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REPORT

OF THE

INTERNATIONAL METEOROLOGICAL COMMITTEE.

ST. PETERSBURG, 1899.

Published by Authority of the Meteorological Council.

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REPORT

OF THE

INTERNATIONAL METEOROLOGICAL COMMITTEE.

Meeting at St. Petersburg, September, 1899

First Meeting, Saturday, September 2nd.

The meeting was opened at 2h. p.m. in the Lecture Room of the Imperial Academy of Sciences.

Present: His Imperial Highness the Grand Duke Constantine Constantinovitch, MM. Mascart (President), von Bezold, Billwiller, Hepites, Hildebrandsson, Pernter, Rykatcheff, and Snellen.

On the motion of M. Rykatcheff, M. J. Kiersnovsky was invited to attend the meetings, in order to render assistance in keeping the minutes of proceedings.

The Grand Duke opened the meeting, and bade the members of the Committee welcome in the following words:—

"Gentlemen,—It is with sincere and heartfelt pleasure that I see you assembled within the walls of this Academy, and I bid you a very hearty welcome.

"The domain of science which you cultivate, Gentlemen, has from the earliest times attracted the attention of our Academy, for our first meteorological observations took their origin at the very epoch of its foundation.

"Seventy years have gone by since the day when the celebrated Alexander von Humboldt, in this very lecture room, put forward his ideas on the necessity of extending the system of magnetic and meteorological observations in Russia. The eminent member of our Academy, M. Kupffer, was fired by this appeal, and framed a scheme for a complete system of observations which were to be instituted and to be discussed in a special central institute to be created for that object. Twenty years later, thanks to the Emperor Nicholas, the august protector of science, Kupffer saw the fulfilment of his dream—the foundation of the Central Physical Observatory of St. Petersburg.

Far from being content with this success, he sought to create a vaster organization, which should institute a uniform system of magnetical and meteorological observations to be carried out over the entire globe.

"In 1850, at the meeting of the British Association at Edinburgh, Kupffer submitted a memorandum in which he suggested the establishment in every country of a system of meteorological and magnetical stations, which should be connected with a central institution serving as their point of reunion. The directors of these institutions should, according to Kupffer's scheme, meet together at certain intervals in order to arrange their plans of action for the future. He contemplated, in one word, such meetings as that which is now about to begin, and we may therefore consider his project as actually in the way of its full accomplishment.

"The year 1899, Gentlemen, is a twofold jubilee for the Nicholas Observatory, the services of which to science you have so gracefully recognized in your addresses. It is the fiftieth since its foundation, and the centenary of the birth of its founder. We have been deeply touched by the congratulations which you have been so good as to convey to us from the illustrious institutions which you administer, and I have the pleasure of expressing to you, not only in my own name, but also in that of the Academy of which I have the honour to be President, our sincere recognition of the sentiments you have expressed to us.

"Thanks to the labours of the earlier meteorological Congresses and Conferences, and of the meteorological committees, many important investigations have been brought to successful issues. A certain uniformity in the methods of observation has been secured throughout the civilized world, and these mark incontestable progress; but we cannot stop. Science can never be stationary; new problems are proposed; new questions arise; and no matter how great the results which your zeal and energy have attained, science calls you to fresh efforts, to continued work.

"Proceed with your work, Gentlemen, so important for the progress of science and the prosperity of the human race, and believe in the sincerity of our hope that the most brilliant and perfect success will crown your noble efforts."

The President replied:

"Monseigneur,—I feel myself happy, in the name of my colleagues, in expressing to your Imperial Highness our thanks for the great honour you have conferred on us in presiding over this the first meeting of the International Meteorological Committee of Paris, and in welcoming us so graciously within the walls of the Imperial Academy of Sciences.

"Our chief object in selecting St. Petersburg as the place of meeting has been to enable us to present our congratulations, and to give utterance to our cordial good wishes on the double occasion of the jubilee of the foundation of the Nicholas Observatory and the centenary of the birth of its founder.

"On the present occasion several of our members, even of those who have been most regular in their attendances, have found it impossible to be present. They have charged me to express their sincere regret, and, at the same time, have assured me of their lively interest in our proceedings.

"The remembrance of Kupffer, which your Highness has recalled to us in such graceful words, is peculiarly appropriate to the present occasion. Kupffer was a man of science, and also of originality. His personal work on most of the questions of Terrestrial Physics and his active propaganda may be said, in one word, to have furnished the basis for the programme of operations which the International Meteorological Committee is seeking to carry into general adoption by common agreement between all civilized nations.

"We could not have commenced our proceedings under more propitious auspices."

The President announced that Mr. Scott, the zealous Secretary of the Committee, had been detained in England by the sudden death of Lieutenant Baillie, and he requested M. Hildebrandsson to be so good as to undertake the duties of provisional Secretary. This proposal was unanimously accepted.

M. Hildebrandsson expressed his thanks to the Committee and took his seat as Secretary.

The President reported, that a provisionary programme of the questions to be submitted to the Committee had been distributed to the members. This programme was adopted with the addition of certain proposals made respectively by MM. von Bezold, Neumayer, Paulsen, and Pernter. (Appendix I.)

M. Mascart read the subjoined report on the action of the Committee since the meeting of Paris, prepared by Mr. Scott :—

"The Conference of Paris in 1896 entrusted the International Meteorological Committee with the duty of collecting reports on several questions which had been handed over for consideration to special Committees nominated by the Conference.

"We have the honour to submit the reports which have reached us up to date.

"In addition the Conference had adopted the following resolution :—

The Conference requests the International Committee to convene a meeting of the Directors of the different offices which deal with Ocean Meteorology, in order to establish uniformity in the methods of observation and of publication. It hopes that a report on the subject may be prepared for the next Conference.

"In order to carry out this resolution, we have consulted the Chiefs of the different offices which deal with Ocean Meteorology. We have not received any definite proposal, and we are of opinion that the moment for convening a Maritime Conference has not yet arrived.

"On April 20, 1898, we received from Dr. Hann an intimation of his desire to give up his seat on the Committee, and a proposal that M. Pernter, his successor at the Central Anstalt at Vienna, should be elected in his place.

"On the 9th of May, 1898, we consulted our colleagues by a circular containing the following proposals :—

(1.) The co-option of M. Pernter.

(2.) The acceptance of an invitation from General Rykatcheff for an eventual meeting of the Committee at St. Petersburg.

"The majority of the replies as regards the two proposals were favourable, and M. Pernter was elected a member of the Committee.

"As regards the date of the proposed meeting, a circular of March 24, 1899, had proposed the 25th of August, in accordance with General Rykatcheff.

"Later, M. von Bezold had requested the Committee to defer the day of meeting for a week, and to fix it for September 2. This proposal was communicated to the members of the Committee by a circular of the 30th of May. No objection was raised to it, and the date of meeting was definitely fixed for September 2.

"Unfortunately, several members have found it impossible to come to St. Petersburg. MM. Davis, Eliot, Moore, and Russell find the difficulties of a visit to Europe too serious, M. Capello excuses himself on the score of health, M. Paulsen on account of the duty which has devolved upon him of directing a scientific expedition in Iceland, M. Mohn is unable to quit Christiania owing to the meeting of the International Statistical Institution, and M. Tacchini has engagements which retain him in Rome.

"Quite lately, owing to the sudden death of Mr. Baillie, who took charge of the Meteorological Office during his occasional absences, Mr. Scott has been unable to obtain permission to leave the United Kingdom during the autumn.

Robert H. Scott."

The report was adopted.

The President read the report by Professor Rücker on the proceedings of the Committee for Terrestrial Magnetism and Atmospheric Electricity, and particularly on the proposals made by that Committee, at its meeting at Bristol in 1898, as to the future organization of magnetic conferences (Appendix II.).

These proposals were unanimously adopted.

M. Hildebrandsson, as Chairman of the Cloud Committee, read his report on the proceedings of that Committee since the last Conference in Paris in 1896 (Appendix III.). At the same time he handed in the publication which he had issued on the subject, and also that of M. le Père Algué, S.J., of Manila.

The President, in the name of the Committee, thanked the Cloud Committee for the important results they had already secured, and he most particularly complimented M. Hildebrandsson on the very important part that he had taken in the organization of these researches, which possessed scientific interest of a very high order.

The President communicated to the Committee (1) a report by M. Hergesell on the proceedings of the Aeronautical Committee (Appendix IV.), (2) a letter from M. Teisserenc de Bort on the experiments he had made with unmanned balloons and with kites at Trappes (Appendix V.), and (3) a report by Mr. Rotch on the use of kites at his observatory at Blue Hill (Appendix VI.).

Dr. von Bezold made some remarks on the aeronautical experiments carried out at Berlin under the direction of Dr. Assmann.

The Committee requested Dr. von Bezold to furnish, at a later meeting, a note on this subject (Appendix VII.).

The President gave a summary of an interesting memoir by M. Violle, Chairman of the Committee for Radiation and Insolation (Appendix VIII.).

The special questions raised during the discussion of the above reports were deferred to subsequent meetings.

The meeting was closed at 3h. 40m. p.m.

SECOND MEETING, MONDAY, SEPTEMBER 4TH.

The meeting was commenced at 9h. 30m. a.m.

Present:—MM. Mascart, President; Hildebrandsson, Secretary; von Bezold, Billwiller, Hepites, Pernter, Rykatcheff, Snellen, and Kiersnovsky, Assistant.

The minutes of the first meeting were read and confirmed.

The discussion was opened on:—

Question 5. "Is it desirable that the Committee should interest itself in seismological observations?"

The President read a letter from Mr. John Milne on this subject (Appendix IX.).

The Secretary gave a summary of the action of the Committee on this subject at previous meetings (Rome, 1879, and Berne, 1880).

M. Rykatcheff gave an account of what had already been effected in Russia, and of what it was proposed to carry out in conjunction with the Imperial Academy of Sciences.

M. Billwiller observed that for the last twenty years a Seismological Committee had existed in Switzerland.

After some discussion, in which MM. Mascart, von Bezold, Pernter, and Hepites had taken part, the following resolution was adopted unanimously:—

"The Committee recommends that Meteorological Institutions should take part in seismological investigations."

Question 6. "Antarctic explorations."

M. von Bezold stated that Germany and England were at present organizing scientific explorations in the Antarctic regions, and that it was desired that, during these expeditions, simultaneous magnetical observations should be carried out over all the terrestrial globe, and that more frequent meteorological observations should be recorded in the Southern Hemisphere. It would be very desirable that other countries should, if possible, organize expeditions for the same year—1902.

After some discussion the Committee requested M. von Bezold to draw up a report on the subject (Appendix X.), and adopted the following resolution:—

"It is extremely desirable (1), that the results of these expeditions should be completed by data furnished by existing observations or by ships; by the establishment of new observatories, and above all by the organization of magnetic observations in the Southern Hemisphere, and (2), that the magnetic observations over the entire globe should be carried out in accordance with those of the expeditions."

Question 7. "On the establishment of observatories at the centres of action of the atmosphere."

M. Hildebrandsson read a report on this question (Appendix XI.).

The majority of the members took part in this discussion, and the Committee adopted unanimously the following resolution :—

"The Committee recognizes the great importance which M. Hildebrandsson has pointed out of securing observations carried on regularly at certain spots which appear to possess special importance in indicating the general character of the movements of the atmosphere. The Committee have been much gratified by the particulars supplied by MM. von Bezold and Mascart of the project of His Serene Highness the Prince of Monaco, for the establishment of a meteorological and magnetical observatory in the Azores."

Question 8. "The determination of the meteorological day."

After an exhaustive discussion, the following resolution was adopted :—

"If the calculation of diurnal means is not made by the use of the exact formula

$$\left(\frac{0 + 24}{2} + 1 + 2 + 3 \quad . \quad . \quad . \quad + 23\right) : 24,$$

it will suffice to include the midnight observation as belonging to the previous day, as is already done at most stations, and to adopt the formula

$$(1 + 2 + 3 \quad . \quad . \quad . \quad + 24) : 24."$$

The meeting was closed at noon.

THIRD MEETING, TUESDAY, SEPTEMBER 5TH.

The meeting was opened at 9h. a.m.

Present :—MM. Mascart, President; Hildebrandsson, Secretary; von Bezold, Billwiller, Hepites, Pernter, Rykatcheff, Snellen, and Kiersnovsky, Assistant.

The minutes of the preceding meeting were read and confirmed.

The discussion was opened on :—

Question 9. "Instructions for the use of sunshine recorders."

M. Hildebrandsson gave a summary of the resolutions already adopted at the Munich Conference (Report, p. 17), and at Paris (Report, p. 42).

M. von Bezold explained the method of observation adopted at Magdeburg.

After a brief discussion, the Committee decided—

"That it is best to refer the sunshine observations to true time, and to indicate this at the head of the tables. In the actual condition of the question, the Campbell recorder appears to maintain its position as the instrument which furnishes the most comparable values."

Question 10. "Rules for determining the temperature of the surface of the soil."

After discussion the Committee adopted the following resolution :—

"The temperature of the surface of the soil is still an element which is ill-defined, and which depends in great measure on the character of the soil, but which has a real practical importance from the point of view of agriculture. It is to be recommended that the observations should indicate the exact conditions under which the thermometers are exposed."

M. von Bezold added that it would be very useful to determine also the physical constants of the different layers of soil, such as the thermal capacity for unit of volume, the conductibility, etc.

Question 11. "Precautions to be adopted in the use of spirit thermometers."

The Secretary reported that the Committee had adopted various resolutions on this subject. Zürich (Appendix V., p. 14), and Munich (Appendix VII., p. 67).

M. Rykatcheff handed in a note on this question (Appendix XII.), and several members described the precautions taken in their respective systems of observation to check spirit thermometers.

Question 12. "Symbol to indicate low fog."

The Secretary pointed out that at Munich (p. 22) it was decided to introduce the symbol (≡) to indicate a fog which does not exceed the height of a man.

M. Rykatcheff remarked that it was rather difficult to introduce a strong lower line in the international symbols, and that he preferred replacing it by a wavy or zigzag line.

The Committee considered that it was not advisable to alter the international symbols. In the manuscript schedules the wavy line might be employed for the lowest line, and the printer might be instructed to replace this by a dark line.

Question 13. "The precise definition of the circumstances in which the symbol for thunderstorms should be employed."

M. Rykatcheff remarked: "As thunder cannot occur without lightning, the symbol T can have no other meaning than that of a distant storm. In our special instructions for thunderstorm observations we have adopted the symbol T for distant storms, when the thunder is heard more than 10 seconds after the flash. It is desirable that a future Conference should again consider this question, and that if the symbol T be maintained, its meaning should be more precisely defined."

No resolution was adopted on this subject.

Question 14. "The protection of magnetic observatories against the influence of industrial electricity."

M. von Bezold submitted some magnetic curves obtained by M. Eschenhagen in order to show the disturbances produced by electrical tramways at different distances. It was quite evident that the whole science of terrestrial magnetism was seriously compromised by electrical industrial establishments, and that it was absolutely necessary to adopt some measures to meet this danger, which is daily increasing.

After some discussion, the Committee requested M. von Bezold to prepare for the Appendices a note of the results of the very interesting researches carried out on this subject (Appendix XIII.).

The Committee felt that it could only place on record its sincere concern at the very serious situation of affairs, which would necessitate the removal of magnetic observatories to a distance from centres of population.

The meeting was closed at noon.

Fourth Meeting, Wednesday, September 6th.

The meeting was opened at 9h. a.m.

Present:—MM. Mascart, President; Hildebrandsson, Secretary; von Bezold, Billwiller, Hepites, Pernter, Rykatcheff, Snellen, and Kiersnovsky, Assistant.

The minutes of the last meeting were read and confirmed.

Question 15. "A proposal to publish, in a special form, tables of the diurnal march of temperature in each country."

The President read a letter by Dr. Hann on this subject (Appendix XIV.).

After discussion, the following resolution was adopted:—

"The Committee recognize the interest and importance of Dr. Hann's proposal, but consider that the question has a general bearing, and ought to be considered by a Conference, after specimens of the proposed tables and the form of publication have been submitted. Pending an ulterior decision, the Directors of the different institutes expressed their readiness to supply to Dr. Hann, in manuscript, if he required them, the results of observatories at stations of the First order."

Question 16. "The importance of actinometrical observations."

The President read a report by Dr. Hann (Appendix XV.).

Dr. Hildebrandsson proposed that the Committee should declare its assent to Dr. Hann's proposal, and expressed the hope that the Committee on Radiation and Insolation would take up this subject, and submit a report thereon to the next International Meeting.

Question 17. "The installation of anemometers in the open country under identical conditions."

The President read a report by M. Teisserenc de Bort (Appendix XVI.).

M. von Bezold remarked that Dr. Sprung, at Potsdam, had taken steps to realise this project. M. Billwiller observed that, with a simple pole, very convenient aspiration anemometers, similar to the English instrument, could be set up. On the whole the Committee was of opinion that the question required consideration at a general Conference.

Question 20. "Is it desirable to restrict the use of the psychrometer as far as possible, and to recommend the employment of the hair hygrometer for the observations at stations of the Second order?"

M. Hildebrandsson gave a summary of the resolutions in relation to this subject recorded at previous meetings; Vienna (p. 58), Rome (p. 13), Munich (p. 12), Upsala (p. 7).

M. Pernter read a report on the subject (Appendix XVII.). He expressed the opinion that for stations of the Second order, where the method of ventilation is not applied to the instruments, it would be best to employ the hair hygrometer. M. Rykatcheff, on the other hand, stated as his opinion that in the actual state of the question the use of the psychrometer is to be preferred, as these instruments remain comparable *inter se.*

M. von Bezold observed that M. Assmann had devised a very simple form of aspirator, which might be employed at stations of the Second order.

M. Snellen stated that he had graduated his hygrometer according to Regnault's method, by the employment of solutions of sulphuric acid, but he thought that it was desirable to employ for that purpose saline solutions of which the vapour tension was well known.

M. Hildebrandsson was of opinion that, below the freezing point, it would be advisable to employ other instruments than the psychrometer, which gives very rough results, but it is to be feared that at present no

other hygrometer exists which is practically useful for determinations at low temperatures.

The Committee considered that it was not advisable to adopt any resolution on this question.

The meeting was closed at 11h. 30m. a.m.

Fifth Meeting, Thursday, September 7th.

The meeting was opened at 9h. 15m. a.m.

Present: MM. Mascart, President; Hildebrandsson, Secretary; von Bezold, Billwiller, Hepites, Pernter, Rykatcheff, Snellen, and Kiersnovsky, Assistant.

The minutes of the last meeting were read and confirmed.

Question 21. "Cable to Iceland."

The President read a letter from our colleague, M. Paulsen, Director of the Meteorological Institute of Denmark (Appendix XVIII.).

This proposal gave rise to a long discussion in the Committee in regard of (1) its scientific bearings, and (2) the possibility of carrying it out.

The Committee adopted the following resolution:—

"The experience of recent years has confirmed the Committee in the opinion it has already expressed on more than one occasion on the actual importance of telegraphic communication of information as to the weather in Iceland. It can only express its sincere wishes for the success of the measures undertaken with this object by the Danish Government."

Question 22. "International ten-day weather bulletins."

A report was submitted from MM. von Bezold and Neumayer (Appendix XIX.).

M. von Bezold gave detailed explanations on the scientific ideas which had led him to propose this scheme, and on the extent of co-operation required from foreign institutions. Several members took part in the discussion.

The Committee thanked M. von Bezold for his communication, and expressed the view that it would be well to have a definite plan of the proposed publication, so that each office could consider its bearing, and how it could itself contribute to its realization.

M. Pernter remarked on the difficulties experienced in the service of meteorological telegraphy, and proposed to form a committee to examine into the improvements which could be introduced into the service of meteorological telegraphy for weather forecasting (Appendix XX.).

The proposal was adopted, and the Committee was formed as follows:—MM. Pernter, Chairman; Billwiller, Snellen, Rykatcheff, Mohn, Tacchini, and Neumayer.

M. von Bezold read a short report on the researches of Dr. A. Schmidt at Gotha, relative to the simultaneous magnetic observations made on February 26, 1896, at different observatories.

The Committee requested that M. von Bezold would draw up for the Appendices a note on this interesting investigation, with some figures to indicate the results obtained (Appendix XXI.).

Date of the next meeting.

The President read a letter from M. Tacchini, in which he proposed a meeting in Paris in 1900.

After a discussion, the Committee requested M. Mascart to arrange, if possible, for Paris, next year, in the first half of September, on the occasion of the open Meteorological Congress, for meeting of (1) the International Meteorological Committee, (2) the Special Committees for Terrestrial Magnetism, for Scientific Aeronautical Enquiries, for the Clouds, for Insolation and Solar Radiation, and for the improvement of the service of Weather Forecasting.

Before concluding the meeting, the President expressed to M. Rykatcheff the profound acknowledgments of the Committee for the truly magnificent reception which had been accorded to it, and especially for the cordial and affectionate character of that reception, of which each of the members would carry away the most precious recollections.

M. Rykatcheff replied:—

"I have been deeply touched by the cordial words which your President has just expressed to me. I thank you heartily for your indulgence, and at the same time I propose to assure our President, M. Mascart, of our most sincere recognition of the way in which he has conducted our proceedings with such zeal and ability."

In conclusion the President expressed the thanks of the Committee to our colleague, M. Hildebrandsson, for having undertaken the heavy task of the secretaryship; and he also thanked M. Kiersnovsky for the valuable assistance he had rendered to the Committee, and for his extreme courtesy to each of its members.

The minutes of the meeting were read and confirmed.

The meeting was closed at 11h. 45m. a.m.

APPENDICES.

APPENDIX I.

Provisional Programme.

1. Report of Professor Rücker on Terrestrial Magnetism and Atmospheric Electricity.
2. Report of M. Hildebrandsson on Clouds.
3. Report of M. Hergesell on Balloon Ascents.
4. Report of M. Violle on Radiation and Insolation
5. Is it desirable that the Committee should deal with Seismological Observations ? (M. Rykatcheff).
6. Antarctic Exploration (Dr. von Bezold).
7. Centres of Atmospheric Action (M. Hildebrandsson).
8. Definition of the Meteorological Day (M. Rykatcheff).
9. Instructions for the use of Sunshine Recorders (M. Rykatcheff).
10. Rules for the determination of the temperature of the soil (M. Rykatcheff).
11. Precautions to be adopted in the use of alcohol thermometers (M. Rykatcheff).
12. The symbol to indicate low fog (M. Rykatcheff).
13. Further definition of the meaning of the symbols employed to indicate thunderstorms (M. Rykatcheff).
14. Protection of magnetic observatories against electric supply currents (Dr. von Bezold).
15. Proposal for the publication, in a special form, of the diurnal march of temperature for each country (Dr. Hann).
16. The importance of actinometric observations (Dr. Hann).
17. The installation of anemometers in the open country under identical conditions (M. Teisserenc de Bort).
18. The use of carrier pigeons by Transatlantic steamships for transmitting information about the weather off the European coasts (M. Teisserenc de Bort).
19. Publication fortnightly, or every ten days, of recent reports of weather in the Atlantic portion of the Northern Temperate Zone (Dr. von Bezold).
20. The desirability of restricting the use of the psychometer as far as possible and of recommending the use of the hair hygrometer for r gular hygrometric observations (Dr. Pernter).

21. The establishment of a telegraph cable to Iceland (M. Paulsen).
22. International ten-day weather reports (MM. von Bezold and Neumayer).
23. Date of the next International Conference.

APPENDIX II.

The Magnetic Conference.

The International Magnetic Conference met at Bristol in September 1898, during the meeting of the British Association.

The President (Prof. Rücker) in his address gave a complete account of the circumstances which had led to that meeting. The names of the gentlemen present, and the subjects discussed, will be found in the report of the Conference.

The meeting was a success from all points of view. A report of its proceedings has appeared in "Terrestrial Magnetism"; separate copies of the portion of the British Association Report which concerns the Conference will be distributed in the course of the coming year.

The Permanent Magnetic Sub-Committee has decided that its resolutions shall be submitted to the next meeting of the International Meteorological Conference, and that the attention of that meeting should be specially called to the proposals for the future organisation of the Magnetic Sub-Committee.

A. W. RÜCKER,
Chairman.

December 22nd, 1898.

The future organisation of the Magnetic Sub-Committee.

In view of a future organisation, the Sub-Committee passed the following resolutions :—

It is desirable that Terrestrial Magnetism should continue to be within the scope of the International Meteorological Conference, provided that :

(1.) Invitations to attend the Conference are issued as widely as possible to directors of magnetic observatories, and to all students of terrestrial magnetism.

(2.) That the Permanent Committee on Terrestrial Magnetism and Atmospheric Electricity, as established at the Paris Conference, be continued.

(3.) That in future there shall be a Magnetic section of the International Meteorological Conference, which shall elect a Permanent Magnetic Committee.

(4.) That the Magnetic Committee have power to summon an International Magnetic Conference at times other than those at which the whole of the International Meteorological (and Magnetic) Conference may meet.

The Sub-Committee also considers that the President of the Permanent Magnetic Committee should only hold office between two successive meetings of the International Meteorological (and Magnetic) Conference.

A. W. RÜCKER.

September 13th, 1898.

APPENDIX III.

Report of the Proceedings of the Cloud Committee.

The International Meteorological Committee, which met in Paris in 1896, elected a Cloud Sub-Committee, the members of which were MM. Hildebrandsson, President, Riggenbach, Secretary, Mohn, Rotch, Rykatcheff, Sprung, and Teisserenc de Bort.

At its first meeting, held in Paris, September 23rd, 1896, the Committee adopted the proposal of M. Rykatcheff, that the international observations on clouds should be continued until January 1st, 1898, wherever it was possible, so as to secure that a synoptic series of observations for one complete year at least should be available.

According to a resolution of the Sub-Committee, the Secretary invited by circular all the directors of meteorological services to send in to the President a note on the actual condition of cloud observations in their respective countries, and to indicate therein :

A. The stations at which either *direct observations* are made. or simple nephoscopes are employed.

B. If *exact measurements* are made, and in that case to indicate briefly the instruments and methods employed,

C. If there is an intention of establishing fresh stations in the future.

We now give a succinct summary of the information received up to date.

At the present moment the results have been published, as far as possible, in accordance with the proposals of the Committee (see below); in two countries, Sweden and the Philippine Islands, these publications have been received at the office of the Committee.

1. *Sweden* (M. Hildebrandsson).—In Sweden direct observations have been taken at Skara, Linköping, Wisby et Luleå. Exact measures have been made at Upsala, by means of photogrammeters, from May 1, 1896, to April 30, 1897, by MM. A. Lundal and J. Westman. The results are given in a publication of the meteorological observatory of the Royal University of Upsala, bearing the title "*Études internationales des nuages*," 1896–97. La Suède I.–III.

2. *The Philippine Islands* (Père José Algué).—Direct observations have been made at several stations in the Philippines. Exact measures have been taken at Manila by two pairs of photogrammeters. The results are published under the title "Las Nubes en el Archipiélago Filipino. Colaboración al trabajo internacional de medición de nubes."

3. *Norway* (M. Mohn).—Direct or nephoscopic observations have been made at Christiania, Bergen, Trondhjem, and Lödingen, in the summer at 8h. a.m., 2h., and 8h. p.m., but in winter only at noon. Exact measures were made at Bossekop from May 29 to September 30, 1896, by means of theodolites. The number of these determinations is about 1900. In winter such observations are impossible in that latitude. The results will probably be published towards the close of the year 1899.

4. *France* (M. Teisserenc de Bort).—Observations and measurements of clouds by means of photogrammeters were carried regularly up to May 18, 1897, at the dynamic meteorological observatory at Trappes, belonging to M. Teisserenc de Bort. M. Åkerblom, assistant at the Upsala Meteorological Observatory, has been engaged for several months at Trappes in order to ensure complete concordance between the operations at these two observatories. About 2,000 points have been measured, and the results will be published in the spring of 1900.

5. *Russia* (M. Rykatcheff).—In Russia the international observations on clouds have been principally organised by the Nicholas Central Physical Observatory, which has collected the documents. These observations are divided into three categories :—

I. Observations made according to the international programme on the forms of clouds and the velocity of their movements. These are only determinations by estimate, without any instrument. Such observations have been sent in to the observatory by :—

188 stations from May 1, 1896, to December 31, 1897.

48 " " " April 30, 1897.

(These include stations in Turkey, at Bokhara, and in China.)

The interval fixed by international agreement having elapsed, many of these stations continue their observations to the present day. In 1899 observations from 206 stations were received.

At the observatory of Irkutsk similar observations were carried out three times a day from May 1896. In 1897 they were almost hourly. In that year 2,300 observations were secured, and in 1898 the number exceeded 3,400.

At the observatory of Ekaterinburg the forms of clouds, their movements, and the amount of cloud, have been observed hourly, according to the international scheme. From July 1, 1896, they began to observe the velocity of the movement of the clouds. These observations are continued up to the present day.

II. Observations of the direction and angular velocity of the clouds by use of a nephoscope.

Observations were sent in by six stations in 1897, 11 in 1898, and eight in 1899.

They have not been calculated as yet.

III. Observations made with photogrammeters or with theodolites.

a. Pavlovsk; the Constantine Observatory.—The observations have been made by the aid of photogrammeters conformably to the international scheme. The number of pairs of really successful plates reached 175 in 1896, 559 in 1897, and 38 in 1898. The calculation of the altitudes measured in 1898 is not yet finished. The number of consecutive pairs, for determining the direction and the velocity of movement of the clouds, is 125 in all. Meanwhile, almost always, at the same time as the measurements with the photogrammeters were being made, determinations with a nephoscope were also made of the angular velocity and the direction of the clouds.

b. Irkutsk Observatory.—In winter, January to May 1895, measurements were taken of the altitude of clouds by the use of theodolites (*vide* Bull. de l'Acad. Imp. des Sciences de St. Pet., series V., vol. IV., No. 3, 1896).

c. Ekaterinburg Observatory.—The altitudes of the clouds have been measured with theodolites. Of quite successful determinations there have been obtained 284 in 1895 and 267 in 1897, making in all 551 measurements. The calculations are not quite finished.

6. *Germany*:—

a. Potsdam (Dr. Sprung).—At Potsdam the measurements were made three times a day, from May 1 to June 30, 1896, with theodolites at the extremities of a horizontal base of the length of 367·8 metres. Since that date a pair of photo-theodolites by M. Koppe have been employed for a year, using a base 1,469·4 metres long, during the period from July 1 to November 9, 1896, and from May 3 to June 30, 1897. During the intermediate period in winter they used the base of 368 metres,

As far as possible the observations were taken every two hours during daylight, and every day except Sundays. In all they obtained 1,126 double, or rather quadruple, clichés, for there were always two successive photographs taken on the same sensitive plate to obtain the direction and velocity of the clouds. With the same object a Sprung's nephoscope was also employed as a

supplementary instrument. On these clichés we have measured 7,470 points.

The calculations will be completed in 1899, and after that the discussions will be undertaken at once.

Towards the close of 1897 an automatic photogrammeter was installed.* With this apparatus we secure simultaneously photographs of the zenithal region at the two ends of the 1,469 metre base. This procedure is certainly preferable for the upper and the intermediate clouds; for the lower clouds the base is generally too long, and is not suitable when it is wished to measure the summits of *cumuli*. We are about to remedy these two inconveniences by means of adjustable mirrors, which will reflect downwards the images of clouds on the horizon.

We have received observations on the direction and relative velocity of the clouds from several stations in Prussia, and from Hohenheim in Württemberg.

All these observations and measurements will be given in a special publication conformable with the international scheme, and this will appear in the course of next year, 1900,

b. Dantzig (M. A. Momber).—At Dantzig, exact measurements have been taken with the same apparatus of M. Kayser and according to the same methods as before August, 1899.† Since May 1, 1896, a base of 1,500 metres instead of the old one of 679 metres has been employed. The calculations and discussions are completed, the only delay is about the cost of printing.

c. In the other parts of Germany neither observations nor special measuremenis have been organized. The stations of the *Seewarte* alone have been requested to observe very carefully, in 1897, the forms and movements of the clouds.

7. *Portugal* (M. de Brito Capello).—Nephoscopic observations have been made at the meteorological observatory of the Infante Don Luis. Exact measurements have also been made, since 1896, with the equatorials of the pattern Lettry-Ekholm, between that observatory and the astronomical observatory of Lisbon. The difference of altitude is not more than one or two metres, but the distance is 3,327 metres, which is too great for the lower clouds.

8. *England* (Mr. R. H. Scott).—No regular system of cloud observations has yet been organized. Mr. A. W. Clayden, alone, has made at Exeter exact measurements. The method adopted is to photograph the cloud and the sun on each of two plates simultaneous exposed at opposite ends of a base line. The time of exposure is noted and reduced to local apparent time, and the altitude and azimuth of the sun's centre computed. The image of the sun then serves to determine the altitude and azimuth of the

* A. Sprung: *Proposition pour simplifier l'exécution des photographies simultanées des nuages.* (*Rapport du Comité météorologique international*, Réunion d'Upsal, 1894, and *Zeitschrift für Instrumentenkunde*, Vol. XIX., 1899, pp. 112 and 129.)

† *Vide Schriften der Naturforschenden Gesellschaft in Danzig*, Vol. IX., 1, p. 93.

selected cloud point. The lenses used have a focus of 45·4 cm., and the plates used measure 16 × 22 centimetres.

9. *Denmark* (M. Paulsen).—The forms and directions of the clouds are observed usually three times a day at Copenhagen, Hinnerup, and Askow, in Denmark, at Thorshaven in the Faroes, at Reykjavik in Iceland, and at Angmagsalik on the east coast of Greenland. The observations have been taken with Sprung's nephoscope. A station will be established at St. Cross, in the West Indies. No altitude determinations have been made.

10. *The Netherlands* (M. Snellen).—The participation of the Netherlands in the international cloud observations has been confined to the direct observations which have been taken at the seven stations subjoined :—

Rotterdam	from August 1, 1896	to July 31, 1897.
De Bilt	„ January, 1, 1897	„ December 31, 1897.
Utrecht	„ May 1, 1896	„ April 30, 1897.
Wintersmyl	„ May 1, 1896	„ December 31, 1897.
The Helder	„ July 1, 1896	„ December 31, 1897.
Flushing	„ October 7, 1896	„ December 31, 1897.
Amsterdam	„ August 1, 1896	„ October 31, 1898.
Haarlem	„ December 1, 1896	„ October 31, 1897.

The observations and discussions are in the press, in the same form as the Swedish ones.

11. *Belgium* (M. Lancaster).—In Belgium there are about 100 observers who note twice a day at 8h. a.m. and 9h. p.m. the character of the clouds, their direction and velocity, according to the international decisions. A good number of observers make a third observation between 7h. and 9h. p.m. No accurate measurements are carried out. The want of funds and assistants has prevented the publication as yet, but M. Lancaster is in hopes that this will ere long be remedied.

12. *Austria* (Admiral Kalmar).—At Pola, observations with Fineman's nephoscope are made daily at 8h. and 9h. a.m. Noon, 2h., 5h., 7h., and 9h. p.m. on the relative velocities on the scale 0—4. At midnight, 2h. and 5h. a.m., the only observation is of the amount of cloud.

At Sebenico and at Teodo, and on board all the ships of the Austrian Navy, observations are taken every watch. Observations on radiation, undulation, etc., are always made.

13. *Spain* (M. Arcimis).—In Spain the forms and directions of the clouds are observed daily, either directly or by the use of nephoscopes, at Madrid, San Fernando, Oña, and La Guardia, but exact measurements are not made.

14. *Bulgaria* (M. S. Watzoff).—Observations of the direction and apparent velocity of the clouds have been made with a mirror four times a day, at 7h. a.m., 2h., 7h., and 9h. p.m., since September 1, 1896, at Sofia, and since January 1, 1897, also, at 11 Bulgarian stations and at Salonika in Turkey. No altitude measurements are made.

15. *The United States* :—

(*a*.) *Washington Weather Bureau* (Mr. Moore).—Nephoscopic observations are taken at 14 stations in the United States. Exact measurements are made with theodolites at Washington, in general three times a day. Prof. Bigelow was in charge of this work. His report was completed and received at the office, July 1, 1899, and is in the press. It contains *in extenso* the observations made with theodolites or nephoscopes ; the discussions and plates are conformable with the international instructions. There is also a large amount of new discussion relating to the clouds and the movements of the upper strata of the atmosphere. The report will appear at the close of 1899 as Part VI. of the *Report of the Chief of the Weather Bureau.*

(*b*.) *Blue Hill Observatory* (Mr. A. L. Rotch).—Nephoscopic observations of the directions and relative volocities of clouds have been made three times a day for the Weather Bureau at Washington. Exact measurements have been made, if possible, twice a day, with theodolites, at three stations situated on the same line. There are, accordingly, three bases of the lengths of 2,590 metres, 1,178 metres, and 1,412 metres. The differences in altitude are 189, 126, and 63 metres respectively. In addition, we employ in certain cases one of the four following methods :—

(1.) From one single station the positions of the cloud, the sun, and the shadow of the cloud are measured.

(2.) Kites are used.

(3.) At Blue Hill the altitude is taken of the reflected light from the lighting of a town, the distance of which is known.

(4.) The altitude of the base of a cloud on the slopes of the hill is noted by direct observation.

1,360 measurements, exact enough to be utilized, were effected in 253 days.

All the calculations have been completed. We have also employed for the definitive mean results the measurements made in 1890–91. The nephoscopic observations of 1896–97 will be published *in extenso*. The discussions have been carried out by Mr. Clayton.

The publication, in conformity with the international instructions, with additions, is in the press, and will shortly appear as an *Appendix to the Blue Hill Observations for* 1897 *and* 1898, in the *Annals of the Astronomical Observatory of Harvard College*, Vol. XLII., part II., and its title will be ; *Measurements of the Heights, Velocities, and Directions of Clouds during the International Cloud-Year, with a discussion of the results by H. Helm Clayton.*

This publication will be the eleventh of the important memoirs on the upper regions of the atmosphere, for which meteorologists are indebted to Mr. Rotch and his able assistants.

16. *Canada*. We have not received a report from the director, Mr. Stupart. MM. Moore and Rotch have informed us that measurements have been made with theodolites at Toronto,

and with nephoscopes at other stations. The measurements made at Toronto during April, May, and June 1897, have been published in the *Canadian Weather Review* for the respective months.

17. *India* (Mr. John Eliot).—Observations have been made with Fineman's nephoscope at :—

Allahabad	since	December 1, 1895.
Jeypore	„	January 1, 1897.
Lahore	„	August 1, 1896.
Madras	„	July 5, 1895.
Simla	„	March 1, 1895.

The observations have been continued up to the present time.

Measures with photogrammeters have been made at Calcutta during the year 1897, and at Allahabad in 1898.

The measurements made at Allahabad, and the nephoscopic observations made during the cloud year, will be published as soon as possible, in accordance with the international instructions.

18. *Victoria* (Mr. P. Baracchi).—Direct observations according to the instructions of the Sub-Committee have been made at 25 stations in Victoria. At Melbourne exact measurements have been made at two stations at a distance of 2,079 metres by the Kew method. Two apparatus with lenses of a focal distance of 112 mm. are placed vertically, and the plates measure 16 cm. square.

19. *Adelaide, South Australia* (Sir C. Todd).—It was the intention to commence nephoscopic observations with May, 1897.

20. *Batavia* (M. J. P. van der Stok).—From the 22nd of May, 1897, exact measurements have been made with two photogrammeters of the model of those at Upsala. The length of the base is 1,625 metres.

M. van der Stok has left Batavia to enter the service of the Meteorological Institute of the Netherlands. Consequently it is not known when and how the observations of that important station will be published.

21. *Finland.*—Immediately before the meeting at St. Petersburg we received the subjoined particulars through M. Hinrichs, of Helsingfors, in the name of the director, M. Biese. Direct observations according to the international cloud atlas have been made in Finland since 1896 at Helsingfors, Wiitasaari, and Torneå.

Since January, observations have been taken at the two latter stations, using the Cloud Atlas published by Hildebrandsson, Köppen, and Neumayer. The observations will be published according to the instructions of the Sub-Committee.

The first portion of the great international undertaking, the collection of observations, is approaching its termination, and the Committee had to organise the other part, the publications. It has followed the example of the Polar Commission, which fixed a programme, which was universally adopted. A model scheme which fixes the minimum of data required from all stations has been prepared by MM. Hildebrandsson and Mohn.

This scheme has been sent to all the members of the Sub-Committee by the Secretary, M. Riggenbach. The objections having been considered, the definite scheme was adopted, and forwarded to all the directors. It is according to this scheme that the observations in the different countries have been, or will be, published.

H. HILDEBRAND HILDEBRANDSSON,
President of the Committee.

APPENDIX IV.

Report of the Proceedings of the International Aeronautical Committee.

(*Up to March*, 1899.)

At the Conference of the Directors of Meteorological Institutions, which took place in Paris in 1896, several discussions took place with the object of ascertaining what services aeronautical science could render to meteorological enquiry. The outcome of these conversations was the appointment of the International Aeronautical Committee by the Conference.

A sub committee had been previously appointed, composed of several members of the Conference, and it held two meetings under the presidency of M. de Fonvielle. MM. von Bezold, Erk, Hergesell, Jaubert, Rotch, Rykatcheff, and Teisserenc de Bort, with others, took part in the deliberations. The subjoined proposals were drawn up and submitted to the Conference at the meeting of September 23, 1896, and they were accepted unanimously.

"(1.) The Conference recognizes the great importance for the science of meteorology, of aeronautical observations, and expresses a wish that balloon ascents should be encouraged and multiplied.

"(2.) The Conference expresses a wish that aeronautical experiments, either with ordinary or with unmanned balloons, should be made simultaneously at different stations.

"(3.) In the actual state of the question, the Conference is unable to recommend special methods or to specify instruments, but it expresses the wish that, in the case of simultaneous ascents of unmanned balloons, the patterns of instruments employed should be, as far as possible, identical.

"(4.) It is of the highest importance that the actual observations should be published as quickly as possible, especially in the case of simultaneous ascents.

"(5.) It is desirable that observations in unmanned balloons should be conducted in a regular manner.

"(6.) In consideration of the satisfactory results which have been obtained at Blue Hill, by the use of kites carrying self-registering instruments to the height of 2,000 metres, it is very desirable that similar experiments should be carried on elsewhere."

The International Committee fixed the following constitution for the Aeronautical Committee: M. Hergesell, President; MM. Assmann, Erk, de Fonvielle, Hermite, Jaubert, Pomortzeff, and Rotch, with express permission to co-opt other members if found desirable. The numerous subjects which have been taken up by the Committee, and especially the desirability of extending the aeronautical researches over as many countries as possible, have rendered it necessary to employ this power of co-option very frequently. Up to the present date the Committee has been able to find representatives in most countries, and its composition at present is as follows:—

MM. Hergesell, President,	MM. Hildebrandsson.
W. de Fonvielle, Secretary.	Hintertoisser.
Andrée.	Jaubert.
Angot.	Kovanko.
Assmann.	Moedebeck.
Besançon.	Pernter.
Berson.	Pomortzeff.
Prince Roland Bonaparte.	Rykatcheff.
Bouquet de la Grye.	von Siegsfeld.
Cailletet.	Tacchini.
Erk.	Teisserenc de Bort.
de Fonvielle.	Rotch.
Hermite.	

I. *International simultaneous ascents.*

The Committee held that its first task was to carry out all the resolutions adapted by the Conference. With this object, it at once set to work to arrange for as many ascents as possible at various points in Europe.

Seven of these ascents have taken place on the dates subjoined.

a. Ascents anterior to the Meeting of the International Aeronautical Committee at Strassburg.

November 14, 1896. Stations: Paris, Strassburg, Munich, St. Petersburg, and Warsaw.

Balloons.—Manned, 5; unmanned, 3.

February 18, 1897. Stations: Paris, Strassburg, Berlin and St. Petersburg.

Balloons.—Manned, 4; unmanned, 3.

May 13, 1897. Stations: Paris, Strassburg, Munich, and St. Petersburg.

Balloons.—Manned, 3; unmanned, 3.

July 27, 1897. Stations: Paris, Strassburg, St. Petersburg, and Vienna.

Balloons.—Manned, 2; unmanned, 3.

b. Ascents subsequent to the Meeting of the Committee at Strassburg, March 31, 1898.

June 8, 1898. Stations: Paris, Trappes, Strassburg, Brussels, Munich, Berlin, Vienna, and St. Petersburg. The observatory on Etna, in Sicily, also took part in the research.

Balloons.—Manned, 8; unmanned, 4.

October 3, 1898. (In connection with the Swiss ascent on the Alps.) Stations: Paris (Trappes), Sitten (Spelterini, Meteorological Institute of Strassburg), Munich, Berlin, Vienna, and St. Petersburg.

Balloons.—Manned, 5; unmanned, 3.

March 24, 1899. Stations: Paris, Trappes, Limoges, Strassburg, Berlin, Vienna, and St. Petersburg.

Balloons.—Manned, 4; unmanned, 7.

The first ascents, especially those which took place before the meeting at Strassburg, were principally employed in testing the equipment of the balloons, and, in fact, the apparatus. The importance of that meeting resulted specially in the following principle, which was established for all the members:—"Simultaneous ascents can possess no scientific value, and cannot expect any success unless the equipment of the balloons, manned or unmanned, at all stations is as nearly as possible identical."

Thermometers and barometers were tested by several members of the Committee from different points of view.

These researches have given rise to an interesting series of discussions, which later on have advantageously served as a basis for the proceedings of the meeting of the Committee. These proceedings have been printed, and they contain the discussions *in extenso* and the resolutions adopted. We may therefore refer to these protocols for matters of detail.*

The following is, in general terms, the actual condition of these important questions.

The equipment of the manned balloons was definitely fixed by the Committee.

As regards the unmanned balloons, it has not seemed practicable as yet to prescribe a normal equipment. The Committee expressed the wish that, concurrently with the Richard's recorders, fitted with Bourdon's tubes, which are always employed, we could construct and employ other thermometers, less inert, which could, owing to their superior sensitiveness, adapt themselves to rapid variation of temperature.

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* Rapport de la Commission aéronautique internationale, Réunion de Strasbourg Publié par l'Institut Météorologique de Strasbourg.

Instruments of that character (thermometers) have been lately constructed by MM. Teisserenc de Bort, Cailletet, Assmann, and Hergesell.

The experience obtained with these apparatus during the last two ascents at Strassburg have shown how much the accounts of the inertia of the Bourdon tubes have been exaggerated. This exaggeration arose from the comparison between the temperatures on the ascent and the descent. The investigations carried out with the thin strip thermometer have shown that the great retardation of temperatures of descent (accounted for by the high co-efficient of inertia in the upper regions attributed to the Bourdon tubes) was not, in fact, due to the sluggishness of the thermometers, but very frequently to the condensation of water on the instrument.

We must therefore give more credit to the data from Bourdon's thermometers (when these data refer to the curves of ascent) than has hitherto been done. The question of thermometer ventilation has been carefully studied by several members of the Committee.*

As to the question of the altitude attained by the balloons, it has been almost unanimously recognized that it is necessary to check the barometrical calculation by other methods.

The trigonometrical and geodetical measurements, which were taken on different ascents from Berlin, Strassburg, Paris, and St. Petersburg, have been completed in the most advantageous way by the photogrammetric method of MM. Cailletet and Finsterwalder. For the calculation of the barometrical heights a common method has been adopted, which M. Teisserenc de Bort has described in the Appendices to the report of the meeting.

The simultaneous ascents, organized by the Committee after the Strassburg meeting, have furnished as result very good meteorological determinations.† They have specially given rise to the idea of not fixing the date of simultaneous ascents beforehand, but to arrange for their taking place under certain meteorological conditions, agreeing to wait until these conditions should present themselves. This would allow of studying the atmospheric situation during the ascent.

The Swiss ascent of October 3, 1898, served, up to a certain point, as a sort of preparation, and abundantly showed the possibility of such an enterprise.‡

The ascent of March 24, 1899, the second ascent organized completely in accordance with the idea above mentioned, was crowned with success from every point of view.

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* *Vide* the papers by MM. Rykatcheff, Teisserenc de Bort, Assmann, and Hergesell, in the appendices to the report of the meeting.

† *Vide* the notes by MM. Hermite, Besançon, Teisserenc de Bort, and Cailletet in the *Comptes Rendus of the Paris Academy*, and the papers of M. Assmann (*Zeitschrift für Luftschiffahrt*), Hergesell, Maurer, and Erk (*Meteorol. Zeitschrift*), and M. Snelling (*Mémoires de l'Academie de St. Pétersbourg*).

‡ *Vide Meteorol. Zeitschrift*, 1899, Heim, Maurer, Spelterini: Ascent of the *Vega* over the Alps.

M. Teisserenc de Bort, to whom the choice of weather for that ascent had been left, had taken advantage of a very interesting phase of weather for his investigation.

The ascents of March 24 have proved *inter alia* that even at very great altitudes (10,000 metres) great changes of temperature may take place.

For the future, one of the principal objects of the Aeronautical Committee in its investigations will be to carry out the study of interesting meteorological conditions by means of simultaneous ascents organized for that purpose.

It appears from the table of simultaneous ascents that up to the present time the Meteorological Institutes or stations of Trappes, Paris, Strassburg, Munich, Berlin, and St. Petersburg, have taken an effective part in the work.

It is much to be desired in the interest of these researches that the list of collaborations should be increased, *e.g.*, by extending the area of ascents to the South-west, South (beyond the Alps), the West and the extreme North of the Continent. I should here point out the possibility of Meteorological Institutions which have not the apparatus for manned balloons for co-operating in these simultaneous ascents. In fact the sending up of unmanned balloons is very simply effected, and the fittings of these aerostats is so far perfected that certain results may be guaranteed.*

II. *Attempts and investigations made by members of the Committee to establish permanent meteorological stations in the free atmosphere.*

The results obtained by the international ascents are certainly very interesting and very valuable, but the attempts to make continuous observations in the free air, that is, to use the captive aerostats for the service of observations, have been neglected, notwithstanding the numerous balloon ascents organized by the members of the Committee during the period of its activity.

Permanent stations in the free air are of great importance for meteorological research, for they give us the opportunity of completing these researches in many directions where the results obtained by free balloons leave much to be desired.

The free meteorological balloon pursues its course and remains almost entirely in the same meteorological conditions as existed when it ascended. It only gives us the vertical constitution of the selected type of weather.

On the other hand, the kite remains sensibly at the same point in the atmosphere. This fact confers on this method a special stamp, for by the aid of kites we can prosecute the study of certain phenomena which succeed each other during the period of observation. These two methods, the kites and the free balloons, are complementary to each other in a very satisfactory manner. Where one fails the other comes to our aid.

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* *Meteorologische Zeitschrift*, January, 1900.

It is the great merit of our colleague, Mr. A. L. Rotch, to have largely improved the technical arrangements for kite ascents. Some time ago, at the Paris Conference, he had announced the beautiful results which he had obtained by using kites for meteorological observations. He has greatly increased his successes since that date. The kites have attained greater and greater altitudes, and have remained flying for a considerable time. On the last occasion they reached the height of 4,000 metres. On another occasion, which was perhaps even more interesting, a characteristic variation of the meteorological situation was registered and recorded by means of aerostatical apparatus.*

We may therefore, in the future, look for even more suggestive results from the establishment of our colleague, Mr. Rotch.

The brilliant results obtained by kites in the United States, where the Weather Bureau has organized stations on the same plan over its immense territory, have raised a wish in Europe no longer to neglect such researches. We should mention here, in the first place, the valuable work of the St. Petersburg Observatory, and the magnificent researches, so rich in results, of M. Teisserenc de Bort, at Trappes, a gentleman whose active co-operation in international balloon ascents has already been frequently mentioned.

At the Trappes Observatory, which is very completely fitted up, ascents of kites are organized to explore the upper regions of the atmosphere, whenever this is possible. We may already count, with great interest, upon the store of results which this work cannot fail to render.

At Strassburg, too, the Meteorological Institution has not neglected the use of kites. We must, however, note the presence of a very disagreeable circumstance, which is perhaps not so serious a disadvantage at other kite stations on the Continent. This is simply the insufficiency of wind force. If, by chance, the wind is fresh, it changes in force every instant, and so produces many disturbances in the flight of the kite. The situation of Strassburg, enclosed in the narrow valley of the Rhine, is singularly unfortunate in this respect.

On the other hand, I think that the extraordinarily favourable position of the Blue Hill Observatory, near the coast, under the influence of fresh sea breezes, blowing regularly, and, moreover, built on a high point, is such a situation as it would not be easy to find for another observatory.

The remarks made about Strassburg have also been found very frequently to apply to Trappes, according to the accounts of M. Teisserenc de Bort. In fact, they are probably more or less applicable to all continental stations. It frequently happens that the kite will not rise for want of wind. If the kites have risen, there are sudden gusts of wind which may break the wire, so that the kite breaks away.

These defects have given rise in Germany to the attempt to use captive balloons for meteorological observations. The spherical

* *Vide* the interesting papers by Mr. Rotch and his assistants in the Reports of the Blue Hill Observatory.

balloon can only rarely be employed for such work, for it can only be used at a time when there is a relative calm. It has therefore been decided to use the kite balloon, which has for some time been employed for military purposes, and which therefore can lend itself to meteorological requirements.

In the course of the International Meeting at Strassburg there was an opportunity of demonstrating the use of one of these kite balloons, made by the firm of Riedinger, of Augsburg. The balloon, which remained up for several days and nights, was left up during the meeting, carrying its freight of recording apparatus. This practical use of the aerostat has shown how useful it would be if employed as a permanent meteorological station in the free atmosphere.

The spherical balloon can also remain up for some time at the same level if the conditions are favourable. This fact was proved by an experiment I made during the fifth international ascent. I succeeded in keeping up a captive balloon for twenty-four hours, with observers in it, at an altitude sensibly unchanged, and I thus obtained valuable results for the daily march of temperature.*

The experiments with the kite balloon are not intended to show that this apparatus can completely take the place of the kites which have been so usefully employed.

On the contrary, the convenience of management will always give the preference to kites when it is possible to use them. The kite balloon can, however, always be employed instead of the kite when there is not wind enough to raise the latter.

I consider, further, that it is impossible to attain with the kite balloon such altitudes as have been reached by the kites in the ascents already mentioned. Possibly a combination of these two types of aerostats may be, in future, the most profitable for our researches. On the other hand, it is probable that the kite balloon alone is able to maintain itself in the atmosphere for a long period.

The experiments and results which have been mentioned, and which have been carried out either by the Committee or by some of its members, have directed particular attention to the subject, and have everywhere met with emphatic approval.

Thanks to the zeal of M. Teisserenc de Bort, there is now at Trappes a model aeronautical establishment, the organization of which leaves nothing to be desired. In Prussia the energy of MM. von Bezold and Assmann have already secured the vote of funds for the establishment of a first-class aeronautical observatory. At Hamburg the *Deutsche Seewarte* will undertake important experiments with kites. At Munich our active colleague, M. Erk, has made experiments in raising these kites.

It is to be hoped that in a short time all the great observatories will employ this powerful method of exploration, which is the outcome of modern meteorology.

* *Vide Meteorol. Zeitschrift*, 1899, p. 49.

The Aeronautical Committee, the youngest of those created by the International Committee, sees in this success of its operations, after so limited a number of years, the best justification of its existence. It sees also an encouragement to continue its researches on the same subject, and with the same aim.

The plans which will be followed in the future may be summarized as follows:—

(1.) The simultaneous international ascents should be continued, so as to study different meteorological phenomena at high altitudes by means of balloon observations.

The number of stations taking part in these operations should be, as far as possible, increased, and care should be taken to extend these stations gradually over countries where these do not yet exist. The participation of the South and East of Europe is most desirable. We must consider that the value of manned balloons depends on the services which these can render in the examination of the lower strata of the atmosphere.

(2.) If it is desired to undertake regular observations on atmospheric conditions at different levels by means of permanent atmospheric stations, either by means of kites or of captive balloons, it is absolutely necessary to establish aeronautical observatories. It is only these observatories which can manage kites, or conduct observations on meteorological phenomena with kite balloons and other apparatus.

(3.) No matter what interest attaches to the operations of the Aeronautical Committee, and, on the other hand, to those of the aeronautical observatories, and although these ought, in the first line, to facilitate our study of the meteorological conditions of the atmosphere, we must not lose sight of the fact that other branches of physics should also be objects for our attention.

The questions of electricity and of the composition of the atmosphere present themselves at once, and have already been dealt with by our able fellow workers, MM. Börnstein, Le Cadet, Cailletet, and others, but the question of Terrestrial Magnetism will become the subject of aerostatic experiments as soon as we shall have discovered the method of treating such. We must, in the first instance, await the extension of our knowledge of the vertical distribution of magnetism. The first difficulty in these observations consists in the fact that observations which require time to make them are impracticable in the movable observatory of the balloon car. It will be the duty of our successors to discover other methods of observing. If the information I have received is correct, several members of the Committee are already working on this question.

H. HERGESELL,
President of the Committee.

Strassburg, July 25, 1899.

APPENDIX V.

Report on the Experiments carried on in the Atmosphere by Unmanned Balloons and Kites at the Observatory of Dynamical Meteorology at Trappes.

The importance of a knowledge of the distribution of the different meteorological elements in the vertical direction, and notably of the temperature, is of the first order in regard of the advancement of meteorology It is for this reason we have undertaken, independently of the attempts to sound the atmosphere by means of kites, to carry on a methodical exploration of the atmosphere by unmanned balloons.

After some preliminary attempts, our really useful ascents were commenced at Trappes in April, 1898, and have been repeated frequently in each month since that date. The ascents have been repeated occasionally at short intervals in order to follow up the modifications which occur in the atmosphere.

In this way we have collected a mass of scientific documents which is probably by far the most important of all the facts we have previously brought together on this subject. It is based on more than 100 ascents of unmanned balloons, of which seven have risen above 14,000 metres, 24 above 13,000 metres, and 53 have reached 9,000 metres.

From the data thus collected we have been able to deduce the following facts:—

1. The temperature at different altitudes presents considerable variations in the course of the year, much more considerable than had been concluded from the earlier balloon observations.

The temperature of 0° C. is found at very different levels, and this gives a satisfactory explanation of the temperature of the ground, which itself sometimes exhibits that temperature in winter, while its temperature is raised very much in summer. Accordingly, the isotherm of 0° C., which at certain times is at the ground level, or which does not appear at all in the atmosphere (when the ground temperature is below 0° C.), moves away at other times, and in summer may be met with above the level of 4,000 metres.

The isotherm of −25° C., which generally is far distant from the earth, is also subject to great variations of altitude. It is found at about 3,000 metres in winter, and above 7,000 metres in summer. In September we have met it even above 8,000 metres. This gives a change of altitude of 5,000 metres in six months (and very possibly we have not caught the extremes).

The isotherm of −40° C. has lowered itself frequently to about 6,000 metres, and is usually found at about 9,000 metres, rising even above that level towards the end of summer.

The temperature of −50° C. has never been met with much below 8,000 metres, and its greatest altitude has been recorded in

September, 1898, and in July, 1899, at 12,000 metres. It, therefore, varies to the extent of 4,000 metres at least.

We see, therefore, that at that altitude, where we have passed through two-thirds of the mass of the atmosphere, the variations of temperature are very considerable.

2. It appears from these observations that even up to the level of about 10,000 metres there is a sufficiently marked tendency to an annual variation of temperature, the maximum occurring towards the end of summer, the minimum towards the end of winter; but this phenomenon is disturbed by the variations from day to day, due to changes in the atmospheric situation, which are very decided. For example, in one season we met with the isotherm of $-40°$ at 8,500 metres on March 14, and at 6,600 metres on March 24, 1899.

3. The character of the decrease of temperature in the vertical direction is different in the barometrical maxima and minima. In the former the temperature decreases slowly in the lower strata, and then takes up a normal rate of decrease, which it keeps up to 11,000 or 12,000 metres (our observations are not sufficient to show us what takes place at higher levels). In the latter, the minima, on the contrary, the decrease is rapid in the lower strata. It generally has a higher value on the whole than what is found in the maxima, at least up to the level of 8,000 metres, which has been attained often enough in bad weather.

Kites.

We have continued our kite experiments whenever the weather permitted, and in the course of June, 1899, we have reached, and even somewhat surpassed, the heights reached at Blue Hill in February.

Thus we have reached :—

3,940 metres on	June	14.
3,590 „	„	15.
3,300 „	July	3.
3,500 „	„	24.

We have introduced several important improvements in the apparatus used to work the kites. Thus, we drive the windlass by electricity, the action of which can be adjusted very exactly, according to the puffs of wind, in such a way as to avoid reaching the breaking stress of the wire.

The windlass and its dynamo are placed in a sentry-box, mounted on a turn-table, with the opening facing away from the wind. This arrangement allows the bobbin of wire, and the person who manages the windlass, to be protected from rain.

Several foreign men of science, in particular M. Lancaster, M. Hepites, Dr. Assmann, and M. Berson, came to Trappes to inspect the new arrangements as well as the mode of construction of our kites.

The ascents, which are more than 120 in number, exhibit several interesting facts, which I have communicated to the Academy of Science. I shall recount them here.

In a general way the decrease of temperature in the lower strata exhibits a character different over the areas of low pressure from that exhibited over those of high pressure.

1. In the former, as soon as you reach a height of a few hundred metres you see the decrease of temperature slacken, and often you find inversions of temperature (this phenomenon is frequent even in summer).

2. In the latter, on the contrary, the decrease is rapid, and can even reach the value indicated by the adiabatic expansion of the air, more or less humid, as the case may be.

With regards to the winds, our ascents show :—

(1.) That in fine weather, and with high pressure, the wind velocity generally decreases as you ascend from the ground (except for the first 50 metres, which are directly influenced by obstacles, trees, &c.) up to an altitude which varies between 1,500 and 3,000 metres.

(2.) On the contrary, with cloudy weather and low pressures the wind increases sensibly with height, especially in the vicinity of the layer of lower clouds.

Supplementary Note.—Since the completion of this note we have succeeded in making, on August 21, with a N.W. wind, a flight of kites to a great height, and we got above 4,300 metres. The temperature at the ground (171 metres) was 22° C.; at 4,300 metres it was $-5{\cdot}1°$. The sky was clear, with a few cirro-stratus clouds.

LÉON TEISSERENC DE BORT.

Trappes, August 12, 1899.

APPENDIX VI.

REPORT ON THE EXPLORATION OF THE ATMOSPHERE BY KITES AT THE OBSERVATORY OF BLUE HILL AND OTHER STATIONS IN AMERICA.

Kites were first employed to raise self-recording meteorological instruments at Blue Hill in August, 1894. Since that date the altitudes reached by meteorographs have constantly increased, as the subjoined table shows :—

Years.	No. of experiments.	Height above sea-level.	
		Mean height of the maxima at each experiment.	Maximum reached.
1894, August 1895	2	567	631
1896	28	510	759
1897	86	845	2,843
1898	38	1,450	3,571
1899	35	2,240	3,679
1899, up to May	10	2,342	3,792

The station at Blue Hill is 192 metres above the sea.

This improvement in the heights attained is the result of progress in the construction of the kites, and in the method of controlling them. The form adopted at present is that of a Hargrave's kite with curved surfaces, which allows of its being held at a great angle with the horizon. An automatic bridle governs the angle of incidence on the wind and thus prevents it from exerting on the line a tension greater than a certain value which has been determined beforehand. The practice of attaching the kite to different points of the wire, and of distributing the tensions in this manner, has rendered it possible to reach great altitudes. With the crank employed excessive tensions on the drum have been avoided, while the measure of the unrolled line has been taken and the traction measured continually.

Fergusson's meteorograph weighs about 1·3 kilog., and records barometrical pressure, the air temperature, the relative humidity and the velocity of the wind. We believe that the method of exposure of the thermometer gives the true temperature of the air.

I have given a general description of the apparatus, of the methods of work, and of the meteorological results in the *Quarterly Journal of the Royal Meteorological Society* for October, 1898. The recent apparatus has been described by Mr. S. P. Fergusson in the Blue Hill observations for 1896 (*Ann. Harvard Coll. Observatory*, vol. XLII., part I.), and in the same publication Mr. H. Clayton discusses the meteorological results of the first three years. Some subsequent observations relative to diurnal changes in the free air and the phenomena of cyclones and anticyclones are discussed by the same author in the *Bulletins of Blue Hill Observatory*, No. 2, 1898, and No. 3, 1899. Details of the actual apparatus are given in the *Bulletin*, No. 3, 1899.

There is no doubt that we shall be able to reach greater heights. The Blue Hill kites require a wind of at least six metres per second, but in case of calms and light airs, it is probable that a gas balloon might be able to raise the kite into a stratum of wind of sufficient force, although the method has not yet been employed in America. A hot air balloon, tried at Blue Hill, has not given good results.

In 1898 the U.S. Weather Bureau fitted out 16 stations, in the central part of the country, with kites, &c., from which it was hoped to obtain good results, by raising them to the height of a mile so as to construct a synoptic chart for the purpose of forecasting. This is the largest collection of kites which has ever been made, and it is to be regretted that the lightness of the winds at some of the stations have prevented the realization of the project.

I have had the honour of presenting a report on the employment of kites in meteorology at the meeting of the International Aeronautical Committee at Strassburg (*Rapport de la Commission aéronautique internationale*, Annexe XVIII.), and I am glad to learn that this American method of exploring the air will be largely used in Europe.

At Paris, Berlin, Hamburg, and St. Petersburg the meteorological institutions are preparing to fit out kites, and I have taken great care to answer all the enquiries made of me for designs and models of the apparatus at Blue Hill.

A. LAWRENCE ROTCH,
Director of the Blue Hill Observatory.

APPENDIX VII.

The Aeronautical Scientific Experiments at Berlin.

A. *The Scientific Ascents made by the Society for the Advancement of Aeronautics.*

The scientific ascents made during the last ten years were inaugurated by the invention of Dr. Assmann's psychrometer in 1887.

Experiments to determine the true temperature of the air were commenced in 1886, and have led to the invention of the instrument just mentioned. The experiments have led to the conclusion that almost all the temperature data furnished by all previous balloon ascents were more or less affected by error.

For this reason it seemed highly important to recommence the examination of the atmosphere by the use of balloons furnished with improved apparatus.

The German Association for the Advancement of Aeronautical Science, founded by Dr. W. Angerstein in 1881, became interested in the question. After some experiments, MM. von Siegsfeld and V. Kremser made a first ascent, July 19, 1886, with a balloon belonging to M. von Siegsfeld.

The funds supplied from private sources, and notably by Dr. Werner von Siemens, and by the Prussian Academy of Science, permitted the continuance of the researches with balloons, both captive and free, all fitted with well-known self-recording apparatus.

The good results which were obtained encouraged the Association for the Advancement of Aeronautical Science to petition His Majesty the Emperor for funds to continue the researches on a larger scale.

The request was graciously granted. His Majesty himself took great interest in the experiments. From March 1, 1893, we commenced a great series of 75 ascents with free balloons, of which 65 were manned, and in these direct observations were taken. The 10 other ascents were made on the plan of MM. Hermite and Besançon, that is with unmanned balloons, equipped in the well-known manner.

It is these last which have reached the greatest altitudes. April 27, 1895, the balloon *Cirrus* went up to the height of

21,800 metres, according to the exact indications of the barograph, which was thoroughly compared and verified after the ascent, and the balloon reached ground in a Danish Island, Laaland.

The same balloon made two other ascents, and the points at which ground was reached was in Bosnia and at Wilna (Russia). The altitudes reached were 17,425 metres and 17,210 metres. At the last mentioned point the temperature shown on the curve was − 68°, but there is some uncertainty about the figure, as is the case with all the data furnished by unmanned balloons.

The results of the 65 manned balloons may be classed among those which have risen to the greatest altitudes. We may first mention the ascent of M. A. Berson, December 4, 1894. The most careful calculations give an altitude of 9,155 metres. The corresponding temperature was − 47·9°.

The next ascent in this series of very high ascents was made by M. Berson from the Crystal Palace, London, September 15, 1898, when he reached the height of 8,320 metres and observed the temperature of − 34·1°.

Following the order of the altitudes, we should mention the ascent of M. Süring, March 24, 1899, who rose to the altitude of 7,955 metres, with a temperature of − 48·2°, the lowest that has ever been registered by direct observation in a balloon.

Formerly, Captain Gross and M. Berson had mounted, May 11, 1893, to nearly the same height (7,928 metres). The temperature observed was − 36·7°.

Among the other ascents there were four which rose above 6,000 metres and seven which rose above 5,000 metres, so that we have collected a very considerable number of facts from levels, which are certainly the highest accessible to man.

The mean maximum height of the 75 ascents has been 4,489 metres; the mean distance covered has been 222 kilometres; the velocity, 9·2 kilometres; and the duration of the ascent, 6h. 30m. The total duration of all the ascents has been 488h. 24m., or 20 days, 8h. 24m., and the total distance covered 16,638 kilometres.

These ascents extend over the different seasons, and over different hours of the day. They have been made in every month and at every hour of the day and night. Although we can reckon seven night ascents and five made very early in the morning, the greater number were made in the day-time.

On the whole, there were 52h. 30m. of observation in winter, 132h. in spring, 138h. 30m. in summer, and 102h. 30m. in autumn.

The scientific results of this undertaking will shortly appear under the title *Wissenschaftliche Luftfahrten*, published by MM. R. Assmann and A. Berson, in three volumes, published by F. Vieweg & Son in Brunswick.

We now give, in a few words, an extract from this book :—

(1.) "The results deduced formerly from Glaisher's ascents were certainly erroneous, for it has since been found

that the decrease of temperature is not greater below than above; on the contrary, for the same difference of altitude, it is greater above than below. From the direct observations which have been made at high levels, from 8,000 to 9,000 metres, we see that it closely approaches the adiabatic decrease of temperature in dry air, and that agrees exactly with the theoretical results.

"The temperature at the ultimate limit of the atmosphere (if one can venture at that altitude to attribute a temperature to the *air*) is much lower than has hitherto been believed, for we have frequently observed directly temperatures of − 48°, and automatic registers of unmanned balloons have marked − 68°.

(2.) "According to the first ascent made at Berlin, the idea was formed that at the altitudes of 7,000 to 8,000 metres the temperature was almost independent of the seasons. The last ascents have proved that this was not the case. We can see that the oscillations of temperature at that altitude are very considerable, if we examine the results supplied by the balloon of October 3, 1898, where the temperature of −28·8° was found at the height of 7,377 metres, and again those from the balloon of March 24, 1899, which gave −48·2° at 7,955 metres.

"It appears that in those very high regions the lowest temperatures occur in spring and the highest in autumn, and we may conclude from this that aloft the temperature in relation to the seasons is much more delayed than close to the ground.

"We may also say that, in every case, all the temperatures observed in the highest regions are much lower than those which Glaisher had recorded at the same level.

(3.) "The decrease of temperature with height is not quite regular, as appears from Glaisher's figures. It has been found that during very fine weather the oscillations in humidity in the different strata of the air are very great, to such an extent that in the middle altitudes the air is sometimes nearly absolutely dry.

(4.) "The temperature of the different strata has an intimate connection with the relative humidity of those strata (we should make an exception as regards the variations of temperature by day and by night in the lower strata). In consequence of this stratification, the superposition of currents in different directions is noticed, which end by reaching the upper limit of the clouds.

"In consequence it results from what has been said that the superior limit of condensation is fixed by the presence of hot and dry currents, and that the regular increase of temperature above the clouds, due in the first instance to warm currents, is increased by the reflection of the solar rays at the upper surface of the clouds.

(5.) "From this stratification of the atmosphere different systems of superposed circulations may be derived; if we take account of this we shall be better able to explain the movements of the air between cyclones and anticyclones. The explanations correcting the theory generally received hitherto will be supplied in the book which has been mentioned."

"We shall treat also of the direction and the velocity of the wind, and of the solar intensity, and the decrease in the electrical potential."

It is apparent from the results recently obtained that meteorological observations should be continued in the upper regions of the atmosphere, either by direct observations or by unmanned balloons, so as to complete the task which we have set ourselves.

With this object, a new section of the Meteorological Bureau has been created, which commenced work July 1, 1899, as soon as it received the funds requisite for its organization.

I now give in few words the organization of this section.

B. *Aeronautical Section of the Royal Meteorological Bureau.*

In order to carry on methodical observations up to the level of 4,000 metres, kites and balloon-kites will be employed. The undersigned is director of this section, M. A. Berson is his principal colleague, M. Elias is his assistant. Besides these officers three workmen are engaged for the manual service of the balloons.

At the *Fegeler Schiessplatz*, close to the place reserved for experiments in military ballooning, a large shed has been constructed for balloons—its length is 15 metres, breadth 10 metres, and height 10 metres; and also a small office. A wooden tower, 27 metres high, and a hut are also being built. In the cellar of the office there will be placed a Lilienthal steam engine of eight horse-power, with a spiral boiler, a dynamo, and a battery of 60 accumulators for light and for the electromotor working the cable for the kites. In the cellars it is proposed to arrange a small laboratory with a machine to produce cold, and the principal pieces of physical apparatus. There will also be a workshop with a lathe. On the ground floor there will be the office, rooms for the director and for his assistants, accommodation for the balloonist, and a room for each of the two assistants. On the first floor there are two rooms in reserve.

The building is situated at the foot of a small ridge, 5 metres in height, crossing the ground reserved for the observatory. To the east is the wooden tower, of which the lower half contains several rooms serving as store-rooms, instrument-room, &c. In the middle height is a kind of raised pavilion, the sides of which

are almost entirely of glass, and in this are placed the winch for the cable, with the electro-motor, which will drive it when the iron kite-wires are wound up. The wires have a diameter of from 0·7 mm. to 1·5 mm.; the thickest should resist a force of 400 kilog. The wire passes from the cylinder to a dynamometer, attached to a warning apparatus in case of need, and next through the ceiling of the pavilion to the summit of the tower, where an arrangement of tubes and pulleys allows it to turn freely with a minimum of friction. According to the direction of the wind, one can send up the kites from the top or the bottom of the tower, or from one side or the other. The object of working the pulley at the height of 27 metres has been to avoid interfering with the balloonists close by or the passers by on the rifle range. One also avoids thereby the interference of adjacent trees, whose crowns are about 25 metres high.

The tower is also very convenient for watching the descents of the kites if the line breaks.

The task of the observatory is to examine the air strata up to the height of 4,000 metres. In order to carry out this object kite-balloons will be employed, two-thirds full of hydrogen, and united to a line, as light as practicable. Such a balloon can rise with its self-recording apparatus even when the air is perfectly calm. The weight of a kite-balloon of 37 cubic metres does not exceed 17 kilog., or with its apparatus, 19 kilog. One of these kite-balloons, three-quarters full of hydrogen, and attached to a line of 0·9 mm. (which will bear 160 kilog.), can rise above 2,000 metres. Whenever there is wind a stronger line must be used, but if you add several kites you can probably attain an altitude of more than 4,000 metres. When the wind slackens one can take off the kites, and the balloon alone suffices to keep up the instruments. It is better to work in this way, at night especially, so that there shall be less chance of the apparatus falling on the ground.

In order to work the winch, which is so arranged that the bobbins carrying weaker or stronger lines can be interchanged, an electric motor of six horse-power is used, which can be set in action at the will of the operator. Seven rheostats, interposed in the primary circuit, in combination with a system of gearing, allow the speed of winding to be regulated within the limits of 0·15 and 3·6 metres per second. If the motor is not overworked the electricity of the accumulators ought to be enough to keep it in action for several hours. For harder work the boiler of the steam engine must be heated.

Besides the lamps which are used for lighting the rooms and the workshops of the observatory, two electric arc lamps will be installed at the height of the tower to illuminate the site, and an electric reflector mounted on the top of the tower will give light for working the balloons.

The hydrogen necessary for the observatory will be furnished by the corps of military balloonists, in bottles of five cubic metres in which the gas is at a pressure of 150 atmospheres. The hydrogen which is produced by the electric current is very pure, so that every cubic metre can raise 1·1 kilog. A space has been reserved for making the gas in future.

For the construction of the buildings and for first outfit 50,000 marks have been allotted. Of that sum 25,000 marks have been spent for the office, for the balloon shed, the tower, and the small glasshouse; 6,000 marks are wanted for balloons, kites, and lines, &c.; 10,000 marks for the steam engine, the motor, the accumulators, and the winch for the kites, &c.; 4,000 marks for the self-recording and other instruments; 2,000 marks are intended for a motor car, to bring back the balloons and kite-balloons which have fallen to the ground.

The budget for current expenses deals with the pay of the first assistant and his two other assistants (the salary of the director is charged in the estimate for the Central Institute), of the balloonist and his two workmen (9,350) marks. For furniture, light, and fire there are 1,000 marks. For the repair of balloons lines, and instruments, 3,000 marks. For publications, 1,000 marks. For necessary chemicals and hydrogen, &c., 5,000 marks. The total annual estimate amounts to nearly 27,000 marks.

DR. R. ASSMANN.

APPENDIX VIII.

REPORT ON RADIATION; BY M. JULES VIOLLE.

Among the phenomena which attract the attention of meteorologists, solar radiation takes the first place. It holds all under its control, the energy of meteorological action, as well of all the beings which have appeared on the face of the earth. This will suffice to indicate the interest we have in studying it. Nevertheless, although remarkable discoveries have largely augmented the field of our investigations in the last few years, and although important researches have been carried out on various subjects, no general agreement has as yet been arrived at between scientific men in different countries to establith a systematic study of this question, such as has been fortunately established for other scientific enquiries. The International Meteorological Conference which met in Paris in 1896 considered that it was time to collect together the isolated attempts which had been made to study radiation, and if possible to weld them into a common action on a definite programme. It has thought well to refer the question to the gentleman who furnished the report to the Congress of Rome in 1897, and has requested him to draw up a report on the actual condition of the question at the present date.

In the last twenty years our knowledge of solar radiation has made great progress. The expedition of Mr. S. P. Langley on Mount Whitney in 1884, and his masterly investigation of the distribution of energy in the solar spectrum up to wave-lengths entirely unsuspected, became the starting point of the admirable work on Radiation which has characterized the close of the century.

At the same time, the apparatus for observing has been improved. Efforts have been made to increase its sensitiveness and accuracy, and, at the same time, to render its manipulation simpler. It has been sought to design instruments more easily portable, and it has also been required that they should be continuously self-recording.

In fact, in addition to the regular work carried on at the observatories, mountain stations have been multiplied; and quite recently the unmanned balloons have furnished the means of reaching elevated regions eminently favourable for actinometrical experiments.

Finally, special attention has been devoted to the formulæ which serve to connect the observations *inter se*, and to deduce therefrom the part played by the atmosphere, and also the quantity of energy which we receive from the sun at every instant.

Setting aside the researches into the spectrum which belong to physics, we shall treat here alone that part of actinometry which incontestably belong to Meteorology.

History. *

Solar radiation may be considered with regard to its luminous, calorific, or chemical effect.

The luminous effects were the first to be studied. The variation in light during the different hours of a fine day first attracted notice. Bouguer† established the law of absorption exercised by a homogeneous atmosphere on a simple ray. Studying the same question, Lambert,‡ although concerned with light alone, undertook measurements with a thermometer, the readings of which in the sun were compared with those of a thermometer in the shade. The actinometer reconstructed at Montsouris about 1870 by Marié-Davy with the remains of an old apparatus of Arago's, and composed as is well known of two thermometers, a black bulb and a bright bulb, enclosed in glass vacuum jackets, measures the light more than the heat of the sun, as the glass envelope stops in great measure the dark heat of solar radiation.

It has also been endeavoured to measure the light of the sun in comparison with that of terrestrial sources. Bouguer§ found that the brilliancy of the sun (at an altitude of 31° above the horizon) equalled that of 230,000 candles. Leonhard Weber‖ estimated the intrinsic brightness of the sun (outside the atmosphere) at 720,000 candles in the red, and 1,910,000 in the green. W. H. Pickering,¶ M. Vogel,** M. Crova,†† and quite

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* For fuller details *see* my previous report.

† Bouguer, *Traité d'optique sur la gradation de la lumière*: a posthumous work, published by the Abbé de la Caille. Paris, 1760.

‡ Lambert, *Photométrie*, Paris, 1760.

§ *See* Mascart, *Traité d'Optique*, Vol. III., Paris, 1893.

‖ *Centralblatt für Elektrotechnik*, Vol. X., p. 760, 1888.

¶ *Proceedings of the American Academy*, Vol. XV., p. 236, 1880.

** *Monatsberichte d. k. Preuss. Akad. der Wissenschaften*, p. 801, 1880.

†† *Comptes Rendus*, Vol. XCIII., pp. 512 and 959, 1881.

recently M. Gaud,* have compared the light of the sun with that of a lamp (either of oil, or petroleum, or an incandescent lamp) for different simple radiations.

Photography seems eminently adapted for these researches. As long ago as 1844, Fizeau and Foucault† employed a daguerrotype plate to compare the light of the sun with that of the positive carbon of the electric arc. The effect of the voltaic light was shown to be 0·38 of that of solar light.

Since that date the chemical actions of light have been much employed in measuring the solar energy.‡ Bunsen and Roscoe§ have used the sensitive mixture of chlorine and hydrogen, which had been already employed by Draper.‖ E. Marchand¶ made use of the mixture of ferric chloride and oxalic acid, which was also suggested by Draper.** But these mixtures give rise to exothermic phenomena in which, as Berthelot†† remarks, the light does not effect the principal part of the work. "It plays the part of the match which is used to set fire to a funeral pile." On this account it becomes necessary to modify the action suitably so as to establish a proportionality between the effect and the cause. Bunsen and Roscoe had taken great pains to make the values obtained from their photometer comparable. Lemoine‡‡ has determined the conditions under which the mixture of oxalic acid and ferric chloride can furnish exact results. Duclaux§§ has studied in a general way the action of solar light on hydrocarbonated substances.

For ordinary observations Bunsen and Roscoe employed the chloride of silver paper, proposed by Sir John Herschel,‖‖ with their photographic actinometer, which was soon converted into a self-recording actinometer. Roscoe¶¶ collected a large mass of observations on the chemical action of the sun and of diffused light. Since that time photographic actinometry has been almost entirely given up, as photometric investigations have shown the difficulties of the question, which deserves, however, to be taken up afresh.

E. Becquerel*** measured the chemical action produced by the light by observing the electric current which was set up. His electrochemical actinometer consisted essentially of two plates of silver covered by a thin layer of chloride, bromide, or iodide of silver, and inserted in a small cup filled with acidulated water. These two plates, as nearly identical as possible, communicated with a galvanometer, the sensitive side of one of them being

* *Comptes Rendus*, Vol. CXXIX., p. 759, 1899.
† *Ibid.*, Vol. XVIII., pp. 746 and 860, 1844.
‡ Radau, *Les radiations chimiques du Soleil*, Paris, 1877.
§ *Pogg. Ann.*, Vol. XCVI., 1855, and Vol. C., 1857. See also *Moniteur scientifique, analyses de Radau*, 1863 *et seq.*
‖ *Silliman's Journal*, 1843.
¶ *Étude sur la force chimique contenue dans la lumière du Soleil*, Paris, 1875; and in extract *Ann. de Chim. et de Phys.*, 5th series, Vol. II., p. 160, 1874.
** *Phil. Mag.*, 1857.
†† *Ann. de Chim. et de Phys.*, 4th series, Vol. XVIII., p. 83, 1869.
‡‡ *Ibid.*, 7th series, Vol. VI., p. 524, 1895.
§§ *Ann. de l'Institut agronomique*, Vol. X., 1886, and *Ann. de l'Institut Pasteur*, 1890.
‖‖ *Phil. Trans*, 1842.
¶¶ *Phil. Mag.*, 4th series, Vol. XLVIII, p. 220, 1874.
*** *Ann. de Chim. et de Phys.*, 3rd series, Vol. IX., p. 268, 1843; and Vol. XXXII., p. 176, 1851.

turned to the radiation which was to be measured. By combining together two of these instruments M. Egoroff* has succeeded in making a differential electric actinometer of great sensitiveness, and M. Rigollot† has recently carried out a series of interesting researches with an electrochemical actinometer.

But it is principally in the form of heat that the solar energy has been measured.

Leslie‡ attempted some determinations with his differential thermometer, having one bulb blackened and one bright; but the apparatus was shown to be too much affected by currents of air. However, we ought also to note that about 1870 the Abbé Allégret mounted this thermometer on an axis so as to form a sort of balance which he called the "solar counter," and which as it became tilted, more or less, according as the sun's heat drove the liquid from the black bulb into the bright one, recorded on a revolving cylinder the difference of temperature of the two bulbs. Canon Bellani had, in 1834, constructed a collector of heat, often described as a vaporization actinometer,§ in which the distillation of a volatile liquid from the black bulb into the bright one took place, and which lends itself to a mode of registration quite similar to the foregoing. This plan has been lately (1898) adopted by M. Dosne in his investigations into the strengths of certain tinctorial colours.

De Gasparin‖ made at Versailles, at Orange, and on the Great Saint Bernard, experiments on the power of accumulation of heat in the interior of a globe of copper, blackened and exposed to the sun. I have added a gilt globe to the blackened globe, and have thus obtained a system of conjugate globes¶ which allows the march of solar radiation to be followed with great ease.

In England, an actinometer, attributed to Sir John Herschel, which had been designed by him about 1830, is generally employed. It consists of a black bulb thermometer placed in a jacket which preserves it from currents of air.

We have already seen how Marié Davy constructed an actinometer by means of two identical thermometers, one with a blackened bulb the other with a bulb bright, placed *in vacuo*.

These instruments have the advantage of requiring no orientation, nor any manipulation, but what do their indications represent? The steady excess shown by a thermometer in the sun, is only proportional to the intensity of radiation, if the loss of heat by cooling, the loss being equal to the gain, is itself proportional to the excess. Now this demands (1) that this excess shall be slight, (2) that the co-efficient of cooling shall be constant. The first condition, very frequently disregarded, is easily fulfilled. In order to realize the second, no plan seems

* *Journ. de Phys.*, 1st series, Vol. V., p. 283, 1876.
† *Ibid.*, 3rd series, Vol. VI., p. 520, 1897.
‡ *Trans. Roy. Soc. Edinb.*, Vol. VII., 1814. Compare Frankland, *Proc. Roy. Soc.*, Vol. XXXIII., p. 331, 1882.
§ Hirn (*Comptes Rendus*, Vol. XCVIII., p 324. 1884) measured the sum of the heat received by the earth in a day by means of the weight of bisulphide of carbon distilled from a cylindrical vessel with blackened walls, oriented along the axis of the earth.
‖ de Gasparin, *Comptes Rendus.*, Vol. XXXVI., p. 974, 1853.
¶ Violle, *Ann. de Chim. et de Phys.*, 5th series, Vol. XVII, 1879.

easier than to place the bulb of the thermometer in a glass envelope exhausted of air. But, on the one hand, the glass modifies the radiation by suppressing the components possessing the greatest energy, and, on the other hand, the temperature of the outside air is not given by an independent thermometer placed near by (English method), nor by a bright bulb in a similar envelope (Montsouris' actinometer). My conjugate globes indicating the variations of cooling allow me to work in the open air.

The dynamical method introduces the observation of cooling into the measurement of the radiation. Employed for the first time by Sir John Herschel,* it consists essentially in observing the heating of a thermometer by the sun during a definite interval, and its cooling in the shade during an equal interval (before and after the exposure). Unfortunately the instrument which Herschel employed, Saussure's† heliothermometer, also used by Forbes and Kämtz‡ in 1830 and 1841, and for 12 years (1842–1853) by Quételet,§ was very defective.

Much superior to all these is the apparatus employed since 1837 by Pouillet,‖ and to it are due the first approximately accurate measurements of the heat of the sun. As is well known, Pouillet's pyrheliometer consists of a thermometer, of which the bulb is enclosed in a thin, flat, metallic box filled with water. The upper surface of the box, carefully blackened, is directed perpendicularly to the rays of the sun. The heating g corresponding to five minutes' exposure to the solar action is noted, and then the coolings r and r' during the five minutes which have preceded and succeeded that exposure (a screen having been placed before the apparatus). Pouillet assumes that "the elevation of temperature produced by the heat of the sun is $g + \frac{r + r'}{2}$. If M is the mass of water in the apparatus, this elevation of temperature t corresponds to a quantity of heat Mt falling in five minutes on the surface S of the box, that is to say, to a quantity $\frac{M}{5\,S}t$ received in one minute by a square centimetre of surface." The experiments of Pouillet have been the starting point for all subsequent researches.

Before he employed the pyrheliometer, Pouillet¶ had made some use of an actinometer, consisting of a thermometer, the bulb of which was placed in the centre of a double envelope maintained at 0° C., and pierced by a hole for the admission of the solar rays. This arrangement, which he unfortunately abandoned, was taken up again by Ericsson** in America, by Padre Secchi†† at Rome, by M. Soret‡‡ at Geneva, and finally by myself§§ at Grenoble.

* *Edinburgh Journ. of Science*, Vol. III., p. 107, 1825.
† *Voyage dans les Alpes*, Vol. IV., pp. 88 and 227, Neuchatel, 1803.
‡ *Phil. Trans*, 1842, Part II., p. 225.
§ *Météorologie de la Belgique*, Brussels, 1867.
‖ *Comptes Rendus*, Vol. VI., pp. 848, 889, and Vol. VII., pp. 15 and 24, 1838.
¶ *Eléments de Physique expérimentale et de Météorologie*, 1st edition, Vol. II., p. 703. 1830.
** *Nature*, Vols. IV., V., XII., and XIII., 1862, *et seq.*
†† *Le Soleil*, 2nd edition, Vol. II., p. 233, Paris, 1877.
‡‡ *Association Française : Congrès de Bordeaux*, p. 282, 1872.
§§ *Comptes Rendus*, Vol. LXXVIII., pp. 1425, 1816, 1874.

In the same class with actinometers with envelopes of constant temperature must be placed the thermo-electric pile. Already used by Secchi* to determine the temperature of different parts of the sun, it was applied to the study of solar radiation by Desains,† Rossetti,‡ and Fröhlich.§ As discussed by Ferrel,‖ we shall find it among the most recent forms of apparatus.

I. *Apparatus.*

We shall now proceed to study the different instruments, pyrheliometers or actinometers, taking them in order of date, but without separating those of the same author.

Violle's actinometer, 1874.¶—This apparatus consists essentially, as is known, of a thermometer bulb, placed in the centre of spherical envelope, of constant temperature, which is pierced by an orifice for the admission of the solar rays.

The only modifications of any importance which have been introduced into it have been with regard to the mounting, which M. Savélief** has arranged so as to materially facilitate the installation and orientation of the apparatus.

Without wishing to reply at present to criticisms, of which the major part have fallen to the ground, I wish to establish the fact that my previous report was not sufficiently explicit in this respect; that the statical method has never been used in my actinometer, in fact, that I have always employed the dynamic method.

The method employed with my actinometer is the following :—To observe for some minutes the heating of the thermometer exposed to the incident radiation, to intercept this and observe the cooling ; and then calculate from these observations the velocity of heating V and the velocity of cooling U for each excess of temperature θ. To each value of θ corresponds one and the same value of the sum $V + U$, although separately, V and U change with θ. This constant value $V + U$ represents the action of the source of heat ; it is the heating which the isolated body would experience during the unit of time if it was withdrawn from the action of cooling. If this is multiplied by the water value, M, of the portion of the thermometer which is heated, it constitutes the numerical expression of the quantity of heat which falls on the thermometer.

This is assuredly the dynamic method, and this is the way in which I employed it on Mont Blanc in 1875. I followed the heating and the cooling until the stationary stage was reached, in order to ascertain that stage, itself an interesting observation, and

* *Le Soleil*, 2nd edition, Vol. I., p. 203.
† Desains (at first conjointly with Branly) *Comptes Rendus*, Vol. LXIX., p. 1133, 1869 ; Vol. LXXVIII., p. 1455, 1874 ; Vol. LXXX., p. 1420, 1875 ; and *Annuaire de Montsouris*, p. 195, 1888.
‡ *Memorie della Società degli Spettroscopisti italiani*, Vol. VIII., 1878.
§ *Repertorium für Meteorologie*, Vol. VI., No. 1, 1876 ; and *Annales de Chimie et de Physique*, 6th series, Vol. III., p. 500, 1884.
‖ *American Journal of Science*, Vol. XLI., p. 378, 1891.
¶ *Comptes Rendus*, Vol. LXXVIII., pp. 1425 and 1816, 1874 ; Vol. LXXIX., p. 476, 1874 ; Vol. LXXXII., pp. 662, 729, 876, 1876 ; Vol. LXXXVI., p. 818, 1878 ; and *Annales de Chimie et de Physique*, 5th series, Vol. X., p. 289, 1877 ; and Vol. XVII., p. 391, 1879.
** *Annales de Chimie et de Physique*, 6th series, Vol. XXVIII., p. 394, 1893 ; Vol. XXIX., p. 260, 1893 ; and 7th series, Vol. IV., p. 424, 1895.

to accumulate experimental data, so as to improve the exactitude of the result. Then I have calculated the sum V + U by making all the observations contribute to that result. With that object I have sought for a formula combining the observations, and from that formula I have calculated V + U. Because a formula in accord with Newton's Law happens to represent the observed numbers pretty well, it does not indicate in any way that my formula supposes that law. The experimental data might have been used in an entirely different way, either analytically or graphically. The only important point is to deduce from them the most probable value of the constant V + U in every case. I have never said that you should follow the heating or the cooling to the very end, and far less have I said that Newton's Law should be considered applicable, whatever be the thermometrical difference. The application of that law, under all conditions, cannot be too much criticized.

Only one word on the apparatus itself. I have been severely criticized for employing the thermometer itself as the calorimetric body. It is, however, a very legitimate method of simplifying the apparatus if the thermometer is good. I have employed thermometers by Baudin, with very thin envelopes, and thoroughly known in all their elements. It is therefore easy to calculate the water equivalent (M) of the thermometer bulb. It is preferable to measure it directly by a calorimetric experiment, which will do away with the corrections due to the disturbing action of the stem, corrections which, for that matter, can be easily enough calculated separately. The employment of several thermometers with different sized bulbs, which is useful on other grounds, will supply useful verifications. One can therefore measure very accurately (to some thousandth parts) the water equivalent of the calorimetric body. How otherwise with other arrangements could the temperature of that body be measured more accurately? As to the isolated surface S, which it is desirable to take as smaller than a great circle of the calorimetric bulb, its measurement presents no difficulty. You can deduce from it immediately the value of the co-efficient M/S.

My actinometer was employed on the expedition to Mount Whitney in 1884, and Mr. Langley* has published a most exhaustive criticism on the subject, worked out by M. Chvolson.†

It has been used by M. Savélief, with certain improvements, as I have said above.

It was employed by M. Joseph Vallot‡ in a series of very interesting simultaneous observations which he has made with M. Henri Vallot on the summit of Mont Blanc (4,810 m.), and at Chamonix (1,040 m.) on the 28th, 29th, 30th, and 31st of July, 1887. These observations, although made in very fine weather, have given for the radiation values much lower than those which I had obtained with similar instruments at the same localities twelve years before. In fact, whereas I had got 2·39 *calories* on Mont

* *Researches on Solar Heat and its absorption by the Earth's atmosphere* (Report of the Mount Whitney Expedition), Washington, 1884.

† Chvolson. *Repertorium für Meteorologie*, Vol. XV., No. 1, St. Petersburg, 1892.

‡ *Annales de l'Observatoire Météorologique du Mont Blanc*, Vol. II., p. 77, 1896.

Blanc and 2·02 *calories* at Les Bossons, the maxima observed by M. Vallot have been 1·56 *calorie* on Mont Blanc and 1·33 *calorie* at Chamonix, and the relative deficiency of these new results cannot, as it seems to me, be due to any instrumental error. In fact, M. Vallot took the trouble to compare one of the instruments which he had used, and of which I had supplied the thermometric constant, with a Crova's actinometer, standardized by the inventor, and he found an agreement between the two instruments, which gave him great confidence in the figures he had got on Mont Blanc. From these figures there will come out a solar constant approximately 2·0 *calories* per minute.

M. Rizzo* has also used my actinometer in his simultaneous observations at four stations on the Rocciamelone in the Val Suza:—

	M.
Mompantero (Suza)	501
Trucco	1,722
Casa d'Asti	2,834
Summit of Rocciamelone ...	3,537

For these observations he had introduced into my actinometer some modifications, the principal of which was taking as the calorimetric body a hollow sphere of solid silver, pierced with a small hole, into which was introduced the cylindrical bulb of a very delicate thermometer; around that bulb, which was thickly silvered, he had pressed a quantity of finely pulverized silver, so as to secure a perfect continuity in all the calorimetric mass. He also took care to place a diaphragm in the orifice of admission so as to prevent any rays impinging on the sphere but those which were sensibly normal to the absorbing surface. I do not mention the particulars of installation and orientation, which varied according to circumstances.

If one treats the results for each station by a formula of Crova's, one can calculate for each of these stations the value of solar radiations Q_1 referred to the zenith. The subjoined table gives, besides the observed pressures H, and the values of Q_1 thus deduced from observations, the values derived from the formula

$$Q_1 = 1{\cdot}3084 + 0{\cdot}0528 \quad (760 - H) \tfrac{1}{2}.$$

——	H.	Q_1 Observed.	Q_1 Calculated.
	mm.	cal.	cal.
Mompantero	722	1·61	1·63
Trucco	622	1·98	1·93
Casa d'Asti	544	2·09	2·08
Summit of Rocciamelone	499	2·13	2·16

M. Rizzo concludes from this that the solar constant is about 2·5, but remarks that certain radiations have doubtless escaped measurement.

* *Memorie della Società degli Spettroscopisti italiani*, Vol. XXVI., 1897, and *Accademia reale delle Scienze di Torino*, 1897–8.

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Violle's conjugate bulbs (1879)*.—Two hollow bulbs of copper, one blackened, the other gilt, each enveloping the bulb of a very sensitive thermometer, furnish an instrument which can be easily made self-recording on Richard's method. That with some precautions useful indications may be obtained from it is shown by the experiments made at the Meteorological Observatory of the Petrovsky Academy, near Moscow.†

Violle's actinometer for unmanned balloons‡ (1899).—From 1892, I had arranged an actinometer which Captain (now Colonel) Renard§ proposed to use with one of the unmanned balloons, which was the first invented. Various unavoidable circumstances stood in the way of the realization of the experiment. In the actual model I employ a thermometer which has been very happily devised by Richard, and which is formed of a spiral of very thin copper, filled with *toluene*, fixed at one end, and at the other acting on a long vertical rod of aluminium which is attached to a Richard's registering apparatus, also of aluminium. This thermometer is placed in a sphere of copper, carefully blackened, and exposed to the rays of the sun. The whole forms an apparatus which is at the same time light and strong, and which will bear very well the cold of the upper regions. We may, moreover, remark that the thermometric sphere, which is isolated, and on the one hand radiates in a rarefied and (relatively) motionless atmosphere at a well defined temperature, and on the other hand receives on the several parts of its surface successively the solar rays is, excellently circumstanced for actinometrical observations. A cylindrical screen with double walls is raised at definite intervals so as to put the sphere in the shade, and to allow the cooling to act freely; it is then lowered, the rays of the sun strike the sphere again, and you find on your record all the data of the problem.

For the rest, experience will show the improvements which it is desirable to introduce into the apparatus. I have already studied a thermoelectric system of which the wires descend far below the balloon, and carry the actinometric arrangement. I have also tested a photographic actinometer, measuring the chemical effect of radiation.

In its actual condition this actinometer, at the height of 12,000 metres, shows + 10° at an air temperature of − 60° C., and, what is a remarkable fact, it shows only a very slight variation of radiation with the thicknesses of atmosphere passed through by the rays.

Crova's actinometer (1875).‖—I shall not reproduce here the description of this well-known instrument, which is especially

* *Annales de Chimie et de Physique*, 5th series, Vol. XVII., p. 391, 1879.

† Colley, Michkine, and Kazine, *Annales de Chimie et de Physique*, 6th series, Vol. XXVI, p. 265, 1892.

‡ Violle. *Comptes Rendus*, Vol. CXXV., p. 627, 1897; and Vol. CXXVI., p. 1848, 1898.

§ *Revue de l'Aéronautique*, Vol. VI., p. 1, 1893.

‖ *Comptes Rendus*, Vol. LXXXI., p. 1205, 1875; Vol. LXXXII., pp. 81 and 375, 1876; Vol. LXXXIII., p. 269, 1876; Vol. LXXXIV., p. 495, 1877; Vol. LXXXVII., p. 106, 1878; Vol. XCIV., p. 943, 1882; and *Annales de Chimie et de Physique*, 5th series, Vol. XI., p. 433, 1877; Vol. XIX., p. 167, 1880; and 7th series, Vol. XVII., p. 22, 1899.

valuable owing to the ease with which you observe with it. I shall only say that it is formed of a large alcohol thermometer with an index of mercury. The bulb is carefully blackened, and receives the solar rays by a narrow aperture pierced through the walls of a thick protecting envelope of brass.

In his first researches M. Crova employed thermometric bulbs, 40mm. in diameter, and orifices of admission of 30mm. Now he takes bulbs of 20mm. diameter, and along the path of the solar rays, in order to destroy the effect of the wind, he arranges, following Langley, a series of diaphragms of aluminium, polished in front and blackened on the back; the last diaphragm has less than 1cm. aperture, so that the cylindrical beam which it allows to fall on the bulb has a direction scarcely varying from the normal. This bulb has been covered by electro-deposition of rough copper, coated with platinum black. With the flame of a taper a thin sheet of black is deposited upon this, and is washed in alcohol. If the smoking and the washing is repeated several times a surface is obtained, which is quite homogeneous, and of which the co-efficient of absorption is 0·98, according to a special investigation by M. Crova. The tube of the thermometer should have two bulbs, one above and one below, so as to receive the mercury forming the index.

Before an observation the instrument is oriented by means of screens which keep the envelope in the shade. The thermometric bulb being itself in the shade, you observe the movement of the index from minute to minute, or, better, from half-minute to half-minute. If this movement is regular for at least two minutes, you proceed with the series of observations as follows:—

(1.) Observe in the shade for one minute, and remove the screen.

(2.) Observe in the sun for two minutes, and replace the screen.

(3.) Observe in the shade for two minutes.

The uniform march during the second minute in the sun, augmented by half the sum of the coolings in one minute, before and after the exposure to the sun, gives the corrected range.

This corrected range, multiplied by the co-efficient of reduction,* represents the calorific intensity.

It was thus found in one experiment† that:—

(1.) The initial heating in the shade was uniform, and amounted to four divisions per minute

(2.) After the first minute of insolation, which corresponds to the variable stage, the heating in the sun during the following minute was uniform, and equal to 21·2 div., and then it became slower.

(3.) The final cooling was *nil*.

* To standardize the instrument, Crova's mercury pyrheliometer may be used, or, preferably, his absolute actinometer, which requires only very short times of exposure. They can even be exactly synchronous with those of the alcohol actinometer, so that the co-efficient of reduction will be independent of the fluctuations of the solar radiation, however rapid they may be.

† The readings of the experiment are given in an *addendum* on p. 101.

The corrected range is therefore $21{\cdot}2^{\text{div.}} - \frac{4{\cdot}0}{2} = 19{\cdot}2^{\text{div.}}$

M. Crova is of opinion that the alcohol actinometer, with which it is possible to make a great unmber of exact observations in a very short space of time, allows you, better than any other apparatus, to follow the fluctuation of solar radiation, and to draw easily hourly curves in calories, with an approximate accuracy of 1 or even of 0·5 per cent., if you make use of all the corrections. This approximation is closer than you can obtain by other methods.

M. Crova's apparatus has been studied in detail by M. Chvolson and by M. Savélief.

It has been employed by M. Savélief* in his observations at Kieff for several years.

It has been employed by M. Joseph Vallot† in the series of simultaneous actinometrical measurements which he carried out on Mont Blanc and at Chamonix with Madame Gabrielle Vallot on September 18 and 19, 1891, and which have led him to results entirely in accord with those which he had obtained in 1887 by means of my actinometer.

Finally, besides the continual use which M. Crova has made of his apparatus at Montpellier and at Palavaz, and also besides the determinations which he has made, or caused to be made, at several stations, and particularly at l'Aigoual, we ought specially to mention the measurements he made, in conjunction with M. Hanski, on the summit of Mont Blanc (4,810 m.), on the Brévent (2,525 m.), and at Chamonix (1,040 m.) during the summer of 1897.‡ The series obtained on the summit of Mont Blanc, September 30, 1897, and at Brévent, August 26, 1897, are particularly regular. They are exactly represented by the formulæ :—

Summit of Mont Blanc ... $Q = \frac{3{\cdot}241}{(1+\epsilon)^{0{\cdot}6641}}$.

Summit of Brévent ... $Q = \frac{3{\cdot}032}{(1+\epsilon)^{0{\cdot}742}}$.

They lead to a solar constant greater than 3.

Crova's Registering Actinometer or Actinograph (1888).§

In order to register solar radiation in a continuous manner, M. Crova receives it on one of the junctions of a thermo-electric element, of which the other is kept in the shade, and registers the

* *Annales de Chimie et de Physique*, 6th series, Vol. XVIII., pp. 458, 462, 1889 ; Vol. XXV., p. 567, 1892 ; Vol. XXVI., p. 289, 1892.

† *Annales de l'Observatoire Météorologique du Mont Blanc*, Vol. II., p. 115, 1896.

‡ On his scientific expeditions M. Crova made use of a direct-sighting actinometer, by which the adjustment of the direction of the instrument was greatly facilitated.

§ *Comptes Rendus*, Vol. CI., p. 418, 1885 ; Vol. CII., pp. 511, 962, 1886 ; Vol. CIV., pp. 32, 1231, 1887 ; Vol. CVIII., p. 35 (with Houdaille), pp. 119, 482, 1889 ; Vol. CXXV., pp. 804, 917 (with Hanski), 1897 ; and *Annales de Chimie et de Physique*, 6th series, Vol. XIV., pp. 121, 541, 1888 ; Vol. XXI., p. 188 (with Houdaille), 1890.

produced current photographically. Each junction has the form of a disc, composed of two superposed plates, iron-german silver, 10 mm. in diameter and 0·1 mm. in thickness, each of them prolonged by a narrow strip, which serves to establish communications between the junctions on the one hand and the galvanometer on the other. The two discs are mounted in a tube which is carried along by an equatorial movement, so as to be constantly directed to the sun. One of the discs receives on its blackened face (german silver) the solar rays, which reach it through a series of parallel diaphragms of aluminium. The other disc, placed behind the first, is separated from it by a double screen of aluminium, which keeps it in the shade without interfering with the circulation of the air in the lower part of the tube, which is closed on that side by a double nickel-plated screen. In front there is a similar screen, pierced by the orifice of admission. The galvanometer is an instrument fitted astatically, of small resistance, conveniently protected by a double screen of sheet iron. The deviation of the astatic system is measured by Poggendorff's method, and the movable luminous spot is photographed on the paper by a Mascart's registering arrangement. It traces thereon a curve, of which the co-ordinate is at each instant proportional to the thermometric excess of the exposed junction, which is always very small. This excess will be itself proportional to the radiation if no disturbing cause acts on either junction.

The apparatus is very sensitive, and gives traces with numerous sinuosities. M. Crova interprets these traces on the supposition "that it is the maxima of radiation which are most important, and that instead of trying to trace out a mean curve, one will come nearer the truth if one traces the curve which passes through the maxima of radiation." From that curve the absolute value of radiation is deduced by a preliminary estimation by the use of an absolute actinometer.

M. Crova has constantly used his actinograph for fifteen years, and has deduced from the observations the laws of diurnal and annual variation of solar radiation at Montpellier. According to the researches of M. Colley the radiation at Moscow follows the same laws. A generalisation of these researches would be desirable. Certain days like September 9, 1886, give regular curves, which appear specially suited for calculation. M. Crova has employed them in the determination of the solar constant. The values so obtained in 1886 and 1887 vary from 1·8 to 2·7, "and in the most favourable circumstances approach the number 3 which Langley's experiments appear to indicate."

M. Savélief has also obtained very regular curves at Kieff on November 10, 1891, and February 25, 1892, and has deduced from them a value of the solar constant equal to 3·6.

Crova's absolute actinometer (1898).*—M. Crova has recently constructed an absolute actinometer, which combines in itself several advantageous arrangements. The calorimetric mass is a broad thin disc (diameter 4 cm., thickness 0·5 cm.), made of

* *Comptes Rendus*, Vol. CXXVI., p. 394, 1898.

copper, suspended from three fine threads at the bottom of a tube, composed of two concentric tubes with water between them, and fitted with successive diaphragms. This disc receives the rays of the sun on its blackened face (the other is polished) and is heated; and if we take a cylinder sufficiently far from the edge, having 1 cm². base, and for height the thickness of the disc, the fundamental equation actually gives us

$$c\, d\theta = aq\, dt - m\, \theta\, dt,$$

θ being the excess of the temperature of the disc above the temperature of the environment at the time t, c the mass of the water equivalent of the cylinder, a the absorbing power of lampblack, q the quantity of heat falling during the unit of time on 1 cm², m the sum of the coefficients of loss by radiation and conductibility for the same cylinder. From this, integrating and putting $\frac{m}{c} = \alpha$,

$$\theta = \theta_0\, e^{-\alpha t} + \frac{aq}{\alpha c}\left(1 - e^{-\alpha t}\right).$$

The coefficient α is determined by observing in the shade; then $q = o$ and $\theta = \theta_0\, e^{-\alpha t}$.

If the thermometric excess is slight, the exponentials are reduced to the first term of their developement, and you have for the cooling and heating a uniform march, which brings the calculation to the very simple form which Pouillet had given it.

The constant c is easily determined.

The installation and orientation of the actinometer present no difficulties, the shadow of the disc is caught on an unpolished disc at the base of the tube.

The delicate point is the measurement of the excess θ. In order to obtain it you employ a thermo-electric couple of Iron-*constantan*. One of the junctions is in the centre of the disc, and and a fine wire of *constantan* is fixed at the back of the disc. This wire is attached at the other end to a copper wire, with which it forms the second junction, immersed in the water of the double envelope at the height of the disc. One of Deprez-d'Arsonval's galvanometers is intercalated in the circuit.

Ångström's differential pyrheliometer (1885).*—In 1886, M. Knut Ångström published in the *Transactions of the Upsala Society* the theory and description of his differential pyrheliometer.

The apparatus is composed of two identical discs of copper, each carrying a thermo-electric junction, and exposed alternately to the action of the sun. A galvanometer placed in the circuit of the two junctures measures their difference of temperature. You note the time that that difference takes to pass from $+\delta$ to $-\delta$, and then, after inversion, from $-\delta$ to $+\delta$, and so on. If the intensity of radiation has not changed during the experiment, the times corresponding to these successive variations have the same value t. Let C be the water equivalent of one of the discs,

* *Actes de la Société royale des Sciences d'Upsal*, 1886.

S the surface exposed to the sun, and a its coefficient of absorption; you see easily that the intensity of radiation is given by the formula

$$Q = \frac{2\, C\, \delta}{a\, S\, t}.$$

Each calorimetric disc has a diameter of about 3 cm., a thickness of 0·5 cm., and weighs 32·334 grammes. The surface on which the rays fall is carefully blackened (M. Ångström estimates the coefficient of absorption of that surface at 0·98), and all the rest of the exterior surface is platinized and polished. At the centre of the disc is screwed on a thermo-electric juncture of copper-german silver; the german silver forms a rod going directly from one disc to the other, the copper forms a wire circuit in the course of which is included a dead-beat galvanometer. The deflexion is observed by means of a telescope and divided scale. The discs being placed normally to the rays of the sun, you displace a screen so as to uncover alternately each of them whenever the galvanometric deviation reaches 200 divisions on one side or the other of zero, and you note the times of passage of the index across the divisions 150, 100, − 50, − 100, − 150, and then −150, −100, −50, 50, 100, 150, and so on. If you combine the times corresponding to +150 and −150 (the time differences should then be divided by 3), to +100 and −100 (these differences should be divided by 2), to +50 and −50, you obtain three values of t corresponding to the passage from + 50 to − 50. Take for example the observations of July 10, 1885:—

———	Deviations.	Times of Passage. Deviations, Positive.	Times of Passage. Deviations, Negative.	Intervals of time for $\delta = 50$.
		h. m. s.	m. s.	seconds.
1st Series	150	1 33 9	35 11	40·7
	100	0 33 27	34 48	40·5
	50	0 33 44	34 24	40·0
2nd Series	150	0 37 28	39 29	40·3
	100	0 37 46	39 6	40·0
	50	0 38 4	38 45	40·0
			Mean ...	40·7 = 0·673 min.

A previous calibration had shown that a galvanometer deviation of one division corresponded to a difference of temperature between the two junctions equal to 0·0195°; we have thus:

$$\delta = 50 \times 0{\cdot}0195° = 0{\cdot}975°.$$

The other constants of the instrument were

$$C = 3{\cdot}039 \text{ grm.}$$
$$S = 7{\cdot}162 \text{ cm}^2.$$

We have then

$$Q = \frac{2 \times 3{\cdot}039 \times 0{\cdot}975}{0{\cdot}98 \times 7{\cdot}162 \times 0{\cdot}673} = 1{\cdot}25$$

This is, as will be seen, a form of the dynamical method which may be termed *the method of equal differences*.

M. Chvolson, who has studied Ångström's pyrheliometer with especial care, prefers to operate on a different method which he terms *the method of equal times*. At epochs which are equidistant and close together, 0, t, $2t$, he observes the numerical values δ_1, δ_2, δ_3, the differences of temperature of the two bodies. He then arranges them so that δ_2 shall be sensibly 0 and δ_1 and δ_3 have, in consequence, contrary signs. We have then, with an approximation which is quite sufficient,

$$Q = \frac{2C}{St.} \quad \frac{\delta_1 \delta_3 + \delta_2^2}{\delta_1 + \delta_3}$$

If we take, for example, the observation made at Pavlovsk, August 12 (24), 1892, at 3h. 22m. p.m.

$$t = 1 \text{ min.}$$

$$\delta_1 = 2{\cdot}61^\circ,\ \delta_2 = +\,0{\cdot}16^\circ,\ \delta_3 = 1{\cdot}79^\circ$$

The + sign of δ_2 shows that in the course of the first minute the difference of temperature has been reduced from 2·61 to 0·16, at the end of the second minute the body which was previously the colder has become the warmer to the extent of 1·79°. We find

$$Q = 2{\cdot}131\ \frac{C}{S}$$

with an approximation of 0·5 per cent., all instrumental errors being disregarded.

M. Chvolson has reduced the losses by radiation and conductibility (1) by gilding the discs over all their external surface except the blackened part, and (2) by replacing by a fine line the german silver rod soldered to the two calorimetric masses, and by supporting each of them by four threads to a fixed external ring.

M. Crova has, as we have seen, adopted this mode of suspension, but he employs only one disc, and places it in an isothermal space, returning to the only trustworthy mode of proceedure, whatever may be said of it.

Ångström's registering pyrheliometer (1885)*.—On the same principle as his pyrheliometer, M. Ångström has constructed a very ingenious self-registering apparatus, in which the discs are replaced by hollow bulbs united by a glass tube so as to form a differential thermometer. In the middle of the tube there is an index of mercury, which a platinum wire soldered into the middle of the tube connects constantly to one of the poles of a battery. Two other platinum wires soldered into the glass tube, at small and equal distances from the two extremities of the index, communicate with the other pole. When one of the bulbs is

* *Actes de la Société royale des Sciences d'Upsal*, 1886; and *Bihang till K. Vet.-Akad. handlingar*, Vol. XV., No. 10, 1889; or *Wied. Annalen*, Vol. XXXIX., p. 294, 1890.

warmed by the action of the sun the index is pushed towards the side of shade. It touches the thread on that side; the electrical circuit is closed, and under the action of a mechanism set in action by a relay the differential thermometer revolves through half a turn; the action of the bulbs is reversed. At the same time a pencil descends a very small distance on a drum turning with a uniform movement. It will descend again the same amount when the bulb actually exposed to the sun exhibits the same excess of temperature as the other bulb showed just before. Thus a continuous curve is traced by small steps, the angular coefficient of which at each point will be proportionate to the radiation at the corresponding epoch. The coefficient of proportionality is determined by comparison with the normal pyrheliometer.

M. Angström has obtained with this registering apparatus on the Island of Yxeloe, during the summer of 1888, very important results, to which we shall hereafter have to return.

Ångström-Chvolson's actinometer (1892).*—The differential thermometer has been simplified by the inventor himself, and subsequently by M. Chvolson, for ordinary observations. M. Ångström had reduced it to the classical form of the differential thermometer, as in his registering apparatus. M. Chvolson lets the two bulbs be independent of each other. Each of them is composed of a box of copper, 3·5 cm. in diameter and 0·7 cm. in thickness, containing the reservoir of a mercurial thermometer. This reservoir, in the form of a spiral flattened, in a plane perpendicular to the tube, was silvered chemically and then coppered galvanically, and is plunged in copper very finely pulverised. The tubes of the two thermometers are bent twice so as to place them normal to the plane of the covers of the two boxes which have been carefully blackened. Between the two tubes a carriage is moved by a micrometric screw, and to this are fixed (1) an index sliding underneath the tubes, (2) a fine thread sliding above them, (3) a large lens over which you place the eye, so as in every case to superpose the thread on the trace so as to avoid any error of parallax. The readings are thus rendered easy and accurate.

M. Rizzo† has adopted an analogous arrangement. He preserves M. Angström's metallic discs, and makes a hole in them on the side, in which he inserts the bulb of a calorimetric thermometer, which is thickly silvered, securing its perfect contact with the mass of the disc by means of finely-powered silver. The tubes of the two thermometers lie parallel to each other, in opposite directions, in a plane parallel to the absorbing faces; and again the reading of the difference of temperatures can be easily made.

Ångström's compensating pyrheliometer (1893).‡—M. Ångström has communicated to the Royal Society of Upsala the description of a compensating pyrheliometer, founded on a

* *Repertorium für Meteorologie*, Vol. XVI., No. 5, St. Petersburg, 1893.
† *Memorie della Società degli Spettroscopisti italiani*. Vol. XXVI., 1897.
‡ *Actes de la Société royale des Sciences d'Upsal*, 1893; and *Annalen der Physik und Chemie*, Neue Folge, Vol. LXVII., p. 633, 1899.

principle entirely different from that he had employed in his first instrument. He takes as his calorimetric body two thin strips of metal, identical in every way, and then exposes one to the rays of the sun, while into the other, kept in the shade, he passes an electric current, the intensity of which is regulated so that the heating of the two bands is the same. He tests this agreement by the fact that the actions of the two identical thermo-electric elements in contact with the two strips compensate each other on a sensitive galvanometer. The energy received by the incident radiation in the unit of time is therefore equal to that which is received in the same time by the electric current.

Let q be the radiation per second per square centimetre, b the width of the strips, a the absorbing power of their blackened surface, r their resistance per unit of length, and i the intensity of the compensating current.

We have

$$b\,a\,q = \frac{r\,i^2}{4{\cdot}18}$$

From this

$$q = \frac{r\,i^2}{4{\cdot}18\,b\,a}\left(\frac{\text{calorie}}{\text{second}}\right)$$

or

$$Q = \frac{60}{4{\cdot}18}\,\frac{r\,i^2}{b\,a}\left(\frac{\text{calorie}}{\text{minute}}\right).$$

This method does away with all corrections for cooling. We determine b, a, and r (as well as the variation of r with temperature).

In order to make an absolute measure we have only to observe i.

The metal strips are of platinum, with a thickness of 1 mm. or 2 mm., and their breadth is 2 mm. On their posterior base they have a junction of *constantan* (or nickel)—copper. Their anterior face is blackened. They are mounted, side by side, at the bottom of a tube containing several diaphragms. In front of the tube there is a movable perforated screen which allows the radiation which is to be measured to fall alternately on one strip or the other. The bottom of the tube carries four terminals, two for the circuit of the thermometric junctions with which is connected a very sensitive galvanometer, and the two others for the circuit of the compensating current, which comprises a Daniell's or Leclanché's element (or, better still, an accumulator), a sliding contact resistance box, and a milliampèremeter (or, better, a special electrodynamometer). A commutator permits the putting one or other of the strips in the circuit.

The strips are cut off on the dividing engine and their breadth b is known in consequence. Their resistance r per unit of length is measured by means of a Lippmann's capillary electrometer, by means of which the differences of potential (1) between two knife edges resting at a known distance apart on the strip which is

being tested, and (2) between two contacts, one fixed, the other movable, on a standard wire, both strip and wire being included in on the same circuit are adjusted to equality. In order to take account of the variation of r with temperature, M. Ångström determines separately the effect of the initial temperature of the wire (the temperature t of the surrounding space) and that of the heating due to the passage of the current of intensity i. He thus knows the resistance $r_{t,i}$ in the conditions of the experiment. Direct observations have shown that the absorbing power a of the surfaces blackened by Crova's methods may be taken as equal to 0·985.

The course of an experiment is as follows:—The apparatus being adjusted for direction, you first ascertain that the radiation falling simultaneously on the two strips does not alter the position of equilibrium, at zero, of the galvanometer connected with the thermometric junctions. You then arrange the screen so that only one of the strips is exposed to the sun, and, at the same time, you direct into the other the compensating current, the intensity of which is regulated so as to bring the galvanometer to zero. You read the intensity of the current by the milliampèremeter (or by the special electrodynamometer). Then you change at once the position of the screen and of the commutator so as to reverse the action of the two strips, and make another measurement. You then revert to the original arrangement and take a third reading. Here is an example of an observation made by M. Ångström on the top of the Peak of Teneriffe (3,700 m.) on June 25, 1896, at noon. The right-hand strip D_1 having been first exposed, and then the left-hand one G, and finally the right-hand one again D_2.

	i^2
D_1	0·0704
G	0·0713
D_2	0·0702

$$\text{Mean } \frac{D_1 + D_2 + 2G}{4} = 0{\cdot}0708.$$

The temperature of the tube enclosing the whole being 21°, $r_{t,i} = 0{\cdot}3614$ and we have $Q = 1{\cdot}626$.

A very interesting double series was obtained a few days later, on July 3, by M. Ångström at Guimar (300 m), and by his assistant, M. Edelstam, at Alta Vista (3,252 m.), between 5h. 30m. a.m. and 5h. 13m. p.m. The series of values obtained at Alta Vista were remarkably regular. The readings at noon were:—

Guimar	Q = 1·384
Alta Vista	Q = 1·618

We should add that M. Ångström has taken care to compare his compensating pyrheliometer with his differential pyrheliometer. "The very satisfactory agreement in the values obtained from the two instruments, which are so different in principle and in their manipulation, speaks, as it seems to him, in favour of their accuracy. It should be added that with the new apparatus the constants are more easily determined, the sensitiveness can be more easily increased, and an observation is more rapidly effected."

M. Chvolson's apparatus (1893).*—In our study of the apparatus of M. Chvolson, as derived from M. Angström's, we have followed M. Chvolson himself. We ought, however, to point out the modifications which he has introduced, particularly into the actinometer of which he has changed the mode of use and transformed the construction, have made them his own instruments. After a careful examination, Chvolson's actinometer has been adopted, under a slightly modified form, by the Meteorological Service of Russia, and has been employed for several years in a regular manner. If we set aside the spectrobolometer, the micro-radiometer, and, generally, the apparatus intended to deal with the simple radiations of the spectrum, apparatus which require a special installation, and confine ourselves to the instruments by means of which we propose to register the total energy of radiation, we see that these have been multiplied within the last twenty years. Although the bulbs may be always employed as calorimetric bodies, it is the discs which offer over all their base a surface normal to the solar rays, if properly directed, which seem to be specially preferred; on the other hand, the mercurial thermometer tends to be replaced by the thermo-electric element, which is more sensitive. Finally, we commence to revert to the use of the isothermal enclosures, which has always seemed to us to be peculiarly advantageous.

II. *Formulæ.*

When we have obtained a series of observations relating to solar radiation under different conditions, it is necessary to connect all these observations by a formula from which to deduce the intensity of the radiation emitted by the sun and the absorption which that radiation undergoes in passing through our atmosphere.

According to Bouguer's fundamental law, the intensity of a simple ray transmitted through a homogeneous atmosphere is

$$I = A p^{\epsilon}$$

p being the coefficient of transparency of that atmosphere and ϵ the mass traversed.

If we call z the zenith distance of the sun, and $h = 90° - z$, its height above the horizon, we know that z differs very little from sec.z (= cosec.h) as long as h is greater than 30°. For smaller values of h you must refer to a more complex formula given by Bouguer. The calculation has been recently taken up by M. Angot,† by M. Maurer,‡ and finally by M. Chistoni.§ I shall

* Chvolson, *loc. cit.*

† *Annales du Bureau central Météorologique*, 1st part, p. 121, 1883.

‡ *Archives des Sciences Physiques*, 3rd period, Vol. IX., p. 374, 1883.

§ *Atti della Società dei naturalisti di Modena*, 3rd series, Vol. XVI., p. 165, 1899.

only reproduce the simple form given by M. Angot to Bouguer's formula :—

$$\epsilon = \frac{1}{\sin. h}\left\{1 - \frac{1}{2a}\cot.^2 h\left(\frac{4}{3} - \frac{1}{a\sin.^2 h}\right)\right\}$$

in which a represents the terrestrial ray.

The formula $I = A p^{\epsilon}$, established for a simple ray and a homogeneous atmosphere, cannot be extended to solar radiation. which is complex, and to our atmosphere, which is not at all homogeneous.

Meanwhile Pouillet and many physicists after him have been able, by means of a formula of that nature, have been able to connect together the different values of radiation at the successive hours of a day. But from the fact that a formula represents well enough the march of the phenomenon for the values of ϵ comprised between 3 or 4 and 1·1 or 1·0, has one the right to extend it to the case of $\epsilon = 0$ and from that to deduce the energy of radiation at the limit of our atmosphere? This is what Pouillet did. He found for what he termed the *solar constant*, A = 1·76 *calorie* per minute. By the same formula, he deduced for the coefficient of transparency of the atmosphere, or the *atmospheric constant* p, values variable from one day to another and comprised between the values of ·72 and ·75.

If we consider it simply as an empirical formula, Pouillet's formula is unsatisfactory. Bartoli and Stracciati,* who have made a great number of determinations of intensity, by means of Pouillet's pyrheliometer and Violle's actinometer, in various parts of Italy and Sicily, have recognized that as ϵ increases p increases, and consequently A decreases.

M. Crova† had already proved the increase of p with ϵ, and in order to take account of it, he had been brought to put

$$Q = \frac{A}{(1+\epsilon)^{m}}$$

This formula represents in general, in a very satisfactory manner, the hourly march of radiation.

Bartoli has actually obtained an excellent series of hourly values simply by the formula

$$Q = \frac{A}{\epsilon^{m}}.$$

As an example we shall reproduce, following M. Rizzo,‡ the series obtained September 20, 1894, by Bartoli on the top of the Stelvio pass (2,850 m.), and which is reproduced throughout its extent by the formula

$$Q = \frac{1{\cdot}676}{\epsilon^{0{\cdot}274}}.$$

* *Bullettino mensile dell'Accademia Giœnia in Catania*, 1889, 1891, and 1892. and *Nuovo Cimento*, 1892 and 1895.

† Loc. cit. For the calculation of the solar constant and the coefficient of transparency of the atmospheres, *see* especially, *Annales de Chimie et de Physique*, 6th series, Vol. XIV., p. 541.

‡ *Memorie della Accademia reale delle Scienze di Torino*, 2nd series, Vol. XLVIII., 1897–1898.

ε.	Q. Observed.	Q. Calculated.	Obs. – Calc.
6	1·026	1·026	0
5·5	1·050	1·051	– 0·001
5	1·080	1·079	0·001
4·5	1·122	1·111	0·011
4	1·146	1·147	– 0·001
3·5	1·188	1·189	– 0·001
3	1·242	1·241	0·001
2·8	1·254	1·265	– 0·011
2·6	1·284	1·290	– 0·006
2·4	1·308	1·319	– 0·011
2·2	1·350	1·351	– 0·001
2	1·386	1·387	– 0·001
1·8	1·428	1·427	0·001
1·6	1·482	1·474	0·008
1·4	1·536	1·529	0·007

Obviously one would not think of extending the formula in the case of $\epsilon = 0$.

As to Crova's formula, which suits almost equally well the Stelvio observations by the expression

$$Q = \frac{2{\cdot}079}{(1+\epsilon)^{0{\cdot}3675}}, \text{ from which comes out } A = 2{\cdot}079.$$

M. Rizzo has shown how the values given by its application to several series of observations of indisputable accuracy differ *inter se*, such as those of Forbes and Kämtz at Brienz(724m.) and at the Faulhorn (2680m.) [A = 2·598 and 3·253], and of Langley at his three stations on Mount Whitney (1460m., 3543m. and 4426m.) [A = 2·036, 2·474, and 2·271], of M. Rizzo at his four stations at Rochemelon (501m., 1722m., 2834m., and 35237m.) [A = 4·039, 6·204, 4·557, and 3·697]. We do not speak of M. Crova's own observations.

Forbes,* in order to represent the simultaneous observations made by himself at Brienz (724m.), and by Kämtz at the Faulhorn (2680m.), September 25, 1832, had taken a formula of two terms:

$$Q = A + B\,p^{H\epsilon}$$

thus dividing the radiation into two parts, one which traversed the atmosphere, or at least the portion of the atmosphere considered, without sensible absorption, and the other which was absorbed according to Bouguer's Law, and for that he took account of the influence of the atmospheric pressure, H, on the mass which was traversed.

If we apply this to the case under consideration, this formula becomes—

$$Q = 0{\cdot}587 + 2{\cdot}333\,(0{\cdot}412)^{\epsilon},$$

and from this the solar constant is

$$A + B = 2{\cdot}82.$$

* *Phil. Trans.*, Part II., p. 225, 1842.

M. Radau* has found that in several cases you may take

$$Q = A_1 + A_2 \left(\frac{2}{3}\right)^{\epsilon}.$$

In dealing with numerous actinometrical determinations at Geneva (407m.), and at the Col. St. Théodule (3320m.), and on the highest peaks of the Alps, Mont Blanc (4810m.), the Faulhorn (2680m.), the Dent des Morcles (2980m.), the Görnergrat (3040m.), the Breithorn (4170m.), Soret† proposed to take into consideration the aqueous vapour, which is always more abundant in the lower strata of the atmosphere, writing simply

$$Q = A\,p^{H^2\epsilon}$$

For my part,§ in order to separate the action of aqueous vapour, I introduced into Bouguer's formula a term to represent the absorption by vapour, and I put

$$Q = A\,p^{\frac{H + K(Z - z)f}{760}\,\epsilon}$$

where K is a new constant and f the mean tension of aqueous vapour, in the column comprised between the place of observation at the altitude z and the stratum entirely free from sensible vapour at the altitude Z.

If this is applied to my observations carried out simultaneously at the summit (4,810 m.) and at the base (1,200 m.) of Mont Blanc, August 16, 1875, and to these we may add those of the following day at the Grands Mulets (3,050 m.), this formula gives A = 2·54. Certainly, one may be doubtful whether this is the real value of the solar constant. At all events the uncertainty of extrapolation as far as $\epsilon = 0$ diminishes according as the observation is extended to lesser values of the mass traversed, and as freed from the lower strata of our atmosphere, which are more or less contaminated, this mass gradually approximates to theoretical homogeneity.

In 1884 Langley‡ made a very great advance in the study of radiation by his memorable expedition to Mount Whitney, and by the admirable researches into the subject, which he carried on for 10 years after his return. By means of his spectrobolometer he measured the energy of the different simple radiations of a definite wave-length λ, and detected an important part of the energy in the infra-red as far as $\lambda = 3\,\mu$ and later even to $\lambda = 6\,\mu$.

Langley wished to measure at one time the total energy of solar radiation with the actinometer, and the energy of each simple radiation with the spectrobolometer, by simultaneous observations at three stations, placed at successive altitudes on the mountain. Lone Pine (1,460 m.), Mountain Camp (3,543 m.), and Whitney Peak (4,426 m.); but this last station, situated on the very top of the

* Radau *loc. cit.* † Soret *loc. cit.* § Violle, *loc. cit.* ‡ Langley *loc. cit.*

mountain, was so exposed that only a few isolated observations could be secured there. At the two other stations it was possible to carry out, in addition to the actinometrical observations, a sufficient number of observations with the spectrobolometer (unfortunately these were not simultaneous), each for different thicknesses of the atmosphere, for 10 different wave-lengths, 0·350μ, 0·375μ, 0·400μ, 0·450μ, 0·500μ, 0·600μ, 0·700μ, 0·800μ, 1·000μ, 1·200μ. For each of these 10 wave-lengths he calculated the two constants A_λ and p_λ of Bouguer's formula,

$$Q_\lambda = A_\lambda p_\lambda^{\frac{H}{760}\epsilon}.$$

He then divided the spectrum into narrow bands corresponding to the different radiations he had measured (and observations subsequently made at Alleghany enabled him to extend these to 2·400μ), and taking the sum of all the partial energies referring to each of these bands, he obtained for the solar constant the number 3·068.*

We may criticize the numerical value thus obtained, which is the mean of two numbers considerably different (and of unequal weights), 2·630 and 3·505, but we cannot set too high a value on the analysis carried out by Mr. Langley with his spectrobolometer, and the consequences which have resulted therefrom.

Mr. Langley shows, in an indisputable way, that the formula with a single term can never, with a mean value of A and only one value of p, replace the formula with several terms.

$$Q = A_1 p_1^\epsilon + A_2 p_2^\epsilon + A_3 p_3^\epsilon + \quad . \quad . \quad .$$

We must, therefore, henceforward entirely renounce the "barbarous" expression, to use Dr. Pernter's† phrase, of a single coefficient of transparency relative to the action of our atmosphere on the total radiation of the sun. But after the results of the researches carried out or suggested by Langley, how complicated does this absorption appear! Let us consider, for example, the curve published by Langley‡ in 1888 to represent the distribution of energy in the solar spectrum at Alleghany. In that curve, constructed by taking for abscissæ the wave-lengths, and for ordinates the values given by the bolometer, what a disconcerting profusion do we find of saw-tooth edges, of depressions of every breadth and every depth. Doubtless, if we place along side of it the well-known telluric rays of the visible spectrum, the zones of absorption of aqueous vapour and of carbonic acid, as we can trace them from the recent works of Ångström, Julius, Paschen, and Rubens, we can clearly see the influence of the different elements of our atmosphere. Not less clearly comes out the difficulty of establishing a complete formula, even if we admit that each isolated action obeys Bouguer's law.

* The values of p_λ varied from 0·35 (for $\lambda = 0{\cdot}375\,\mu$) to 0·97 (for $\lambda = 1{\cdot}200\,\mu$) in the very pure atmosphere above Mountain Camp. In the lower layers the coefficient of transparency is still more variable, from 0·10 to 0·96. We have, practically, $p = e^{-x\lambda^{x}}$, x and x being two constants.

† *Zeitschrift für Meteorologie*, p. 207, 1886.

‡ *American Journal of Science*, 3rd series, Vol. XXXVI., p. 397, 1888.

Meanwhile, we cannot set aside the loss of intensity which the radiation of the sun undergoes by diffusion through the solid or liquid particles in suspension in our atmosphere. This effect, which appears to account for certain depressions of Langley's curve, may become very decided in a cloudy atmosphere. Lord Rayleigh* has been brought to represent by $I = I_0 e^{-K\lambda^{-4}\epsilon}$ the intensity of radiation of initial magnitude I_0, of length of wave λ, transmitted through a cloudy medium of thickness ϵ, K being a co-efficient depending on the properties of the particles in suspension. The experiments of M. Mänz, M. Nichols, Captain Abney, and General Festing, and above all those of Ångström, have only given an imperfect verification of that formula. Fresh researches are required.

Among the constituents of our atmosphere the carbonic acid has particularly attracted M. Ångström's attention.† Assuming that nitrogen, oxygen, and aqueous vapour exert on the whole of the solar rays an absorption which is uniform and weak, while the carbonic acid produces a strong action, he puts

$$Q = A_1 p_1^\epsilon + A_2 p_2^\epsilon,$$

A_1 representing the radiations which are affected by nitrogen, oxygen, and aqueous vapour, A_2 those affected by carbonic acid; p_1 and p_2 are the corresponding coefficients of transparency. M. Ångström then takes the observations he made in the island of Yxelœ on July 19, 1888, and which had given him the following results :—

Hours.			ε.	Q. a.m.	Q. p.m.	Q. Mean.
5 a.m.	7 p.m.	...	4·75	0·51	0·48	0·495
6 "	6 "	...	3·09	0·73	0·77	0·750
7 "	5 "	...	2·26	0·89	0·98	0·935
8 "	4 "	...	1·80	1·02	1·09	1·055
9 "	3 "	...	1·53	1·20	1·20	1·200
10 "	2 "	...	1·38	1·28	1·30	1·290
11 "	1 "	...	1·29	1·36	1·34	1·350
12 "	...	...	1·26	1·36	1·36	1·360

He applies to these figures his formula, and with this effect, that he remarks that for values of ϵ superior to 3 the rays absorbable by carbonic acid have, in fact, disappeared, so that from the first two observations relating to $\epsilon = 4{\cdot}75$ and $\epsilon = 3{\cdot}09$

* *Phil. Mag.*, Vol. XLI., p. 107, 1871.

† *Ofversigt af Kongl. Vet.-Akad. förhandlingar*, No. 4, Stockholm, 1889, and *Bihang till K. Svenska Vet.-Akad. handlingar*, Vol. XV., No. 9, 1899; or *Annalen der Physik und Chemie*, Neue Folge, Vol. XXXIX., p. 267, 1890.

he can deduce the constants of the portion of the radiation apart from the action by carbonic acid :

$$A_1 = 1{\cdot}56 \qquad p_1 = 0{\cdot}786.$$

The difference between the observed intensity and that given by the formula $Q_1 = 1{\cdot}56 \times 0{\cdot}786^{\epsilon}$ represents the intensity of the portion which passes through the carbonic acid for each of the six remaining thicknesses.

From his own personal observations, and particularly from the determinations made by M. Lecher on the Altenberg, M. Ångström gives for the carbonic acid $p_2 = 0{\cdot}134$. He then finds from six equations which are quite concordant $A_2 = 2{\cdot}45$.

The intensity of solar radiation is accordingly represented by the formula

$$Q = 1{\cdot}56 \times 0{\cdot}786^{\epsilon} + 2{\cdot}45 \times 0{\cdot}134^{\epsilon}$$

which gives, for $\epsilon = 0$,

$$A_1 + A_2 = 4{\cdot}01.$$

This will be, according to M. Ångström, the solar constant, or, rather, as he very correctly says, the minimum of that constant.

The most satisfactory agreement is shown between the numbers calculated by the preceding formula and the numbers observed. But we have already remarked more than once that such an agreement proves absolutely nothing as to the theoretical value of the formula adopted. M. Radau has given several proofs of this; he has reproduced with equal success the same figures by very different formulæ, and M. Rizzo has shown that the values observed by M. Angström on July 19, 1888, may be just as well obtained from Crova's formula

$$Q = \frac{3{\cdot}239}{(1 + \epsilon)^{1{\cdot}063}}.$$

The simultaneous observations at different altitudes have quite naturally attracted the attention of M. Rizzo,* whose important expedition to Rochemelon is well known. He has endeavoured afresh to establish a relation, more or less approximate, between the total energy Q of the solar radiation at one point, referred to the zenith, and the pressure H at that point. With that object, he has taken into account simultaneous observations (or what may be considered as such) taken by me at Bossons, at the Grands Mulets, and on the top of Mont Blanc, in 1875; by Mr. Langley at Lone Pine, at Mountain Camp, and on the top of Mount Whitney (1st series), at Lone Pine and at Mountain Camp (2nd series) in 1884; by MM. Crova and Hanski at Chamonix, at the Brévent, and on top of Mont Blanc in 1897; and, finally, by himself at Monpantero, at Trucco, at Casa d'Asti, and at the top of Rocciamelone in 1897, and he has found that the formula

$$Q_1 = A + B\,(760 - H)^{\frac{1}{2}}$$

* Rizzo *loc. cit.*

will adapt itself perfectly to these five series with the following values of A and of B :—

Observers.	A.	B.
Violle	1·570	0·045
Langley (1st series)	1·261	0·042
" (2nd series)	1·414	0·039
Crova and Hanski	1·067	0·053
Rizzo	1·308	0·053
Means	1·324	0·046

It will be seen that the coefficient B which, properly speaking, expresses the law according to which the radiation varies with the altitude above sea level, oscillates between sufficiently narrow limits, while A exhibits larger variations. These variations arise either from the fact that the intensities differ really in consequence of the absorptions which have taken place, or more probably from the fact that the measurements are affected by instrumental errors. As this is, without doubt, the principal cause of disagreement, we may take for the most probable value of A the mean of the numbers furnished by the different series, and for B the mean derived from the same series, and with these numbers we can establish a formula which will represent, at least approximately, the law according to which the solar radiation, referred to the zenith, depends on atmospheric pressure, and which will allow us to obtain the value of the solar constant.

Thus, we find

$$Q_1 = 1{\cdot}324 + 0{\cdot}046\,(760 - H)^{\frac{1}{2}}$$

and consequently, for the solar constant, 2·592.

Over and above the inexactitude which results from extrapolation, and which always exists, more or less, in every formula we take, the determination of the solar constant will be always affected by the unknown effect of the upper strata of the atmosphere. This shows us again the advantage of securing observations at the highest elevations we can reach.

Summary.

To sum up, we now possess excellent apparatus for measuring and registering solar radiation at an observatory. We have also portable instruments for scientific researches.

But is there yet an actinometer in existence which unites the conditions of simplicity, strength, and facility of manipulation which are demanded by meteorologists for routine observations?

We know a number of empirical formulæ which will enable us to follow the march of radiation during one entire day. We can also, with a certain exactness, unite the different observations following the same vertical plane. But we have no formula from which we can deduce with any certainty the *solar constant;* the formula $Q = \Sigma A_{\lambda} p_{\lambda}^{\varepsilon}$, over and above the difficulties of its application in the actual state of our knowledge, leaving out of account the absorptions not following Bouguer's law. Moreover, no formula can take account of the unknown absorptions produced in the upper regions.

At all events, the observations at great elevations (on mountains, or, still better, in balloons) will allow us to approximate to the truth, by making it possible to measure the total radiations, as well as every simple radiation up to the levels of 24 or 25 kilometres, which we shall soon be able to reach.

It will be of advantage to multiply these observations, not only on account of the changes which take place on the surface of the sun, but also on account of the variations of the distance from the earth to the sun, and the special actions which the different regions of space traversed by our earth may produce.

JULES VIOLLE.

August 25, 1899.

APPENDIX IX.

Meteorology and Seismology.

The connection between earth tremors and meteorological phenomena is slight, and is but seldom seen, excepting in the analysis of numerous records from large areas, or from small areas where seismic disturbances are frequent. When these are made it is found that seismic frequency and periodicity is more or less connected with varying meteorological conditions.

If, however, the seismologist includes in his work observations on changes in the vertical, diurnal, and semi-diurnal waves, which may in part represent globe distortions, earth tremors, or micro-seismic movements, the records of which do not always refer to movements of the soil, &c., then the explanation of these phenomena are to be found almost entirely in the registers of the meteorologist.

Unusual movements of large bodies of water, like *seiches* and oscillations of sea level, are due sometimes to atmospheric disturbances and sometimes to seismic disturbances.

Where observations of terrestrial magnetism are included in the work of a meteorological establishment, at certain stations, it will often be found that the records of a seismograph will throw light upon unusual perturbations of magnetic needles.

As to whether meteorological and seismological investigations should be associated, the conclusion, based upon the above notes, is that very much depends upon the nature of the observations undertaken by the meteorologist and by the seismologist respectively.

To observe earth tremors is comparatively a simple matter, but there is at present no completely-equipped establishment for the complete record of the various movements of the surface and of the interior of our earth.

JOHN MILNE.

January 5, 1899.

APPENDIX X.

Report on the German Expedition to the Antarctic Regions; by Professor Dr. von Drygalski.

The German expedition to the South Pole will start at the end of August, 1901, and will at once make for the Cape of Good Hope, stopping only from time to time to obtain some soundings in the South Atlantic Ocean. The equipment of the expedition will then be completed if necessary, and the first magnetic observations will be taken.

From the Cape the expedition will proceed to Kerguelen, by Prince Edward's and the Crozet Islands, where, at least, one observation of the magnetic elements will be taken with the standard compass (Fox's apparatus) and with the deviation magnetometer.

While at sea the meteorological observations will be made, as usual, every four hours; about noon, between 11h. 30m. a.m. and 1 p.m., the observations will be more frequent, so as to determine more exactly the maximum temperature of the air over the surface of the ocean.

It is proposed to establish an auxiliary station at Kerguelen, with photographic self-recording magnetometers, a portable magnetic theodolite, one of Heydweiller and Stamkart's apparatus, and an induction inclinometer. The meteorological observations at that station will be similar to those taken at a station of the second order, and self-registering apparatus will be used in addition. The estabishment of this auxiliary station has not yet been decided on, but it may be considered probable.

In November, 1901, the principal expedition will start from Kerguelen. It will proceed eastward as far as the meridian of 90°, and then will turn southwards to Termination Island, and from that to the hypothetical west coast of Victoria Land.

It is there that it is proposed to establish the principal station, which will be in action for a whole year from about the commencement of February, 1902, until the same date in the succeeding year. Accordingly, the return voyage will commence in February, 1903, directing its course westwards towards Weddell's sea, if possible, and from that by South Georgia towards Tristan d'Acunha. On leaving Kerguelen, the magnetic and meteorological observations will be continued at sea as before. At the principal station a complete set of instruments for magnetic variation will be set up, with also a set for absolute observations. From that station sledge journeys will afford opportunities for taking magnetic observations at a distance inland, by means of the portable theodolite. Meteorological observations will be taken at the principal station, as at a station of the second order, with the addition of self-recording apparatus. Observations will also be taken of remarkable phenomena, and especially of those which occur in the upper regions of the atmosphere.

During the voyage one person will be specially charged with the magnetic and meteorological observations. At the fixed hours he will be assisted by the officers of the ship. At each station there will be two observers with assistants.

The ship itself will be of wood, with a complete set of sails and an auxiliary engine. This latter, with its accessories, will be set abaft, so that the iron in its presence shall not affect the magnetic observations which will be carried on on the captain's bridge, forward. For six metres at least all round the position of magnetic observations the use of iron will be excluded as far as possible, having due regard to the question of the strength of the vessel.

Berlin, November 14th, 1899.

APPENDIX XI.

REPORT ON THE ESTABLISHMENT OF OBSERVATORIES AT THE CENTRES OF ACTION OF THE ATMOSPHERE; BY H. H. HILDEBRANDSSON.

It is evident that we shall never discover the laws which govern the general movements of the atmosphere if the only observations we make on the earth's surface are those in certain civilized countries. What is clear is that the atmosphere is a continuous mass resting on the earth and the sea, and that these two react upon each other. Any disturbance which appears at any one point must make itself felt at very considerable distances from that point. We shall often have to seek for the cause of a certain phenomenon in another which has taken place perhaps in another hemisphere. In consequence, a commencement has been made, on the one hand, to study the upper regions of the atmosphere, by establishing, at great cost, observatories on very high peaks. Two international committees are working towards this end. The Cloud Committee has organised cloud observations on an uniform plan in almost all the meteorological services of the world, and the results of the first international discussion have either been published or are in the press. The Aeronautical Committee is carrying out with enthusiasm simultaneous balloon ascents which have already furnished very important results.

On the other hand, Meteorological Congresses and the International Meteorological Committee have, without ceasing, insisted

on the importance of establishing stations in countries at very great distances from the centres of civilisation. It appears to us that the time has come to indicate the positions on the earth's surface whence it would be of the greatest importance to receive observations.

On any isobaric chart for any month there will be found determinate regions of barometrical maxima and minima. The most constant maxima are located over the oceans to the north and south of the equator, near the Tropics, and the most important minima on the oceans near the Arctic and Antarctic circles. On the continents of the temperate zones we have maxima in winter and minima in summer. These maxima and minima have been termed by M. Teisserenc de Bort *centres of action*. In fact, thanks to his investigations and the more recent work of MM. van Bebber and Köppen, we know that a certain relation exists between the general character of the weather of a season in Europe and the variations in the barometrical heights at the surrounding centres of action, that is to say, in Iceland, at the Azores, and in Siberia. A season has a different *type* according to whether Europe is under the influence of one or other of these centres of action. If the depression over the sea near Iceland extends in winter over the whole north-west of Europe, we have a mild and rainy winter, while the winter is severe if either the Azores maximum or that from Siberia extends over Europe, and so on. It seems that direct relations exist between all the centres of action over the globe.

We have thought that it would be useful to make a preliminary study of this question. In two memoirs, *Quelques recherches sur les centres d'action de l'atmosphère*, I., 1897; II., 1899,* we have found interesting simultaneous relations between the barometrical pressure and the rain at different centres of action. So we have shown that there exists a sort of compensation between certain neighbouring centres of action. The monthly barometrical variations at the Azores and in the vicinity of Iceland are almost always opposed to each other. The same opposition exists between Siberia and Alaska, especially in winter, and between Tahiti and Terra del Fuego, situated in the Antarctic minimum. On the other hand, there is a quite remarkable concordance in winter between the atmospheric pressure at the Azores and in Siberia.

The rainfall exhibits analogous relations. A wet winter at the Azores corresponds to a dry one over the Iceland sea, and *vice versâ*, and we have found the same opposition between Siberia and Zi-ka-wei in China, between Mauritius and New Zealand, between Alto da Serra in Brazil and Cordoba in Argentina. There is also a very clear opposition between the centre of the United States of America and the South of Europe. On the other hand, the character of the rain is nearly uniform from the Azores in the west, through central Europe as far as to Barnaoul and Jenisseisk in Siberia to the east.

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* *Kongl. Svenska Vetenskaps Akademiens handlingar*, Vols. XXIX. and XXXII.

We have, moreover, clearly established that very decided relations exist between the different centres of action. We have also found that the study of these relations holds out the hope of its conducting us to *a forecast of the weather for a considerable time in advance.*

In fact we have ascertained that the quantity of water which has fallen from October to March in Siberia is in general inverse to the quantity which will fall in the succeeding rainy season in India. This confirms an old Indian observation that if the quantity of snow on the mountains is small in winter an abundant monsoon rain may be looked for, and *vice versâ*. It is clear that a correct forecast of the weather for the rainy season in India, made six months in advance, would be of very great importance.

Moreover, a very good agreement exists between the amounts of water falling in British Columbia in winter on the north-east shores of the Pacific and that of the following autumn in the Azores.

Finally, what touches us more closely is that we have proved that, with one or two exceptions, the *winter rain* at Thorshaven has the same character as that of the *preceding summer* at St. Johns, Newfoundland, and of the *succeeding summer* at Berlin.

We need not say that we do not pretend to have found, so far, any definite laws or conclusive results. What we have effected has been only a reconnaissance in a country almost unknown, and that we have found some traces of the road we ought to follow. For the present it is impossible to proceed further, *owing to the absence of observations in some of the most important regions.* While there are numerous observatories and well-equipped meteorological stations in the *intermediary* countries in Europe, in India, &c., such establishments are either entirely non-existent or, at best, very imperfectly equipped in the regions where the centres of atmospheric action are situated.

It is true that a commencement has been made, in countries which possess colonies, to organize meteorological stations in those colonies, and also in other distant countries, by means of the consulates. The results are also published in the meteorological annals of those countries. But when we examine these records we can see that the observations do not last in general for more than a few years at each place, and may then be resumed later on at another place. But for the researches in question *we want simultaneous observations, continued for a long series of years, at a large number of stations well situated.*

In Siberia we have observations and excellent stations managed by the Meteorological Institute of Russia.

The Icelandic minimum is also well furnished with stations by the Danish Institute. An observatory of the first order at Thorshaven, situated in the centre of the Gulf Stream, would be, doubtless, of extraordinary importance.

At the Azores we have well-organized Portuguese stations, but they are all of the second order. The Prince of Monaco has

established there a complete magnetic observatory. He would confer a great benefit on meteorology if he would also establish there a meteorological station fully equipped.

At Mauritius, in the region of high pressure of the Indian Ocean, we have a first-class observatory, which was for forty years, under the able management of Dr. Meldrum, a standard station for meteorology.

At Honolulu, in the high-pressure region of the North Pacific, Mr. Lyons' station should be made into a fully-fitted observatory. Let us hope that funds will be found in the United States, inasmuch as the Sandwich Islands belong at present to that country.

In the South Pacific we have Tahiti and Noumea, belonging to France. For several years observations have been made there, but unfortunately they are far from complete.

In the South Atlantic we have St. Helena, belonging to England, and from it we receive absolutely nothing.

It is also very little we receive from the great minimum in Behring Sea. An observatory on the west coast of North America, *e.g.*, at St. Michael's, United States, and another opposite at Petro-Pavlovsk, in Kamtchatka, would be of very great value.

In the region of Cape Horn, Archbishop Faguano is about to organize a meteorological station, supplied with self-recording apparatus, at Punta Arenas. It is very much to be desired that he may succeed in that enterprise, and that his observatory will issue publications which will rank with those of Manila and Zi-ka-wei, both of which are managed by clergymen. There is hardly a spot in the whole world from which observations would be of higher value if they could be obtained.

Finally, it is much to be desired that the two stations of Pará and of St. Paul, or of New Amsterdam, should be supplied with meteorological instruments if the Magnetic Committee succeeds in establishing magnetic observatories there.

The number of observatories we propose to create is not large, but we ought to have at least one well-equipped station at each of the centres of action of the atmosphere, and then it will be possible after some years to carry out the study of the principal changes of the weather from month to month over the entire globe. In almost all the countries lying in the temperate zones we have excellent observations, and in the torrid zone the number of stations is continually increasing. These observations are doubtless indispensable, but they will not suffice if we do not know what is going on at the actual centres of action of the atmosphere.

APPENDIX XII.

The Verification of Alcohol Thermometers; by General Rykatcheff.

It is generally the practice to verify, in the vertical position, minimum thermometers which are ordinarily alcohol thermometers. On the contrary, they are set up horizontally to record the lowest temperatures. At the same temperature the indications of thermometers placed vertically and horizontally will differ, and the discrepancies will, as it is well known, amount to some tenths of a degree. Another circumstance which stands in the way of our using only the corrections supplied to the original verification of the minimum thermometers is that at high temperatures the spirit is volatilized, and sometimes a drop distils into the small cavity at the top of the tube at the opposite end to the bulb. The readings are then too low. Such a drop, separated from the rest of the column, may escape the notice of the observer, if he only marks the point on the scale reached by the end of the index.

In order, as far as possible, to reduce the errors due to this cause, and to render the readings of the minimum thermometers comparable to those of mercurial thermometers, the Nicholas Central Physical Observatory recommends the observers in connection with it to note not only the readings of the index of the thermometer but also of the column of spirit in the instrument. By comparing these readings of the spirit column with those of a mercurial thermometer in a vertical position, we calculate at the observatory, usually for each month, the supplementary corrections which are applied to the minimum readings before they are published in the annals. These comparisons allow us also to see if the corrections of the minimum thermometers have changed with the time.

This rule, which we have adopted for checking the readings of the minimum thermometers, has been introduced into the instructions issued by the Imperial Academy of Science of St. Petersburg for stations of the second order (*vide* the editions of 1893, 1894, 1896, 1897, and 1898). For the other countries, as far as I know, there are only the instructions issued by Professor Mohn for the meteorological stations in Norway, which recommend that attention should be paid to this cause of error, but according to these instructions the corrections are calculated and applied every day,* while we only apply our corrections to the monthly means.

It would be very desirable to know if other central meteorological institutes have adopted any measures to eliminate these errors, and what these measures are. If similar rules are applied it might possibly be useful to publish them in the instructions or in the annals. It may happen, that if notice is not taken of these supplementary corrections, noticeable contradictions may come out between the direct observations and those of the maximum and minimum thermometers.

* *Vejledning til Udförelse af meteorologiske Jagttagelser ved det norske meteorologiske Instituts Stationer*, 1888, Christiania.

APPENDIX XIII.

The Influence of the Stray Currents, from Electric Tramways, on the Instruments for Measuring Terrestrial Magnetism; by Dr. J. Edler, Potsdam.

The researches I am about to describe have been undertaken at the suggestion of Dr. von Bezold, Director of the Royal Meteorological Institute, who, in order to protect the magnetic observatory of Potsdam which is under his direction, has entered a protest against the establishment of an electric tramway, with an aerial conductor, and a return current by the rails and the earth, at a distance of at least 15 kilometres from that observatory.

The instruments employed for this research were those described by M. Eschenhagen.* Firstly, a very sensitive apparatus for recording the variations of horizontal force which will indicate the disturbances caused by currents perpendicular to the magnetic meridian. The sensibility of the magnetometer, that is the value of 1 mm. of ordinate of the curve, has been determined by deflexion, or by the duration of oscillations, or simply by the comparison of the curves with those taken simultaneously at Potsdam. In this way the amount of the disturbance may be found in absolute measure.

On the other hand, two induction coils, with a Dubois and Rubens galvanometer will indicate the relative amount of the disturbances due to currents parallel to the winding of the coils. If the windings are perpendicular to the magnetic meridian, the deviation of the magnetometer for the horizontal component, produced by a sudden change of current, is proportional to the current induced in the coils, and consequently to the deviation of the galvanometer.

The magnetometer having been graduated, the quotient of the two deviations gives the coefficient of reduction which allows of transforming into absolute values the indications of disturbance given by the coils.

As the direction of the coils was arbitrary, it was possible to determine the magnitude of the disturbance for every direction of the current.

The deviations of the magnetometer and the galvanometer record themselves on the same photographic sheet, while the fixed mirror of the magnetometer gives the line of reference.

* *Verhandlungen der deutsch. physikal. Gesellschaft*, 1899, p. 147.

The distances from the cylinder to the mirrors of the magnetometer and to the galvanometer were respectively 165 cm. and 135 cm. The galvanometer being placed on one side, a prism directed, by total reflection, the luminous ray and threw it on the photographic cylinder. A slight cover of red material completed the protection of the apparatus against the effects of stray light.

Experiments were made in this manner in June and July, 1898, first at Berlin at 6, Schinkel Platz, and at 120, Potsdamer Strasse, and then in much greater number at Spandau, close to the electric tramway.

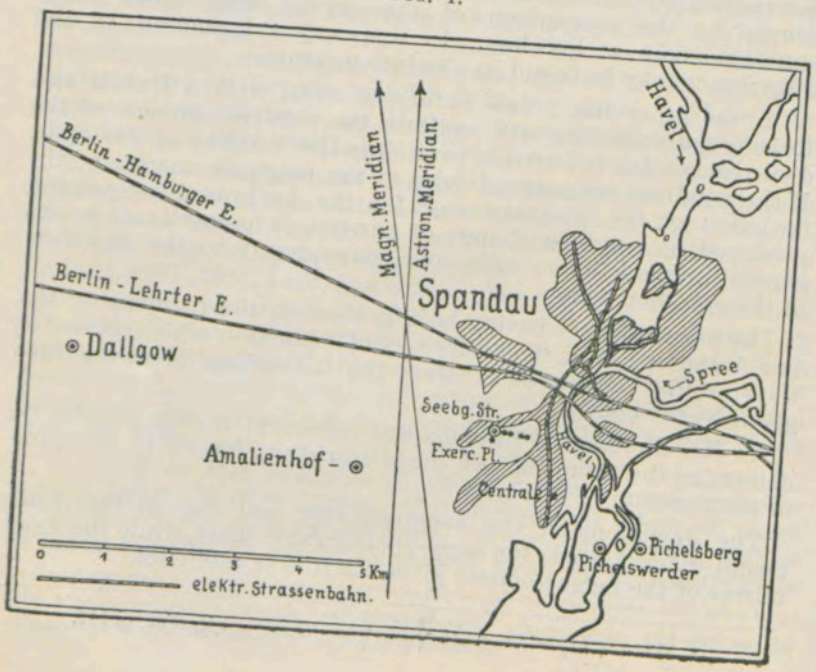
That tramway is particularly well placed for such experiments, because that it runs in only one direction from south to north, and, as it runs alongside a river, it allows of testing the eventual influence of a stream of water on the stray currents.

The distance between the two extreme points of the tramway is about 5 kilometres. The central factory is located near the south terminus, not far from Pichelsdorf. On week-days there are circulating at the same time 15 motor carriages and two coupled carriages. On Sundays and holidays there are 21 motor carriages and six coupled ones.

The current is carried in the usual way by an air line. Two strong cables of copper take the current back from the rails to the machine. The tension is 500 volts. On week-days, under the ordinary conditions, the current varies in intensity between 35 and 150 ampères.

On the small map (Fig. 1) may be seen the different posts of observation relative to the tramway and to the stream of water.

FIG. 1.



The five positions on the *Exerzierplatz*, where induction coils are alone employed, are indicated by points, while galvanometer always remained at the spot marked *Seeburgerstrasse*.

To the west of the tramway measurements were taken, but only with coils, in the *Seeburgerstrasse* at a distance of 0·77 kilometre, and at the *Exerzierplatz* at five points distant from 0·38 to 0·92 kilometre, and others at *Amalienhof* at the distance of 3·01 kilometres, and at Dallgow at the distance of 7·48 kilometres.

Self-registering apparatus was also set up on the left bank of the Havel at Pichelsberg, at the distance of 1·54 kilometre, and at Pichelswerder at the distance of 0·92 kilometre.

On the curves obtained in this way, only the zigzag disturbances have been measured, the total length of which was 200 mm. for each station, and then the mean amplitude of the disturbance has been determined by taking the distance of the extreme points of the zigzags.

In the table given below will be found, not the directions of the currents, but those of the components of the magnetic field produced by the currents.

The following is the signification of the letters :—

D is the minimum distance from the tramway ;
H the elevation above the level of the river Havel ;
and taking as unit $\gamma = 10^{-5}$ C.G.S.,

a_x is the magnitude of the disturbance N. – S ;
a_y is the magnitude of the disturbance E. – W. ;
a_z is the magnitude of the vertical disturbance ;
a_t is the magnitude of the horizontal disturbance ;

$$a_t = \sqrt{a_x^2 + a_y^2}$$

TABLE I.

Site of observations.	D.	H.	Horizontal.			Vertical.	—	
			a_x	a_y	a_t	a_z	Da_t	Da_z
	km.	m.						
Spandau Exerzierplatz	0·38	3·5	3·31	8·94	9·53	23·17	3·62	8·80
,, ,,	0·47	3·5	2·98	7·27	7·86	17·88	3·69	8·40
,, ,,	0·64	3·5	2·32	5·63	6·09	10·60	3·90	6·78
,, ,,	0·72	3·5	1·99	4·98	5·36	7·62	3·86	5·49
,, Station ...	0·79	3·5	1·82	4·30	4·67	5·62	3·69	4·44
,, Exerzierplatz	0·92	3·5	1·66	3·98	4·31	4·32	3·97	3·97
Amalienhof	3·01	3·5	0·74	1·10	1·33	0·95	4·00	2·86
Dallgow	7·48	3·5	0·20	0·39	0·44	0·24	3·29	1·80
Pichelsburg	1·54	16·0	1·25	0·71	1·44	1·04	2·22	1·59
Pichelswerder	0·92	8·0	1·28	1·14	1·71	1·13	1·57	1·04

The extreme disturbances are often twice and sometimes even three times as great as the mean value of the disturbance.

At one station, near New Zehlendorf, to the south of Berlin, it is only the great disturbances which make themselves felt.

In June of the present year (1899) the magnetic observatory has also established self-registering apparatus in the magnetic pavilion of the Physikalisch-technische Reichsanstalt at Charlottenburg. The disturbance of the horizontal component there was 0·8γ on the mean and 2γ in the maximum. The disturbance of the declination almost reached one minute in certain cases.

Naturally, all these results refer only to the instrument employed, of which the damping and the period of the oscillation vary a little with the magnitude of the disturbance. For the magnetometer the logarithmic decrement of the oscillations was 0·64. With equal impulse the proportion borne by the deviation of one needle, without damping, to the value observed was 1·32.

The impulses of the current follow each other very rapidly, and their intervals are less than the period of oscillation of the needle.

These oscillations are thus continually modified by new impulses; however, if we calculate the mean duration of an oscillation for a sufficiently long interval by the number of zigzags, the value obtained agrees well with the period of free oscillation of the needle.

For comparison we may also give the disturbances observed at some other observatories near which electric tramways are now passing.

TABLE II.

—	Mean distance of tramway.	Disturbance.			—
		Declination.	Horizontal force.	Vertical force.	
Greenwich ...	Kilom. 6·8	Not measurable.	4 γ to 7 γ	4 γ to 9 γ	Serious disturbance by stray currents.
Toronto ...	0·12	5′ to 10′	60 γ to 120 γ	150 γ to 300 γ	
Washington...	0·42	About 1′	About 10 γ	About 300 γ	Some disturbance from electric lighting.

The last columns of Table I. give the products D a_t and D a_z. It may be seen that for all the stations west of the tramway D a_t remains sensibly constant, so that if the stations are situated in the same positions as regards the tramway, the horizontal disturbance decreases simply in proportion to the distance. On the contrary, the vertical disturbance is much the more important near the tramway, but it also decreases more rapidly as the distance increases.

To the east of the tramway, at the other side of the Havel, the disturbance is decidedly weaker. It is not impossible that the

water may intercept a part of the stray currents; but the stations are so placed with reference to the tramway, and, above all, with reference to the central works, that, without taking account of their greater elevation above the level of the water, the current lines reach them less easily. If we admit that D a_t = 3·75 on the average for all the western stations, we have, for D = 8 kilom., a_t = 0·47. That is the average disturbance, but the maximum disturbance will be two or three times greater, that is, about 1 γ.

With a view to determine the minimum distance to which magnetic observatories should be removed from tramways, we must offer a few remarks.

The observatories which confine themselves to photographic registration and continuous register of the elements of terrestrial magnetism, maintain a sensibility of 3 γ to 5 γ for a millimetre of ordinate, and publish in their reports definite values to about 1 γ. The diurnal range, the determination of which presents special interest, varies in intensity in the middle latitudes from about 15 γ in winter to 50 γ in summer. An error of 1 γ would therefore involve in these values an uncertainty of from 7 per cent. to 2 per cent.

In the actual conditions it is necessary to limit the error to 1 γ, because it is difficult to determine the absolute values more closely.

For this it would be necessary that the individual curves should exhibit such precision that this magnitude may be easily appreciable. This precision will disappear at once if the curves are blurred by the disturbances arising from the tramways and so lose their definition. We must not forget that an entire displacement of the curves is not impossible, at least for the vertical component, and that absolute measurements remain uncertain.

If, on the other hand, it is proposed to take up, at an observatory of the first order, the special duty of determining the smallest oscillations, it is necessary to increase the accuracy of the individual values. Inasmuch as, for that object, you can easily reach a sensibility of 0·5 γ, and even determine a fraction of that amount, according to the experiments made at Potsdam with what are called *sensitive* magnetometers, disturbances of that magnitude (0·5 γ) will not allow you to determine whether they correspond to phenomena which are natural or are produced by an artificial disturbance.

Observations made at a distance of 8 kilom. at the side of a tramway with ordinary traffic, and running for a distance of 5 kilom., have given a maximum disturbance of 1 γ, and have shown that the disturbance diminishes, proportionally, with distance. It must therefore be concluded that we cannot fix *a priori* the distance at which we need no longer fear the action of disturbances due to an electric tramway sending its return currents by rails and earth.

In any case, with a tramway of ordinary traffic, the observatory must be placed at least 8 kilometres from the line.

If the traffic of the tramway is heavier, as at Greenwich, it would be necessary to determine the distance at which the disturbances do not exceed 1 γ following the above-mentioned law of decrease in simple ratio with distance.

As to the observatories which have in view special researches with the most delicate apparatus, even a distance double what has been mentioned, a distance of 15 kilom., as has been proposed up to now for Potsdam, is not sufficient to secure complete protection against the influences indicated.

We shall seek to determine in that case, by means of comparative observations, made in different places, whether in reality there is any fear of artificial disturbances being perceived there.

The reduction of the distance to 8 kilom. will consequently signify for such observatories a diminution in the field of their researches, and a lowering of their rank and their capacity.

It is to be desired for the magnetic observatory of Potsdam, which is at present the only spot in Germany where the apparatus necessary for such special researches exists, that the field of its activity should be preserved until a new observatory shall have been constructed, fitted with all the necessary apparatus, at a place which is secure from disturbance for a long period to come.

APPENDIX XIV.

The Publication of Tables of the Diurnal Range of Temperature, in the form of Deviations of the Hourly from the Daily Means; by Hofrath Dr. J. Hann.

Since the date of the publication of tables in this form in Wild's great work on the temperature conditions of the Russian empire, the daily range of temperature has been determined for many places over the earth's surface. But the results of these observations are scattered over numerous publications which are only discoverable with great difficulty. In fact, the tables in Wild's work, which has just been cited, are not easily to be got at, owing to the great and voluminous publication in which these tables appeared.

Now, the climatologist is constantly in need of such tables, in order to reduce temperature means to true means, and he is frequently unable to select the best stations for such a purpose. Even a man who is fortunate enough to have access to the library of one of the large meteorological establishments will find himself compelled, in order to reduce a single station, to spend an inordinate amount of time in finding out the proper tables of reduction, inasmuch as he has to search through several voluminous publications in order to discover them.

Moreover, the theoretical meteorologist would find it a great assistance in all his investigations into the diurnal range of temperature, and into all that depends thereon, if he were provided with a general conspectus of results of all the observations on this phenomenon over the globe.

For this reason I venture to submit to the Committee that it should recommend with emphasis to the chiefs of the different central offices that they should collect and publish in an easily accessible form (and not merely in their annual reports—Jahrbücher) all the tables of the diurnal range of temperature for their area, or, better, for their country.

As a single page 4to. can easily contain the data for two stations, 16 stations will come to eight pages 4to. or 16 pages 8vo., so that the expense cannot be serious.

The head of each system is best able to select such stations as fairly represent the conditions of his country (if he has at his disposal more results than can easily be published), and can give in the introduction very useful particulars as to the situation of the stations, exposure of the instruments, &c.

These publications should be printed in an uniform shape and size, viz., that of the publications of the International Polar Stations, so that they can be bound up together, and about a hundred copies of this should be on sale by booksellers, so that they may be easily accessible to any worker on climatology.

It would be also of great importance to give in this publication stations which do not belong to any extensive system.

Possibly the Committee might find some expert who would be ready to calculate the daily range for such stations. It is most probable that some learned society or academy would publish the results for such stations in the proposed form if the Committee were to recommend the step.

Such a collective work on the daily range of temperature all over the globe, as far as the observing stations exist, would be of the highest value for the progress of meteorology and climatology, and would not be without importance for the geographer.

APPENDIX XV.

Actinometry; by Hofrath Dr. J. Hann.

The Committee is requested to insist upon the high importance of the registration of the intensity of solar radiation at various latitudes, and in various climatic areas, on some method which admits of reduction to absolute measures.

It is only in this way that we can attain a knowledge of the thermal economy of the atmosphere.

The Committee should recommend the continuation of experiments in the construction of an actinograph, furnishing indications reducible to units of heat, and should request the heads of meteorological systems to encourage actinometrical investigations over their areas.

APPENDIX XVI.

THE INSTALLATION OF ANEMOMETERS ON LEVEL GROUND UNDER IDENTICAL CONDITIONS; BY M. LÉON TEISSERENC DE BORT.

The methods for determining exactly the velocity of the wind have been the subject of numerous discussions at several of the international meetings, and these discussions have been chiefly devoted to the choice of instruments and the mode of calibrating them.

Thanks to the increasing progress made in the application of mechanical principles to self-recording apparatus, and thanks also to the care which is taken at most of the principal meteorological stations to take off in a simultaneous manner the indications of different anemometers, we may feel assured when we compare the published figures that the differences between different instruments tend to reduce themselves gradually. In certain cases the indications of the instruments are so far comparable that we may without great inconvenience substitute the indications of one for those of another. I shall quote in particular, in this connexion, the two instruments employed at the Bureau Central Météorologique, at the station on the Eiffel Tower, by M. Angot, the Richards' and the Robinson's anemometers, and the analogous instruments on the turret at Trappes.

We may therefore consider that at the present moment we know, for a sufficiently large number of stations, the velocity of the wind (with an approximation not yet determined, but which, as it appears to me, cannot be less than within 15 per cent.) for the strata of air in which they are placed. It now becomes indispensable, if we wish to proceed to general conclusions, to secure the installation of anemometers in air strata which move under comparable conditions, and it is on this last point that I wish to insist.

At most of the observatories the precaution has for some time been taken to install the anemometers on towers, turrets, masts, &c.; but, without speaking of the absolute altitude, which is naturally varied—

(1.) The altitude of the instrument above the ground is variable.

(2.) The relative contour of the ground around the stations is different in the different azimuths.

This last circumstance plays a most important part, as is shown by the very interesting researches lately carried out in England.

As to the effect of the height of the cups above the ground, I shall report here that:

At the observatory for dynamical meteorology, we have taken care, for several years, to record the velocity of the wind on the top (191 m.), and at the foot (171 m.) of the turret, at the epochs

of direct observations which take place several times a day. It appears immediately that the relation between the velocities above and below is absolutely variable with the different characters of the weather. With a rather fresh S.W. wind the velocity is much greater above than below, in fact double as great, while the two are nearly equal with a N.E. wind and a high barometer. I have noted the same phenomena as between the Eiffel Tower and the Bureau Central Météorologique for several years past. The frequent employment of kites, which at once draws the attention to the phenomenon, shows that it is so striking that it attracts the notice of the workmen assisting in the experiments.

It will therefore be understood that it would be illusory to endeavour to obtain accurate results from comparisons between anemometrical values obtained at different heights above the ground, especially if these heights are small. On the contrary, it would be feasible to erect, at a moderate cost, at some points, comparable stations, either in a large flat country, or on a largish level space clear of obstructions, a small wooden tower of scaffolding poles of 20 metres high (this height is easily to be reached with the timber generally used), carrying small masts or pipes raising the anemometers 2·50 metres above the top platform. It is easy to get at the instrument by means of a sort of ladder to enable you to attend to it. Precise data as to the velocity of the wind could thus be secured in the course of a few years.

I therefore ask the International Committee if these propositions appear to them to be justifiable, and I hope for a favourable response.

1. To prepare for the coming Conference a report on the different anemometrical stations in every country, with information as to their altitude, topography, &c.

2. To make attempts, as soon as possible, to establish at certain points modest anemometrical stations like those above described, even if the observations at these points could not be carried out at all seasons of the year.

APPENDIX XVII.

THE USE OF THE WET AND DRY BULB THERMOMETERS AT STATIONS OF THE SECOND ORDER; BY DR. J. M. PERNTER.

The psychrometer had not been invented when de Saussure, more than a hundred years ago, affirmed that every one would revert to the hair hygrometer after having tried all the other instruments.

However, at the present day, the psychrometer appears to have dethroned the hair hygrometer. All the world believes that in the psychrometer they possess an excellent instrument, accredited by theory, and based on a theoretical formula, deduced by a very pretty mathematical calculation. It is true that Regnault gave a severe and just criticism of that formula, nevertheless he accepted it as an *empirical* expression, and he endeavoured to find the pyschrometric factor A, by a long series of experiments. Although this factor changes considerably with the velocity of the wind, it has been been considered to be correct to take the mean of all the values corresponding to different velocities of the wind, as if it had been the arithmetic mean of numbers obtained under the same experimental conditions. In consequence of this inaccurate procedure psychrometrical tables have been calculated, and from them the values of humidity and vapour tension have been calculated, without taking any account of the wind, and yet its influence might doubtless be eliminated by some artificial modification of the constant.

I shall now give an example by calculating the humidity F and the vapour tension e by the psychrometric formula for different air temperatures, with three different values 0·0006, 0·0008, and 0·0012 of the factor A. The first corresponds to a velocity of the wind equal or superior to 3 metres per second. The second is nearly the same as that of the psychrometrical tables in use, and the third satisfies the condition of calm. Here is the result of the calculation :—

t	t'		A = 0·0006	0·0008	0·0012
$t = 32$	$t' = 25$	e ...	20·81	19·92	18·11
		F ...	59	56	51
$t = 29$	$t' = 25$	e ...	21·98	21·44	20·43
		F ...	74	72	69
$t = 27·5$	$t' = 25$	e ...	22·55	22·23	21·59
		F ...	83	82	79
$t = 20$	$t' = 15$	e ...	10·74	10·15	8·81
		F ...	62	58	51
$t = 18$	$t' = 15$	e ...	11·51	11·13	10·35
		F ...	75	73	68
$t = 17$	$t' = 15$	e ...	11·90	11·64	11·13
		F ...	83	81	77
$t = 14·5$	$t' = 10$	e ...	7·40	6·82	5·66
		F ...	60	56	46
$t = 12·5$	$t' = 10$	e ...	8·17	7·85	7·21
		F ...	76	73	67
$t = 11·5$	$t' = 10$	e ...	8·56	8·37	7·98
		F ...	85	83	79
$t = 5$	$t' = 2$	e ...	4·11	3·73	2·95
		F ...	63	57	45
$t = 3·8$	$t' = 2$	e ...	4·57	4·34	3·88
		F ...	76	73	65
$t = 3·2$	$t' = 2$	e ...	4·81	4·65	4·35
		F ...	84	81	76

I add also two ordinary cases with Föhn :—

$t = 10$	$t' = 4$	e ...	4·14	3·50
		F ...	45	38
$t = 5$	$t' = 0$	e ...	2·64	2·00
		F ...	41	31

Without discussing these results it is easily seen that the unventilated psychrometer gives, with the psychrometrical tables, values which almost always vary so much from the truth that it is impossible to attribute to the psychrometer the character of a standard instrument for the determination of humidity, in the degree to which it has been thought right to consider it so. De Saussure therefore, was right, for we shall have to return again to the hair hygrometer, which has also the advantage of giving good values of humidity, when the temperature is below the freezing point.

I know well that all meteorologists will not agree with me, but without having recourse to ventilation, it is certain that we shall not be able to preserve the psychrometer from discredit, notwithstanding the elegance of the theory on which it is based. Belli had already recommended the addition of an aspirator to the psychrometer, and Regnault had established the fact that the influence of the variable velocity of the wind is reduced by the rotation of the psychrometer round its axis. Later it has been proved by Sworykin that the factor A remains sensibly constant when the velocity of the wind passes a certain limit. Accordingly ventilated psychrometers have been constructed so as to secure for the observations a velocity of aspired air of at least 3 metres per second. In America recourse has been had to the sling psychrometer. The factor A has been determined for these instruments, and has been found to be constant enough for us to be assured that the ventilated psychrometer gives values for air humidity which are very close to the truth.

It must not, however, be forgotten that the psychrometric formula is purely empirical for the ventilated instrument, much more so than for the ordinary arrangement. After the publication of beautiful investigations of Maxwell and Stefan for calm air, an attempt has been made to apply the formula to the case of air in motion. After all these attempts we find ourselves obliged to return to the conclusion arrived at by Regnault, who give the subjoined criticism of the psychrometer and of the formula in the following words :—"*The psychrometer should be considered to be an empirical instrument analogous to the hair hygrometer of Saussure.*" (*Comptes Rendus*, XXXV., p. 938.)

On the other hand, the employment of ventilated psychrometers at stations of the second order is practically impossible. On this subject I shall again cite what Regnault has said : "If you give to the psychrometer a rapid movement of rotation in a circle round a vertical axis, you reduce the action of the variable motion of the air, and the influence of local conditions, *but you destroy the simplicity which is the principal merit of the psychrometer*" ; and further on : "I shall also remark that in operating as M. Belli proposes, the observation of the psychrometer gives rise to an operation at least as complicated as that observation of the condensation hygrometer, and I cannot see any reason for preferring its use to that of the latter instrument, of which the indications are absolutely correct." (*Comptes Rendus*, XXXV., p. 936.)

In fact, even the most complete accuracy of its indications is not enough to make an instrument applicable to the use of meteorological stations. Our observers require an instrument simple in its construction, in its installation, and in its management and observation. This observation itself should not take up more time than is wanted for a simple reading of the instrument. We should find only a very few observers who would be ready to engage to make three times a day a true physical experiment. It is the absence of simplicity in the ventilated psychrometer which has confirmed us all in the opinion that it can never be introduced generally at our stations.

We arrive, therefore, at the following conclusion:—The unventilated psychrometer, with the tables it requires, gives values too discordant with the truth for us to accept it as a correct instrument; the ventilated psychrometer gives values correct enough, but it does not possess the simplicity necessary for our meteorological stations. We must, therefore, seek for another hygrometer which is easily managed and is simple for these stations. Such an instrument already exists. It is the hair hygrometer which will return to the place of honour, as Saussure foresaw.

At the first International Meteorological Congress at Vienna, in 1873, the question of the relative merits of the hair hygrometer and the psychrometer was discussed. M. R. Wolf, of Zurich, had prepared an experimental memorandum for the use of the Congress entitled "Psychrometer or Hair Hygrometer." The conclusion of the report, drawn up by Cantoni, and accepted by the Congress, was that it was useful, and to be recommended that the hair hygrometer should be used in addition to the psychrometer. At that time it was feared that the hair hygrometer, unless controlled by the psychrometer, was too uncertain and variable. But since Koppe has investigated the hair hygrometer these difficulties have been overcome. The self-recording hair hygrometer has yielded very satisfactory results, so that we can without the least hesitation confine ourselves to the use of the hair hygrometer, if we check the point of complete saturation, 100, from time to time by the simple method proposed by Koppe.

Accordingly we know the humidity by the direct indications of the hair hygrometer, and from it we can easily find the vapour tension. The tables required to reduce the observations to values of vapour tension are, at the same time, very short, and are applicable to all instruments and all conditions of the atmosphere.

The accuracy of the results reached in this way is certainly greater than those from the unventilated psychrometer, and are possibly less accurate than those from the ventilated instrument. I feel myself therefore justified in recommending to my colleagues the use of the hair hygrometer at the meteorological stations of

the second order. Meanwhile I recognise the necessity of a new and thorough inquiry, and an experimental verification of the hair hygrometer, which I hope shortly to carry out.

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APPENDIX XVIII.

ON THE CABLE TO ICELAND; BY DR. PAULSEN.

In a paper, *Sur les tempêtes de l'Atlantique Septentrional*, &c., published in Copenhagen in 1880, M. Hoffmeyer, at that time Director of the Meteorological Institute of Denmark, pointed out the great importance to the science of meteorology and its practical application, of the establishment of a system of daily observations, and the publication of daily reports for the regions of the North Atlantic, which do not as yet possess telegraphic facilities. With this object he proceeded to point out that among the important spots which should be connected by international cable with Europe were especially the Azores, the Bermudas, the Färoe Islands, Iceland, and Greenland.

Meteorologists were not slow to recognize the importance of the investigation M. Hoffmeyer had carried out. Scarcely six months after the appearance of his paper the International Committee at its meeting at Berne, in 1880, adopted the following resolution :—

> "The Committee, without examining the practical details of the question, fully agrees with Captain Hoffmeyer that the establishment of telegraphic communication with the Färoe Isles and Iceland, with Greenland and with the Azores, would be of the highest importance for the progress of the science of weather-forecasting in Europe, and expresses its hopes for the realization of the scheme."

In order to realize the scheme large sums were required, and one could not think that the problem was at once to be solved. Nevertheless, meteorologists did not lose sight of the subject at their subsequent gatherings. When the International Committee met at Copenhagen in 1882, the following resolution was unanimously adopted :—

> "The Committee, with reference to their former utterances on the subject, consider that storm warnings cannot realize all the advantages which may be anticipated from them until regular telegraphic communications are established with the stations situated to the west of Europe, viz., Iceland, America, and the Azores. The prospect of the establishment of this network does not appear to be near at hand, but the Committee is of opinion that a cable uniting Iceland to Europe would meet the requirements which are most urgent; they therefore recommend such an undertaking, and request M. Hoffmeyer to draw up, for distribution by the executive officers of the Committee, a memorandum which will set forth the general interest of the scheme."

At the Meteorological Conference at Munich (1891) the question was again brought forward by Dr. Neumayer, Director of the Deutsche Seewarte, who pointed out at the meeting of September 2nd how important it would be to realize M. Hoffmeyer's scheme, and was warmly applauded by the meeting.

Encouraged by what had been said at the meeting at Berne, and by the promises which several members of the Committe had had the kindness to make, viz., to endeavour to obtain from their respective Governments an effective support of the project, the Danish Government set vigorously to work in 1881–2 to organize telegraphic communication between Great Britain, the Färoes, and Iceland. With this object it addressed itself to the Great Northern Telegraph Company, which has its head-quarters in Copenhagen, in the hope that its great telegraphic system of submarine cables in the north of Europe might be able to secure the realization of the scheme.

All these efforts failed, for such an enterprise could never be self-supporting, owing to the slight commercial importance of the Färoes and Iceland, and the sparseness of their population (13,000 and 70,000 respectively). Another difficulty was the great outlay requisite to maintain the cables in the waters of high latitudes.

No one would take up the scheme unless it were supported by substantial financial assistance from the Governments interested. Unfortunately none of the approaches made in this direction met with any encouragement.

However, since that date the Azores, Bermuda, and the Shetlands have been connected with the international telegraphic system.

A glance at the map will show that the observations in the above-named stations, conjointly with those already existing on the west coast of Europe, in Newfoundland, and on the east coast of America, nearly complete a semicircle round the portion of the Atlantic which is daily traversed by the crowd of ships engaged in the mail service, in emigration, and in the enormous commerce between Europe on the one side and the United States and Canada on the other. In order to complete the circle it is only necessary to include in it the Färoes, Iceland, and Greenland; but the last named country is practically inapproachable by a telegraphic cable, on account of the amount of ice on its coasts. If we could only succeed in closing the curve by means of the Färoes and Iceland, we should evidently supply meteorology with daily reports from all the observatories round the circle, and should be able to state the actual conditions of the weather over the northern region of the Atlantic Ocean, and to issue forecasts of weather with much greater certainty than we can at present. It is impossible to rate too highly the services which such an organization would render to the navigation and the commerce of the two continents, as well as to the navigation along the coasts of both of them, and to their agricultural interests. It is not too much to say that, from one side of the ocean to the other, the great centres of navigation and of commerce will soon come to consider a daily weather chart of the Atlantic Ocean to be quite as indispensable as is the daily publication of the courses of Exchange at the present day.

In the opinion of the Danish Meteorological Institute, the moment has now arrived to realise the establishment of the cable to the Färoes and Iceland, and the chances of success have materially improved. Accordingly the Danish Government has again made application to the Great Northern Telegraph Company, and this latter has evinced a great amount of disinterestedness in proposing to lay, manage and maintain a telegraphic cable, starting from the Shetland Islands, with the authority and support of the British Government, touching at the Färoes, and landing in Iceland even, if the Government would procure guarantees, even very moderate ones, for raising the capital, and replacing it in the interval of about 30 years.

The calculations, which have been carefully examined by the Danish Government, state that £120,000 would be the minimum cost for laying the cable to Iceland and establishing an air line of telegraphs between the meteorological stations on the east, north, and west coasts of that large island. If we put the interest of the capital as low as 4 per cent. and fix the term of its replacement at 28 years, these two services will require about 6 per cent. per annum, or annually £7,200 for 28 years. The cost of keeping

up of the cables will be £3,200 a year, and a somewhat similar sum is required for maintaining the telegraphic stations, etc. This gives a total of about £13,600 per annum.

The commercial business will be insignificant and the receipts from it almost *nil* for a number of years, but we may feel some confidence, and wait until we see them rise in the long run, and the Great Northern Telegraph Company is prepared to undertake the cost of keeping up and administering the cables, provided that the above-mentioned revenue of £13,600 a year is guaranteed to them for 20 years. In other words, it gives up all claim for the last eight years of the term of replacement of capital for all succeeding years.

The Governments of Denmark and of Iceland as well as the legislative bodies of both countries, consider the conditions so favourable, and attach such importance to the affair, that they have promised to the Company an annual subvention of about £5,000, but there remains still to be made up the balance of £8,600 a year.

The sacrifices which the Danish Government has already made, and the fact that it is also ready to undertake the hydrographical researches required for laying the cable, makes us hope that other Governments will be disposed to follow this good example, on a more moderate scale, if their respective Meteorological Offices will make the demand. We propose to approach the subject in the following way. The Meteorological Institutes which are most interested in learning the condition of the atmosphere over the North Atlantic should obtain from their Governments authority to subscribe an annual sum to be continued for 20 years, and will receive daily telegraphic weather reports from the Färoes and from Iceland. These telegrams will be transmitted gratuitously over all the lines of the Great Northern Telegraph Company for the 20 years above-mentioned, and after. It is evident that the greater the number of subscribers, the less will be the amount of subscription for each.

We only propose to count provisionally as subscribers the following Institutes:

First category.—Great Britain, France, Germany, Russia, Austria-Hungary, Italy, and the United States of America.

Second category.—Norway, Sweden, Belgium, Spain, Portugal, and Canada, without counting Denmark, because it has already promised its contribution as above mentioned.

If, for example, the large States were to subscribe £800 a year each, and the small ones £400, the grand total would be £8,400 a year. Supposing that any of the States enumerated declined to support the scheme, the deficit would be covered by slightly increasing the amount paid by certain States, such as Great Britain, France, Germany and Norway, which are strongly interested in the fisheries and commerce of Iceland and the Färoes.

At all events it is evident that as an example we have represented as above a distribution of the several proportions. It will always be possible to modify this. The chief matter is to raise the money required.

The Danish Government undertakes to found the meteorological observatories which are necessary in the Färoes and Iceland. Moreover, in return for the concessions it has made to the Company, that same Government will reserve to itself the right of superintending and controlling the laying and administration of the lines, so that the service shall leave nothing to be desired within reasonable limits. In case of failure the Government will reserve to itself the right to take over the entire system, if the management provided by the Company should show itself to be inefficient.

The Danish Meteorological Institute is full of confidence in the interest which several Institutes have evinced for the matter, and as it enjoys the support of its Government, it ventures to put forth the proposal above stated in order to hasten its realisation, and it requests the parties interested to be so good as to turn the project over in their minds, and, when the time comes, to draw the attention of their respective Governments to the plan.

APPENDIX XIX.

Explanatory notes on the proposal made by von Bezold and Neumayer with reference to the publication of ten-day reports of the weather.

There is no doubt that meteorologists feel a great desire to receive, as early as possible, intelligence as to important non-periodic variations of weather, over a more extensive area than is now available. Up to the present date, however, we are forced, if we want to study weather anomalies lasting for a long time, and affecting a considerable area, to search through volumes of annual publications. These do not come out until long after date, and until a time when the personal impression of the meteorological events, and the actual interest in them has, in a great measure, faded away.

Researches which have been prosecuted by MM. Blanford, Teisserenc de Bort, Hildebrandsson, Eliot, Köppen, van Bebber, and others, on the dislocations of, and the variations of, intensity in the great centres of action of the atmosphere, have given life to systematic enquiries into the subject. The work which MM. Pettersen and Meinardus have carried out, on the meteorological relations between the North Atlantic and Europe, have shown clearly that by similar enquiries we might achieve the discovery of the actual connexions between the consecutive anomalies of weather. It is needless to say more as to the necessity and the utility of prosecuting these researches.

But the continuance of this work will require a mode of publication of observations which is more convenient and more

prompt than at present. The observations should cover a considerable portion of the globe, and a period long enough to eliminate local and secondary influences.

The reflections above stated have led us to propose the following scheme.

1. If the central meteorological offices of Europe and America are ready to supply the necessary contributions, the *Deutsche Seewarte* undertakes to publish ten-day reports of the weather for the belt of the North Temperate Zone comprised between the east coast of North America, and the Pacific coasts of Asia, three times a month, *i.e.*, on the twentieth day after the termination of each decade, or on the 1st, 11th, and 21st of each month.

2. Each International Ten-day weather report will contain :—

a. A table of ten-day mean values, actual and normal, of pressure (to 0·1 mm. reduced to sea-level and lat. 45°), of temperature (to 0·1 C.), and of atmospheric precipitation (to millimeters) for some 90 stations.

For 8 other mountain stations the barometers will not be reduced to sea-level.

For 4 additional sea-coast stations the temperature of the sea surface will alone be given.

b. A tabular and graphical summary of the individual observations of pressure, sea temperature, force and direction of wind, and, if necessary, of drift ice in the tracks of steamers between the Channel and North America.

c. A chart on which will be given for each station, the values of pressure, and the differences of temperature from the normal.

d. A space reserved for notices of extraordinary phenomena (such as squalls and storms, cyclones, and extremes of heat and cold), and for observations on the depth of snow at the commencement of each ten-day period.

3. A list of the proposed stations, 102 in number, arranged in the order of the Bulletin, is given below. In that, preference has been given to stations which send daily telegraphic reports to their own central offices.

4. The central offices are to undertake the immediate calculation of the ten-day values of pressure, temperature, and rain, for their own stations, and will forward them to the *Deutsche Seewarte* by post, at latest on the third day after the conclusion of the period.

5. The normal values for each ten-day period are also to be calculated by the central offices.

It would be well to take, once for all, as a point of departure, the same period, viz., 1861–90.

6. From time to time appendices will appear which will render it possible to fill up *lacunæ* arising from delays in receipt of reports.

7. In summer the communication with Greenland being established, the ten-day bulletin may be completed by observations made at several stations in the Färoe Islands, Iceland, and Greenland, as an appendix, in case of the delay in the transmission of these reports. With the co-operation of the Meteorological Institute of Denmark, it will be possible to give, at the beginning of the summer, a supplement giving all the ten-day values for these stations, for the period of the preceding winter.

The subjoined table of stations, arranged in geographical order, will give an idea of the work apportioned to each central office :—

LIST OF STATIONS.

1. *U.S. Weather Bureau, Washington, D.C.*
Merida, Colon, Kingston, St. Thomas, Barbados, Portland (Or.), San Francisco, San Diego, Phenix, Salt Lake City, Miles City, Dodge City, Galveston, Duluth, St. Louis, Key West, Jacksonville, Washington, Boston, Blue Hill, Edmonton, Minnedosa, Toronto, Father Point, Sydney, St. John's, Bermuda 27

2. *Observatorio do Infante Dom Luiz, Lisbon.*
Angra, Funchal, Lisbon 3

3. *Instituto Central Meteorológico, Madrid.*
Corunna, Madrid, Mahon 3

4. *Bureau Central Météorologique de France, Paris.*
Oran, Biskra, Tunis, Iles Sanguinaires, Clermont, Paris, St. Mathieu, Pic du Midi, Puy-de-Dôme 9

5. *Meteorological Office, London.*
Valencia, Hurst Castle, Fort William, Ben Nevis, Sumburgh Head, Lerwick 6

6. *Norsk Meteorologisk Institut, Christiania.*
Skudesnaes, Christiansund, Bodö, Vardö, Fruholm, Udsire 6

7. *Meteorologiska Central-Anstalten, Stockholm.*
Stockholm, Haparanda 2

8. *Ufficio Centrale di Meteorologia e di Geodinamica, Rome.*
Milan, Naples... 2

9. *Meteorologische Central Anstalt, Zurich.*
Säntis 1

10. *Central-Anstalt für Meteorologie und Erdmagnetismus, Vienna.*
Klagenfurt, Hoch-Obir, Vienna, Lesina, Tarnopol, Athens, Constantinople, Alexandria 8

11. *Ungarische Reichs-Anstalt für Meteorologie und Erdmagnetismus, Budapest.*
Szegedin 1

12. *Meteorologische Centralstation, Sofia.*
Sofia 1

13. *Meteorologisches Institut, Bucharest.*
Bucharest ... ...

14. *Nederlandsch Meteorologisch Instituut, de Bilt, Utrecht.*
The Helder 1

15. *Dansk Meteorologisk Institut, Copenhagen.*
Skagen, Bornholm 2

16. *Deutsche Seewarte, Hamburg, und Preussisches Meteorologisches Institut, Berlin.*
Keitum, Neufahrwasser, Magdeburg, Brocken, Breslau, Schneekoppe, Bamberg, Friedrichshafen, Heligoland 9

17. *Physikalisches Central-Observatorium, St. Petersburg.*
Tammerfors, Vilna, Odessa, St. Petersburg, Kharkov, Moscow, Archangel, Astrachan, Viatka, Orenburg, Yekaterinburg, Batoum, Lenkoran, Tashkend, Omsk, Barnaul, Irkutsk, Nertshinsk, Vladivostok, Nikolayevsk 20

APPENDIX XX.

SUB-COMMITTEE TO CONSIDER THE IMPROVEMENT OF THE TELEGRAPHIC WEATHER SERVICE. A PROPOSAL BY M. J. M. PERNTER.

My proposal to nominate a committee for the improvement of the telegraphic whether service is based on the following considerations :—

The daily synoptic chart of the weather is drawn on the hypothesis that the observation put down in it are synchronous, but the difference *inter se* between the times of observation is too great to be disregarded. In fact, the observations in the different countries are made according to local time, and these may differ as much as three or four hours. The difference in longitude between Ireland and Moscow, for example, is about three hours; the observations being made at Moscow at 7 o'clock and in Ireland at 8 o'clock a.m., so that the difference in time of these observations, used for the construction of synoptic charts, differs by four hours. In Austria also, the hour of observation is 7 a.m., and that produces for our stations, owing to the difference of longitude, a difference of time of three hours or more, if they are compared with observations from England and Ireland. These facts are enough to indicate the actual condition of accuracy of our synoptic charts. It cannot be proper for institutions of high authority, like the central meteorological offices, to publish isobaric charts with data differing so much in time. Moreover, any forecasts, based on such charts, will probably be faulty.

There is another inconvenience in weather telegraphy, in that the observations arrive too late. The Committee ought to discuss the means of overcoming this delay. To obtain this object the Committee should take into consideration the introduction of the telegraphic system termed the "circuit system."

For the weather forecasts, a considerable improvement might be secured if it were possible to receive the noon observations of a certain number of stations, selected according to the requirements of each central office. In Austria, we already receive such a message from twelve of our stations, and it consists of a single word in cypher. It might be possible to obtain similar reports from some stations belonging to other systems and the results would be very satisfactory.

The foregoing are the principal improvements in weather telegraphy which I have in view. The Committee will no doubt add other proposals. The importance of the subject is evident.

APPENDIX XXI.

The Results of the International Simultaneous Magnetic Observations of February 28, 1896, by Dr. A. Schmidt, of Gotha.

Among the international simultaneous magnetic observations, made at the request of the Meteorological Institute of Prussia, at the suggestion of Dr. Eschenhagen, those which were made on February 28, 1896 (between 6h. and 7h. Greenwich time), coincided with a very great magnetic storm. The observations have been published, in curves, in the *Ergebnisse der magnetischen Beobachtungen in Potsdam im Jahre*, 1896.

This mode of representation, which we were obliged to adopt in order to avoid delay in publication, is not well suited to allow of a more careful examination of the causes which produce magnetic storms.

In order to affect this object, it would be better to determine the acting forces, and to combine them in a suitable table as MM. Rücker and Thorpe have done in their enquiry into local disturbances, and also Mr. Bigelow in his investigation of the storm of January 5, 1892.

M. Schmidt as well as the reporter have already shown, more than once, the necessity which exists to study magnetic disturbances in this way, and, when we are dealing only with the horizontal components, to prepare synoptic charts for very brief intervals of time. For that purpose the demand for simultaneous observations made at very brief intervals, has been one of the considerations which has led to the organization of such observations.

M. Schmidt has undertaken the very meritorious labour of calculating the acting forces during the above-mentioned hour of observations, and laying down, on charts, the resultants of the horizontal components at different moments.

Meanwhile it has not been possible to employ individual values, for at several stations, owing to insufficient damping, the free oscillations of the needles were apparent in too many of the isolated observations, and this led to errors which were far too great to be negligeable. We then combined the 12 observations taken in each minute, from 0″ to 55″, and it is the mean of each minute which has been employed for the calculation of the vectors. The result was that at the moment of deviations, which were great, and were changing rapidly, the acting forces at neighbouring stations converged sensibly to a point situated at some distance, or emanated from that point, while they had a tendency to remain parallel during the moments of relative calm.

From this it appears that magnetic storms should be attributed to electrical currents which move round centres of action, like the cyclones or anticyclones of our atmosphere.

At the moments of the greatest agitation of the needles these centres of action should be sought for at distances from the point of observation which are relatively small, while during the relative calms, when the vectors are nearly parallel to each other, we may conclude the existence of a centre of action which is at some distance.

The velocity with which these whirls displace each other is nearly of the order of magnitude of 1 kilom. per second. If we consider the vertical forces also, which permit us to determine the position of the system of currents according to the simple law of Ampère, it comes out that we must look for the currents, at any rate according to the general character of the phenomenon, above the surface of the ground, *i.e.*, in the atmosphere. Naturally an external whirl, which changes its intensity and its position, should induce currents in the soil and in the interior of the earth. This effect may be remarked in the phenomenon.

A better idea of what has just been said may be found by a study of the small maps Dr. Schmidt has drawn.

The stations which have been used, and are indicated on the maps by their initials, are Kew, Utrecht, Wilhelmshaven, Kiel, Paris, Darmstadt, Göttingen, Potsdam, Pola, and Vienna.

Each map bears on its face the minute to which it refers, and no other explanation is required. Nevertheless Fig. 5, which relates to the same minute as Fig. 4, presents an appearance which is quite different. This point will be dealt with subsequently.

If this system of vectors be considered, it must be remembered that, in general, the direction of the current is normal to the direction of the force. This relation is only approximative because the observed force is produced not only by the part of the current which is nearest to the post of observation; it is in reality the resultant of all the currents acting at that moment over the entire surface of the globe.

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Consequently, the more regular and circular is the whirling current, the more intense will it be, and the angle formed between the vector of the force and the direction of the current will approximate to a right angle.

We see, in fact, that in particular in Figs. 2, 3, and 6 the lines of current have an almost circular form, and are always normal to the *radii vectores*.

If the maps are examined from this point of view it will easily be remarked that Figs. 2, 3, and 4 tend to agree *inter se*, and indicate a whirling motion becoming stronger and stronger. Fig. 5 also shows a whirling motion, but at a greater distance, that is to say, with a more distant centre.

FIG. 2.

6h. 2m.

FIG. 3.

6h. 7m.

At the same time certain irregularities appear in Fig. 5, and these may be attributed to the more distant whirl. In this way Fig. 6 has been obtained, which allows us to detect a new circular

FIG. 4.

6h. 12m.

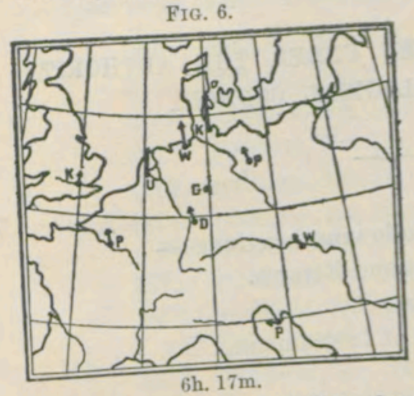
FIG. 5.

6h. 17m.

whirl appearing in the north. The direction of its current is moreover opposed to that of its predecessor. So it should be noted that while the vectors in Figs. 2 to 5 diverge, those which

refer to the new whirl, and are represented in Fig. 6, tend to converge, and the same is perceived in Fig. 7.

FIG. 6.



6h. 17m.

FIG. 7.

6h. 39m.

Numerous magnetic disturbances, noted in the older systems of magnetic observations, have led to analogous conclusions when they are studied in the same way.

We can only refer the reader, for the other considerations deduced from his observations by Dr. Schmidt, to his original paper, *Ueber die Ursache der magnetischen Ströme*, which came out in the number of the *Meteorologische Zeitschrift* for September, 1899.

It has appeared sufficient for me to point out here the principal results, in order to show the importance of organizing additional simultaneous observations, to be studied in the way which has been explained, and which has proved so extraordinarily fruitful in results.

VON BEZOLD.

ADDENDUM TO M. VIOLLE'S REPORT ON RADIATION.

READINGS OF CROVA'S ACTINOMETER, REFERRED TO ON PAGE 51.

——	Time. m.	Time. s.	Position of Index.	Differences.
1.—In Shade	0	0	89·8	2·0
"	0	30	91·8	2·0
"	1	0	93·8	2·1
"	1	30	95·9	2·0
"	2	0	97·9	2·0
"	2	30	99·9	1.9
"	3	0	101·8	7 7
2.—In Sun	3	30	109·5	11·0
"	4	0	120·5	10·6
"	4	30	131·1	10·6
"	5	0	141·7	10·3
"	5	30	152·0	9·4
"	6	0	161·4	—
3.—In Shade	6	30	161·4	—
"	7	0	163·5	—

The index fixed itself at 163·5, and remained there stationary for several minutes.

LIST OF PUBLICATIONS ISSUED UNDER THE AUTHORITY OF THE METEOROLOGICAL COUNCIL.

The list is arranged under the following headings :—

1. Periodical Publications and Reports.
2. Occasional Publications.
3. Instructions in the use of Instruments.
4. Marine Meteorology.
5. Miscellaneous Publications.

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1. Periodical Publications.

Daily Weather Report. Subscription, £1 per annum. Single copies, 1*d*. each.

Weekly Weather Report. With Appendices and Monthly Supplements priced separately :—

*1888. Vol. V. (Official, No. 85.) 4*d*. per week. Annual subscription, including Supplements and Appendices, 21*s*. 2*d*.

1889–1900. Vols. VI.–XVII. (Official Nos. 86, 87, 96, 100, 107, 111, 116, 121, 128, 133, 138, 144.) 6*d*. per week. Annual subscription, including Supplements and Appendices, 30*s*.

Monthly Weather Reports :—

1884. (Official, No. 62.) Jan.–March, May–Nov., 1*s*. 6*d*. each ; April (with two Appendices), 2*s*. 6*d*. ; Dec., 1*s*. 9*d*.

1885. (Official, No. 65.) Jan. to Dec., 1*s*. 6*d*. each

1886. (Official, No. 68.) Jan. to Dec., 1*s*. 6*d*. each.

†1887. (Official, No. 77.) Jan. to April, 1*s*. 6*d*. each ; May to Dec., in wrapper, 12*s*.

* The publication of the Weekly Weather Report began in February 1878. Annual subscription, including supplements and appendices, post paid, 1878–1883, 12*s*. 6*d*. ; 1884–1887, 21*s*. 2*d*.

† The publication of the Monthly Weather Report was continued after this date as a Supplement to the Weekly Weather Report.

1. Periodical Publications—*continued*.

Quarterly Weather Reports :—

1869. (Official, No. 7.) Parts I. to IV. 5*s*. each.

1870. (Official, No. 9.) Parts I. to IV. 5*s*. each.

1871. (Official, No. 14.) Parts I. to IV. 5*s*. each.

1872. (Official, No. 16.) Parts I. to IV. 5*s*. each.

1873. (Official, No. 19.) Parts I. to IV. 5*s*. each.

1874. (Official, No. 25.) Parts I., II., and IV., 5*s*. each ; Part III., 5*s*. 9*d*.

1875. (Official, No. 30.) Parts I. to IV. 5*s*. each.

1876. (Official, No. 33.) Part I., 6*s*. ; Parts II., III., and IV., 5*s*. each.

1877. (Official, No. 52.) Part I., 10*s*. ; II., 5*s*. ; III., 4*s*. 6*d*. ; IV., 6*s*. ; Appendices and Plates, 27*s*.

1878. (Official, No. 55.) Parts I. to IV., 6*s*. each. Appendices and Plates, 28*s*.

1879. (Official, No. 49.) Parts I. to III., 6*s*. each ; IV., 5*s*. 6*d*. ; Appendices and Plates, 27*s*.

1880. (Official, No. 50.) Parts I. and II., 6*s*. each ; III., 4*s*. ; IV., 6*s*. ; Appendices and Plates, 28*s*.

ANNUAL Volumes :—

Reports of the Meteorological Committee :—

1867. (Official, No. 1.) 1*s*.

1868. (Official, No. 5.) 5*d*.

1869. (Official, No. 6.) 10*d*.

1870. (Official, No. 10.) 10*d*.

1871. (Official, No. 15.) 10*d*.

1872. (Official, No. 17.) 1*s*.

1873. (Official, No. 22.) 4*d*.

1874. (Official, No. 26.) 6*d*.

1875. (Official, No. 29.) 4*d*.

1876–77. (Official, No. 31.) 3*s*. 5*d*.

Reports of the Meteorological Council :—

1877–78. (Official, No. 35.) 1*s*.

1878–79. (Official, No. 38.) 5*d*.

1879–80. (Official, No. 41.) 1*s*.

1880–81. (Official, No. 42.) 1*s*. 2*d*.

1881–82. (Official, No. 48.) 1*s*.

1882–83. (Official, No. 58.) 10½*d*.

1883–84. (Official, No. 64.) 1*s*. 2*d*.

1884–85. (Official, No. 67.) 4*s*. 4*d*.

1. Periodical Publications—*continued.*

ANNUAL Volumes—*continued.*

Reports of the Meteorological Council—*continued.*

1885–86. (Official, No. 72.) 8*d.*
1886–87. (Official, No. 75.) 8*d.*
1887–88. (Official, No. 79.) 1*s.*
1888–89. (Official, No. 84.) 5½*d.*
1889–90. (Official, No. 91.) 7½*d.*
1890–91. (Official, No. 99.) 5½*d.*
1891–92. (Official, No. 104.) 6*d.*
1892–93. (Official, No. 109.) 8*d.*
1893–94. (Official, No. 112.) 7½*d.*
1894–95. (Official, No. 119.) 8½*d.*
1895–96. (Official, No. 122.) 8½*d.*
1896–97. (Official, No. 130.) 8*d.*
1897–98. (Official, No. 136.) 11*d.*
1898–99. (Official, No. 140.) 7½*d.*

Observatories and Stations.

*Hourly Readings from the Self-Recording Instruments at the . . . Observatories under the Meteorological Council :—

1881. (Official, No. 51.) Part I., 10*s.* 6*d.*; Parts II. III., and IV., 21*s.* each.

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