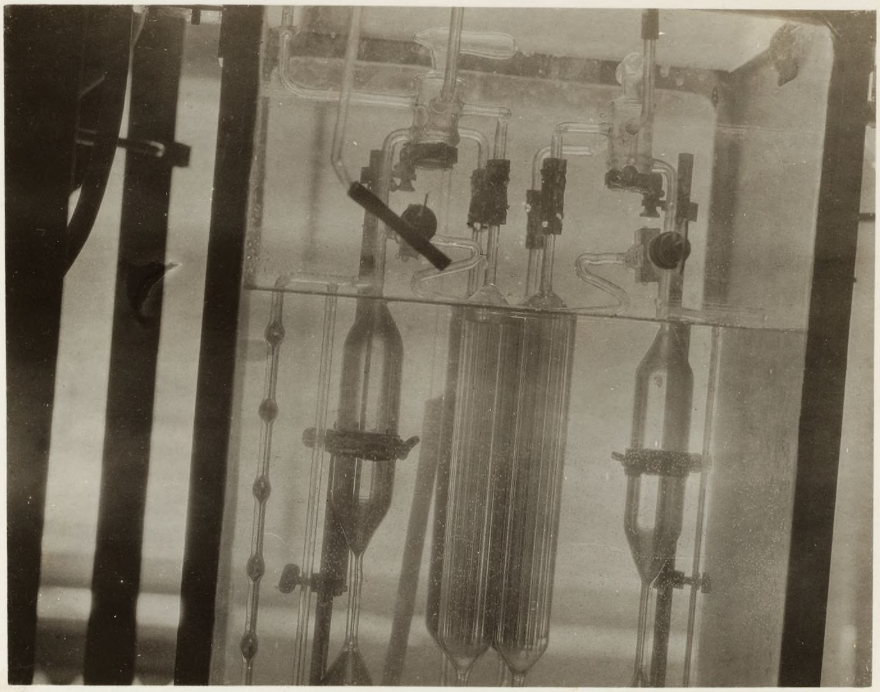


Fig. 54. Details of the Zuntz differential gas-analysis apparatus in the Tierphysiologisches Institut in Berlin.

Fig. 55. A general view of the Zuntz gas-analysis apparatus in Berlin.

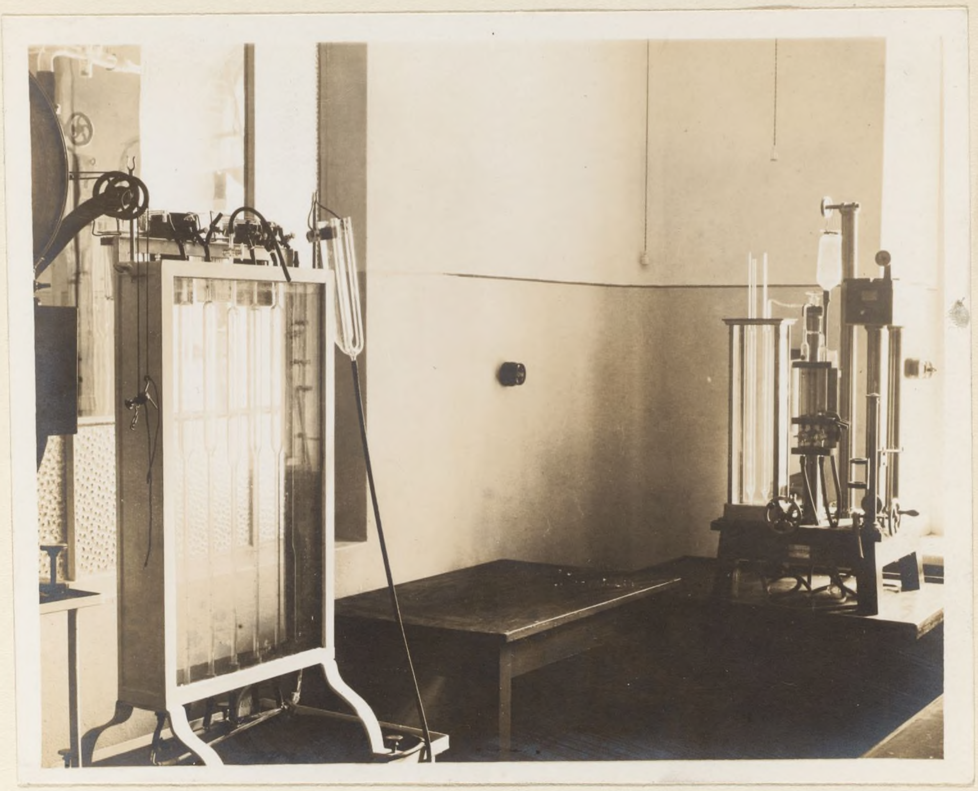


Fig. 56. A corner of the laboratory for gas analysis. Zuntz's laboratory, Berlin.

composition of the atmosphere, and was having the same difficulty that we had. Professor Zuntz confessed that the book was lying on the desk at his house, and he had never shown it to Dr. Markoff. This seemed to me a very singular thing. Dr. Markoff was working on a method for getting ozone-free oxygen and hydrogen-free oxygen by electrolysis.

In discussing the question of the possibility of high oxygen poisoning animals Zuntz told me of the transportation of fish from Munich to Berlin when the air was highly charged with oxygen. The fish were all sick when they reached Berlin. Zuntz said that Bornstein had also shown that with dogs a long inhalation of oxygen had very serious effects.

The new bicycle ergometer of Zuntz's is made by Gustav Voigt, Neuenburgerstrasse 12, Berlin. The weight of the arm and the scale pan is 1.80 kilograms. The circumference of the brake is 3 meters. There is a very thin band of steel passing around the wheel. Different weights may be hung on. The bicycle appears to me quite clumsy. It seems to be an adaptation of the Bremse ergometer on the bicycle principle and is not a particularly felicitous construction, I should say. (See figs. 57 and 58.) Other apparatus in Zuntz's laboratory are shown in figures 59 to 62.

The more one sees of Professor Zuntz the more one realizes what a marvelous man he is, but it must be admitted that he has done his fair share of work and that the institute is now clearly getting out of his hands. It is one of the most mismanaged institutes imaginable and although it is only a few years old, it looks like an old place and is thoroughly dirty. Everything is topsy-turvy and there is no system or organization of any kind. The men run into his office

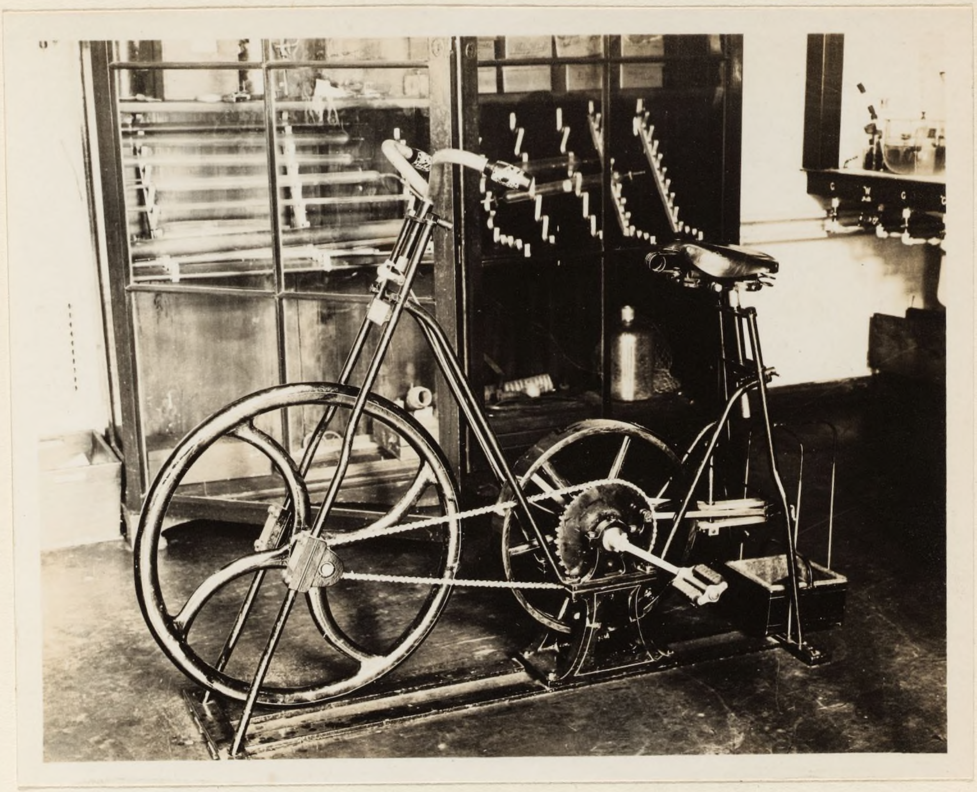


Fig. 57. Side view of Zuntz's new bicycle brake-ergometer in Berlin.

The fly-wheel is in front and the brake is on the wheel where the pedals are attached.

Fig. 58. Details of portions of Zuntz's new bicycle brake-ergometer in Berlin.

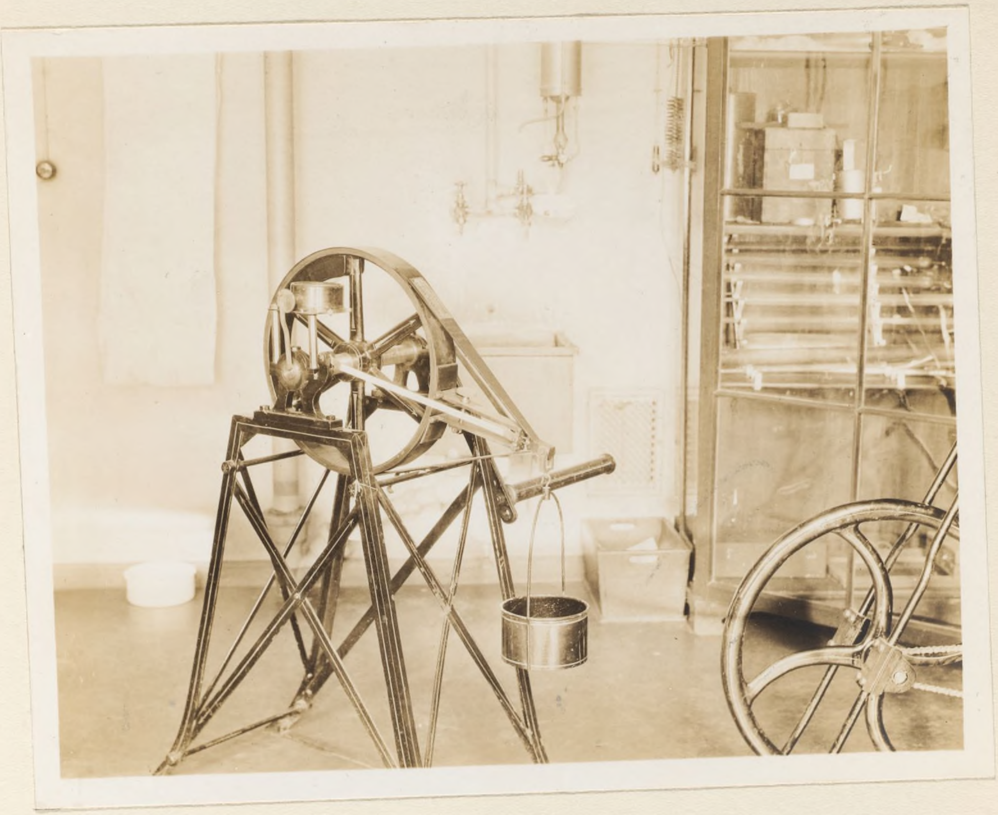


Fig. 59. General view of the Gärtner ergostat in Zuntz's laboratory in Berlin.

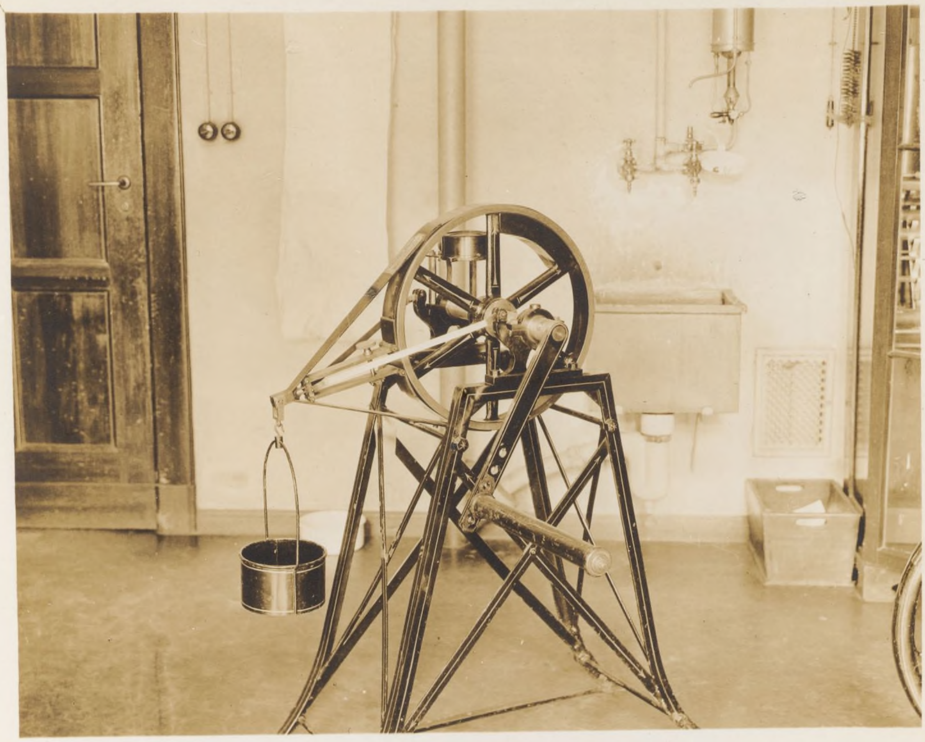


Fig. 60. General view of the Gärtner ergostat with brake effect in Zuntz's laboratory in Berlin.

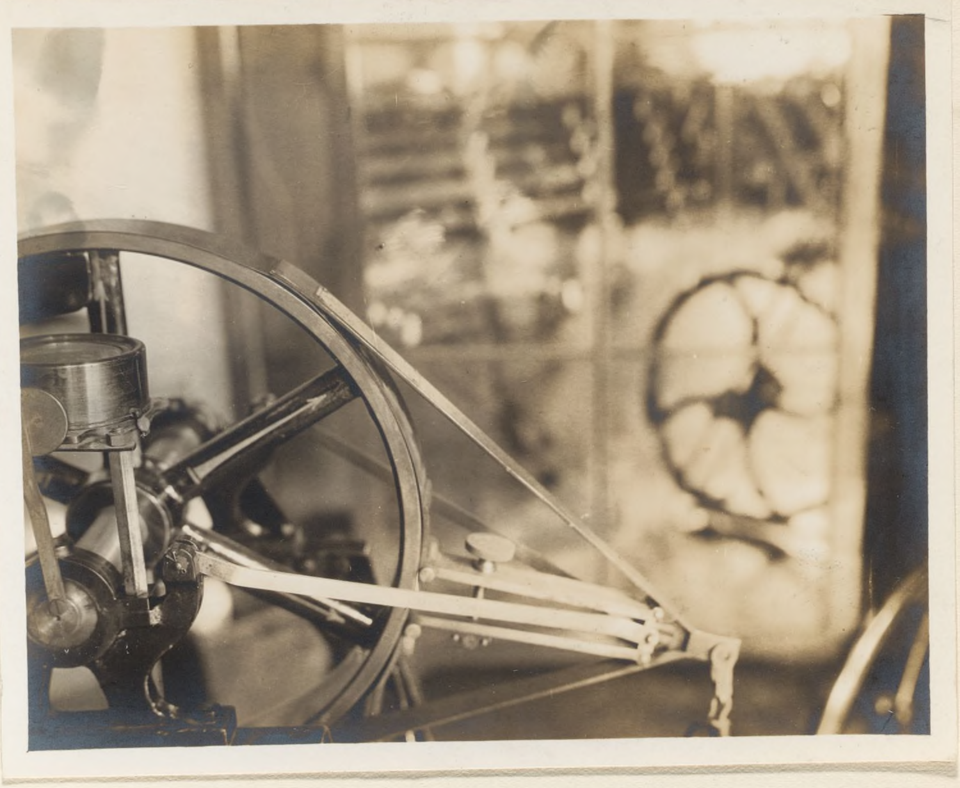


Fig. 61. Details of the brake and release used on the Gärtner ergometer in Zuntz's laboratory in Berlin.

The same principle was applied to the bicycle ergometer of Zuntz.

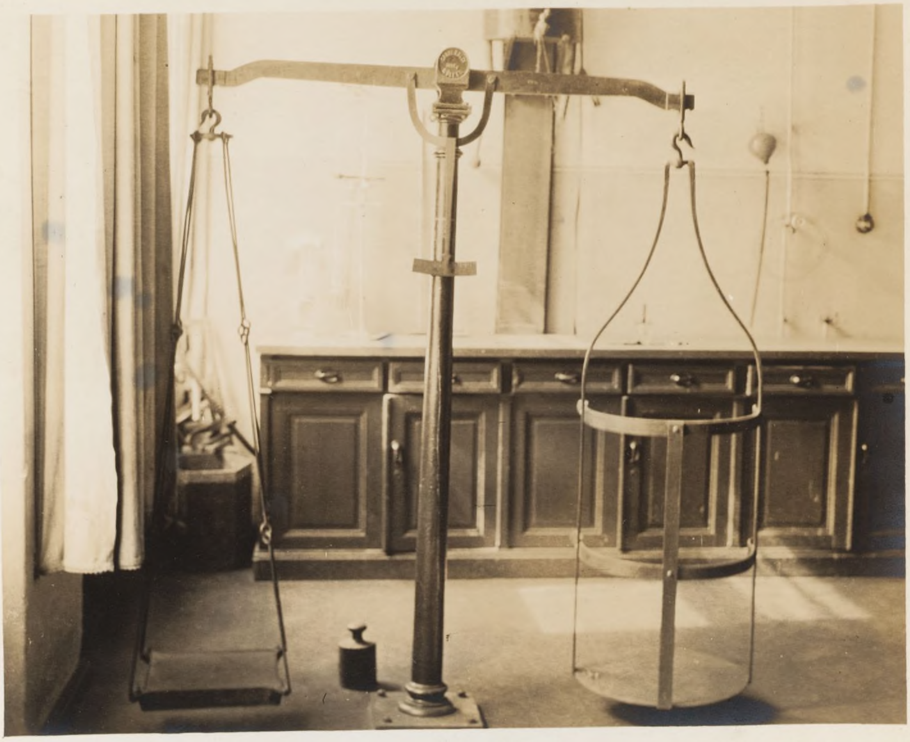


Fig. 62. Large balance for weighing man in Zuntz's laboratory in Berlin.

The balance is of great interest as it has been used for so many years in many experiments made in this laboratory on the loss in weight of different subjects.

every moment and give him no peace. In spite of his very efficient secretary, who is much distressed over the continual disturbances, there seems to be no help for it. He has not the moral courage to tell people he will not let them in. On the other hand, I think he is far more intelligent than any of his assistants and that he has no natural successor. I am perfectly sure that many of the subjects that he has written about erroneously are based upon a false belief in the accuracy and faithfulness of his associates. This undue confidence has, I believe, brought him into trouble a great many times. In spite of all this he still remains the most stimulating and the most interesting man to talk with along scientific lines that I have met anywhere in Europe. I count every hour spent with him as clear gain. He has withal such a pleasant personality that it is a delight to spend an hour with him.

Professor Caspari.

While in Berlin I had opportunity to secure further information regarding the research work in cancer now being carried on in Europe, especially in Berlin. The consensus of opinion in Berlin appears to be that through the efforts of Wassermann, Ehrlich, and Emil Fischer the cancer problem will surely be solved and at a not very late date. Although I did not go to see Fischer, as I had too many other things on hand, I inferred that he was most active in working upon a substance to be injected for cancer cure.

I talked with Caspari chiefly on account of his interest in cancer, although frankly of all the over-rated incompetent scientists in Europe I consider him the worst. As a worker he appears to me as being a lazy, good-for-nothing chap, but perhaps as a historian he may be a little better. There is working with him a very intelligent man, Neuberg, who really seems to be the man behind the whole thing. Neuberg is the chemist and Caspari the physician.

Their work is chiefly with mice but Caspari is also using injections in private practice. It seems that Caspari and Neuberg became interested in the cancer problem after Neuberg noticed the affinity of certain tissues for radium and discovered some new heavy metals allied to radium.

The composition of Neuberg's solutions is kept secret. Caspari said that any chemical factory could make them, but that Professor Orth had advised their not making the composition public as much misuse of the formula would be made. In doing this they are following exactly the same practice as that established by Behring and Koch, who prefer secrecy. Personally I do not at all approve of this scientific secrecy, but it seems to be the proper procedure in Germany.

Caspari maintained that Werner in Heidelberg always used choline in combination with some heavy salt and that this procedure is really dual therapy and dual therapy, he believed, is the solution of the problem. He thought that some day from the thousands of possible combinations that have yet to be tried out, some one will discover just the right "carrying agent" for the heavy metal.

Caspari told me a little regarding the history of Werner's choline scheme. Werner noticed that radium had a special affinity for growing cells, testicles, and sexual organs. Werner thought this affinity was due to the presence of lecithin, but Caspari maintained that there is no evidence that radium acts especially on the nervous system where there is much lecithin, and that this assumption of Werner is, in consequence, a false one. Considering the relation of lecithin to rapid growth and the divisions into which lecithin would split, Werner concluded the choline was probably the active part of lecithin and therefore always used it in combination with selenium or some other metal. The choline of course was good only as a "Bindungsstoff" with the heavy salts.

Caspari had injected Neuberg's metal solution into a vein in a mouse's tail. In implanted tumors he got the best results with cobalt, but had also obtained good results with silver. He is now studying human cancer. He has found that with certain of Neuberg's solutions he can hold uterine cancer in check with two injections per week, but if he stops giving the injections for one week the cancer grows rapidly, making up in three days for the previous good effect. Caspari told me that if the solution is <u>too weak</u> it acts as a stimulus to the growth of cancer. If the case is a dangerous one, there must be repeated doses of weak solutions given. Strong solutions are poisonous and some of the solutions change in toxicity if allowed to stand any length of time."

Caspari told me of a strange case of cancer which had recently attracted considerable attention. A man suffering from cancer, so severe that according to the best authorities in Berlin, Orth among others, it was not advisable to operate, had nevertheless continued to live, and after his death several years later from an entirely different cause, a post mortem examination showed complete recovery from the cancer. This case is of fundamental importance in proving that spontaneous recovery can actually take place, a fact long surmised but exceedingly difficult to prove.

Professor Franz Müller.

Franz Müller, one of the most agreeable and probably the most intelligent of the assistants of Professor Zuntz, although not especially intelligent, is working upon the determination of the amount of blood that passes through the lungs or the heart in one minute, using a mixture of nitrous oxide, oxygen, and air. He has two bag spirometers of special construction, the larger one having a writing lever which writes on prepared black paper furnished and patented by Zimmermann. This paper is 6 centimeters wide and is in a long roll about 4 centimeters in diameter. The volume of air during respiration is recorded on this paper. He also takes samples of the alveolar air as a check at the end of the experiment.

Müller does not think much of the Plesch carbon monoxide method of determining the total amount of blood. He maintains that the carbon monoxide dissociation curve depends upon the carbon dioxide present, the amount of temperature, etc. Müller has worked with Barcroft and says that this is likewise Barcroft's opinion. For his determination Müller places a mixture of nitrous oxide, one half per cent of carbon dioxide, oxygen, and nitrogen, in a spirometer and freezes it. He used to analyze the gas in a refractometer which he finds is very good for his work.

He absorbs the carbon dioxide by potassium hydroxide and determines the oxygen by sodium hydrosulphite but finds it necessary first to saturate the solution with a little of the gas to have it saturated with nitrous oxide.

I was present at an experiment he was making on Dr. Klein. As a matter of fact the thing was handled very comically. There were too many things to be done by too few people and the experiment was a failure. I took two or three photographs. (See figs. 63 to 67.) I found that no description had been published of the gasometers which he was using but they seemed to be rather clever.

Franz Müller is much interested in the metabolism of children. He has a place near the North Sea where he is studying the influence of climate on metabolism. He studies the intake and output for a number of weeks, also the output of nitrogen, sulphur, phosphorus, chlorine, etc. He says that he feeds the children the equivalent of 4000 calories per day when in the North Sea region, as they are running about actively all day long. This is an enormous calorie requirement and is based solely upon the amount of food fed to the children. I am not sure that this amount is actually controlled. It may be simply the amount of food purchased.

Franz Müller impressed me as being by far the most genial and companionable of Zuntz's assistants but I was not very favorably impressed with the accuracy of his work.

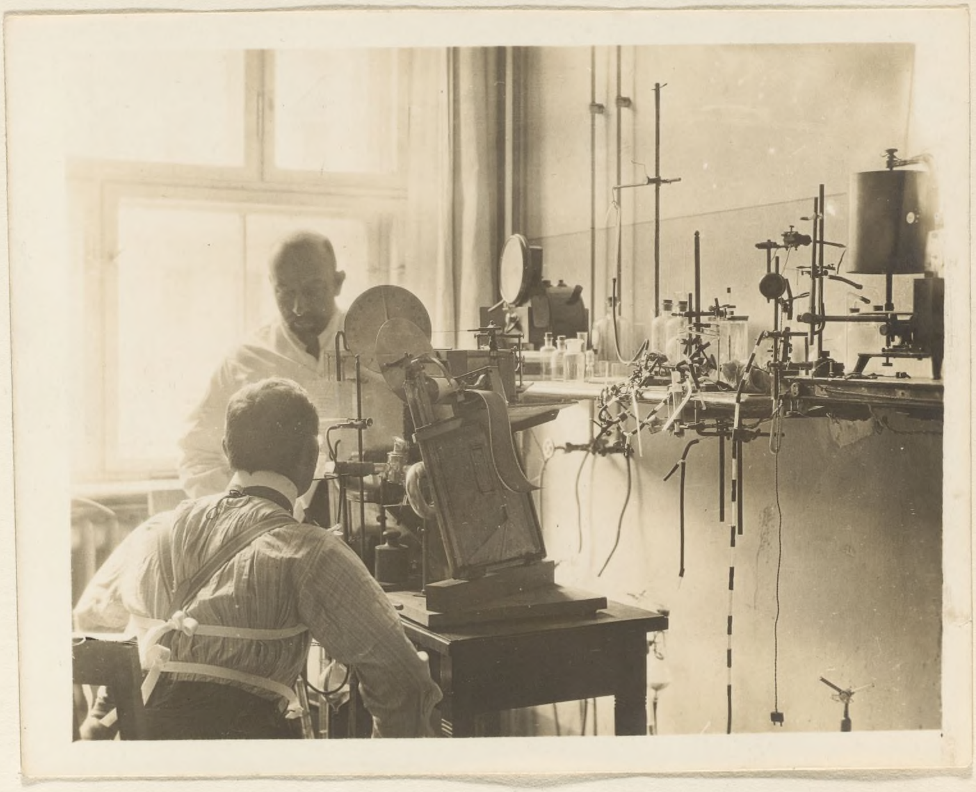


Fig. 63. Professor Franz Müller (facing) making an experiment in Zuntz's laboratory on the volume of blood in the body, using two gas meters of special construction, containing nitrous oxide, carbon dioxide, nitrogen, and oxygen.

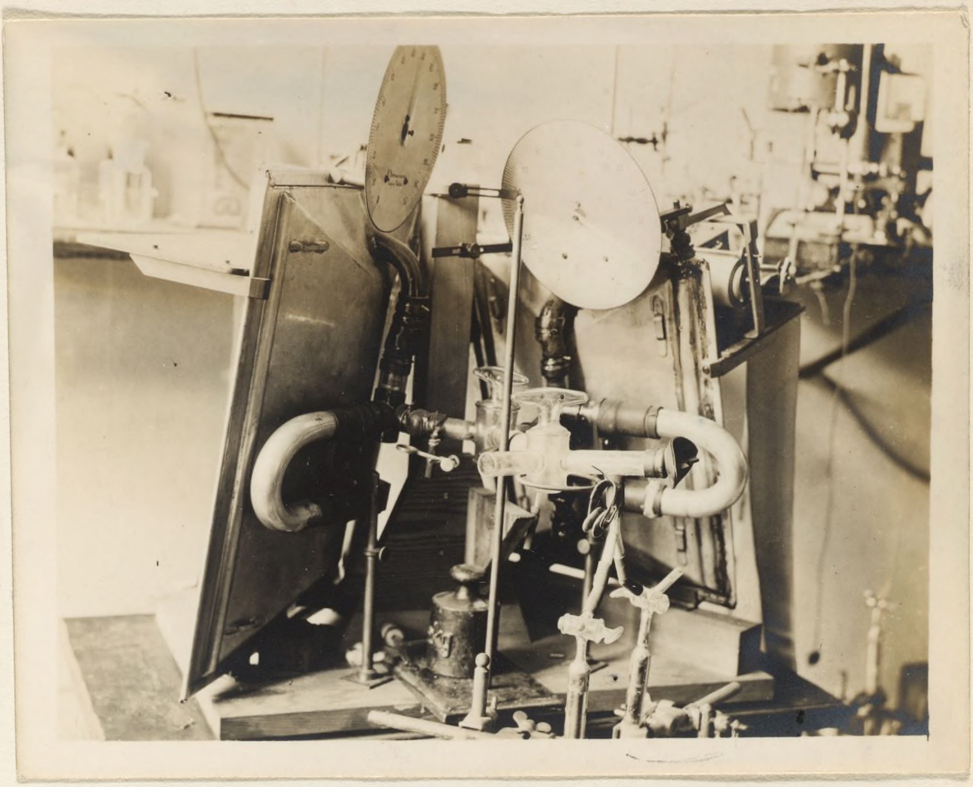


Fig. 64. Details of Professor Franz Müller's respiration apparatus in Zuntz's laboratory in Berlin.

The two peculiar spirometer constructions are shown in the immediate foreground, together with the mouthpiece and the sampling pipette for the alveolar air. The dials indicate the volume of air remaining or being removed from the spirometer.

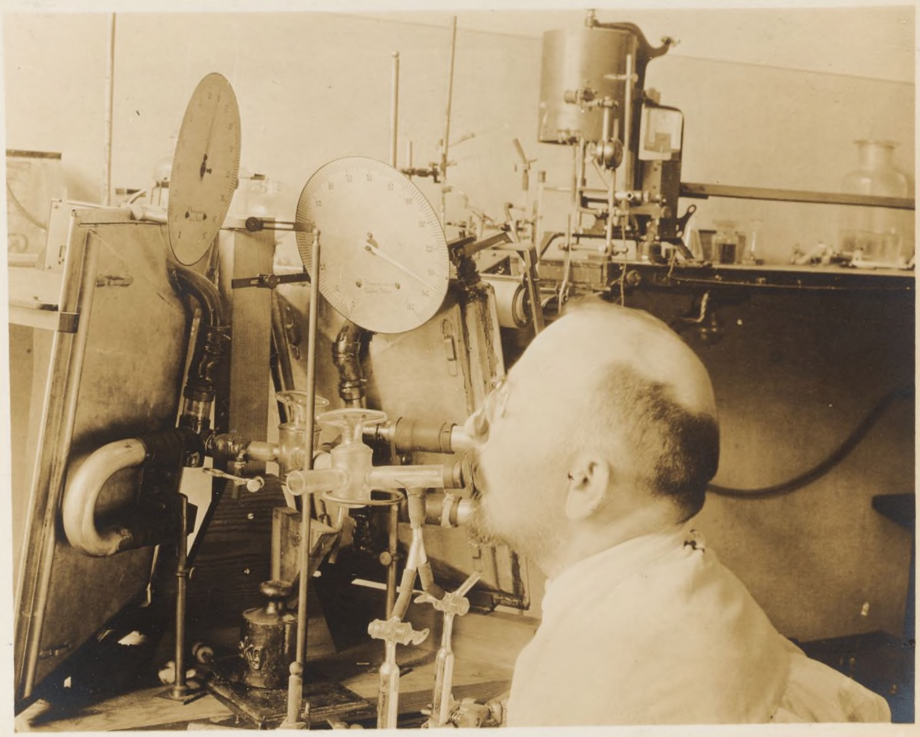


Fig. 65. View of Professor Franz Müller using his apparatus for studying the flow of blood in the body.

The two spirometers, the mouthpiece, and the half-inch tubes for taking the alveolar air samples are in the foreground.

Fig. 66. Dr. Klein with a pneumograph attached for the experiment with Franz Müller's respiration apparatus for studying the volume of blood in the body.

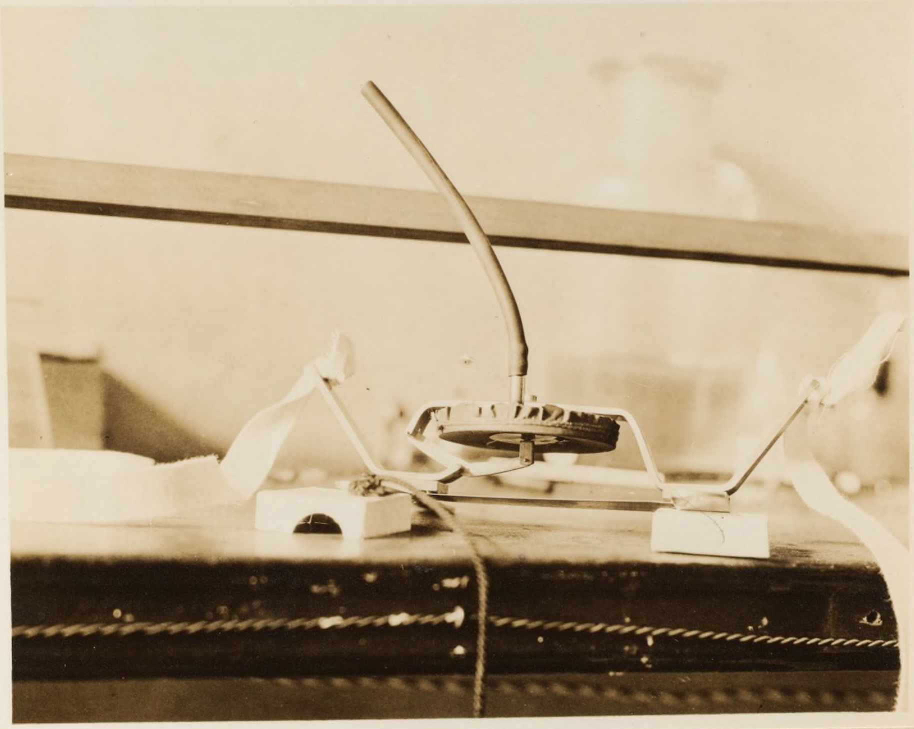


Fig. 67. Details of pneumograph fastened around the chest for recording pneumograph tracings of respiration in Franz Müller's experiments in the Zuntz laboratory in Berlin.

MOSCOW, RUSSIA.

Frauen-Hochschule.

Professor Schaternikoff.

Since his break with the University of Moscow Professor Schaternikoff has had a post in the Frauen-Hochschule. (See fig. 68.) This is a beautiful modern building of concrete with wonderful lecture rooms and is situated some distance from the city. Here Professor Schaternikoff has a few rooms quite well fitted up for physiological research. He evidently is a man of many ideas and interests. When he told me of the very small amount of money he had available for purchasing apparatus and supplies and paying assistants, I was utterly dumbfounded at what he had accomplished with almost nothing.

Of particular interest to me, of course, was his respiration chamber. (See figs. 69 to 76.) This is large enough for a person to sit in and has a capacity of 10 cubic meters, its dimensions being 2 x $2\frac{1}{2}$ x 2 meters. It is painted white inside and has a mercury pump of four barrels on the Blakeslee annular space principle. He uses sodium hydroxide absorbing bottles much as one uses a Müller valve, one set before the pump and one after it. These form the valves of the pump itself. The apparatus had not been completed at the time I was there and Schaternikoff was still working on it. He had devised a scheme for making the air bubble through strong caustic soda or strong caustic potash. Although this scheme was of course very desirable from a certain standpoint, yet to one who has had experience with soda lime the method seems very troublesome and very bulky.

I was much interested in his reconstruction of the Zuntz valves

Fig. 68. Professor Schaternikoff in the room containing the respiration apparatus in the Frauen-Hochschule in Moscow.

Fig. 69. Entrance to the respiration chamber of Professor Schaternikoff in Moscow.

Fig. 70. Another view of the Schaternikoff respiration chamber in Moscow showing the four-cylinder pump.

In the immediate foreground is a large box with coil of pipe for maintaining temperature equilibrium, either with cold or with warm water.

Fig. 71. Professor Schaternikoff of Moscow and his respiration apparatus for women, showing the four-cylinder pump and the large valve.

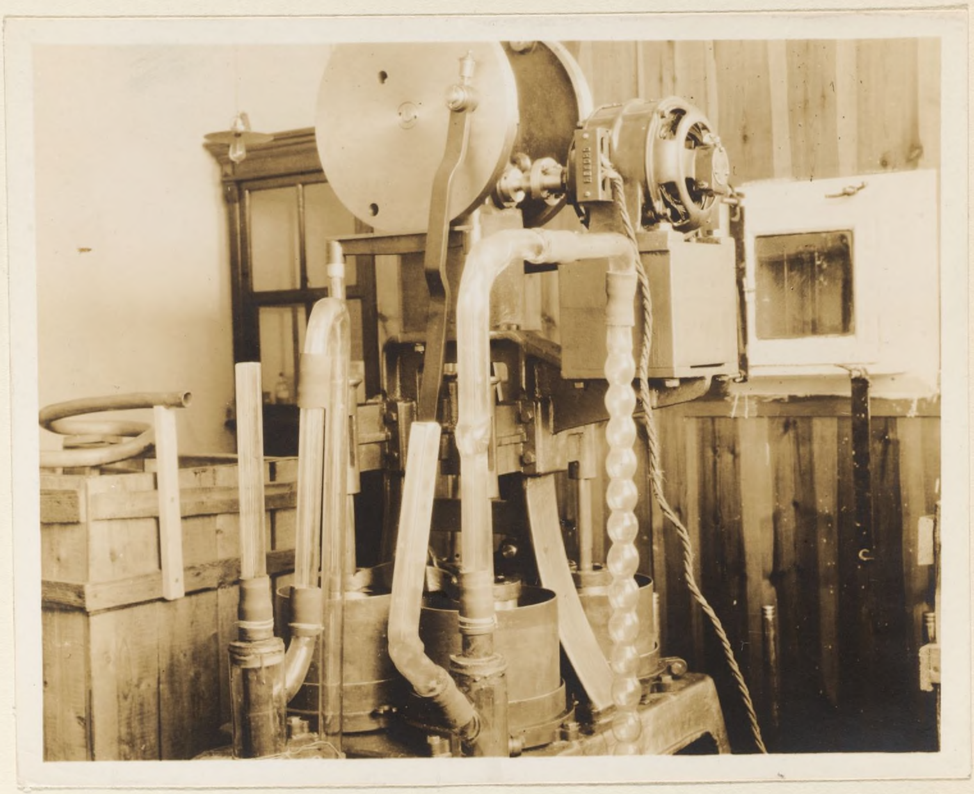


Fig. 72. Another detail of Schaternikoff's respiration pump in Moscow.

Fig. 73. A nearer view of the details in the valve system of the Schaternikoff respiration apparatus in Moscow.

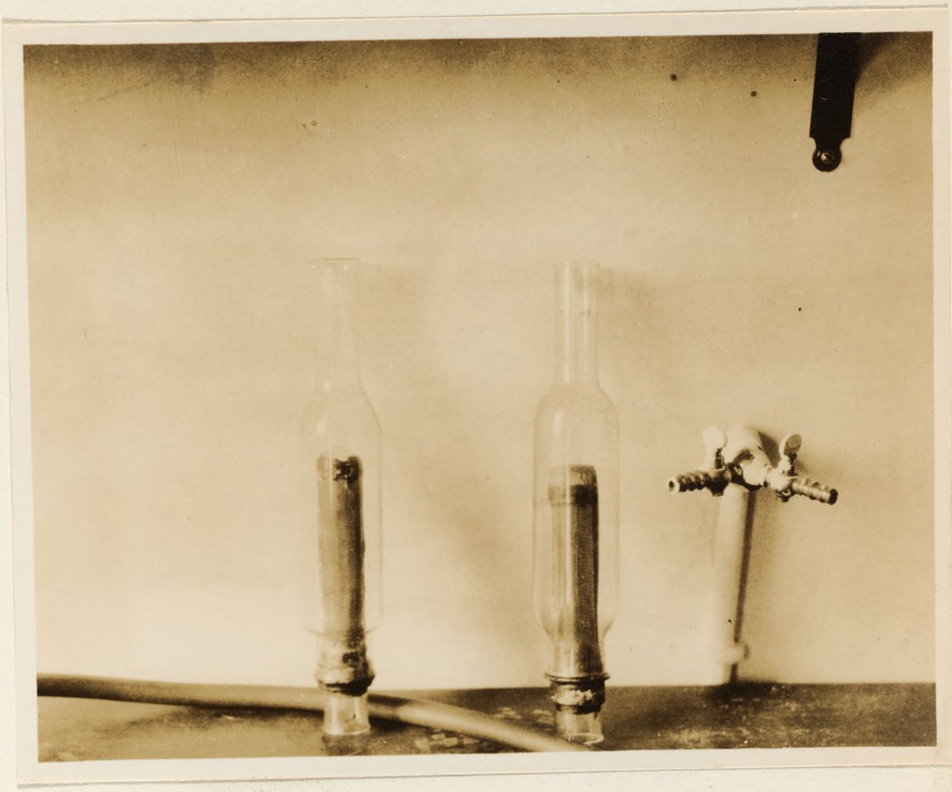


Fig. 74. Modified Zuntz valves used by Schaternikoff in Moscow.

To prevent the rubber form from collapsing and entering the opening Professor Schaternikoff uses a piece of wire gauze which he wraps around the opening, and which seems to be very practical.

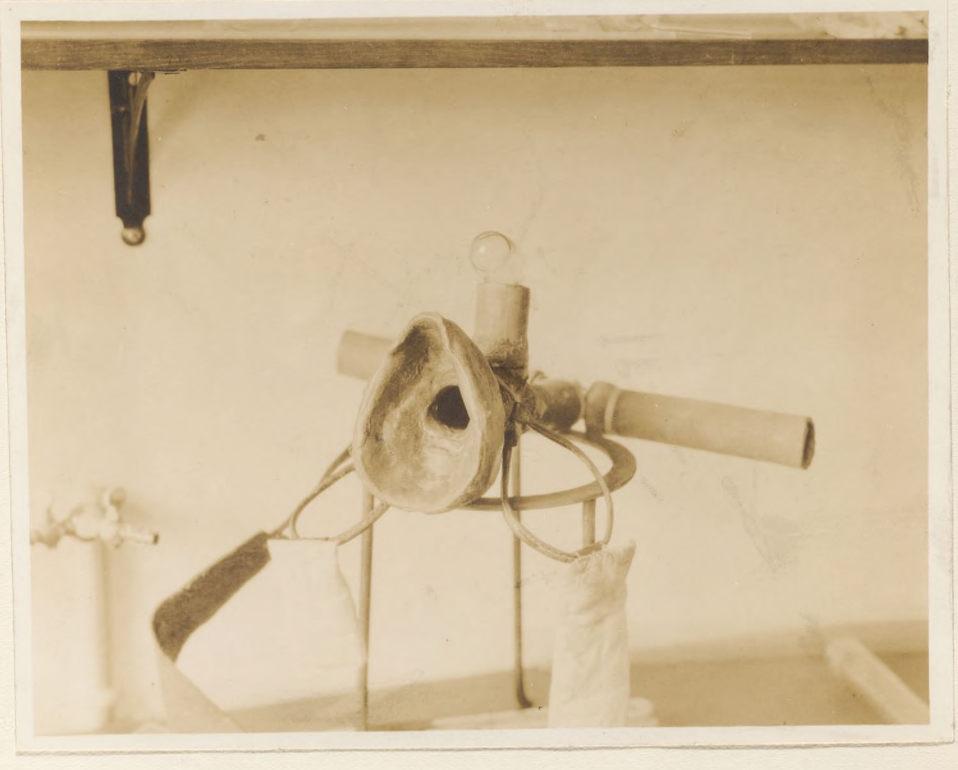


Fig. 75. Mask used by Schaternikoff in mouth breathing for respiration apparatus.

Fig. 76. Details of carbon-dioxide absorbing cylinders and valves in the Schaternikoff respiration apparatus in Moscow.

The valves act as large Müller valves. The numerous perforations in the plates show where the air bubbles through the caustic soda. The plates are made of porcelain or hard rubber.

for use in respiration experiments. He has placed wire gauze over the slit in the glass tube to prevent the rubber from collapsing. This seems to me rather a practical point.

Although his father died the second day after I arrived in Moscow Professor Schaternikoff was extraordinarily kind in taking me about and in showing me every attention. I enjoyed extremely every moment of conversation with him and consider the visit to his laboratory well worth the time. Although he reads English very little, we were able to converse very comfortably in German. Of course he was somewhat upset by the severe illness and death of his father and I did not have time for extended scientific discussion, much to my regret, for he impressed me as being a good and extraordinarily keen man, and a capable worker.

Agricultural Experiment Station.

I spent one morning at the Agricultural Experiment Station which is likewise some miles out from Moscow and found some work being done on the extraction of material. I saw many interesting things, but none of particular value from the standpoint of the Nutrition Laboratory. A method for the determination of carbon dioxide by gas analysis which I had heard of proved on investigation to be of no use to us as it had to do only with soil.

University of Moscow (Physical Institute)

Professor Swientoslawski and the laboratory of Professor Louguinine.

The laboratory of Professor Louguinine has been the great thermochemical centre of Eastern Europe for many, many years. (See figs. 77 to 79.) Louguinine was a very rich man and had bought a great many pieces of expensive apparatus, paying enormous sums to fit up his laboratory and making it practically a rich man's hobby. His private laboratory was at first located in Paris but it was subsequently removed to Moscow. While in Moscow he had a great many students and was very much beloved by them. In the laboratory I saw a very fine enlarged photograph of Louguinine and since my visit there his successor has sent a copy of the photograph to the Nutrition Laboratory. Professor Louguinine died some years ago and part of his family now live in Paris and a part in St. Petersburg. Professor Louguinine was a disciple of Regnault, following in his footsteps closely, and the whole laboratory technique shows the influence of Regnault and also of Dumas. Louguinine is unquestionably one of the foremost of those who have contributed to calorimetry in this decade.

Louguinine's direct successor was Schukarew who wrote a book with him (Methods of Calorimetry). The present holder of the chair is Dr. Swientoslawski, a young man from Kiev, an engineer, I think. He is not very highly estimated in Moscow but was given the position there because there had been a change of professors made by the university administration and it was difficult to get a satisfactory man for the place.

The laboratory itself is remarkably clean, neat, and well kept.

Fig. 77. A general view of Professor Louguinine's laboratory in Moscow.

Over the door is a painting of Professor Louguinine. On the left are seen many forms of bomb calorimeters enveloped or water-jacketed.

Fig. 78. A general view of the right side of Professor Louguinine's laboratory in Moscow, showing the various apparatus for studying the specific heat and the expansion of metals by heat.

Fig. 79. Study formerly occupied by Professor Louguinine in Moscow.

At the left is shown a calorimeter for use with bomb, and the reading telescope.

I think I have never seen its equal. The most complicated apparatus is kept in the most perfect order. For instance, I saw a Golaz-Berthelot bomb calorimeter containing 1500 grams of platinum, which was 27 years old and which had been used for about 15,000 combustions and yet, as it rested under a bell-jar cover, it looked as if it were absolutely new. This illustrates the value of handling instruments with the greatest care. (See figs. 80 and 81.)

Dr. Swientoslawski was much interested in bomb calorimetry. He says he has determined the heat of combustion of benzoic acid by five methods and that they all agree, with the use of the Regnault-Pfaundler law. For example, he would burn the benzoic acid in water and obtain a rise of 3 degrees and again in petrol and obtain a rise of 6 degrees. With the specific heat of petrol about 0.433, there would be in the second case a radiation altogether different from that in the first case, and yet by means of the Regnault-Pfaundler formula, he got the same results. He remarked that his values for benzoic acid and sugar were far different from those of Fischer and Wrede.

Dr. Swientoslawski uses fine platinum wire for ignition but he also likes for this purpose small collodion bags or pouches. These are made by coating the inside of a glass cap, such as is employed to keep the dust off of the stopper of a reagent bottle, with collodion and allowing the collodion to dry for a number of hours or days. These little collodion sacks can then be removed and weighed. Usually they weigh not more than 50 milligrams and as the heat of combustion of collodion is generally not more than 2.5 calories per gram, the correction is very small. Dr. Swientoslawski says that this is a very good method and that he gets fine results with it, even with naphthalene. He usually develops about 8,000 calories inside his

Fig. 80. The Golaz-Berthelot bomb calorimeter used by Professor Swientoslawski in Moscow.

In the immediate foreground, under one of the bell-jars, is the Golaz-Berthelot bomb; at the right is seen a Kroker bomb. The bomb in the centre is interesting inasmuch as it has been used for about 1500 combustions and is still in perfect condition. It did not show any more wear than would a diamond. There was an enormous mass of platinum in this bomb. This illustrates how carefully all the instruments in this laboratory are kept.

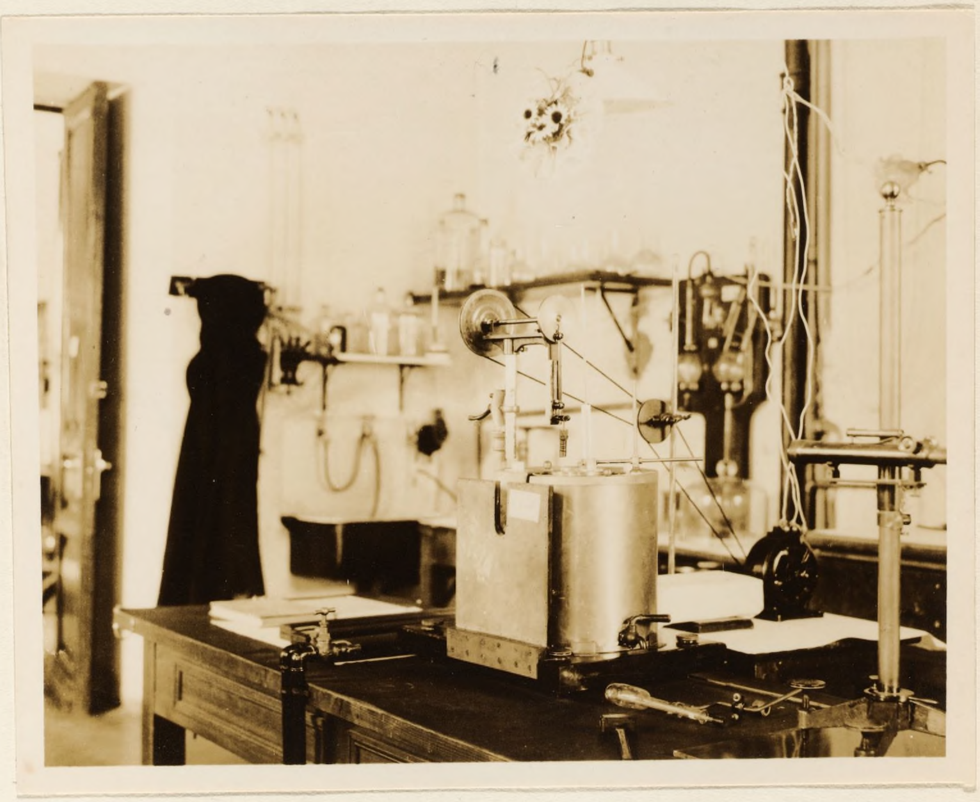


Fig. 81. Details of a calorimeter in Professor Louguinine's laboratory showing the reading telescope at the right and the stirring arrangement.

bomb, i.e., a rise of 3 degrees. He has had a great deal of trouble with oxygen and suspects there are traces of hydrocarbon. If there is even 0.001 per cent of hydrocarbons present, he maintains it will affect his results.

In discussing Professor Richards's adiabatic calorimeter Swientoslawski expressed very little belief in its theoretical correctness and still less in the bomb calorimeter developed by Higgins and myself. He says that the temperature curve for the first three minutes can never be exactly approximated either by adding the acid in Professor Richards's case or the electrical heat in ours, for with each substance there is a different rapidity and different curve and at some time or other there must be a loss or storage of heat for a few seconds. Personally he thinks the Regnault-Pfaundler method of correcting for cooling is the right one. He had not tried out the adiabatic problem thoroughly himself, but was discussing the work from a theoretical standpoint. He is now spending considerable time repeating work of his own and some of Professor Richards's work. I told him I would ask Professor Richards to send some of his reprints as I think he ought to understand exactly what Professor Richards means.

Swientoslawski does not like the Jaeger and Steinwehr calibration of the Fischer and Wrede bomb with the manganin wire about the outside for he maintains that the latent period is considerable, i.e., the heat is not instantly given up. He has personally tried the experiment and finds that a considerable amount of heat is not so quickly equalized in the bomb as one would expect. He has used a manganin coil inside of a fake bomb and found that a large amount of heat was not given off quickly. He expects to publish his results soon but probably in Russian.

In speaking of the large number of scientific instruments in the laboratory (see figs. 82 to 84), I asked Swientoslawski about a cathetometer. He said that he had had no luck with them and does not believe in them at all.

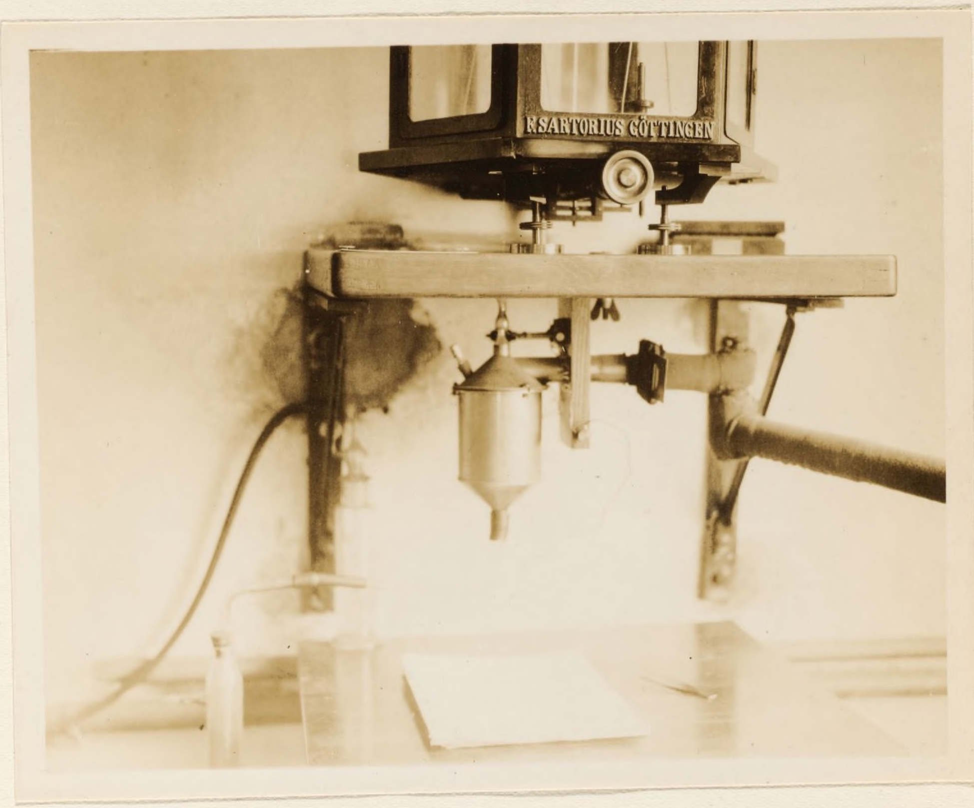


Fig. 82. Apparatus for determining specific heat in the laboratory of Professor Louguinine in Moscow.

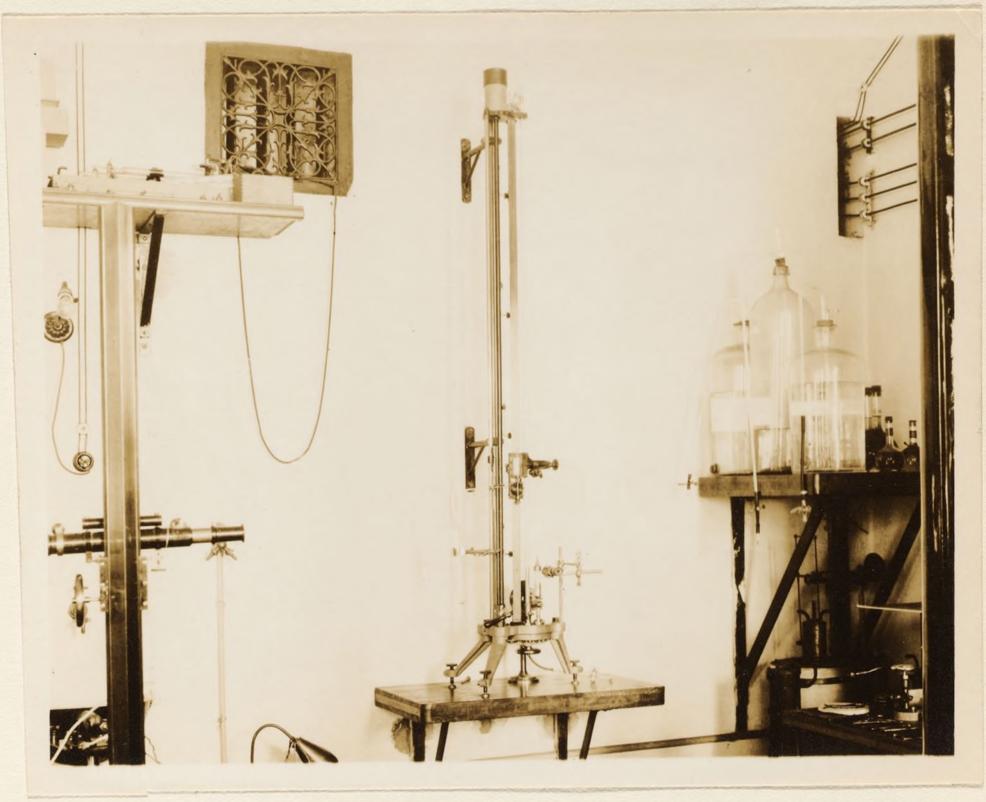


Fig. 83. A very delicate hydrogen gas thermometer in Professor Louguinine's laboratory in Moscow.

Fig. 84. Another view of the hydrogen gas thermometer in Professor Louguinine's laboratory in Moscow.

Professor Bachmetjew.

Professor Bachmetjew, who is a professor in the University of Sofia, called at my hotel one evening and I saw him the next day also. We discussed his experiments on freezing animals. He has been freezing cold-blooded animals slowly and finds he can keep them frozen for weeks if not, indeed, months and then thaw them out and have them return to life, that is, if he does not freeze them below a certain temperature. He maintains that there is a basal metabolism which can be altered by giving the animal different percentages of carbon dioxide, i.e., he can get one-fourth of the ordinary metabolism by giving carbon dioxide or can get a higher level. In working with rabbits he first gives them carbon dioxide to produce hibernation, so to speak, and later he freezes them. He has not succeeded in bringing the rabbits to life again but maintains the preliminary experiments are very satisfactory indeed.

In taking the rectal temperature of cold-blooded animals during the freezing process Bachmetjew has noted that as the animals cool, the temperature gradually falls off to a certain point and then suddenly rises, this change corresponding somewhat to the rise in temperature noted when glacial acetic acid is supercooled in the determination of the freezing point by the Raoult method. Bachmetjew is very anxious to try his methods on warm-blooded animals and says that he has already tried it with bats.

The experiments were still in progress in Sofia where he is a professor in the University. He is going to St. Petersburg to continue the experiments on apes and then finally to Paris to work on animals and men. There is a new law in Paris which permits criminals, particularly those convicted for a capital offence, to choose between undergoing some scientific experiment or being decapitated or hung. If they choose the former, the experiments must require a major operation or something of that kind. Bachmetjew proposes to freeze some of the criminals and see if he cannot thaw them out as he has animals.

He told me of an interesting experiment he made with certain larvae which had been fused into a Geissler tube with oxygen and then left for a month in a temperature of -5° C. They produced no carbon dioxide during this time. When thawed out they came to life again.

Bachmetjew seemed to be a rather bombastic old chap but extremely interesting.

ST. PETERSBURG, RUSSIA

Institute for Experimental Medicine

Professor Pawlow.

At the time of my visit Professor Pawlow's new institute was just being completed. It is a marvelous laboratory, rather small but peculiarly constructed. It is so arranged that there is no vibration of any kind; all the stairways are isolated, the walls are very heavy, and each floor is a huge concrete slab set in a cushion. It looks not unlike the Castle of Chillon and, indeed, is called "Castle Chillon".

At present Professor Pawlow has no interest in the use of fistulas at different points in the alimentary tract but tells me that his former associate, Professor Babkin, who is now in another Russian university, does much work in this line.

Pawlow is entirely wrapped up in psychological problems and the so-called "unbedingter Reflex". He had trained one dog to 16 different reflexes after working with him a year. He then operated upon him and removed the large muscle over the skull so that later he might get at the skull easily and remove a part of the brain. A year later he operated again and removed three convolutions of the brain, the skin centre and the ear centre. I personally witnessed this last operation. (See fig. 85.)

Pawlow has a great many women assistants who are working in hospitals and who also work outside with him. At the time I was there he had 20 different dogs under experimentation. He invariably does his own chloroforming and does not lose any dogs. He said that a few

Fig. 85. Professor Pawlow and his assistants operating in the operating room of Professor Pawlow's laboratory in the Institute for Experimental Medicine in St. Petersburg.

years ago, when he had assistants do the chloroforming, he lost a great many of the animals as the assistants would become careless and look around while giving the chloroform. He says that when he has been working with one dog for a year and it has cost him several thousand rubles, he cannot afford to take any chances of losing the dog. He consequently does his own chloroforming. After he gets the dogs into the first stages of anaesthesia, the assistants go ahead with the work. Pawlow is most careful in operating and his remarkable aseptic precautions are almost perfect. Of special interest to me was the fact that he allowed no smoking anywhere in the laboratory.

Most of his work is done with the reflexes of the salivary gland. A permanent salivary fistula is attached to the dog which causes no great inconvenience for the dog wears it for months at a time. After the insertion of the fistula the animal is subjected to a certain stimulus and immediately thereafter fed. A typical illustration will suffice. A dog which had been provided with a salivary fistula was stimulated by placing an electric iron on the shoulder. The first two days the hot iron caused the dog to flinch, yelp, and howl, but it was fed immediately thereafter and in a relatively short time there was no reaction to pain, no twitching, no protective reflexes of any kind; the only reflex was the rapid flow of saliva. Finally he could subject it to almost any test, such as sticking a needle into it or even burning or cutting out a part of the flesh and the dog would not wince, draw away, or yelp, the only effect being the flow of saliva. The intensity of the flow was determined by pneumatic transmission, a rather ingenious device of Hanicke. A little manometer was placed in the fistula and the drops of saliva counted by noting the pressure on the manometer.

Pawlow discussed quite freely a number of men, particularly Dr. Yerkes. He said there were two individuals in Yerkes, the man and the experimenter, and he found it very hard to unite the two characters.

Jennings and for Parker.

Pawlow thinks that all methods of studying animal psychology are wrong. In discussing a maze, he says "What is it? You can't say what it is or what it means. Open a door, what is that? It is extremely complex and incapable of being analyzed." Personally he believes the simplest things are the best. In speaking of the electrocardiogram he said the whole operation was too complicated. On the other hand he thought the flow of saliva was the simplest reaction and was a very happy solution.

In discussing the question of the pain reflex I wondered if it were not due to the fact that on heating the same point continuously he had simply killed the nerve, and that the dog had no pain because it had no nerves. Pawlow replied that he had tried out this point and that the absence of pain was not due to affecting the nerves at a particular point.

In speaking of his preparation of "Magensaft" he told me that after all the Russian fast days there was invariably an increased consumption of "Magensaft" which was a proof that the people compensate by overindulging after fasting. The "Magensaft" sold for about 80 kopecks, i.e., about 40 cents for a 200 c.c. bottle.

With regard to his general field of research Pawlow said he wanted to keep the reflex field for himself for a while as otherwise much difficulty would arise; experimenters would get a lot of wrong results which would upset the whole thing because they could not easily acquire the technique.

Pawlow says that no one else has an assistant equal to Hanicke who is a wonder and very skilful with fingers and hands. Hanicke is very much interested in the new building.

In speaking of vivisection Pawlow argued that until a law is passed against hunting no word should be said against vivisection. He is not willing to discuss the matter until that point is settled. With regard to Bachmetjew's idea of freezing individuals Pawlow says he wants to be frozen and thinks it would be a good plan to have a city cellar where they could freeze men for fifty years and then thaw them out. He wished he could be frozen and thawed out again at the end of fifty years. He personally suggested the idea of freezing individuals to Bachmetjew.

It is almost impossible to follow Professor Pawlow in his great variety of ideas and rapid conversation. His brilliancy and his intense energy is shown in everything he takes hold of. He certainly is a very remarkable man. One can easily see why he stands so high in Russia. Pawlow has been a professor in the Military Medical Academy and the Institute for Experimental Medicine, and is also connected with another research laboratory. He goes to one laboratory in the morning and another in the afternoon. He is a very busy man but is intending to give up some of his duties. He says that some of the conditions at the Military Medical Academy have been quite intolerable and as he finds his position there very difficult, he wants to sever his connection with it.

Women's Medical College

Professors Likhatscheff, Albitsky, Kartaschefsky, and Sskolow.

Professor Likhatscheff is now the secretary of the Women's Medical College and is very busy in academic work. (See fig. 86.) Perhaps this accounts for the fact that there is nothing new in his laboratory. I found him much interested in the alcohol program. He told me about a lot of work he had done giving alcohol to dogs in large amounts. He used the Nicloux method and found that alcohol did great damage to the kidneys, even when diluted. The percentage of alcohol excreted after ingestion remained always the same even with large doses. He also found that alcohol appears in mother's milk.

He spoke to me about an extensive bibliography in Russian by Viazemsky of the alcohol literature and since my visit he has sent the bibliography to the Nutrition Laboratory and it has been translated into English.

Likhatscheff told me that there was a new institute recently opened for cancer research but he had never been there himself and did not know where it was located other than that it was a short distance out from the city.

I was not able to get in touch with Dr. Boldyreff as he had left St. Petersburg but Professor Likhatscheff will secure information with regard to Boldyreff's spirometer and send it to me later.

There are now in St. Petersburg two or three copies of the small Regnault-Reiset respiration apparatus devised by Professor Likhatscheff. I also saw at the Women's Medical College a Likhatscheff apparatus with an enormous pump used for an exceedingly small ventilation.

Fig. 86. A group of Russian scientists at the Women's Medical College in St. Petersburg.

At the left in the lower line is Professor Likhatscheff, at the lower right is Professor Albitsky. The man standing in uniform is Lieutenant Kartaschefsky.

Professor Albitsky has left the Military Medical Academy after thirty years' service, owing to difficulties there. He now has an appointment in the Women's Medical College and is making carbon dioxide determinations on the rabbit. At the time of my visit the investigators at the Medical College were much interested in the injection of isotonic and hyper- and hypo-tonic solutions. There was no control of muscular activity. If one solution produced pain and the rabbit was uneasy the results were entirely worthless. I spoke of this point and promised to send to Albitsky, Likhatscheff, and Kartaschefsky a description of the work done on our respiration apparatus showing the effect of body activity.

Dr. Kartaschefsky is now an assistant in the laboratory of the Women's Medical College and is also still at the Military Medical Academy but has very little to do with it. He does most of his work with Professor Albitsky.

Professor Likhatscheff took me to see the new surgical clinic which is a gift from a member of the Nobel family. It is wonderfully fitted up in every way.

Professor Sskolow likewise has a nice clinic. I was especially interested in the isolation house which has four rooms, each with two entrances, with double glass doors. Everything is perfect for observation. There is no communication so there can be no contagion. The different wards are separated by glass walls, so that one can see the whole length. I spent considerable time in studying Professor Sskolow's differential pneumograph which is extremely simple, showing inequalities in the lung ventilation very clearly indeed. He illustrated the pneumograph with a doll and a baby. This interesting apparatus should be of value to us. I took various photographs of it (see figs. 87 and 88) and Professor Sskolow gave me a curve and a reprint in connection with it.

I was not very much impressed by the rather slipshod appearance of the Women's Medical College but found the new surgical clinic much neater in appearance. There was a sign over each wash basin which attracted my attention and which Professor Likhatscheff translated as "Don't be afraid of water. Wash yourself often." On the whole the infant clinic and the new surgical clinic impressed me very favorably indeed, as marking a great advance in Russian surgery and medicine.

Aside from the experiments in the laboratory of Professor Likhatscheff and Professor Albitsky there was not much work in the line of metabolism going on in St. Petersburg at the time of my visit and yet it was most interesting to meet the men who had done so much, Albitsky in particular. My only regret was that Albitsky does not speak German; indeed, it was only through the kindness of Professor Likhatscheff that we were able to carry on any conversation together.

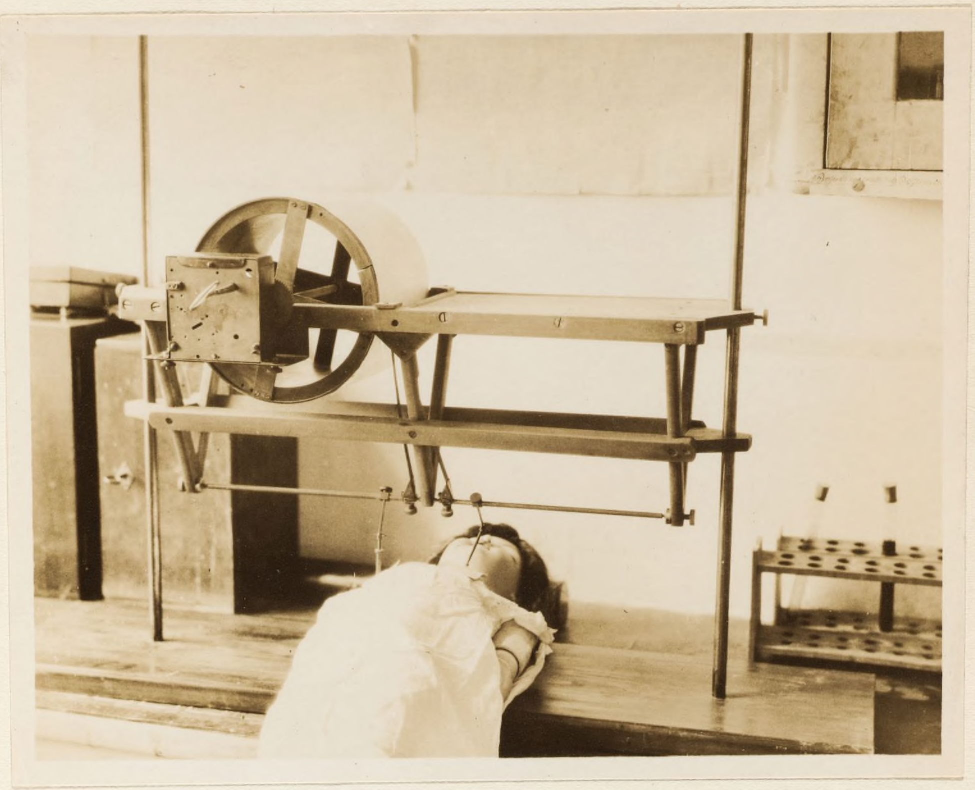


Fig. 87. The double writing pneumograph of Professor Sskolow in St. Petersburg.

The apparatus is demonstrated in this photograph on a doll. Two delicate levers are placed upon the chest, and by the rise and fall of the chest wall any inequalities of respiration are recorded on the drum above.

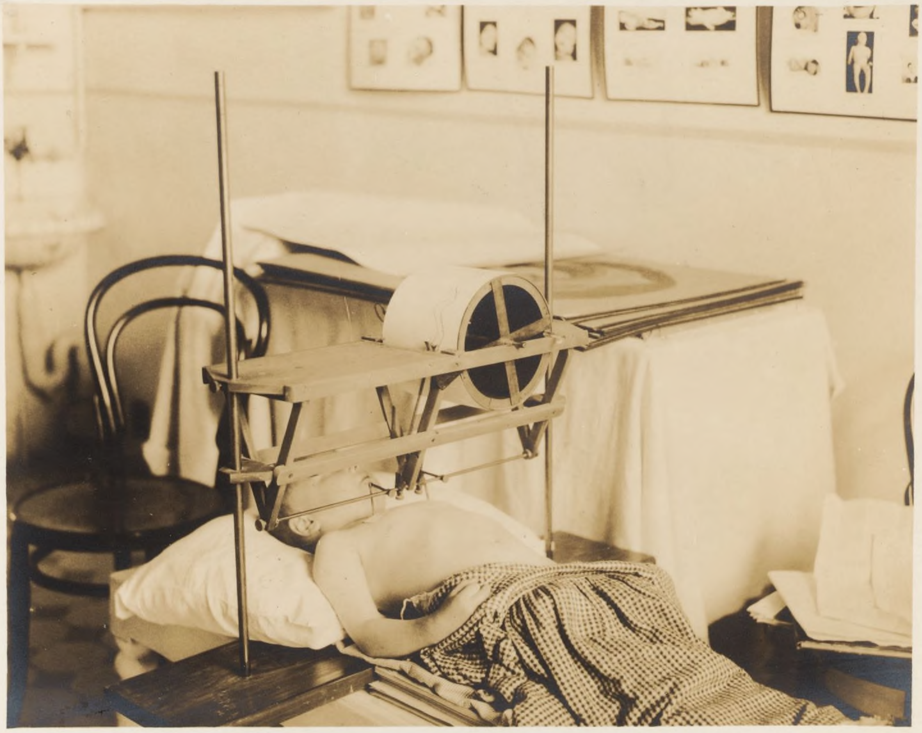


Fig. 88. Demonstration on a living child of the Sskolow differential pneumograph in St. Petersburg.

The tracing is shown very clearly on the drum.

HELSINGFORS, FINLAND.

University of Helsingfors (Physiological Laboratory)

Professor Robert Tigerstedt

On my previous visit to Helsingfors I was so favorably impressed by the wide range of experience and wonderful knowledge of literature that Professor Tigerstedt possesses that I looked forward, as I always do, to seeing him again and having a personal chat with him. In recent years he has devoted himself chiefly to literary work, being the editor of the Skandinavisches Archiv für Physiologie and having produced a number of compendious volumes. Nevertheless he is carrying out several researches in the Institute, ably assisted in these by his son, Dr. Carl Tigerstedt, who is a Privat-Docent of the University and an assistant in the laboratory.

In discussing the matter of writing with Professor Tigerstedt, he maintained that the "Handbuch" or textbook is a most important part of physiology. In other words, he said that the Handbuch is the life of physiology, being handed on from generation to generation. A man rarely looks up old literature but he depends upon a handbuch; it is therefore a great service to the progress of science to prepare good handbooks. This would naturally be his opinion as he has written such admirable handbooks.

In discussing the physiologists of Europe in general and the German physiologists in particular, Tigerstedt felt that German physiology is now at its lowest ebb and that the English physiologists are at the highest flood of their success. He said that the Germans are now going to England for ideas. Among the English physiologists he rated Dr. Barcroft very highly.

Professor Tigerstedt has expressed his regret to many people that

Professor Johansson is so occupied with administration matters and commission affairs in Stockholm. As Tigerstedt puts it, "Johansson drives everything except physiology". In other words, he gives his attention to everything except physiology.

In speaking of Rubner Tigerstedt maintained that Rubner is not clear in his writings. He contrasted him very strongly with Helmholtz. Helmholtz, he said, would never have to tell his reader "You misunderstand me" because his reasoning, although not always correct, is very clear. Tigerstedt thought this was in striking contrast to most of Rubner's writings.

Tigerstedt's opinion of Professor Fano was very poor and also his opinion of Professor Loewy of Berlin. He felt that Zuntz had done a good deal of work but had not been of very great service to physiology in general.

In his own laboratory he was still much interested in body temperature measurements by a resistance thermometer, particularly with regard to the influence of work. He had carried out considerable research in cooperation with his son, Carl, on fatigue in work. Carl had devised a lever for writing the number of times the leg is raised during work.

Using the large respiration chamber, they had been studying the respiration during the severe muscular work of breaking rocks, the zinc lining of the chamber showing dents where the bits of flying rock had struck. Practically all of the results of this study on muscular work, regarding both the carbon-dioxide output and the influence of the environmental temperature on the carbon-dioxide output, were contrary to the views that Rubner had put forth.

In operating Tigerstedt told me that he used a great deal a mixture of iodine and xenophone (?) powder which he found a perfect antiseptic.

Dr. Carl Tigerstedt was working with a platinum string galvanommeter and a Gaerten photographic apparatus with which he was able to get a velocity of 5 meters per second.

I spent considerable time in my visit to Helsingfors in talking over with Professor Tigerstedt and his son, Carl, the possibility of the latter's coming to America and working in the Nutrition Laboratory as Research Associate. After my return to America this was arranged for and Dr. Tigerstedt came to the laboratory in the early fall of 1913. He worked here until about the first of January, 1914, when he returned to Helsingfors.

Professor Tigerstedt impressed me with his wonderful knowledge of literature. He has a marvelously interesting and unique library which possesses every special paper one could think of on any branch of physiology. The amount of work he has accomplished personally in writing is stupendous. Probably he has published more handbooks, text books, and things of that kind on physiology than any other one man. Most large handbooks in physiology are written by several men, the editor simply putting the different sections together, but a great deal of Professor Tigerstedt's handbooks have been written by himself. He certainly has a wonderful command of literature. He himself is charming in his very graceful humor and willingness to talk and chat with you by the hour upon any phase or branch of physiology, and is, withal, one of the most genial and pleasant men one meets anywhere in Europe. (See fig. 89.)

Fig. 89. Professor Robert Tigerstedt of Helsingfors in his library.

The small picture on the wall is that of Professor Atwater.

STOCKHOLM, SWEDEN

Karolinska Institute (Physiological Laboratory).

Professor Johansson.

Professor Johansson is apparently as occupied as ever with commissions, administrative duties, military matters, and general public affairs. He evidently feels that his best service to Sweden as a whole is in not confining himself solely to scientific research at the Institute, but in utilizing his ability in figures and in statistics in working on large commissions. He has been for many years actively identified with some of the most important commissions on moral, social, and military problems in Sweden, particularly with regard to military hygiene and gymnastics. He says, however, that his activity in certain commissions is now nearly at an end and he expects soon to resume work in physiology.

The Institute has been considerably enlarged since I was there three years ago. The new part is now completed and ready for use. I was impressed by the fact that although the Institute is a very good one, there is very little opportunity for work and there are very few workers engaged there at present. Since Dr. Landergren's death no one has actively taken his place, although one or two young assistants are interested in some research work. The original Sondén-Tigerstedt apparatus had been destroyed. A small chamber was being constructed in the basement and also a subsidiary chamber about the size of our bed calorimeter. These were not completed when I was there but I took a photograph of the latter. (See fig. 90.) They were setting up the Johansson gas sampling apparatus (fig. 91) and apparently expected to begin work with it shortly.

Two assistants had been doing some work on the amount of carbon

Fig. 90. The new respiration chamber of Professor Johansson in Stockholm.

This chamber is built after the model of the bed calorimeter in the Nutrition Laboratory.

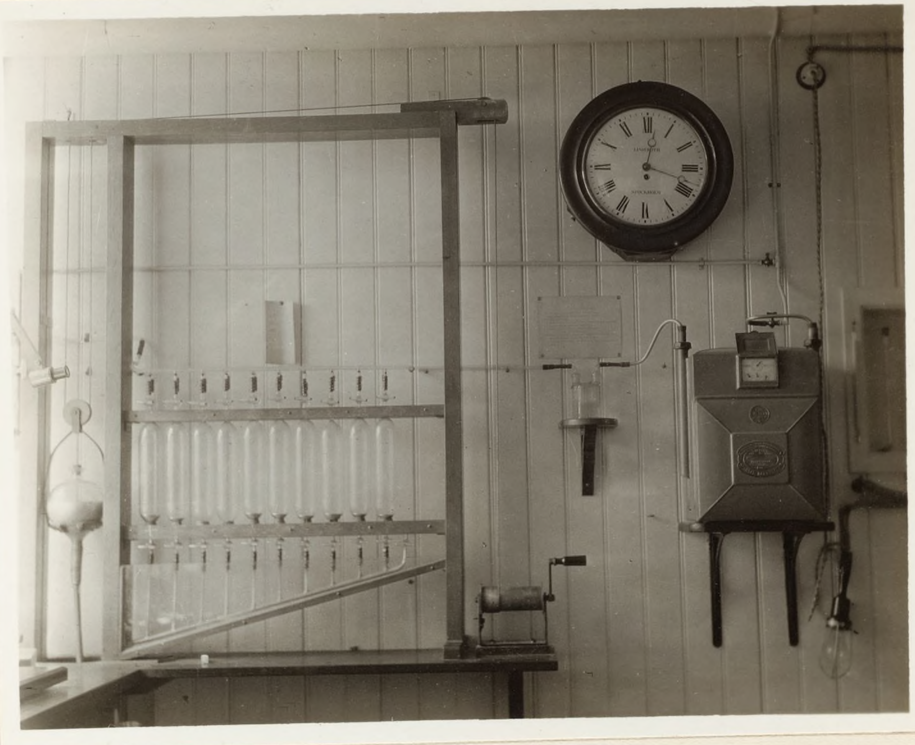


Fig. 91. The sampling reserve pipette of Professor Johansson in Stockholm.

This is on the back of the medium sized respiration chamber.

dioxide that can be pumped out during forced ventilation. They used a mask over the face and the carbon dioxide, as soon as expired, was sucked out by a suction pump into a spirometer and weighed. They also used Lovén valves, which I saw for the first time and photographed, together with the mask. (See fig. 92.) The mask did not appear to me as particularly satisfactory. They find no difference in the carbon-dioxide output of the subject in the three positions of lying, sitting, or standing. This is very hard for me to believe, especially as they told me that the subject was not leaning when standing up but actually stood free.

In discussing the question as to what is the best method of determining the respiratory exchange, I advocated strongly that Johansson use the Jaquet principle. I pointed out to him that they have there in Sweden the wonderful Sondén-Pettersson apparatus for determining carbon dioxide and oxygen with the greatest accuracy. The Jaquet chamber has a ventilating circuit on the general principle of the Sondén-Tigerstedt apparatus, but modified by Jaquet. If Johansson and his assistants could determine the oxygen with a Skandinavian apparatus, namely, the Sondén-Pettersson apparatus, it seemed as if local pride would justify them in adhering to the Jaquet principle. Furthermore the Jaquet principle is theoretically correct and since their analyses showed that the oxygen remained constant, this fact would simplify matters enormously. Since my return I discovered that Hasselbalch used the so-called Jaquet principle for infants simultaneously with Jaquet's use in Basel.

I had a number of very interesting discussions with Professor Johansson, particularly in regard to his idea that all food material is first deposited in the body before it is burned. He argues that his depot idea is as good as the respiratory quotient argument,

Fig. 92. Details of Lovén valves, mouthpiece and mask in Johansson's laboratory in Stockholm.

Unfortunately I could not follow him entirely. In fact, I have written to him once or twice since I returned from Europe, in an attempt to get his ideas specifically. Formerly I maintained that his whole point of view was wrong because he neglected to take changes of the respiratory quotient into consideration. He uses the amount of carbon dioxide produced as the sole basis of his calculations, not realizing in the first place that the character of the katabolism might change when sugar is ingested, and secondly, that there might have been an actual increase in the katabolism as a result of introducing the sugar. He maintains that his idea is just as good whether he takes the respiratory quotient into consideration or not. On writing me recently he says that he prefers to have more experimental data before publishing his ideas.

Of very great practical value from the physiological standpoint was his large and very extensive series of lantern slides in which he had photographed the exact pages of the originals of a large number of the classics in physiology, giving the exact page number, shape of table, the size and the general appearance of the originals themselves. It seemed to me advisable to have these slides to show to students.

On the roof of the Institute they are raising their own dogs. One of the dogs was very large, weighing from 25 to 30 kilograms. I also saw the dog upon which Dr. Carrel operated when in Stockholm at the time the Nobel prize was awarded a year or so ago. They maintain that the dogs thus raised on the roof are really "at home" and do not bark or make any noise. It seemed to be an admirable place for them.

Laboratory of the Stockholm Board of Health.

Dr. Sondén

In company with Professor Johansson I visited the laboratory of Dr. Sondén, although there were no especially new features of practical application to the work of the Nutrition Laboratory. A number of photographs were taken (figs. 93 to 95) to show one or two minor points in connection with the laboratory. One is the method of collecting mercury when using gas analysis apparatus by means of a groove around the edge of the table. The other is a simple pressure reduction apparatus for use in connection with the oxygen bomb.

Dr. Sondén is chiefly occupied with his Health Bureau and has given but little time to gas analysis in the past three years. His work is in large part bacteriological in connection with the water supply and sewerage disposal of Stockholm. I have always regretted that this wholly remarkable experimenter has been confined to a bureau where his technical skill is of minimal value.

Fig. 93. Professor Johansson and Dr. Sondén in the laboratory of Dr. Sondén in Stockholm.

Professor Johansson is at the left in the photograph and Dr. Sondén in the middle.

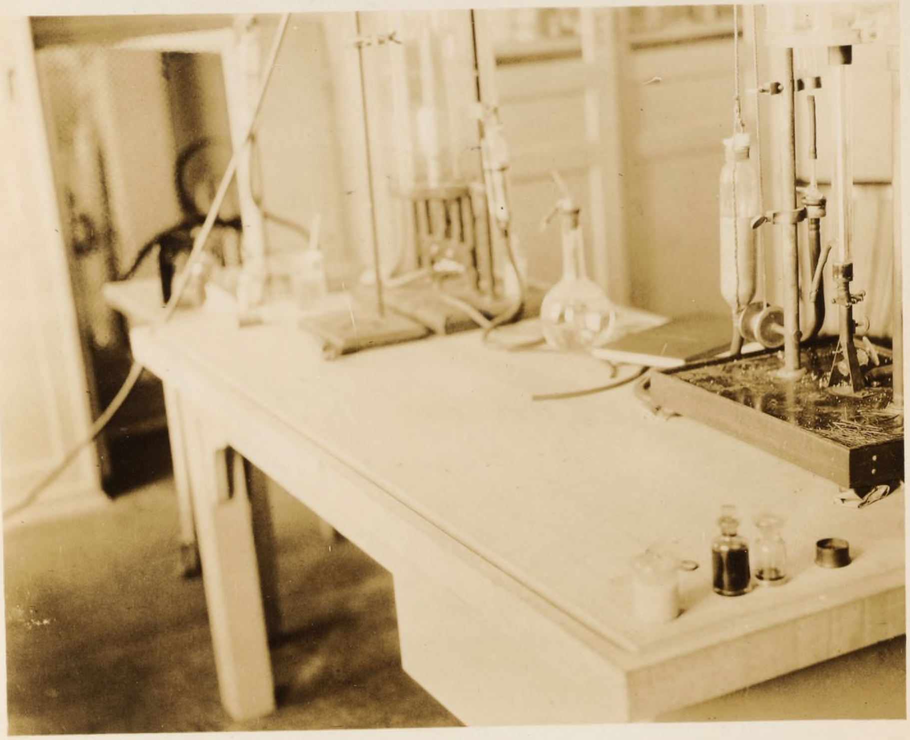


Fig. 94. Table in Sondén's laboratory in Stockholm.

The groove around the edge is designed to catch any mercury spilled upon the table. The Sondén apparatus is shown at the right.

Fig. 95. Pressure regulator for regulating the flow of gas in Sondén's laboratory in Stockholm.

This has a mercury seal consisting of an iron spirometer with mercury in it, and iron pressure weights. The gas comes out of an oxygen cylinder.

Karolinska Institute (Pharmacological Laboratory).

Professor C. G. Santesson.

One of the most charming gentlemen that I have met for many years is Professor Santesson, the professor of pharmacology in the Karolinska Institute. His new laboratory, which is in the same wing as Professor Johansson's and, I think, one flight above his, is modern and well fitted up in every way, simple and yet extremely practical. Santesson laid great emphasis on the fact that he had a pharmacological museum in which the students could see all the materials that were used and the different preparations of pharmacological material.

COPENHAGEN, DENMARK

University of Copenhagen (Physiological Laboratory).

Professor Henriques

The influence of Professor Bohr who died but a few years ago is noticeable in the fact that at present Copenhagen has many institutes working along physiological lines. Professor Henriques is the successor of Professor Bohr in the Physiological Laboratory of the University. He has made no material alterations in the original equipment; indeed, he is still too new in his present post, with all his courses and lectures to arrange, to be actively engaged as yet in scientific research. Furthermore his health has always been somewhat precarious.

Of special note in this laboratory was an electrolytic cell with which Henriques was able to separate completely pure hydrogen from pure oxygen. There was also a large battery and a Bohr pump with water pressure to raise and lower mercury.

The laboratory has its original cleanliness and air of exact nicety. It is probably the neatest laboratory devoted to physiology in the world.

Zoophysiological Laboratory.

Dr. Krogh.

Dr. Krogh, a former pupil of Bohr, has a special institute for animal physiology in Copenhagen. He is certainly one of the most ingenious and exceptional men to be found anywhere in Europe. His laboratory is small but well lighted and practically arranged. For example, all the tables can easily be changed if necessary and all the plumbing is removable. He has very few students and unfortunately no assistants.

The lower floor of the laboratory is a small workshop in which admirable work can be done. Krogh is certainly an extensive experimenter and impresses one as doing very careful and exact work. He has an ingenious respiration apparatus on the closed-circuit principle in which he removes all the carbon dioxide from the expired air by passing it through a large tank of soda lime forming a part of the circuit. The soda lime in this tank had not been removed for about two years and still absorbed all the carbon dioxide expired by the subjects.

Krogh does not determine the carbon dioxide directly but obtains the respiratory quotient by gas analysis. The amount of oxygen introduced is measured by a clever meter devised by Krogh, so arranged that each electrical contact represents 0.88 liter of oxygen. Krogh maintains that he was the first one to use a spirometer in the respiration circuit and that he showed his device to Dr. Carpenter some years ago. The spirometer is not immersed in water, the experiments being so short that Krogh does not think it necessary. It is calibrated by letting one or two liters of water flow into a special glass vessel. (See fig. 96.) As the spirometer rises and falls at each expiration the movements are written in the form of a curve on a graduated brass scale, which reads to 0.1 and possibly 0.05 millimeter, the total volume of expired air being thus represented on the curve. (See fig. 97.)

In discussing gas analysis I spoke to Krogh of our work on the oxygen in the air. He thinks he can determine the absolute volume of oxygen in the air by analyzing the dry gas over mercury. He will probably try this scheme; I sincerely hope he will. He has a most exact method for determining carbon monoxide, using a method of explosion.

In connection with the respiration apparatus a rubber mouthpiece or mask is employed. Krogh thinks it is possible to have a mask tight if a special mask is made for each subject. He also uses micro-valves with a very clever trap for condensed water. This trap consists of a small piece of rubber tubing leading to a finger-cot which is partially collapsed and the condensed water runs into this finger-cot.

Krogh has a method of taking samples of the alveolar air from just above the exit valves by a small capillary tube of lead leading almost to the insert end of the mouthpiece. He turns a stop-cock at each flap of the exit valves, thus getting a sample at the end of the expiration. A small electrical device shows exactly when the mercury is flowing and from what part of the expiration the sample is taken. All the samples are taken in mercury pipettes of about 50 c.c. capacity, usually not by vacuum but by letting the mercury flow out. This method of sampling is much used by Hasselbalch and Lindhard. I had considerable discussion with Dr. Krogh in regard to the changes in the alveolar air and in the dead space and arranged that Mr. Higgins of

Fig. 96. Special glass vessel holding one liter used by Krogh in Copenhagen for calibrating his spirometer.

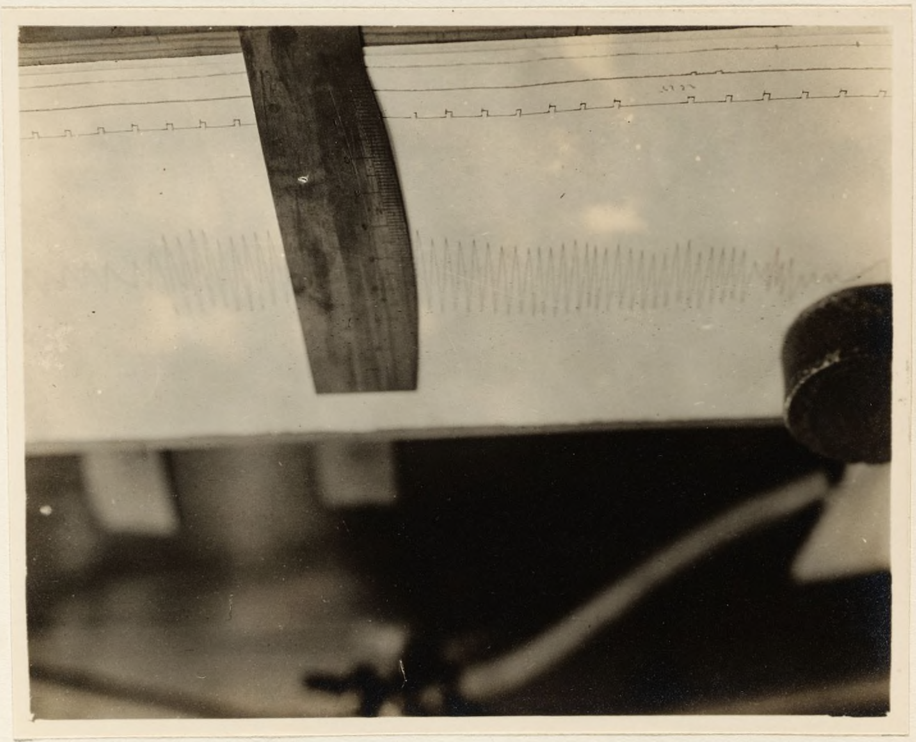


Fig. 97. Graduated scale used for reading the curves obtained with the Krogh spirometer in Copenhagen.

the Laboaratory staff should visit him later in the year. This Mr. Higgins did much to his profit.

Krogh's bicycle ergometer with an electric brake, one of which was being constructed in his shop while I was there, interested me particularly. (See figs. 98 to 102.) It has four magnets, symmetrically arranged, and is in many ways extremely ingenious and much better than the one used in the Nutrition Laboratory. I ordered one for the Laboratory which has already been received. The apparatus is beautifully made and we are very much pleased with the one sent us.

Using the principle of the electric brake Krogh suspends a 1-kilogram weight on the ergometer, each revolution of the disk corresponding to 2 kilogrammeters of work. To remove the wide variations and oscillations a dampening device is provided, consisting of a lead paddle or damper attached to the magnet frame and dipping into a trough containing a thick syrup solution at the bottom of the ergometer. This device is supposed to hold the pointer at the top of the apparatus at 0. It is not automatic, however, and the operator has to change the direction of the current passing through the magnet with each variation of the pointer from 0. I suggested to Krogh the desirability of using electric contacts to open and break, or close and short-circuit the current which would vary the pull upon the magnets with each deviation of the pointer from 0. At my suggestion, also, Krogh substituted for the chain he was using--certainly a very poor one--a so-called "Morse chain" which is much better as it has knife edges and roller bearings.

According to Krogh as each revolution of the disk corresponds theoretically to 2 kilogrammeters of work, all one needs to do to obtain the number of kilogrammeters of work done is to count the number of revolutions of the disk and multiply by 2. Inasmuch as ultimately we

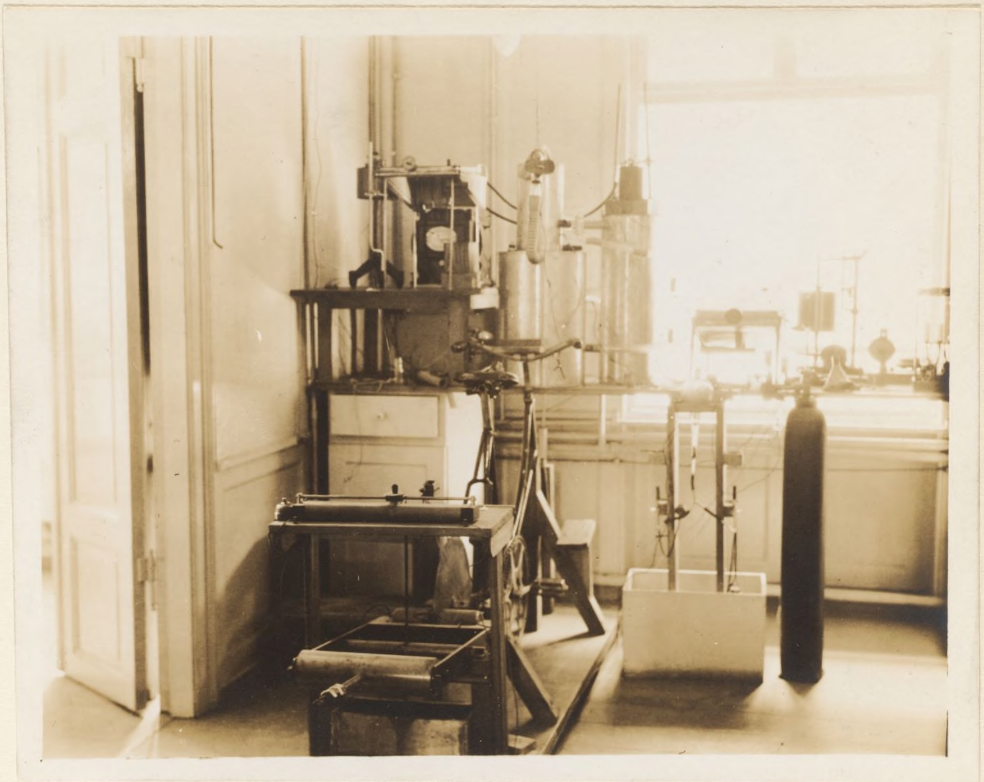


Fig. 98. General view of Krogh's apparatus in Copenhagen for studying muscular work, showing the ergometer in the foreground, the mouthpiece, the soda-lime cans, etc.

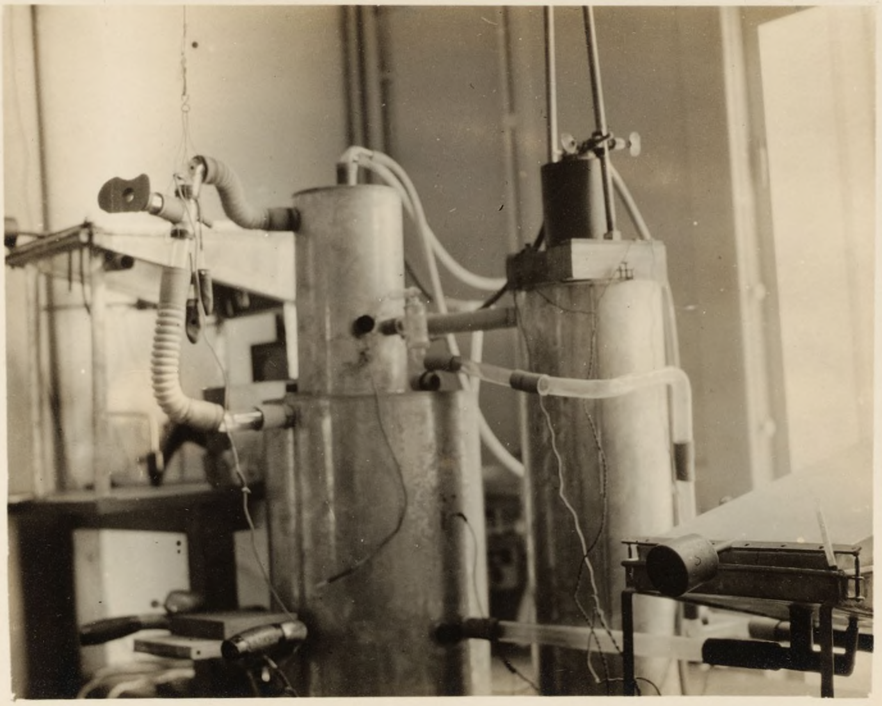


Fig. 99. Details of the respiration apparatus of Krogh in Copenhagen for studying muscular work.

The handle bars of the ergometer and the mouthpiece are in the immediate foreground; the large cans in which the carbon dioxide is absorbed are shown in the middle. At the extreme left and right are the spirometers.

Fig. 100. Mouthpiece of the Krogh respiration apparatus for studying muscular work in Copenhagen.

The small finger-cots with tubes are arranged for collecting saliva and condensed moisture. The small lead pipe is used for drawing off samples of the alveolar air.

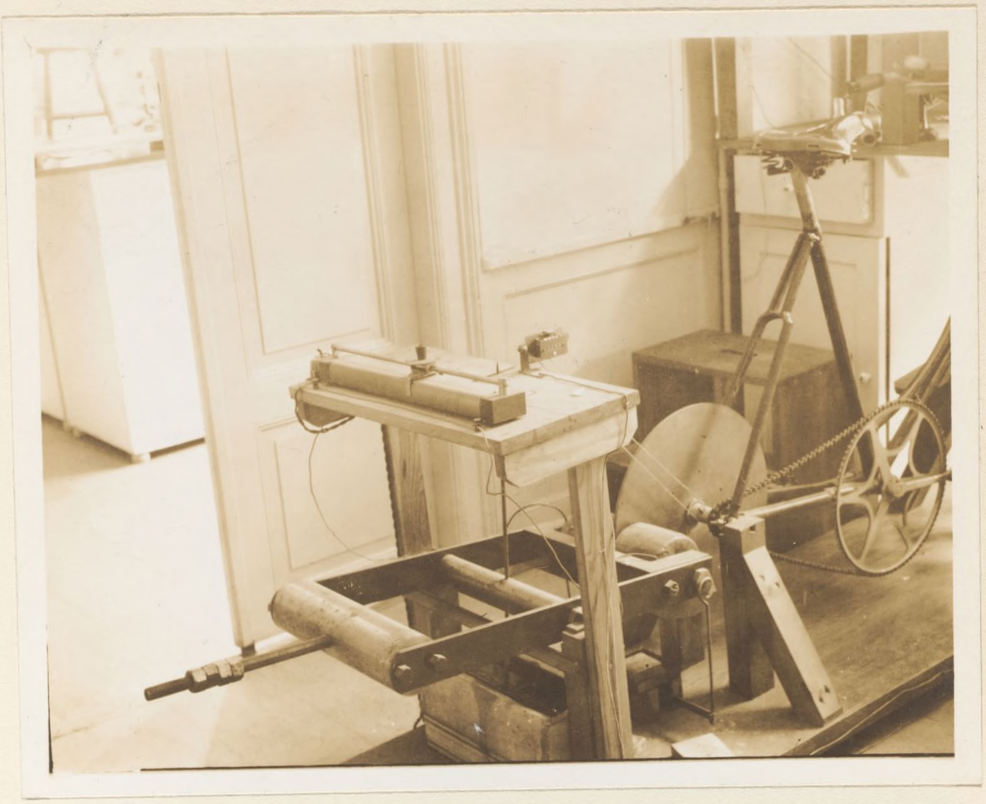


Fig. 101. Details of Krogh's ergometer used with his apparatus for determining the respiratory exchange.

. This is the older form of ergometer and shows the movable magnets, with the copper disk rotating between them. The adjustable rheostat and revolution counter are clearly shown.

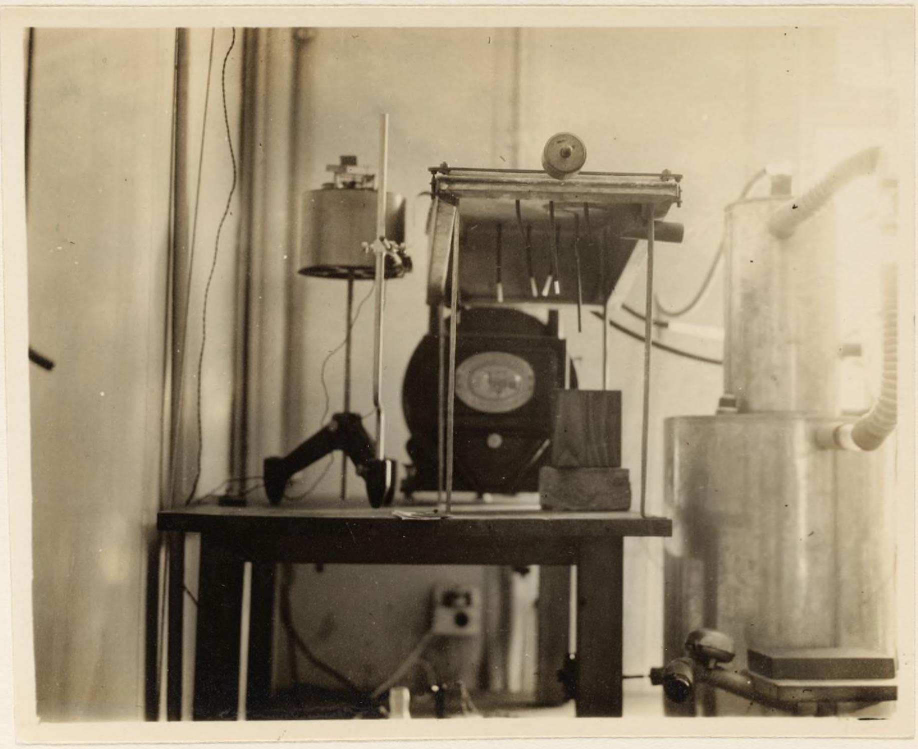


Fig. 102. The gas-analysis apparatus and spirometer used by Krogh in his muscular work experiments in Copenhagen.

wished to determine the exact amount of work put upon the machine by a subject, it seemed desirable to us to calibrate our Krogh ergometer by placing it inside the chair calorimeter and measuring the heat developed. Of course the friction is unknown. The results of our calibration tests will be of interest to all those who own one of these ergometers. (See results published in the American Journal of Physiology, 1915, volume 38, page 52.) Krogh's ergometer appears to be used rather extensively in Europe. Haldane has one in London and I also saw one in Hasselbalch's chamber with decreased pressure at the Finsen Institute.

The Carlsberg Laboratory.

Professor Sorensen.

The Royal Academy of Sciences in Copenhagen owns a large brewery called the Carlsberg Brewery. The income from this plant, which amounts to about one million kronen per year, is devoted to the general advancement of science in Denmark, a picture gallery, a chemical laboratory and a botanical laboratory being among the institutions receiving money from this source. The brewery has its own laboratories in botany and applied chemistry, so that the laboratories which receive gifts from it are really independent of it in every way. Professor Sorensen, who is the successor of Kjeldahl as Director of the Carlsberg Laboratory, told me that his laboratory receives money for research from this brewery, but is not connected in any way with it and is absolutely independent in its selection of lines of research. As a matter of fact some of Sorensen's researches and those of some of his colleagues as, for instance, the department of botany, are of very great value to the brewery. The Carlsberg Brewery has already begun to recognize the importance of substituting other beverages for alcoholic liquors and is now making many non-alcoholic beverages for the people of Denmark.

Professor Sorensen took me through the underground street of the brewery, to see the enormous cooling plant. It was a sight simply beyond imagination. The beer is stored in great vats, several meters underground, the alleys between being as wide as city streets with storehouses on either side.

The Carlsberg Laboratory is very fine, scrupulously neat in every particular and very well kept. Sorensen at the time was very much interested in working on the hydrogen-ion concentration and has developed many forms of apparatus for this work. He was also interested in conductivity and had some very good electrical measuring apparatus, particularly a Hartmann and Braun apparatus. There were many dialysis experiments running in which collodion sacks were used.

Laboratory of the Finsens Medicinske Lysinstitut.

Professor Hasselbalch.

The Finsen Laboratory, presided over by Professor Hasselbalch, is established in a very beautiful part of Copenhagen in an old villa which has been remodeled in part for the purpose. There is an especially good installation here for measuring hydrogen-ion concentrations, including particularly, an apparatus for shaking the gas electrodes. Hasselbalch has really done a great deal of work in this line and says that the hydrogen-ion concentration of the blood always remains remarkably constant. He thinks there is very little to be learned further on the subject.

There was also an exceptionally good outfit of gas-analysis apparatus in the laboratory. Lindhard works with a modification of a large Haldane apparatus which is in size between the small and the large Haldane apparatus. He uses the reagent pipette of Krogh, which is simpler than that in the Haldane apparatus. The Bohr meters find extensive use here as do likewise the Bohr mouthpieces and the Krogh spirometer.

Of special interest to me was the large respiration chamber in the basement for studying the effect of diminished pressures--a most astonishing apparatus. Hasselbalch said that as there were no mountains in Denmark, the country being unusually flat, they had to make their own mountain. He also told me that Rubner had recently been lecturing in Copenhagen and had been inside the chamber with him when the pressure was equal to that at a height of 5000 feet.

Professor Krogh evidently played a very important rôle in designing the apparatus. The chamber--a vacuum-air chamber--has a capacity of 25 cubic meters, is made of steel plates, and has a dome. It cost about 10,000 kronen. A rotary suction pump in the basement with automatic control is used to produce a very high degree of rarefaction. The whole apparatus works remarkably well. There are double doors for the food aperture, an extremely ingenious water-closet device, and very satisfactory folding beds. Two subjects are always used in experiments with this chamber and look after the apparatus contained in it. A 9-day experiment, also an 8-day experiment, had been made with the apparatus, Hasselbalch and Lindhard being the subjects. They lived inside the chamber, were able to communicate with the outside by the long distance telephone, and felt perfectly free.

Inside the chamber was a large electric light for producing ultra-violet rays. They used this particularly in comparing the effect of sunlight and ultra-violet light upon the metabolism. There were also inside the chamber a Bohr spirometer and complete apparatus for studying the respiratory exchange, a Haldane gas-analysis apparatus, and a Krogh spirometer as well as a Krogh ergometer. (See figs. 103 and 104.)

In discussing the determination of sugar by safranin titration, Hasselbalch said that he found an error of about 5 per cent with safranin and thought that thionin was much better. He expects to publish his results shortly.

The Finsen Hospital is carried on to get money for financing Hasselbalch's department, the money coming from the paying patients. There are about 80 cases of lupus received at the hospital each year, but the disease is now treated early and cured for the most part and there are not so many unidentified cases as formerly. They use a large arc flame for hard cases with very good results, as they find that the arc light bath apparently alters the blood flow greatly. As a matter of

Fig. 103. Interior of the Hasselbalch diminished pressure chamber in Copenhagen, showing the complete Krogh gas-analysis apparatus on the table at the rear, and the Bohr spirometer with mouthpiece or mask in front.

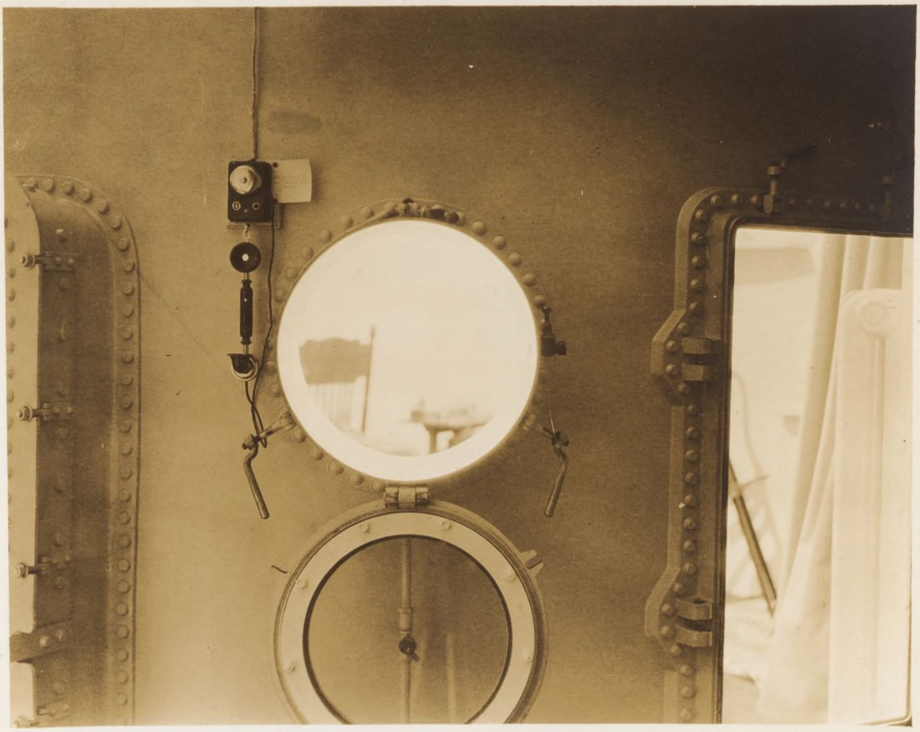


Fig. 104. Window and door of the Hasselbalch diminished pressure chamber of the Finsen Institute, Copenhagen.

Near the door to the right are seen parts of the Krogh ergometer.

fact, Hasselbalch has discovered that it is possible to become sunburned by the light.

Hasselbalch impressed me as being a very serious, studious man with a good head. He is evidently a remarkably good manager. Unfortunately I had no communication with Lindhard. He seemed to avoid me as he would a plague. He speaks very little English or German and is extremely shy. I saw him on the Krogh ergometer inside the respiration chamber and photographed the apparatus with him upon it (see fig. 105) but I could not get a word of conversation with him.

Fig. 105. Dr. Lindhard on the Krogh ergometer inside of the Hasselbalch decreased pressure chamber of the Finsen Institute, Copenhagen.

UTRECHT, HOLLAND

University of Utrecht (Institute of Physiology and Physiological Chemistry).

Professor Pekelharing.

I had never met Professor Pekelharing before this visit to Utrecht but had always looked forward to meeting him for I have been greatly interested in his work on creatine and creatinine and likewise in the work done by his students on the fasting woman, Tosca. I found him a most charming, quiet man, scholarly, unassuming and evidently all wrapped up in his work. (See fig. 106.)

Professor Pekelharing has a combined chair of physiological chemistry and histology at the University. His laboratory is very old and lacks many modern conveniences but is scrupulously clean in every way. All of the doors were kept locked as appears to be the general custom in foreign laboratories; he had to go downstairs three times to get the right key to open a room.

In discussing the alcohol program Professor Pekelharing made many suggestions but later wrote them down on the alcohol program for us. He told me he had done quite a good deal of work on alcohol and had published his results with Zwaardemacher in the Onderzoekingen. Several volumes of this journal are deposited in the Bowditch Library of the Harvard Medical School. When alcohol was injected into the rectum it was found later in the stomach. They also discovered that the mucous membrane of the intestines was more sensitive to alcohol than was the stomach lining. They produced peritonitis very easily in rabbits by rectal injections of alcohol.

With the stomach administration they found that the hydrochloric acid was greatly increased but that after giving a false feeding to

Fig. 106. Professor Pekelharing in his laboratory in Utrecht.

animals no more pepsin was secreted but only a lot of acid. They also found that after a continued injection of alcohol and a stimulation of the acid cells, microscopic slides indicated a degeneration of the cells. Pekelharing showed me as an example the slide obtained from a woman drunkard. There was no inflammation and yet Pekelharing was sure that the acid-producing cells were disintegrating. This work should certainly be thoroughly abstracted.

Among the ingenious laboratory devices that I saw in Pekelharing's laboratory was an exceedingly good galvanometer scale with illumination and a curved mirror which was obtained from Professor Hamburger's mechanician in Groningen. Professor Pekelharing uses filter paper torn up and suspended in water for separating large quantities of material. By placing in a funnel a hard rubber disk with holes punched through it, centering the disk in the funnel by a rod, and covering it with a layer of filter paper, he gets a fairly good filtering medium.

The outfit for studying the hydrogen-ion concentration was very extensive, as Professor Pekelharing is much impressed with the possibilities of working along this line. He is also working on ptyalin reaction. I unfortunately did not know enough about the subject to discuss it understandingly but I know he uses a high voltage with many storage cells and studies the effect upon the ptyalin reaction.

For evaporation at reduced pressure he uses an apparatus devised by a man in Vienna and supplied by Rohrbachs Nachfolger in Vienna. The evaporated liquid condenses and runs off. This apparatus I likewise saw in Groningen.

Professor Pekelharing is especially interested in the subject of creatine, where it is formed, and when it is converted to creatinine. He believes it is converted in the liver because when the liver is held at 37° some creatinine is formed. He believes that ordinarily the carbohydrates have an oxidase or ferment which converts creatine to creatinine and that they are not simply present in the body. Fat does not have this converting power nor protein, so Pekelharing thinks that the carbohydrates have an oxidase or ferment. He believes thoroughly that we should find out how these things take place in the body and that research work should follow along these lines. He is certain that the liver plays a rôle. Normally there is no creatine in urine, but dogs with Eck fistulas always have some creatinine. He thinks that if the liver were cut out of the circulation altogether all the creatine would appear in the urine and that the liver is therefore the place where the creatine is turned into creatinine. He cited some of his earlier work on this point.

Professor Zwaardemaker.

Professor Zwaardemaker, who is a professor of physics in the Institute, is a genius of the first order. (See fig. 107.) He is much interested in physics and has a pitot tube and a differential manometer. He uses a Rota Messer or, as he calls it, an "air bridge". He photographs the Rota Messer mica-plate as it goes up and down and thus studies the respiratory volume.

I looked at his olfactometer in which he has been able to mix four different odors. He said that he could blend nitrobenzene and skatol and the subject could detect no odor whatsoever.

His laboratory is full of most interesting things; I took two photographs of his apparatus for measuring the rate of flow of gases. (See figs. 108 and 109.) As Professor Zwaardemacher was extremely busy at the time of my visit and I had but little time to spend there, I was not able to do his laboratory justice but it is well worth further study.

Fig. 107. Professor Zwaardemaker (right) and Professor Pekelharing (left) in Zwaardemaker's laboratory in the University of Utrecht.

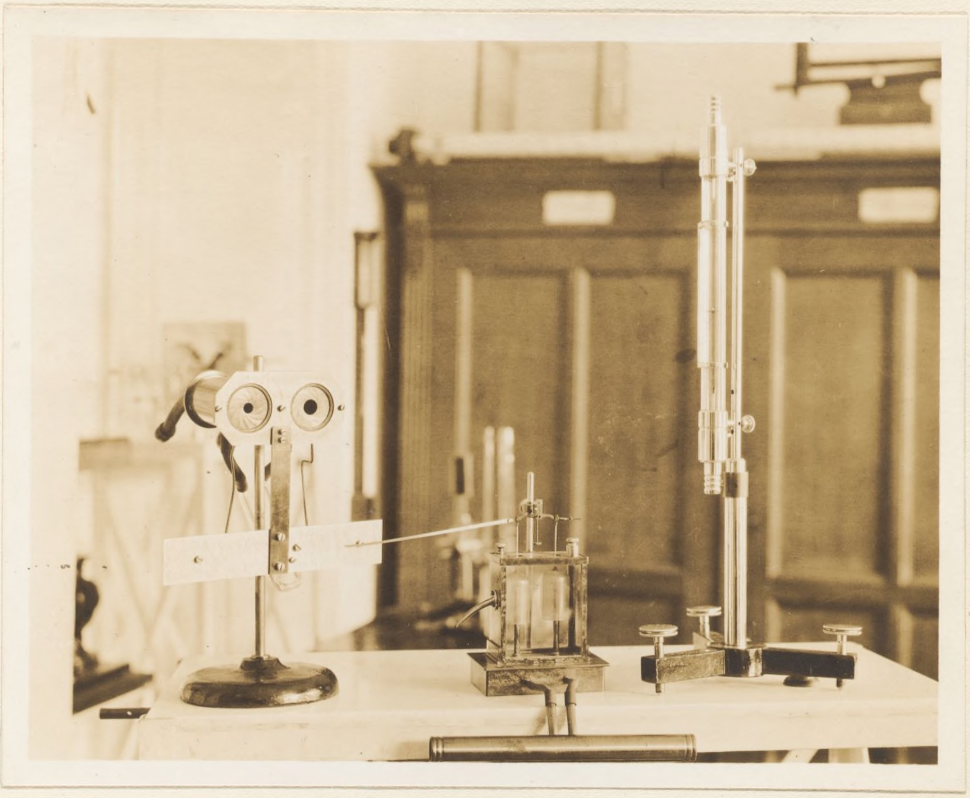


Fig. 108. Several forms of apparatus devised by Professor Zwaardemaker of the University of Utrecht for measuring the flow of gases.

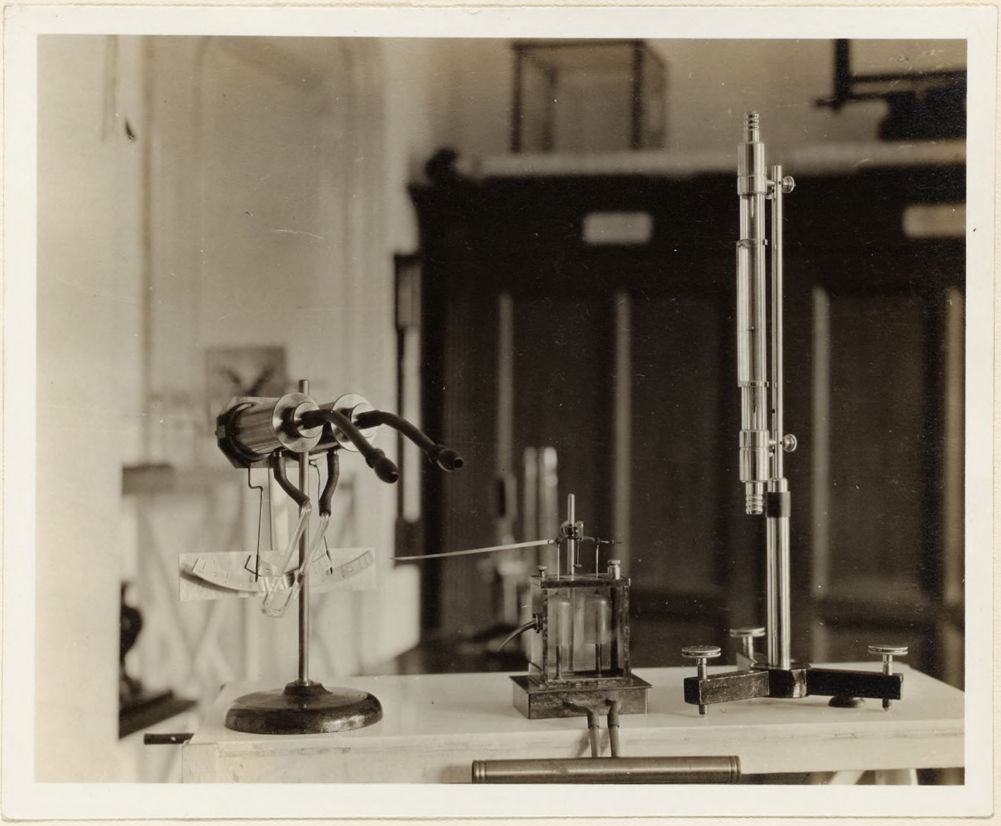


Fig. 109. Zwaardemaker's apparatus for measuring the rate of flow of gases, showing the nosepiece reversed, with the ventilation tubes for each nostril.

GRONINGEN, HOLLAND.

University of Groningen (Physiological Laboratory).

Professor Hamburger.

Although I had not originally planned to go to Holland I found I had two days available and, at the kind invitation of Professor Hamburger, I went to Groningen. This city had a special attraction this year as the International Congress was to be held there in September.

The University of Groningen as a whole and the new buildings in particular impressed me as being built upon a grand scale with a degree of luxury that would not be tolerated, I think, in any American university. I have not seen anywhere so much luxury as I saw in these buildings in the Groningen University. Indeed, Professor Hamburger's office in the Physiological Laboratory is not unlike that of a steel magnate's in America.

The Physiological Laboratory is a large, new building, with extensive grounds. Provision is made for both animals and fish, as some research work is carried out on these. Evidently when the appropriation for the building was asked for from the Government, the men in charge applied for a large amount. In Holland it is apparently the rule to get everything possible from the Government at the first request for thereafter it is very difficult to obtain additional funds. As a matter of fact, in this instance the Government gave them a great deal of money which has been used in the construction and equipment of the laboratory and it is now planned to do a great deal of work.

The administration of the laboratory interested me especially as I had read Professor Hamburger's description of it in the program of the Congress; it is absolutely unique, I think, in Europe. My general impression is that the laboratory is distinctly overdirected and over-organized. The multiplicity of details in its organization points certainly towards a deficiency in output. It seems to be inventoried ad infinitum, each room having its own special inventory and each shelf being specially arranged and numbered. It seems to me it is utterly impossible to carry out this inventory method where there is much work to be done. As a matter of fact there appeared to be but little work in progress. This may have been due to the fact that they were preparing for the large Congress which, of course, kept them very busy indeed.

Hamburger himself is not doing any work at present as the Congress occupies all of his time. He had no suggestions to make with regard to the alcohol program for he was evidently too busy then to read the program over at all critically.

He is especially interested in haemolysis and had just received Richet's Dictionary containing his article on "Isotonie". His chemical assistants were working on blood and sodium chloride and also sodium bromide, using a high power centrifuge and reading off the precipitate on the graduated part of the tube. Hamburger's laws and researches are altogether too difficult for me to understand as they are entirely outside of my province. He is a great admirer of Donders who was formerly his chief.

Hamburger has countless ingenious details of administration. The reprints received go first to his office and after fourteen days are removed and filed. His distribution of reprints is given a given a great deal of attention. He has printed labels for shipping reprints to people on his mailing list, with a special book to check up what is sent to each individual instead of the library card system that we use; I think perhaps the book is better than the card system. On the other hand, I saw a book in Zuntz's laboratory which looked very much crowded and with no chance to expand. Hamburger has a private library at his house with a card index and all the reprints belong to him. These are filed consecutively as they are accumulated. The system is rather unwieldy as the numbers now run to 4500.

The laboratories, of course, were wonderfully clean (see fig. 110), being new and luxurious in all their details. For example, there was a very large room with a large number of arc lights for taking kinematic photographs of animals. It seemed to me an enormous laboratory for a relatively small number of students; I did not see how one half of the rooms in the building could ever be used.

There was a very good machine shop in the laboratory with a tin-cutting machine for cutting out round disks, also a plating department, with buffing wheels, etc. Several machinists are on the staff of the laboratory, each with his own cabinet and kit of the tools most used, marked with his own initials. A very practical idea was a small kit of rough tools put upon a frame in each room in the laboratory for general use. I photographed two of these kits. (See fig. 111.)

In the machine shop the men are allowed to make things after laboratory hours and sell them. They pay for the stock but the laboratory furnishes the power and light. Any one in the laboratory may get a training in the use of tools by going down to the shop, as Hamburger thinks such manual training experience in using tools is good for the general laboratory technique. The mechanician is told to give assistance and lessons to the men if they wish it.

Three or four of the assistants in the laboratory appeared to me to be very bright. Hamburger is the only one who lectures, the others being simply laboratory assistants.

Fig. 110. General view of a typical laboratory room in Professor Hamburger's laboratory in Groningen.

The extraordinary neatness and systematic arrangement of this laboratory is marvelous. It is possible only where there are few workers.

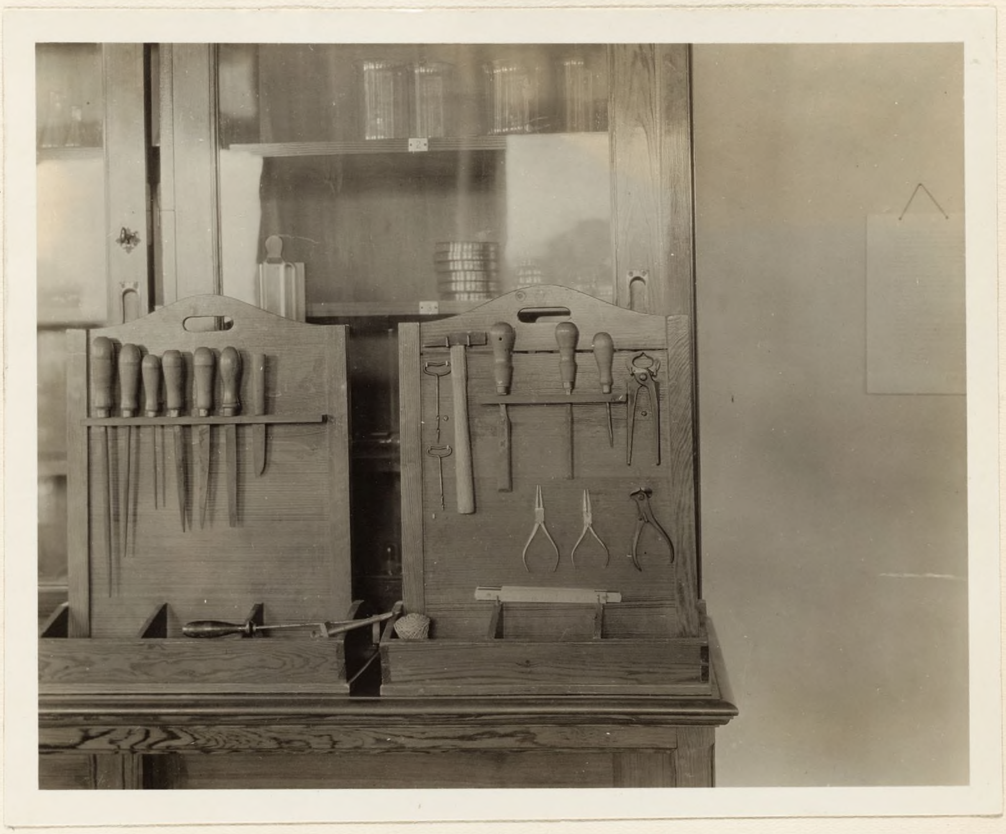


Fig. 111. Ingenious kits of tools used in the laboratory of Professor Hamburger in Groningen.

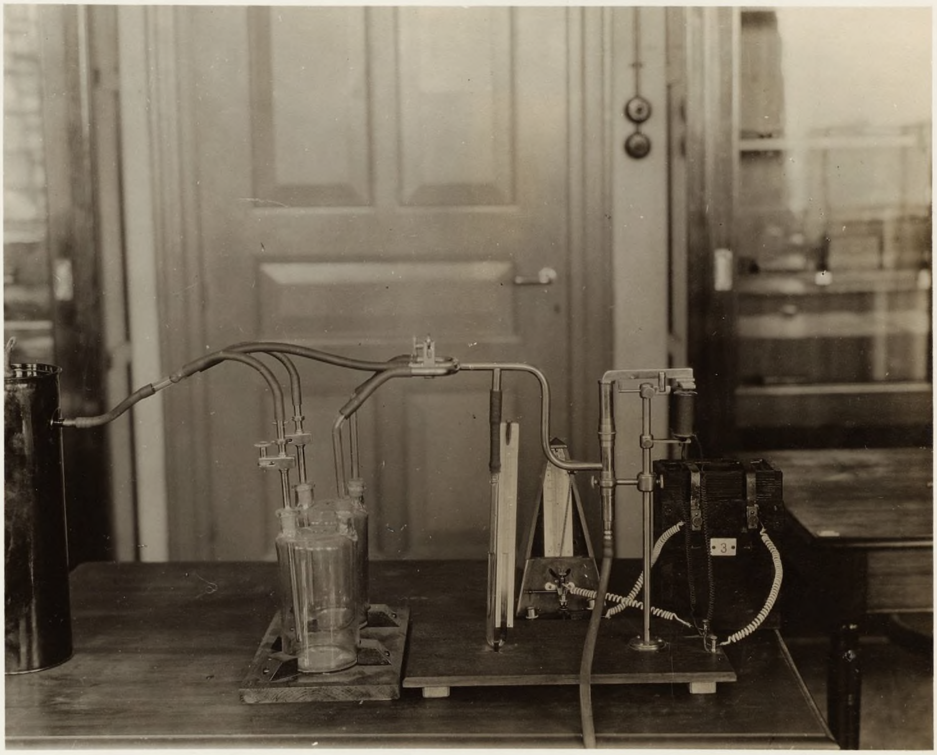
These kits of tools may be carried from room to room or kept in the individual laboratories.

There were many things of value in Hamburger's laboratory and I secured many hints from his work. Among things of special note was a scheme for artificial respiration by using compressed air at constant pressure. I took a photograph of this instrument (see fig. 112). There was also a series of adjustable holders for tambours, some of which were ordered for the Nutrition Laboratory and have since been received. These holders were used for raising and lowering a tambour on a standard.

One of Hamburger's assistants, Dr. Laqueur, impressed me very favorably. Dr. Laqueur has been working upon the stimulus of carbon dioxide on the excised gut in Ringer solution. He was much impressed with a method of determining chlorine in urine, called Fisser's(?) method. This method is based on the oxidation of urine with nitric acid and then with potassium permanganate and finally the titration of the mixture with a ferrous salt as an indicator. The method did not work well in my presence and I was not much impressed with it.

The Zimmerman kymograph used by Laqueur seemed to be very satisfactory. Hot air motors were much used, being generally connected with the water supply for cooling. They were quiet and excellent for long continued use in stirring water baths. On the other hand they were not easily moved about and were apparently not easily started; accordingly they can hardly be equal to electric motors, although they probably cost less.

Considerable use was made of a micro-burner that could be turned down very low and would not snap back. This micro-burner was made by Kindel and Stoessel in Alexandrienstrasse 3, Berlin, S.W.,XIII, and cost only about 1½ Marks.



Fig, 112. The apparatus for artificial respiration by the use of compressed air used in the laboratory of Professor Hamburger in Groningen.

This apparatus is constructed on the principle of using compressed air, aided by an electric contact device with a metronome (shown in the rear of the picture) which opens a valve at each electrical contact. This apparatus was exhibited at the Hygiene Congress.

Psychiatric Laboratory.

Professor Wiersma.

Professor Hamburger had written me that a new pulse registering apparatus devised by Professor Wiersma was to be exhibited at the Congress. I was therefore very glad of an opportunity to see it. Professor Wiersma is the professor of psychiatry in the University and I visited him in his clinic with Professor Hamburger. He impressed me as being an active, keen, bright man.

The pulse registering apparatus consists of a rubber bulb of peculiar design, made of a piece of soft rubber tubing about 20 millimeters in internal diameter, with walls approximately 1.5 millimeters thick, and of a length convenient to hold in the closed hand, i.e., about 10 centimeters. One end of the tubing is sealed by cementing the compressed walls together; the other end has attached to it a circular piece of rubber with a small rubber tube which is cemented to the larger tubing. The bulb is held firmly by the subject and the closed hand and bulb are then carefully bandaged all over. When the bandaging is completed, the subject relaxes the hand and the experiment can begin. A pneumograph is also placed over the abdomen. The pulse and respiration movements are transmitted by means of long rubber tubing to two Marey tambours in another room with levers moving before a slit in a photographic registering apparatus. In this way both the pulse and the respiration can be photographed.

The scheme is very simple and interesting and possibly of great value. Wiersma gets a very fine curve in which the dicrotic notch is well shown. He tried the apparatus on me. It might be tolerated for a night by a Dutchman but an American would fly off the handle in twenty minutes, according to my experience. Wiersma had many all-night curves and other interesting results obtained with the apparatus. The calculations on the character of the respiration and the pulse were very pretty. He has taken samples of the pulse changes with infants and finds that the pulse varies with each respiratory rhythm. Since my visit the apparatus has been slightly modified in the Nutrition Laboratory and has proved fairly useful.

Of special importance to me was the fact that they found no great difficulty in Groningen in handling pathological cases. As is the general practice in most European institutes, the patients were told that the experiments would help them,--a practice which smooths away all difficulties and makes the subjects very anxious to be experimented upon.

LONDON, ENGLAND

University of London (Physiological Laboratory).

Professor A. D. Waller

I was particularly interested in visiting Professor Waller's laboratory and was fortunate in having the privilege of discussing with him his experiences with the Bock-Thoma oscillograph. This apparatus, which permits of making four simultaneous curves of the pulse and the heart action, had been brought to my attention several times in my European tour. Professor Waller assured me that it was very practical, was always in readiness and was very much less troublesome than any string galvanometer of which he knew. There was considerable discussion as to whether or no the oscillograph gave the same character of pulse-rate as did the string galvanometer. This point is by no means settled. I was so much impressed with the oscillograph as I saw it in use and the results that Professor Waller had obtained with it that I ordered one for the Nutrition Laboratory. It has since been installed and has given great satisfaction.

Professor Waller has devoted a great deal of time to studying the length of current from the heart and, indeed, was one of the first to demonstrate this by means of the mercury capillary electrometer. Subsequently the development of the string galvanometer by Einthoven of Leyden in Holland has given great impetus to this phase of experimentation.

Professor Waller was evidently intensely interested at the moment in the subject of heart motions and could talk and think of nothing else. He is particularly interested in the inclination of the electrical axis of the heart and has recently published a number of papers on this point.

Guy's Hospital Medical School (Physiological Laboratory).

Professor Pembrey and Professor Haldane.

Although Professor Pembrey was much occupied in his work with Professor Haldane on mine rescue apparatus and I could therefore see him but a very short time, I was glad to meet him and to see his method of work. Pembrey and Haldane were working upon two firemen who had been using the mine rescue apparatus. Mr. Higgins and myself were present at some of their tests. Unfortunately I could get very little from Pembrey, although he had a few comments to make on the alcohol program. It would seem as the years go on that his judgment is even less to be trusted; he appears to be very set in his ways and not given to a sound consideration of problems.

This was my only opportunity to see Professor Haldane and I had practically no time to talk with him.

University College (Physiological Laboratory).

Professor Starling

Professor Starling was good enough to allow Mr. Higgins and myself to spend several hours in his laboratory while he was carrying out an experiment. I saw there a part of a unit respiration apparatus for use with animals and was able to give him some suggestions with regard to it. At this time he told me that the results of an investigation published by Knowlton and himself a short time ago would have to be retracted as they found them incorrect.

I was particularly interested in watching an assistant read off a stop watch and report the time of the flow of blood as 5.1, 5.3, and 5.6 seconds. I noticed that Professor Starling wrote these down. On asking the assistant how he got these values, he said that he used the stop watch and estimated to within one tenth of a second; if the pointer stopped half way from the mark he called it 0.5 second, instead of two-fifths or three-fifths. From the well known principle of stop watches, this seemed to me a rather surprising way of reading it.

CAMBRIDGE, ENGLAND.

University of Cambridge (School of Agriculture)

Prófessor C. G. L. Wolf.

I was able to visit Cambridge for a day and thus see Professor Wolf in his new location and particularly to discuss with him the very singular experiments made by Professor Friedmann in Berlin. Professor Wolf told me that he made these experiments with Professor Friedmann on curare with dogs and that he personally felt that the experiments were entirely wrong, owing to the false analyses made by the woman assistant. Consequently he disclaimed all responsibility for them.

Professor Wolf had a small laboratory which was full of apparatus. He was carrying out some work on the respiratory exchange of decerebrate animals. He took me into the new chemical laboratory of the agricultural building and showed me the apartments set aside there for himself. I have since heard that his entrance into Cambridge scientific circles has been a matter of discussion.

I did not have the good fortune to meet Professor A. V. Hill but saw Professor Langley for just a moment.

EDINBURGH, SCOTLAND

Professor Schaefer.

I had the great privilege of spending several days with Professor Schaefer at his home in North Berwick but did not go to his laboratory. On this occasion we took the opportunity to talk over matters of physiology in general and I had many most interesting conversations with him. There were no particular points that need to be recorded, other than the general method of attack of scientific problems and the general method of handling laboratories.

While I was there Professor Schaefer invited Dr. Cathcart to spend the week-end and Dr. Cathcart and I devoted most of the time to revising and recasting certain portions of the muscular work book (Publication No. 187) which was about to go to press.

From my conversation with Professor Schaefer I can easily see why he is estimated so highly in English scientific circles as well as outside. He has had a vast experience and evidently is a man of remarkably good judgment. He is singularly free from crank notions or from pet hypotheses or pet notions of any kind and yet distinctly positice as regards his opinions on any subject upon which he is approached. One feels that he has a remarkably judicial mind. It is a matter of some surprise that he does not have associated with him more capable assistants than Dr. Pringle and Dr. Cramer. These men are both fairly good second class men but one would naturally expect to find associated with a man like Professor Schaefer a somewhat larger coterie of really brilliant associates.

Special observations.

While the main object of my visit to Europe was to get in intimate touch with various laboratories and workers in lines of research allied to those being carried out in the Nutrition Laboratory, I was likewise able to make many observations regarding general subjects of interest to us here, some of which I think it desirable to record in this report.

Vivisection.

The agitation against vivisection, which is perhsp more pronounced in England than in any other place, and which is increasing in America, is surprisingly lacking on the Continent. My experience in visiting many different laboratories has brought me into intimate touch with the methods of vivisectors. I believe that the majority of men are extremely careful, considerate, and kind, and do not produce unnecessary pain in any way. Some of them express very great and deep sympathy with the animals and in general I think this may be said to be true. On the other hand, I saw distinct evidences in at least one laboratory, i.e., in the laboratory in Naples, where I thought there was a great misuse of vivisection. A doctor was working there upon the influence on the volume of air expired by the animal of removing portions of the lung. I looked very carefully at the apparatus, the animal, the man who was doing the vivisection, and his method of operation. It seemed to me that the whole experiment was absolutely useless. The dog lay upon his back and was strapped to a board with a mask tied to his face. The expired air was measured by passing through a Verdin spirometer. It was evident that the closure of the mask about the head was not air-tight. If the experiments were carefully made, I can see the desirability of studying this subject and the possibility of the hypertrophy of the other lung compensating for the decreased lung area for the absorption of oxygen. On the other hand, I was perfectly certain that the apparatus, method, and technique were absolutely worthless. One would hardly expect such technique in the hands of a high school boy, yet this man was carrying out these vivisection experiments. Unquestionably many of the animals succumbed to pain, which perhaps was necessary considering the theoretical ideals of such experiments but certainly unnecessary from the point of view of the worthlessness of the experiment. I must confess that I felt strongly that this particular man certainly ought to have been subjected to the most careful and critical oversight in regard to the kind of work he was carrying out and his methods of vivisection. In view of this observation I should almost be willing to have all vivisection work under the regulation of a committee, the committee to be selected not by those agitating vivisection but by a committee of responsible medical men.

I question whether work in vivisection should,in certain instances at least, be allowed to be as free as work with test tubes and work with petri dishes. It is perhaps surprising that in all of my many investigations in various laboratory clinics and hospitals I did not see any other misuse of vivisection and yet this one instance certainly did impress me greatly. There are doubtless a large number of similar cases in the course of a year but this experiment seemed to be so perfectly fruitless that I was shocked by the fact that an animal should be subjected to vivisection for such a purpose.

Use of libraries in foreign laboratories.

In general I think that there is a gross economic waste in the distribution of books in foreign laboratories. Granting the necessity and desirability of departmental libraries, a point which I think no one will now deny, the duplication of libraries in adjoining rooms is certainly to be discountenanced. In the University Laboratory in Naples I saw a great duplication of literature in two departmental libraries, which were separated only by the length of a corridor. It would seem as if this frequent duplication of large libraries were a great waste of time and money.

It is possible that abroad the mechanics of procuring books from the large university libraries is so difficult as to discourage professors from using them and they have therefore accumulated their own libraries at considerable cost. On the other hand they are very chary about allowing their libraries to be used by others; indeed, it is quite usual to find that a professor has his library at his house and that he never takes a book to the laboratory for the use of any of his co-workers. I suppose their experience in having books lost, misused, or damaged without being repaired or replaced has forced them to be careful. This one can easily understand and it justifies the keeping of the libraries isolated at the house.

On the other hand I saw a great many instances where I am sure that material which should have been on file in the university library, such as reprints of current researches, text-books, and monographs, were taken by the professor to his private house and there kept in his private library and never made available to the students. I noticed some marked illustrations of this where copies of several of our publications are sent to the libraries of the various institutes of which certain professors are in charge. In at least one instance I saw the professor had appropriated the monograph, taken it to his own private library, there to be sold for the benefit of the family when he died. There seems to be an insane desire on the part of these professors to accumulate reprints for their personal libraries. They snatch a reprint out of the mail box or out of the library with the avidity of a small boy taking a coin. The lack of moral responsibility for reprints or books is certainly quite impossible to explain.

Distribution of reprints.

On the basis of the above observations it seems important to alter materially our method of distributing reprints in foreign laboratories. It has heretofore been the custom for us to send these reprints directly to the professor who has been doing the work. Occasionally it has been necessary to send the large monographs to the library of the institution with the name of the professor below it. The professor invariably assumes from this that the monograph is a gift to him personally; he appropriates it and takes it to his house and does not leave it available for the workers in his laboratory.

On the other hand, that a personally sent copy may not reach the institute but may remain in the professor's house is illustrated by an incident which occurred during my visit to the laboratory of my dear personal friend, Professor Zuntz. Dr. Markoff, who was working with the Sondén-Petterson gas analysis apparatus, had been working for several months trying to overcome a difficulty that Miss Johnson and I had already worked out and which had been described in Publication No. 166. When I asked Dr. Markoff if he had seen the publication, he replied "No", that Professor Zuntz had never even mentioned its existence to him. A day or two later I saw the publication lying on Professor Zuntz's table where it had been for several months. It had been opened, read, and, indeed, annotated.

It seems highly desirable, therefore, to inscribe on the fly leaf of each of our monographs the statement that it is presented to the library of such and such a laboratory or such and such an institute rather than to the professor in person. In this way the publication, primarily intended for a library and so marked, can not be easily appropriated by the professor and added to his own library.

There are certain difficulties in having this idea carried out, inasmuch as many of the books, after being printed, are immediately wrapped in paper and thus retained ready for shipment. I think this difficulty could be overcome and that the above suggestion would help a great deal in insuring adequate distribution of reprints and monographs. Personally I am inclined to think that the fewer reprints sent directly to professors the better. The reprints should for the most part be inscribed to and addressed to the library of the specific institute. Of course if there are no departmental libraries, the material must be sent directly to the professors. Furthermore it is highly probable that there are certain men whose writings in special lines of work are of such a character that they should not only have copies in their libraries but likewise in their private houses. In such cases a second copy should in fairness be sent to the individual workers.

There seems to be a disposition on the part of foreigners to disregard American work. This may be based in part on the fact, as they say, that they cannot get at our literature; on the other hand, I have known many instances when they could get at it and did not want to get at it. This may partly be helped by Americans sending autoreferate to foreign abstract journals, thus securing proper representation. It may likewise be necessary to republish part of the large monographs in French or German.