OFFICIAL No. 164.

# REPORT

OF THE

# INTERNATIONAL METEOROLOGICAL COMMITTEE.

## SOUTHPORT, 1903.

Published by Authority of the Meteorological Council.

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# REPORT

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# INTERNATIONAL METEOROLOGICAL COMMITTEE.

## SOUTHPORT, 1903.

### MEETING AT PARIS.

The following extract from the Minutes of the Meeting of the Committee held in Paris in 1900, in connexion with the Meteorological Congress there assembled, is reproduced for purposes of reference.

Meeting of the Meteorological Committee of September 15th, 1900.

The Meeting commenced at 2 p.m.

PRESENT:

MM. Mascart, President; Davis, Hepites, Hildebrandsson, Mohn, Pallazzo, Paulsen, Pernter, Rykatchew, Shaw, and Snellen.

Invited to attend for the discussion of special questions:—MM. Walz and Chaves.

1. MM. Palazzo and Shaw were elected Members of the Committee in place of MM. Tacchini and Scott, resigned.

2. M. Hildebrandsson was elected to the office of Secretary to the Committee in place of Mr. Scott, who had performed the duties of this office since the creation of the Committee with unfailing zeal, which would always be remembered.

M. Hildebrandsson thanked his colleagues and took his seat as Secretary.

3. M. Rücker was re-elected President of the Magnetic Committee.

4. On the recommendation of this Committee, it was resolved that the Directors of the Magnetic Observatories should be requested to send to the Secretary of the Committee periodically a list of the days which they considered calm. These documents should be forwarded to the observatories concerned.

5. The Cloud Committee brought forward a request that the Directors of Meteorological Observatories should make simultaneous cloud observations at times to be fixed in advance by the Aeronautical Committee. After some discussion, the Committee instructed the Secretary to inform the Directors that it was considered desirable to organise such observations everywhere.

6. The Aeronautical Committee have expressed the opinion that it is desirable for the military ballooning establishments, and the meteorological offices to be requested by their respective Governments to take part in the international ascents, as is already done in several countries, and request the International Meteorological Committee to approach the French Government with a view to having this resolution carried out in France, and transmitted to foreign Governments through diplomatic channels.

The Meteorological Conference, at a private Meeting held on the same day, had been unanimous in urging the importance of carrying out this request. The Committee requested the President to officially approach the French Government in the manner desired by the Conference.

7. The Telegraphic Sub-Committee submitted the following resolution to the Committee:—

> "The advantages already secured by the extension of the radial system to several neighbouring countries, have induced the Sub-Committee to propose to the International Meteorological Committee that they should take steps to appoint, with as little delay as possible, a committee consisting of official representatives of all States participating, to confer with the International Telegraphic Bureau at Berne, on the most suitable methods for further improving the meteorological telegraphic service."

On the motion of MM. Pernter and Rykatchew, the Committee entrusted the President with the task of taking official steps to secure this end.

8. Mr. Walz presented a letter from Mr. Willis L. Moore on several meteorological questions.

9. M. Chaves read a letter from M. Brito Capello, who was prevented from attending the Meeting.

10. After some discussion, the Committee decided that it would be premature to definitely fix the date of their next Meeting or of that of an International Meteorological Congress.



The Committee expressed their thanks to Mr. Moore for an invitation, which he had sent, to hold a Meeting in Washington, and to Mr. Shaw for a proposal to meet in London.

The Meeting adjourned at 4 p.m.

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## MEETING AT SOUTHPORT.

### Provisional Programme.

1. A letter from Sir Arthur Rücker on terrestrial magnetism.

2. Report by M. Hergesell on aeronautical ascents.

3. Report by M. Hildebrandsson on international work relating to clouds.

4. Report by M. Violle on radiation and insolation.

5. Proposals by the Telegraphic Sub-Committee.

6. Various questions:—

(1.) The Royal Academy of Sciences of Saxony has communicated to us the text of the resolutions adopted on May 14th, 1901, by a Conference of Delegates from various Academies on the subject of the scientific organisation of researches relating to atmospheric electricity. The Conference resolved to forward these resolutions to the International Meteorological Committee.

(2.) M. Paulsen desires to raise a discussion of the measurement of the electrical potential of the atmosphere by means of collectors of radio-active salts. He will show the Committee the apparatus which the Danish expedition for the investigation of the aurora borealis used in Finland during the winter, 1900-1901.

(3.) MM. Pernter, Snellen, Billwiller, and Palazzo think that it is important for the Committee to consider all questions on the programme of the Sub-Committee for International Meteorological Telegraphy. M. Billwiller adds that this is not merely a question of arrangement with the telegraphic services, but also of agreement on several important points between the various Meteorological Offices.

(4.) In order to diminish the number of unforeseen gales, it is important to introduce an exchange of evening telegrams on the day of observation, and thus to organise a night service. It is further desirable to ascertain whether it be possible to introduce such a service immediately.

What steps would have to be taken to realise this project as soon as possible? (M. Rykatchew.)

(5.) On the utility of including observations of the direction of motion and relative speed of clouds in meteorological telegrams. (M. L. Teisserenc de Bort.)

(6.) Could not the Committee make arrangements for days of magnetic perturbation, similar to those already jointly adopted in 1900 for calm days? If the days selected by the Observatories of Greenwich and Paris were known sufficiently early, I have no doubt that many Directors would be willing to publish their figures for the same days. This is very difficult if the dates can only be ascertained by special correspondence. (Rev. J. de Moidrey, Zi-Ka-Wei.)

(7.) Discussion of the relation between Meteorology and Astrophysics; and the desirability of appointing a Committee to deal with these studies. (Sir Norman Lockyer and Mr. Shaw.)

(8.) Observations on radiation at the principal observatories. (M. Pernter.)

(9.) The substitution of the hair hygrometer for the psychrometer, in consequence of some interesting experiments made in Austria. (M. Pernter.)

(10.) The resolution adopted by the Conference held at Copenhagen (1882), that the occasions on which it was snowing, raining, &c., at the hour of observation, be noted in the column headed "Amount of Cloud" (nébulosité), ought to be reconsidered and modified. (M. Hellmann.)

(11.) It would be preferable to measure the height of the layer of snow on the ground every day at the hour of making the morning observation, and to publish this in a separate column instead of inserting the symbol [*] (ground covered with snow) in the "Remarks" column. (M. Hellmann.)

(12.) The value of the height of the anemometer above the level of the ground should always be given at the head of all published tables of wind velocities. (M. Hellmann.)

(13.) The drawing up of general rules of procedure for the Meetings of the International Committee (Feststellung der Normen für die Sitzungen des Internationalen Comités). (M. Pernter.)

(14.) It is necessary to define the relations which should exist between the Committee and the special Committees ("Commissions") which it appoints, more definitely. M. Pernter.)

(15.) The official publication of an international meteorological handbook; containing all important resolutions



adopted by the various Meteorological Congresses and Conferences which have met between 1872 and the present time, together with comments and explanatory notes on the same. (M. Hellmann.)

(16.) Would it not be desirable for those who suggest a subject for discussion by the Committee, to draw up and distribute among the Members of the Committee a short report on the same, a month before the Meeting? (M. Pernter.)

(17.) The convention of a General Meeting of the Directors of the different Meteorological Offices in the year 1905, similar to the Meetings held in Munich and Paris in 1891 and 1896 respectively.

(18.) How are we to distinguish rain which falls from fog? (Mr. Rotch.)

(19.) Should snow which is melting as it falls be recorded as snow, or as rain; how should its thickness be measured in cases in which it becomes appreciable? (Mr. Rotch.)

(20.) The definition of a thunderstorm, adopted by the International Conference which met in Paris in 1896, as the result of a discussion which I had raised, appears to me to be unsatisfactory. Would it not be better to indicate by the symbol ꝶ those occasions on which thunder is heard and rain observed, instead of limiting it to those on which thunder and lightning are observed? It is obvious that if thunder is heard, but no lightning seen, special circumstances must have prevented the latter from being noticed. (Mr. Rotch.)

## MINUTES OF THE MEETINGS.

First Meeting, Wednesday, September 9th, 1903.

The Meeting commenced at 10 a.m. in the Council Chamber of the Town Hall.

**Present:**

MM. Mascart, President; Chaves, Hellmann, Mohn, Paulsen, Pernter, Rykatchew, Shaw, Snellen, and Hildebrandsson, Secretary.

Present by invitation: MM. van Bebber, Hergesell, Rotch, and Teisserenc de Bort.

Prevented from attending: MM. Billwiller, Hepites, Palazzo.

1. The President, in opening the Meeting, referred to the death of M. Capello, and expressed the regret of the Committee at no longer counting MM. Scott and von Bezold among their number. He concluded by welcoming MM. Chaves, Shaw, and Hellmann.

2. Mr. Shaw read a letter from the Royal Society of London wishing success to the labours of the Committee and welcoming them to England (Appendix I.). The President returned thanks in the name of the Committee, who resolved that a letter should be addressed to the President of the Royal Society.

3. The Secretary then read the following report:—

Since the Meeting in St. Petersburg in 1899, several of our colleagues have retired from the Committee, and we have heard with regret of the death of Admiral Brito Capello. In the first instance, we regret the loss of Mr. Scott, who had carried on the duties of Secretary with great zeal since the creation of the Committee. In his place the Committee have elected Mr. Shaw as a Member of the Committee, and M. Hildebrandsson as Secretary. Further, M. Tacchini has been replaced by M. Palazzo, Admiral Brito Capello by M. Chaves and M. von Bezold by M. Hellmann.

At the Meeting held in Paris on September 15th, 1900, it was resolved, on the motion of the Magnetic Committee, that the Directors of Magnetic Observatories should from time to time send a list of the days which they regarded as calm to the Secretary, and that these lists should be communicated to the different observatories. Since that date, such communications have been sent to the Secretary by a large number of observatories. The Secretary has had them copied and distributed to the principal magnetic observatories. It seems, however, that this duty ought not to fall upon the Secretary of the Committee but upon one of the Members of the Magnetic Committee, which will in future carry out this work, especially if it should be increased by the present Meeting in accordance with the proposal of the Rev. J. de Moidrey.

In accordance with the request of the Cloud Committee, the Committee instructed the Secretary to inform the Directors that it was desired that simultaneous observations of clouds should be made at times previously fixed by the Aeronautical Committee. The Secretary communicated this request to the Directors by means of a circular, dated 1st October, 1900, and since then, the observations desired have been made at a large number of observatories.

At the request of the Aeronautical Committee and of the Meteorological Conference, the Committee requested the President to approach the French Government and to ask it to officially request military ballooning establishments and meteorological offices to take part in the international ascents. The co-operation of England and the Scandinavian countries was especially desired, as these countries are situated on the paths most frequently followed by disturbances.



This official application led to no result.

After this, M. Mascart put himself into direct communication with the meteorologists of Scandinavia, and the outcome of this preliminary correspondence was that he formally invited Denmark and Sweden to join France in founding, at joint expense, a station for aerial soundings in Denmark, on the track most frequented by storms, and there to carry out their researches in a systematic manner during several months. Finally, M. Teisserenc de Bort, who has taken so active a part in this work at his observatory at Trappes, announced that he was willing to lend the project effective help.

M. Mascart's invitation was eagerly accepted; the three Governments have each appointed a member of the directorate of the station: M. Teisserenc de Bort represents France, M. Paulsen, Denmark, and M. Hildebrandsson, Sweden; a large tract of moorland country belonging to the Manor of Hald, near Viborg, was gratuitously put at the disposal of the observers by its owner, M. Krabbe; the station was organised during the spring of 1902, and work was carried on without interruption, often continuously by day and by night, from July 11th, 1902, up to May 10th, 1903, under the direction of M. Teisserenc de Bort. The observations have already been published, and we hope that a report on the work will be read before the Committee by M. Teisserenc de Bort.

On the motion of M. Mohn, the Conference in Munich, in 1891, resolved to recommend all meteorologists to give barometric readings reduced to standard gravity, at latest, after 1st January, 1901. At the request of M. Mohn, the Bureau sent a circular to all directors in November, 1900, proposing to put the resolution of the Munich Conference into practice after January 1st, 1901, by adopting the following measures:—

1. The height of the barometer from stations which report to the Central Offices by telegraph, should always be reduced to standard gravity.

2. It should be stated in all published tables of observations whether the barometric heights have been reduced to standard gravity or not, and the value of the correction that has been applied to the readings when the reduction has been made, or

which ought to be applied in the contrary case should be given.

Almost all countries have now adopted these measures.

(Signed) E. MASCART,

H. HILDEBRANDSSON.

Southport, 9th September, 1903.

4. The President read a letter from Sir Arthur Rücker, President of the Magnetic Committee, stating that M. Rijkevorsel wished to retire from the Committee on which he could be more usefully replaced by M. Snellen. Sir Arthur Rücker further desired to resign the Presidency of this Committee, on account of new duties which he had undertaken.

The Committee decided to replace M. Rijkevorsel by M. Snellen, and asked Sir Arthur Rücker to retain the Presidency of the Committee. In case of his insisting on retiring, the Magnetic Committee should be invited to appoint a successor.

5. M. Hergesell, President of the International Aeronautical Committee, read a report on the work of this Committee. (Appendix II.)

M. Rykatchew made some remarks on this report. He thought it was desirable that the construction and verification of the instruments used should be carried out in the country where the observations were made. It would thus be necessary to have in each country an institution whose duty it was to carry out this work, say, for instance, the central meteorological offices. These institutions would have to see that the instruments constructed in different countries yield comparable results.

M. Teisserenc de Bort read a communication on the aerial soundings carried out at Trappes, and on the work of the Franco-Scandinavian Station at Hald (Appendix III.). He handed in a copy of all the observations made at Hald.

Mr. Shaw read a report on the kite observations made at sea by Mr. Dines, in the neighbourhood of Ben Nevis, in Scotland (Appendix IV.).

Mr. Rotch read a note on his experiments at sea which had been the first example of their kind (Appendix V.).

M. Hellmann requested the Committee to print as an Appendix, a report by M. Assmann on the work of the Aeronautical Observatory at Berlin (Appendix VI.).

M. Rykatchew stated that he had made some attempts to raise kites at sea in the Gulf of Finland at the commencement of August, 1903. Experience on a day of calm and on another of strong wind, showed that it was possible to work satisfactorily on both occasions.



6. The Meeting was interrupted for some minutes by the presentation of the Members of the Committee to the Mayor of Southport.

7. Mr. Willis L. Moore took part in the Meeting, and was greeted by the President.

8. M. Hildebrandsson handed in the first part of his report on the international work relating to clouds.* M. Hellmann handed in a volume containing the results of the observations and measurements of the movement and height of clouds at Potsdam in 1896-1897.

9. The President announced that M. Violle had been hitherto prevented from preparing his report on radiation on account of family bereavement.

10. On the motion of Mr. Shaw, the Committee resolved to take part in the Meetings of the British Association on the mornings of the following Thursday, Friday, Monday, and Wednesday. The Meetings of the Committee would be held in the afternoon from 3 to 5 o'clock.

11. Question 6.—"Could not the Committee make arrangements for days of magnetic perturbation similar to those already adopted in 1900 for calm days? If the days jointly selected by the Observatories of Greenwich and Paris were known sufficiently early, I have no doubt that many directors would be willing to publish their figures for the same days. This is very difficult if the dates can only be ascertained by special correspondence." (Rev. J. de Moidrey.)

After some discussion the Committee resolved:—

(1.) That M. Snellen should be instructed to receive and distribute the observations on calm days, and

(2.) That the question of disturbed days should be referred to the Magnetic Committee.

12. Question 7.—"A discussion of the relation between Meteorology and Astrophysics; and the desirability of appointing a committee to deal with these researches."

Sir Norman Lockyer had distributed a report on this subject to the Members of the Committee. (Appendix VII.)

At Mr. Shaw's request, this question was to be discussed at a Meeting of the British Association before the question of the appointment of a sub-committee was considered by the Committee.

The Meeting adjourned at 1 p.m.

(Signed) E. MASCART.

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* The Meteorological Council have arranged with the Council of the Royal Meteorological Society for a translation of this report to appear in an early number of the Society's Quarterly Journal.

Second Meeting, Wednesday, September 9th, 1903.

The Meeting commenced at 3 p.m.

PRESENT:

MM. Mascart, President; Chaves, Hellmann, Mohn, Paulsen, Pernter, Rykatchew, Shaw, Snellen, and Hildebrandsson, Secretary.

Present by invitation: MM. van Bebber, Hergesell, Rotch, and Teisserenc de Bort.

1. Question 2.—" M. Paulsen desires to raise a discussion of the question of the measurement of the electric potential of the air by means of radio-active salts. He reports to the Committee that this method was used by the Danish expedition for the investigation of the aurora borealis in Finland during the winter of 1900-1901."

M. Paulsen read a note on this subject, and exhibited the apparatus used by the Danish expedition in Finland (Appendix (VIII.) M. Mascart said that M. Moureaux had made use of the same method at the Observatory of Parc Saint-Maur and had compared it with that of the water-dropper (Appendix IX.). M. Pernter remarked that radio-active salts had been used for this purpose at the Austrian Observatories for the last two years.

The Committee recorded with satisfaction that the method of using radio-active salts is capable of giving as great sensitiveness as the methods of the water-dropper or the flame electrode, and that it presents many advantages for the study of atmospheric electricity.

2. At the President's request, M. Chaves gave an account of the meteorological organisation at the Azores. (Appendix X.)

On the motion of the President, the Committee unanimously adopted the following resolution:—

> The Committee have heard M. Chaves' account of the meteorological and magnetic work carried on at the Azores, with the active support of the King of Portugal, with great interest. The Committee respectfully beg to thank His Majesty the King of Portugal for the encouragement given to a scientific institution of the greatest importance to the physics of the globe.

3. Question 8.—"Observations on radiation at the principal observatories" (M. Pernter). M. Pernter read a memorandum on this subject.

After M. Pernter's review of the progress which had been made in the construction of actinometers by MM. Ångström, Chwolson, and others had been read, the Committee insisted anew on the importance of observatories making researches on solar radiation of the greatest possible accuracy.



4. Question 9.—" The substitution of the hair hygrometer for the psychrometer" (M. Pernter). M. Pernter gave an account of his experiments with the hair hygrometer. (Appendix XI.)

The Secretary stated that this subject had been discussed at almost all Meetings of Meteorological Committees and Conferences. He also stated that the experiments of M. A. Svensson with Assmann's aspirating psychrometer at very low temperatures had shown that it was possible to determine the humidity of the air with great accuracy by means of this instrument, provided that the true tensions of water-vapour given off by ice were used in the calculations.

M. Rykatchew said that the psychrometer gave identical results when exposed under identical conditions; whereas the hair hygrometer does not always give the same readings when exposed under similar conditions. It is for this reason that the psychrometer is preferable on all occasions except when the temperature is very low, much below the freezing point, and above all when it falls to values equal to or below —20° C. (—4° F.).

M. Rykatchew gave a summary of the results obtained from his experiments in Russia (Appendix XII.). Mr. Shaw was of opinion that it would be useful to make experiments with a view to evolving a satisfactory method of determining the humidity of the air by the formation of an artificial cloud. In nature, the humidity of the atmosphere is one of the most variable, and at the same time one of the most important elements, and one of the most distinctive phenomena which it presents is condensation and evaporation in the free atmosphere, and not only on solid or liquid surfaces or on the fibrous materials used in the construction of hygrometric instruments. He did not think that it would be difficult to determine experimentally the conditions which involve the formation of a cloud, and to deduce from them the humidity of the air.

M. Mascart remarked that it is very difficult for an observer at a station of the second order to know whether his hygrometer was in good order or not. Some further remarks were made by MM. Snellen, Mohn, Hergesell, and Hellmann.

Replying to the different objections, M. Pernter remained convinced on the result of his own experiments, that the use of the hair hygrometer satisfied all demands of Meteorology, provided only that the saturation point was verified at intervals. He did not ask to take a vote of the Committee on this question.

The meeting adjourned at 5.15 pm.

(Signed) E. MASCART.

## Third Meeting, Thursday, September 10th.

The Meeting commenced at 3 p.m.

PRESENT:

MM. Mascart, President; Chaves, Hellmann, Mohn, Paulsen, Pernter, Rykatchew, Shaw, Snellen, and Hildebrandsson, Secretary.

Present by invitation: MM. van Bebber, Hergesell, Rotch, and Teisserenc de Bort.

1. Sir Arthur Rücker stated in a private letter to Mr. Shaw, that, at the request of the Committee, he would agree provisionally to retain the Presidency of the Magnetic Committee until its next Meeting.

2. Question 10.—"The resolution adopted by the Conference at Copenhagen (1882), of noting in the column headed "Nébulosité," the occasions on which it was raining, snowing, &c., at the hour of observation, ought to be reconsidered and modified." (M. Hellmann.)

After a discussion, in which several Members took part, M. Hellmann amended his motion as follows:—"It is recommended that the principal figure in the column headed 'Nébulosité'" should have added to it a suffix signiyfing rain, snow, fog, hail, or sunshine at the moment of making the observation. For example:—

$8_{\bullet}$, $6_{*}$, $7_{\triangle}$, $4_{\odot}$, $5_{\equiv}$ etc."

Without insisting on the adoption of the method here indicated, the Committee think that it is in all cases desirable to state which of the above phenomena was observed at the time of making the observation.

To this M. Rykatchew remarked:—

"M. Hellmann asks us to make and to publish observations of sunshine at the fixed hours. This question has not been announced in the programme, except that in his Note (Appendix XVI.) distributed at the Meeting of the Committee, he speaks of it as though it only concerned the form in which the observations are published. It must be remembered that the international form for the publication of the observations made at stations of the second order, which was agreed to after many difficulties had been overcome, represents the minimum of what is required. Each country is at liberty to increase the number of data contained in the form without being under the necessity of obtaining the sanction of the Committee. But it is necessary to be very prudent if we intend to increase the minimum demanded from all countries. In Germany, observations of sunshine are published in the column 'Nébulosité.' Very good! I believe that we are doing no harm by publishing heliographic and other observations, which are not obligatory, from a large number of stations of the second order, in addition to the minimum demanded.

"M. Hellmann proposes to make a further observation which does not in this case present any difficulty; nevertheless, it increases the labour of the observers whose co-operation is given gratuitously, and also that of the assistants who reduce the observations and prepare them for publication; it also increases the expense. In Russia it would be necessary to issue a special instruction, to correspond with all the 900 observers, to pay more for each form used and to pay for the supplementary labour required to prepare the observations for publication. I shall have to consider very carefully whether we are in a position to introduce this new observation without asking for additional credit or without being obliged to refrain from publishing other observations which are not obligatory for stations of the second order, but which in my opinion are as important as observations of sunshine. I ask you, therefore, not to regard observations of sunshine as obligatory for stations of the second order. I recognise their utility, and I shall try to introduce them if the circumstances I have mentioned do not prevent me from doing so."



3. Question 11.—"Instead of putting the symbol [✱] (ground covered with snow) in the 'Remarks' column, it is preferable to publish the height of the snow layer at the hour of making the morning observation in a special column." (M. Hellmann.)

After a discussion in which a large number of Members joined, the Committee decided to retain the symbol [✱], and to continue to publish the observations in their present form, but they also thought that it was desirable to publish the height of the snow-layer on the ground in the tables of those countries in which such measurements are regularly made.

4. Question 12.—"The height of the anemometer above the ground ought always to be given at the head of all published tables of wind velocities." (M. Hellmann.)

After some discussion, the Committee agreed that it was desirable to have the height of the anemometer above the ground always stated at the head of the tables.

5. Question 13.—"The drawing up of a code of general rules of procedure for the Meetings of the Committee." (M. Pernter.)

After some remarks by various Members, the discussion was adjourned to the following Meeting.

The Meeting adjourned at 5.30 p.m.

(Signed) E. MASCART.

Fourth Meeting, Friday, September 11th.

The Meeting commenced at 3 p.m.

PRESENT:

MM. Mascart, President; Chaves, Hellmann, Mohn, Moore, Paulsen, Pernter, Rykatchew, Shaw, Snellen, and Hildebrandsson, Secretary.

Present by invitation: MM. van Bebber, Hergesell, Rotch, and Teisserenc de Bort.

1. M. Teisserenc de Bort handed in the publication of the observations of clouds made at Trappes in 1896-1897.

2. The discussion of Question 13 was resumed.

The Committee, while appreciating the justice of the remarks made by M. Pernter on various difficulties connected with the Meetings, thought that these might be arranged for in the correspondence which preceded the Meetings.

3. "Would it not be desirable for those who suggest a subject for discussion by the Committee to draw up and distribute among the Members of the Committee, a short report on the same a month before the Meeting." (M. Pernter.)

After a short discussion, M. Pernter's motion was agreed to unanimously.

4. Question 14.—"It appears to be desirable to define the relations which should exist between the Committee and the Sub-Committees ('Commissions') which it appoints, more definitely." (M. Pernter.)

The Committee think that it is desirable for the Presidents of the Sub-Committees to communicate with the officers of the Committee when arranging the dates of their Meetings, a practice which has prevailed hitherto. The Secretary would inform Members of the dates agreed upon.

M. Hergesell stated that he had taken care to consult the President of the Committee about the Meeting of the Aeronautical Committee at Berlin in 1902. He announced officially that the next Meeting of this Committee would be held at St. Petersburg in August, 1904.

5. Question 7.—"Discussion of the relation of Meteorology to Astrophysics."

The Members of the Committee had taken part in a discussion of this subject at a Meeting of Section A of the British Association; and Mr. Shaw proposed that a Committee should be appointed to review and discuss meteorological observations from the point of view of their connexion with solar physics. Mr. Shaw's motion was adopted, and MM. Lockyer, Shaw, Pernter, and Angot were elected to serve on this Committee, with power to add to their number and to elect their officers.



6. Mr. Shaw proposed that the attention of Section A of the British Association be called by the International Meteorological Committee to the desirability of introducing greater uniformity in the units adopted in Meteorology, and that the Section be asked whether it did not think that the time had come for taking steps to secure this uniformity.

After some discussion, the Committee decided to call the attention of Section A of the British Association to the inconveniences which result from the want of uniformity of the units employed in meteorological observations, and to ask the Section to use its influence in securing such uniformity.

7. Question 15.—"The official publication of an international meteorological handbook containing the final resolutions adopted by the Meteorological Committees and Conferences which have met between 1872 and the present time, together with comments and explanatory notes on the same." (M. Hellmann.)

The Committee requested MM. Hellmann and Hildebrandsson to prepare a publication of this nature for distribution to all directors of meteorological institutions.

8. Question 8.—"How are we to distinguish rain which falls from fog?" (Mr. Rotch.)

The Committee think that no sharp distinction can be drawn between these two phenomena, and that it must be left to the discretion of the observer which of the two symbols is to be employed. M. Hildebrandsson remarked that the French language possessed in the word "bruine" a term applicable to doubtful cases.

9. Question 19.—"Ought snow which melts as it falls to be recorded as snow or as rain, and, if it is of appreciable thickness, how ought this to be measured?" (Mr. Rotch.)

After some discussion, the Committee agreed that precipitation which melts subsequently to its fall should be regarded as snow.

10. Question 20.—"The definition of a thunderstorm adopted by the International Conference, which met in Paris in 1896, as the result of a discussion which I had raised, appears to me to be unsatisfactory. Would it not be better to indicate by the symbol ☈ those occasions on which thunder is heard and rain is observed, instead of limiting it to those cases in which thunder and lightning are observed? It is obvious that, if thunder is heard but no lightning seen, special circumstances must have prevented the latter from being noticed." (Mr. Rotch.)

The Committee are of opinion that, if a true thunderstorm was experienced but special circumstances prevented the lightning from being seen, the symbol ꓘ should be used. If rain fell at the same time, the symbol ● should also be used.

The Meeting adjourned at 5 p.m.

(Signed) E. MASCART.

Fifth Meeting, Monday, September 14th.

The Meeting commenced at 3 p.m.

PRESENT:

MM. Mascart, President; Chaves, Hellmann, Mohn, Moore, Paulsen, Pernter, Rykatchew, Shaw, Snellen, and Hildebrandsson, Secretary.

Present by invitation: MM. van Bebber, Hergesell, Rotch, and Teisserenc de Bort.

1. The President read the following telegram from His Majesty the King of Portugal:—

"To the President of the Meteorological Committee, Town Hall, Southport.

"His Majesty the King of Portugal has commanded me to express his thanks to the International Meteorological Committee and to assure them of his best wishes for the progress and development of the meteorological services. The President of the Council, Hintze Ribeiro."

2. The President read a letter from M. Palazzo, in which he made the following proposal: "I think that it would be advisable for the Committee to establish the rule of sending special invitations on the occasion of a Meeting not only to the Members of the Committee, but also to the Governments on which the Members are respectively dependent."

The Committee thought that this demand of M. Palazzo was quite contrary to the custom of the Committee, and that there was no reason for altering the latter.

3. The President handed in the report of M. Violle, President of the Sub-Committee on Solar Radiation (Appendix XIII).

4. Question 1.—"The Royal Academy of Saxony has communicated to us the text of the resolutions adopted on May 14th, 1901, by a Conference of delegates from various academies on the subject of the scientific organisation of researches relating to atmospheric electricity. The Conference decided to forward



these resolutions to the International Meteorological Committee."

M. Hellmann gave a summary of the questions submitted to the Committee by the Conference convened by the Academies of Göttingen, Leipzig, Munich, and Vienna to draw up a programme for bringing about international co-operation in investigations connected with atmospheric electricity.

After various opinions had been expressed on this subject, in particular by MM. Hellmann, Pernter, Mascart, Rykatchew, Shaw, and Paulsen, the following resolution was adopted:—

"The Committee have taken note of this communication with great interest; if the International Association of Academies adopts the scheme, the meteorological observatories will all, without doubt, be disposed to lend it their assistance."

5. M. Hergesell's report (Appendix II.) ended with the following resolutions:—

"(1.) The International Meteorological Committee, in agreement with the decision adopted at Berlin by the International Committee of Scientific Aeronautics, consider that the exploration of the atmosphere over tropical parts of the ocean by means of kite ascents conducted from a steamer equipped for the purpose, an enterprise suggested by Mr. Rotch in 1901, to be one of the most important meteorological investigations of the future."

This resolution was adopted unanimously.

"(2.) The International Meteorological Committee, in agreement with a resolution adopted at Berlin by the International Committee for Scientific Aeronautics, are of opinion that the continuation of the simultaneous international ascents, which is highly desirable in the interests of science, requires the continuation of the regular publication of the results obtained."

The Committee also adopted this resolution unanimously, and expressed their thanks to the German Government for the funds which it had assigned for the publication of the important observations of 1900 to 1904.

6. The President read a letter addressed to the Committee by MM. R. T. Glazebrook and J. Larmor (Appendix XV.). The complexity of the questions therein raised was too great to permit of an answer by the Committee.

7. Referring to Question 3, the Committee expressed the following opinions:—

"(1.) That it is desirable that the English observations which are transmitted by telegraph for forecasting purposes should be made at 7 a.m. (Greenwich time).

"(2.) That it is also desirable that observations by means of kites and balloons should be organised in England as a contribution to international enterprises."

The Meeting adjourned at 5.15 p.m.

(Signed) E. MASCART.

Sixth Meeting, Tuesday, September 15th.

The Meeting commenced at 10.30 a.m.

PRESENT:

MM. Mascart, President; Hellmann, Mohn, Paulsen, Pernter, Rykatchew, Shaw, Snellen, and Hildebrandsson, Secretary.

Present by invitation: MM. van Bebber, Hergesell, Rotch, and Teisserenc de Bort.

1. M. Hellmann moved that a letter be addressed to the German Government to inform it of the vote of thanks carried at the Meeting of September 14th, in connexion with item No. 5.

The Committee accepted this proposal, and instructed the Officers to send a letter embodying the resolution taken at the previous Meeting to His Excellency the Chancellor of the German Empire.

2. The Telegraphic Sub-Committee has discussed the following questions:—

(1.) What is the best method of obtaining the most rapid interchange of meteorological telegrams? For example, in several countries, the meteorological observations are sent from the different telegraphic reporting stations directly and separately to the foreign offices at the same time as they are sent to the home office.

(2.) Would it not be an incontestable advantage both to the telegraphic services and to the meteorological offices if the transmission and receipt of meteorological messages could be accomplished within a specified limit of time, say an hour? Would it be possible to secure this end by an international arrangement similar to that already in force in several European countries; or would it be possible to reserve the telegraph lines between the central meteorological offices for the transmission of meteorological messages for the space of half an hour?



As the result of its deliberations, it proposes the following resolutions to the Committee:—

"(1.) That it is desirable that an official International Committee, comprised of representatives of the telegraphic and meteorological services, should discuss the best methods to be adopted for accelerating the transmission of the information necessary for forecasting.

"(2.) If this Committee could not be brought together, the directors of the different offices should be asked to take steps in their various countries to reduce the delay in the transmission of meteorological telegrams as much as possible."

The Committee adopted these resolutions unanimously.

3. Question 5.—"On the utility of transmitting observations of the direction and relative velocity of clouds in meteorological telegrams." (M. Teisserenc de Bort.)

The Committee thought that this question should be reserved for further study on account of the modification of the existing telegraphic code which it would involve.

M. Teisserenc de Bort added, that he wished to take advantage of the occasion to publish a study on the application of cloud observations to forecasting.

4. M. Snellen announced his intention of retiring from the Committee, as he was no longer at the head of the meteorological service of his country, and thanked the Committee for the sympathy which it had always extended to him. The President thanked M. Snellen for the services which he had rendered in connexion with the work of the Committee; he made himself the spokesman of the Committee in expressing to him the regret and sympathy of all the Members.

5. The Committee proceeded to the election of a member to replace M. Snellen. M. A. Lancaster, having obtained a majority of the votes cast, was declared elected a Member of the Committee.

6. Question 17.—"The convention of a General Meeting of the directors of the different meteorological offices in 1905, similar to those held in Munich in 1891 and in Paris in 1896."

After some discussion, the Committee voted in favour of such a Conference at Innspruck during the second week of September, 1905, and asked the officers to prepare questions for discussion, and to draw up the programme for this Conference.

7. The Committee entrusted the officers with the task of drawing up the Minutes of the present Meeting.

8. At the close of the Meeting, the President proposed to the Committee to send a letter of thanks to his Worship the

Mayor of Southport for the graceful hospitality which had been extended to them; this proposal was adopted unanimously.

The President then thanked his colleagues for the kindness which had been extended to him, and for the cordiality which had prevailed at all their deliberations. He added, that the Committee were particularly indebted to the Secretary, M. Hildebrandsson, for the care and zeal with which he had acquitted himself of his laborious task.

M. Rykatchew thanked M. Mascart, in the name of his colleagues, for the excellent manner in which he had carried out his important duties, and for the tact which he had displayed in all his speeches, while he had guided their labours with great zeal and talent. He also thanked Mr. Shaw for the hospitality with which he had received all the Members of the Committee, and, above all, for the cordial and kindly character of their reception.

The proceedings terminated at 11.45 a.m.

(Signed) E. MASCART.

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## APPENDIX I.

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The Royal Society,

Burlington House, London, W.,

June 30th, 1903.

SIR,

WE have been directed by the President and Council of the Royal Society to convey their welcome to the International Committee of Meteorology when it meets in this country next September, and to express their cordial wishes for a very beneficial Conference in regard to the international relations of this most important Science, in which the Royal Society, through its connexion with the British Meteorological Council, is specially interested.

The Conference comes at a time of year when the sittings of the Royal Society are suspended, but the President and Council do not doubt that interest will be taken by the Fellows of the Society individually in its Proceedings, which will be commensurate with the importance of its work.

We are, Dear Sir,

Yours very faithfully,

M. FOSTER,

J. LARMOR,

Secretaries, Royal Society.

The President,

International Congress of Meteorology,

Southport.

## APPENDIX II.

### REPORT OF THE PROCEEDINGS OF THE INTERNATIONAL COMMITTEE FOR SCIENTIFIC AERONAUTICS.

By M. H. Hergesell.

Since the last report was presented on the occasion of the last Meeting of the Permanent Committee at St. Petersburg. the work of the International Committee for Scientific Aeronautics has been continued on former lines, but its scope has been extended.

In the following memorandum I shall confine myself, in the main, to the work of the actual Committee, but I shall not be able to avoid mentioning the enterprises of some of our members which have no direct relation to the work of the Committee. I hope that my colleagues will present special reports on the work in question.

The Committee assembled twice, and its work was much assisted by this direct and verbal interchange of opinion.

In 1900 our Committee held several meetings during the International Meteorological Conference, held at the time of the Paris Exhibition. An account of the proceedings will be found in the official publications of the Congress in question. For details these must be consulted, and I will here only recapitulate the principal points.

In the first place, I must re-call a proposal made by M. Teisserenc de Bort and myself, calling on the Committee to organise regular ascents of manned balloons, unmanned balloons (ballons-sondes), and kites to take place simultaneously, once a month.

Secondly, I must mention a motion proposed by MM. Sprung and Teisserenc de Bort on behalf of the Cloud Committee (Paris, 1900), to include observations of clouds, and above all, of the upper clouds, in these international ascents, to complete our study of the upper regions of the atmosphere.

Both these proposals were agreed to, and what is more important, they have been carried out.

The organisation of these simultaneous ascents has been attained since November, 1900, and ascents of manned and unmanned balloons and kites have taken place regularly on the first Thursday of each month. These experiments have now been organised at several stations on the Continent. Up to the month of September, 1903, there had been 34 simultaneous international ascents carried out by means of balloons and kites.



The second proposal, regarding the observations of clouds, has not merely appeared in the accounts of the meetings, but it has also been carried out. Since September, 1900, about 30 stations, distributed throughout Europe, have made observations of clouds on the days of the international ascents, as well as on those preceding and succeeding them.

In Europe, the Institutes of Trappes, Chalais-Meudon, Strassburg, Berlin, Vienna, St. Petersburg-Pawlowsk were the main participators in the aeronautical experiments. From America, I am glad to be able to mention the active collaboration of Mr. A Lawrence Rotch, who has conducted kite ascents on the international days with great regularity. Some ascents were also made at Munich and Hamburg; it is to M. Köppen that we owe the kite ascents at the latter place. I have made many efforts to increase the number of stations, and above all, to fill up the gaps in their distribution which exist in the West of the Continent. These efforts met with considerable success especially after the Conference held in Berlin in May, 1902. This Meeting, of which I will present a report to the International Committee, was attended by numerous delegates, among others by representatives of Great Britain, Spain, and Italy, and I have great satisfaction in stating that these countries have taken a more or less active interest in our labours since this date.

In England, we are indebted to Mr. Patrick Y. Alexander for a number of "ballons-sondes" ascents. He had already assisted in experiments of this nature which M. Teisserenc de Bort had carried out. This year Mr. Alexander was not content with procuring balloons and instruments, but he sent one of his scientific collaborators to study the technical side of the ascents, and the method of reducing the results practically. I must also mention the kite experiments which Mr. Dines carried out at Crinan Harbour during 1902.

Also, M. Palazzo, the Director of the Central Institute of Italy, has assigned an important place in his programme to the exploration of the atmosphere by means of balloons and kites, and we are already able to chronicle several highly successful ascents.

In Switzerland, after the first initiative had been taken by Professors Forel and Heim, and after M. de Quervain had carried out some "ballons-sondes" ascents at Berne, I gave a demonstration of our new methods before the Swiss Meteorological Committee, and, in consequence, Switzerland now takes part officially in the ascents.

In Spain, several ascents with manned balloons were made on the international days, through the interest of Dom Pedro Vivès y Vich, the Commandant of the Aeronautical Station.

The active part which M. Arcimis, the Director of the Spanish Meteorological Service, took in one of these ascents makes me hope that in the future we shall be able to reckon on the co-operation of Spain.

Concerning the extension of the field of our operations towards the North and East, mention must primarily be made of the activity of our colleague, M. Teisserenc de Bort. He has fitted out an expedition, which he has entrusted to M. de Quervain, for conducting ascents of "ballons-sondes" in Russia, in the most continental position that was possible, preferably in the neighbourhood of Moscow. M. de Quervain has succeeded in sending up and recovering 25 "ballons-sondes." The results of these ascents have been already published in the "Travaux de l'Observatoire de Météorologie dynamique." I am also presenting them to the Committee. As a result of M. Teisserenc de Bort's enterprise, the meteorological observatory at Moscow has taken part recently in several international ascents.

Finally we must mention the Franco-Scandinavian Station, for obtaining aerial soundings, at Viborg (Denmark). Under the initiative of MM. Teisserenc de Bort, Hildebrandsson, and Paulsen, this enterprise must be reckoned among the most important known to Meteorology. As a special report on this subject will be presented, I will refrain from going into details.

I have already touched on the efforts which our Committee has made, since its foundation, to establish permanent stations in the free atmosphere. Ascents of kites and of kite-balloons are here principally concerned.

In the first place we must mention the work of the Prussian Meteorological Institute, carried out at the Aeronautical Observatory at Tegel. We owe to the activity of M. Assmann the excellent installation which the members of our Committee had an opportunity of inspecting on the occasion of the Meeting at Berlin. But above all, M. Assmann has furnished proof that, with a satisfactory installation, it is possible to make ascents and thus to obtain vertical sections of the atmosphere every day. Since the beginning of this year, the aeronautical observatory has transmitted the results of these daily ascents by telegram to the Seewarte, which has published the results on the day of the ascent. Without doubt we are concerned here with a most important method of meteorological investigation, and we must try to collect permanent records of this nature from as many points on the globe as possible.

In Germany further facilities for such enterprises have already been provided. A kite-station, founded at Hamburg at the instigation of M. Köppen, makes regular ascents almost every day; the results of these are also published in the Daily Reports of the Seewarte.



The selection of a site for kite-stations depends, above all, on the conditions of the wind. Experiments can only meet with a full measure of success if the wind blows with sufficient permanence and force to lift the kites throughout the year, as in the case, for instance, on the shores of the North Sea at Hamburg. In a country far from the sea, experiments have been less successful, because the wind is more changeable. I have tried to raise kites from hilltops in the Vosges, in particular from the "balloon" of Guebwiller, where there is a meteorological station belonging to the Alsatian system. These experiments were carried on during the autumn and winter of 1900. But, in spite of the elevation, the wind was often too light, and on other occasions too strong. The conditions were, therefore, little favourable to continuous experiments.

But it is not on that account less desirable to have continuous records from observatories far from the coast. To realize this ambition, I was led to choose the Lake of Constance as a site for the experiments. As early as July, 1900, I conceived the idea of making use of the motion of a vessel for correcting the wind conditions, and I made some trials with a motor-boat, but without raising any instruments.

In August, 1901, Mr. Rotch, in America, was the first to raise a kite on an almost calm day by using a small steamer which he could manœuvre at will. Subsequently he crossed the North Atlantic on a boat making her regular passage during a period of calm; he obtained observations with kites on six days out of eight. Mr. Rotch's report was presented to the British Association at its Meeting in Glasgow in 1901. The idea was pursued further at the Meeting in Berlin, and the proposal of Mr. Rotch to explore the conditions prevailing over the tropical regions of the ocean with the help of steamers led me to recommence my experiments on the Lake of Constance. Throughout the month of June, 1902, I conducted kite ascents from a motor-boat, or else from a larger steamer, on all the international days, and also on others.

The experiments were sufficiently successful to justify me in maintaining that it is possible, with a suitable vessel, to carry out kite ascents under all meteorological conditions. If the wind is too light the speed of the boat is added to that of the wind, and the sum of the two is always sufficient to raise the kites. If the wind is too strong the vessel is run before the wind, or at a convenient angle with it; this allows of the tension of the cable being admirably regulated. I have succeeded in raising kites to a height of 2,000 metres on days of absolute calm. If the height attained is not very great, the reason is that I had not at my disposal a vessel of sufficient speed or a motor-winch.

There is reason for hoping that, with the assistance of the German Government, a permanent aeronautical observatory will soon be established on the Lake of Constance.

As in Germany, energetic efforts have also been made in other States to establish centres of observation in the free atmosphere. I have already mentioned the great Franco-Scandinavian enterprise. At St. Petersburg, our colleague, General Rykatchew, has founded a kite station, which is maintained in almost continuous activity, and which takes part in the international ascents.

At Paris, M. Teisserenc de Bort has increased his already important establishment by creating a station for scientific aeronautics at Itteville. Whereas the situation of Trappes is specially favourable for kite ascents, Itteville has become the centre for ascents of free-flying balloons. By choosing rather irregular ground, M. Teisserenc de Bort has established a truly magnificent installation, from which it is possible to send up paper balloons of extraordinary large capacity which ascend to 16 or 17 kilometres and so reach altitudes which cannot be attained by other means, except by the india-rubber balloons recently devised by M. Assmann. The discovery of the now famous isothermal zone, at 12,000 to 15,000 metres, was the first worthy result of this great technical triumph and of these indefatigable labours.

We have already pointed out the great improvement which has been attained in the technique of "ballons-sondes," which we owe to the labours of M. Assmann.

We must admit that the limiting height for open free-flying balloons has been practically attained by the paper balloons invented by M. Teisserenc de Bort, but M. Assmann has conceived and carried out the idea of sending up closed balloons with a very elastic envelope. With open paper balloons a capacity of about 100 cubic metres was needed to make them ascend to a height of 15,000 metres. A small india-rubber balloon can attain the same elevation with an initial capacity of only 4 cubic metres.

The launching of unmanned balloons has thus been greatly simplified. There is no longer any need for a shed or for a large staff of assistants. The means at the disposal of every meteorological institute suffice for launching a "ballon-sonde." The small expense of this method constitutes a second important advantage. It is true that the recent increase in the price of india-rubber decreases this latter advantage somewhat.

The improvement which has taken place in the technical problems has been accompanied by a corresponding one in self-recording instruments. The construction of new thermometers has been undertaken at Paris, Berlin, and Strassburg.

M. Teisserenc de Bort has shown how important it is to insulate the thermometric substance from the body of the



recorder with the greatest care, and that the temperatures recorded in the upper regions of the atmosphere approach very near the truth if this precaution is taken.

For my own part, I have directed my attention to the sensitiveness of the thermograph, trying to conceive a thermometric body of minimum thermal capacity which would adapt itself most satisfactorily to the conditions of natural ventilation of a "ballon-sonde" ascent. I have arrived at the construction of a tubular thermometer, a description of which will be found in the account of the Berlin Meeting. I believe that this instrument will show the smallest thermal details, and, above all, the inversions of temperature even in the most rapid ascents.

While discussing instruments, mention must also be made of M. Assmann's baro-thermo-hygro-chronograph, which indicates the temperature as a function of the pressure, and which gives the time also as a function of the pressure. This arrangement avoids to a great extent the inconvenience of the stopping of the clockwork.

Apart from the discussion of technical questions, the programme of future work was discussed and arranged at the Berlin Meeting. The members of the Conference embodied their views in a number of resolutions which will be found in the proceedings of the Meeting on page 60, *et seq.*

The German Government undertook to communicate these resolutions to the Foreign Governments through the regular diplomatic channels, thus, as we hope, materially aiding the advancement of our objects.

I desire to draw your attention in particular to two of these resolutions, which are of the greatest importance to the work of the International Committee for Scientific Aeronautics.

The following is the text of the fourth resolution:—

> "The International Committee for Scientific Aeronautics are of opinion that the exploration of the atmosphere above the oceans and the tropics forms one of the most important problems of the future."
>
> "To attain this end they recommend, as a first step, the organisation of a marine expedition to the region of the Trade winds, which shall examine the physical conditions prevailing in the upper atmosphere by means of kites."

The Committee are aware of the fact that MM. Rotch and Berson have been working at the realization of this project with great zeal, and that they are still engaged on it.

In my opinion, it would be of great service to these projects if the International Committee would make known their point of view on this question, and if they think fit, would support the realization of this work by a favourable resolution.

I have the honour to propose a resolution to this effect to the Committee.

Further, I wish to speak of the third resolution adopted at Berlin.

This resolution states that the International Aeronautical Committee regard the creation of a journal for publishing the results of the international simultaneous ascents and observations as rapidly as possible, and the provision of funds for carrying this out in a manner to be agreed upon, as matters of great importance. This resolution has been only partially put into effect.

On my application, His Excellency the German Chancellor has made the necessary dispositions for publishing the observations of the years 1901, 1902, 1903, and 1904. The German Empire has put at our disposal for this work a sum of 18,000 marks. This has permitted of the publication of the results in question, but not without charging a considerable residue to the Institute at Strassburg.

I have the honour of presenting to the Committee the first two volumes of this publication, as well as the monthly publications for the current year, 1903, which have appeared up to the present. In this connexion I must make mention of the collaboration of Dr. de Quervain; it is to him that we are primarily indebted for the prompt publication of the ascents of 1903.

In accordance with a resolution adopted by the aeronautical Committee, these publications contain only the actual results of the simultaneous observations, carefully reviewed and edited. In this manner we have succeeded in placing the observational matter at the disposal of the Members of the Committee and of all persons interested therein, as rapidly as possible, to serve as a basis for scientific work.

Up to the present, as has been already stated, the expense of these publications has been borne entirely by the German Empire, which has guaranteed the necessary funds up to 1904.

I am convinced that the continuation of this publication is a question of vital importance for the future of scientific aeronautics, but it will only be possible in the future if the other States which take part in the international ascents also contribute towards the cost of publication.

I should be extremely grateful to the International Committee if they would not simply confine themselves to acknowledging the utility of our publications, but would express a distinct wish that this publication should be continued in the



future. The Meeting of the Committee, which it is proposed to hold in St. Petersburg in 1904, would thus have a firm basis for its deliberation. Further, for my own part, as President of the Committee, I should be in a much better position for conducting the necessary negotiations and for securing the funds which will be needed. If we succeed in obtaining the funds necessary for this official publication by international agreement, we shall also have attained the necessary basis for that official organisation of international scientific work in Aeronautics, which is advocated in the third resolution of the Committee. The detailed organisation could then be arranged with little difficulty, and outstanding questions of detail could be discussed and agreed upon by the Conference at St. Petersburg.

In conclusion, I wish to bring before you the scientific programme which our Committee proposes for the future.

There can be no doubt of the necessity of continuing the international ascents during the coming years. The valuable and interesting observations contained in the publications which are before you give direct proof of this. But these observations also show that it is necessary to extend the experiments to render them still more useful to Science.

Up to the present the simultaneous experiments have been carried on for one day only, and have then been stopped no matter how interesting the meteorological situation might be. Experience shows that it is most important to be in a position to continue the experiments on interesting occasions in order to study the changes which follow. We therefore desire to replace the single ascents by a series of ascents. This would, undoubtedly, increase the technical and scientific work and also the expense, but this might to a certain extent be compensated for by decreasing the number of international days by omitting certain months.

On the other hand, I am of opinion that we could economise considerably by giving to our Committee a suitable organisation. At present we pay too much for our instruments. The fact that these instruments are made by different makers, be it at Paris, at Berlin, or at Strassburg, has the effect of making the price too high. If the Committee could agree to establish a small mechanical workshop for their own needs, the instruments could not only be supplied at net cost, but what is more important still, they would all be of the same type. It may be objected that the individual tendencies of the different institutions would suffer by this arrangement. I am of opinion that that could be avoided, and that the proposal would not prevent each observatory from pursuing its own resarches with special instruments and methods.

There will be opportunity for discussing this question in greater detail at St. Petersburg.

Whatever decision be arrived at on this point, it appears to be certain that the next step forward which the work of our Committee must take is that of substituting for the present ephemeral ascents a series of experiments which will allow of all the developments of a meteorological situation over our continent being studied.

In conclusion, I will sum up the requests which the President of the International Committee for Scientific Aeronautics thinks it his duty to bring before the Committee in the following resolutions:—

"(1.) The International Committee, in agreement with the decisions adopted at Berlin by the International Committee for Scientific Aeronautics, consider that the exploration of the atmosphere over the tropical parts of the ocean by means of kite ascents carried out from steamers suitably equipped for the purpose, an enterprise attempted in the experiments of M. Hergesell on the Lake of Constance in 1900, and suggested by Mr. Rotch in 1901, to be one of the most important meteorological investigations of the future.

"(2.) The International Committee, in agreement with a resolution adopted at Berlin by the International Committee for Scientific Aeronautics, are of opinion that the continuation of the simultaneous international ascents, which is highly desirable in the interests of Science, requires the continuation of the regular publication of the results obtained."

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## APPENDIX III.

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## REPORT ON THE AERIAL SOUNDINGS CARRIED OUT AT THE OBSERVATORY FOR DYNAMICAL METEOROLOGY.

By M. Teisserenc de Bort.

Aerial soundings by means of unmanned balloons ("ballons-sondes") have been carried out at Trappes since the spring of 1898; during the last two years, the majority of the ascents have been made from Itteville, 40 kilometres south of Paris, where I have built a large shed on a well-selected site. The number of balloons liberated, up to the end of July, was 815, with the following results:—

| | | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| An altitude of 10 kilometres was reached by | | | | | | | 579 balloons. | |
| " | " | " | 11 | " | " | " | 525 | " |
| " | " | " | 12 | " | " | " | 466 | " |
| " | " | " | 13 | " | " | " | 334 | " |
| " | " | " | 14 | " | " | " | 165 | " |
| " | " | " | 15 | " | " | " | 38 | " |
| " | " | " | 16 | " | " | " | 10 | " |
| " | " | " | 17 | " | " | " | 1 | " |

These balloons, as is well known, are made of paper and are inflated with hydrogen.

I beg to report that our soundings have brought the following facts to light for the first time:—

(1.) Contrary to what was believed up to a few years ago, an annual variation of temperature is found to occur up to a very great height. As I have already pointed out elsewhere,* the amount of this variation is still 10° C. at an altitude of 9 or 10 kilometres above the ground.

(2.) The non-periodic variations of temperature observed by means of balloons and of kites are more marked at a height of

* *See* Comptes Rendus de l'Académie des Sciences, 1899.

several thousand metres than at ground level. Very great variations are still shown, though more rarely, at altitudes of from 7 to 10 kilometres; these occasionally attain an amplitude of 10° C. (18° F.), while the simultaneous changes at the surface do not exceed 5° C. (9° F.).

(3.) The vertical distribution of temperature presents different characteristics in regions of high pressure to what it does in regions of low; at first it decreases more rapidly in the barometric minima than in the maxima, a fact which has also been brought to light by observations on mountains; then, contrary to expectation, the rate of decrease diminishes greatly, or even becomes zero in the minima, but in the maxima it remains constant up to a height of from 11 to 13 kilometres. Hence it follows that at a mean altitude of from 5 to 7 kilometres the temperature is lower in the barometric minima than in the maxima. At a somewhat higher altitude the temperature is the same in both regions, and finally it becomes lower in the maxima.

Direct verification of these characteristics is furnished by the fact that at the same time of the year the lowest temperatures indicated by the balloons at great altitudes are to be found over regions of high pressure. I have already pointed out these main characteristics in a communication to the Meteorological Congress of 1900, and the soundings carried out in the last two years have confirmed these conclusions.



## APPENDIX IV.

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## EXTRACT FROM A REPORT ON METEOROLOGICAL OBSERVATIONS OBTAINED BY THE USE OF KITES OFF THE WEST COAST OF SCOTLAND, 1902.*

By W. N. Shaw and W. H. Dines.

The paper presents the results of the first organised attempt to obtain a series of automatic records of temperature and humidity in the upper air of the British Isles, or neighbouring seas, by means of kites. They are derived from the records of 40 kite ascents, in which instruments were raised, and which were carried out by Mr. Dines and his two sons, under the auspices of the Royal Meteorological Society in co-operation with a committee of the British Association, during the months of July and August, 1902. Two of the ascents were from a small island in Crinan Bay, Argyllshire, the remainder from the deck of a tug steaming in the Jura Sound or neighbouring sea. Kites were raised on 71 occasions, but, on 31 of them, the force of the wind, even when assisted by the speed of the tug at seven knots, was not sufficient to raise the recording instruments. On those occasions an experimental form of registering air thermometer alone was carried. The average recorded height of ascents with instruments was 5,900 feet (1,800 metres), and average computed height of the 71 ascents 4,200 feet (1,300 metres); a height of 12,000 feet (3,700 metres) was passed on two occasions, and 15,000 feet (4,500 metres) was reached once, but the record was lost owing to the breaking away of the highest kite.

The kites and winding gear were designed and constructed by Mr. Dines. Particulars are given in the Quarterly Journal of the Royal Meteorological Society, vol. 29, p. 65, 1903.

The average angular elevation given by the kites with a short length of line was 62° 30′, the greatest height reached with one kite was 5,500 feet (1,700 metres), with two 9,200 feet (2,800 metres), with three 12,400 feet (3,800 metres).

* Phil. Trans. Roy. Soc., A., Vol. 202, 1903, p. 123.

The following table gives the average fall of temperature observed:—

TABLE of FALL of TEMPERATURE in DEGREES CENTIGRADE for each 500 METRES of ASCENT.

| — | July. | | August. | |
|---|---|---|---|---|
| Metres. | Ascents. | C. ° | Ascents. | C. ° |
| 0 to 500 | 22 | 3·0 | 13 | 2·6 |
| 500 „ 1000 | 16 | 2·8 | 11 | 2·8 |
| 1000 „ 1500 | 9 | 2·2 | 9 | 2·3 |
| 1500 „ 2000 | 2 | 2·0 | 7 | 2·1 |
| 2000 „ 2500 | 1 | 2·0 | 3 | 2·0 |
| 2500 „ 3000 | — | — | 2 | 2·0 |
| 3000 „ 3500 | — | — | 2 | 1·7 |

The range of fall for the first 500 metres varied from 4° C. to 1° C. The smallest fall was associated with an inversion of temperature gradient not far from the surface. An inversion of temperature gradient with very dry air above a layer of clouds was shown also on one of the occasions of steepest gradient near the surface. The steep gradients observed in the lower strata are shown to be associated with anti-cyclonic conditions preceding the approach of a depression, and by examples on five occasions it is shown that the characteristic of the passage of a depression is that the isothermal lines of the diagram open out as the depression comes on, the average diminution of gradient for the change of barometric condition amounting to as much as 50 per cent.

The paths of the centres of depressions producing these changes are shown on the maps taken from the monthly weather reports of the Meteorological Office. It appears that they passed the station on all sides at various distances but none actually crossed it. The results show that whatever was the path taken by the centre, the column of air over Crinan became relatively much more nearly uniform in temperature under the influence of the depression, and therefore probably represented a relatively warm column of air.



The average of the values of temperature gradient in columns of air of different heights derived from all the Crinan ascents are as follows:—

| Height of Column. | Temperature Gradient. |
|---|---|
| Metres. | Per 100 Metres. |
| 500 | 0·56 |
| 1000 | 0·56 |
| 1500 | 0·52 |
| 2000 | 0·50 |
| 2500 | 0·48 |
| 3000 | 0·46 |
| 3500 | 0·43 |

It must be remembered that a moderately strong wind was required for the higher ascents, and they therefore refer to a more or less special type of weather. The gradients for the higher columns are accordingly not so generally applicable as those for the lower columns.

The results are compared with temperature gradients observed elsewhere as given in Hann's "Meteorologie," with the theoretical temperature gradient in dry air (1° C. per 100 metres), and with that for saturated air having an initial temperature 12° C. The last differs but little from 0·53° C. per 100 metres for all ranges up to 2,000 metres and then increases. The average Crinan gradient is almost identical with this and with the conventional correction in use in this country for the reduction of temperatures to a common level, viz., 1° F. per 300 feet.

The last part of the paper is devoted to considering the differences between the temperatures as observed in the free air at the same height as the summit of Ben Nevis and those read on the mountain itself. The differences are always in favour of the free air, which is shown to be on the average 2·6° warmer than the mountain summit. Various circumstances are adduced to support the result, and an explanation is sought in the suggestion that the air flowing from the sea over the mountain would be mechanically raised and practically subject to the adiabatic gradient which is not reached in the free air. The consideration of the relative heights of clouds as observed on the hill sides and over the sea is adduced in corroboration.

Observations were again made during the months of July and August of the present year; the results of these will also be discussed.

## APPENDIX V.

### PROGRESS IN EXPLORING THE AIR AT BLUE HILL OBSERVATORY; AND A PROJECT FOR MAKING ATMOSPHERIC SOUNDINGS ABOVE THE EQUATORIAL OCEANS.

By A. Lawrence Rotch, Director of Blue Hill Meteorological Observatory, Massachusetts, U.S.A.

Beginning with December, 1901, kite-flights were attempted upon a fixed day each month that was appointed by the International Committee for Scientific Aeronautics. During 1902, 13 flights were made, of which 10 were within a day of two of International dates specified. In two of the flights the upper kites broke away and were lost in the ocean, but it is probable that the height attained during one of them exceeded 5,000 metres. The average of the highest points reached in each of the 11 flights, from which records were obtained, was 2,420 metres, and the maximum height was 4,285 metres. The reason that flights were not made on all the international days was lack of wind at the ground, a velocity of at least 6 metres per second being required, and sometimes it was not possible to rise higher than the cumulus clouds on account of the wind falling below this velocity at that level. Occasionally, when the upper wind exceeded 30 metres per second, further progress upward was arrested by such accidents as those mentioned, but, since the kites can be launched in a wind of 15 metres per second, starting the kites was never prevented by strong winds at the ground. If it were desired to fly kites every day, or with certainty on any pre-determined day, independent of the wind, duplicate kites and apparatus might be installed on board a small steamer, which, by steaming in Massachusetts Bay, in the neighbourhood of Blue Hill, could create an artificial wind which would raise the kites in calms or could reduce the natural wind to a suitable velocity. This possibility of becoming independent of the natural wind by employing a moving steam-vessel was probably first demonstrated by me in 1901, when, after preliminary experiments with a tug-boat in Massachusetts Bay, the kites were raised over the North Atlantic Ocean in almost calm air on five days of the eight occupied by the voyage from Boston to Queenstown. The results were first described at the Glasgow Meeting of the British Association (Report, 1901, p. 724). The method of flying kites from moving vessels has since been extensively and successfully employed by my European colleagues, as I have related in "Science," vol. XVIII, pp. 113-114.



The most important application of this method would be an investigation of the meteorological conditions above the trade-winds and doldrums, a project that I suggested in Symons' Meteorological Magazine for November, 1901, and further described to the International Aeronautical Congress at Berlin in 1902, and which was approved by the International Committee for Scientific Aeronautics then in session. The paper is published in "Beilage II. des Protokolls über die dritte Versammlung der Internationalen Kommission für Wissenschafliche Luftschiffahrt." The generally accepted theories regarding the motions of the upper anti-trades are not confirmed by the observations of the drift of volcanic dust and high clouds in these latitudes, and neither the height to which the trade-winds extend nor the vertical gradients of temperature and humidity over the ocean are known. Dr. O. L. Fassig, of Baltimore, flew kites the past summer from the Bahama Islands but was unable to ascend in the trades to a greater height than 1,220 metres. Above the lower cloud stratum, which the kites traversed, was another nearly motionless one, and Dr. Fassig believes that by flying the kites from a fast steamer this neutral stratum might be penetrated. It is my wish to make such atmospheric soundings with kites flown from a steamer, having a speed of not less than 12 knots, and cruising between the Azores and Ascension Islands. The route proposed and the objects to be attained, as given by Professor Hildebrandsson, are stated by me in the Washington "Monthly Weather Review" for July, 1902. An application was made by me more than a year ago to the Carnegie Institution for funds to aid in chartering and equipping a steamer for this investigation which, in the course of a few months, might solve some of the most important problems in meteorology and physical geography.

# APPENDIX VI.

## REPORT ON THE WORK OF THE AERONAUTICAL OBSERVATORY OF THE ROYAL METEOROLOGICAL INSTITUTE OF BERLIN.

By Professor Assmann, Director of the Observatory.

The first ascents were made from the Shooting Range at Tegel on the 1st of October, 1899, while the observatory was still under construction. Before the installation was completed these ascents were necessarily very imperfect, and had frequently to be interrupted for long periods; and later on, so many modifications, of which the advantages or defects could only be determined by experiment, had to be made in the machines and instruments as well as in the construction of the kites and captive-balloons, that it was even then impossible to avoid long breaks in the series of ascents.

It was soon seen that the choice of the site of the observatory had been a most unfortunate one. The unforeseen difficulties met with were of such grave character that the necessity of removing the observatory as soon as possible was soon recognised.

Among the principal obstacles to our work the following may be enumerated:—The immediate neighbourhood of a shooting-range in daily use by firing parties, as well as for drilling troops, and for military experiments; the proximity of the town of Berlin and its suburbs (distant from 2 to 5 kilometres), and of the electric tramways with their overhead wires, which almost completely surrounded the observatory, a fact which caused several serious accidents on account of short circuits caused by breakage of the kite lines; but above all, the neighbourhood of the military aeronautical section, which was established during the year 1901, gave rise to repeated incidents of a particularly grave character, in particular the wires and cables of the observatory frequently fouled the kites and balloons of the military wireless telegraphy department, which has been recently added to the aeronautical section. To put an end to this intolerable situation it has been proposed to remove the observatory to the neighbourhood of Lindenburg, 60 kilometres S.E. of Berlin, to a small hill which dominates the surrounding country and attains an altitude of 120 metres above M.S.L. The funds necessary have been provided for in the State Estimates for 1904. If the legislative assemblies pass them, the new observatory will be in a position to commence work during the summer of 1905.



It was only during the summer of 1902 that the preliminary experiments, whose object had been to study ways and means, were sufficiently advanced to enable us to attempt to realise the true aim of the observatory, which is, of course, the continuous study of the atmosphere by making daily ascents. This has proved to be a much more difficult undertaking than was expected, and the staff and material needed has exceeded the estimates considerably. M. Teisserenc de Bort had the same experience in his experiments at Hald.

Since the month of August, 1902, the ascents near Berlin have been carried out almost every day; daily ascents have been made since October, Sundays and public holidays excepted; since 1st January, 1903, these days have also been included, and no break has occurred in the observations.

The experiments group themselves as follows:—(1) kite ascents; (2) captive balloon ascents (balloon-kites), for these the type of balloon-kite of 68 cubic metres capacity appears to yield the most satisfactory results; (3) unmanned balloons, principally of the model of the india-rubber balloons invented by the Director of the observatory; (4) manned balloons; in addition to the balloons of the German Aeronautical Society, the following balloons belonging to the observatory were used:—The "Preussen," of 8,400 cubic metres capacity, presented by M. Enders, and the "Meteor," of 850 cubic metres capacity. With the exception of a number of voyages made for special objects, ascents of the two last kinds were made mainly on the days of international ascents.

The following table gives a summary of the number and heights of the ascents carried out between 1st October, 1899, and 30th September, 1903:—

Table I.

(1.) *Kites.*

| —— | No. | Heights. | | | | Mean. | Max. |
|---|---|---|---|---|---|---|---|
| | | < 1000 m. | 2000 m. | 3000 m. | > 3000 m. | | |
| 1899 from Oct. 1 to Dec. 31. | 2 | — | 2 | — | — | 1298 | 1480 |
| 1900 ... ... | 22 | 14 | 6 | 1 | 1 | 1032 | 4255 |
| 1901 ... .. | 33 | 17 | 12 | 3 | 1 | 1160 | 4088 |
| 1902 ... ... | 100 | 35 | 34 | 25 | 6 | 1542 | 4820 |
| 1903 up to Sept. 30 | 231 | 46 | 90 | 69 | 26 | 1832 | 4565 |
| Sums... ... | 388 | 112 | 144 | 98 | 34 | — | 4820 |
| Mean ... ... ... ... ... ... | | | | | | 1652 | |

(2.) *Captive Balloons (Balloon Kites).*

| — | No. | Heights. | | | | Mean. | Max. |
|---|---|---|---|---|---|---|---|
| | | < 1000 m. | 2000 m. | 3000 m. | > 3000 m. | | |
| 1899 from Oct. 1 to Dec. 31. | 2 | 1 | 1 | — | — | 608 | 1090 |
| 1900 ... ... | 8 | 4 | 4 | — | — | 963 | 1323 |
| 1901 ... ... | 91 | 63 | 25 | 3 | — | 811 | 2673 |
| 1902 ... ... | 154 | 61 | 81 | 12 | — | 1018 | 2457 |
| 1903 up to Sept. 30 | 109 | 24 | 83 | 2 | — | 1271 | 2080 |
| Sums... ... | 364 | 153 | 194 | 17 | — | — | 2673 |
| Mean ... ... ... ... ... ... | | | | | | 1040 | |

(3.) *Unmanned Balloons (Ballons-sondes).*

| — | No. | Heights. | | | | Mean. | Max. |
|---|---|---|---|---|---|---|---|
| | | < 5000 m. | 10,000m. | 15,000m. | > 15,000 m. | | |
| 1899 from Oct. 1 to Dec. 31. | 0 | — | — | — | — | — | — |
| 1900 ... ... | 6 | 2 | 4 | — | — | 5727 | 8005 |
| 1901 ... ... | 28 | 11 | 11 | 5 | 1 | 6136 | 17,345 |
| 1902 ... ... | 17 | 3 | 5 | 3 | 6 | 11,321 | 19,960 |
| 1903 up to Sept. 30 | 15 | 1 | 6 | 8 | — | 7579 | 13,370 |
| Sums... ... | 66 | 17 | 26 | 16 | 7 | — | 19,960 |
| Mean ... ... ... ... ... ... | | | | | | 8217 | |

(4.) *Manned Balloons.*

| — | No. | Heights. | | | | Mean. | Max. |
|---|---|---|---|---|---|---|---|
| | | < 3000 m. | 4000 m. | 5000 m. | > 5000 m. | | |
| 1899 from Oct. 1 to Dec. 31. | 2 | 1 | — | — | 1 | 4509 | 6625 |
| 1900 ... ... | 3 | — | — | 1 | 2 | 5672 | 6447 |
| 1901 ... ... | 9 | 2 | 1 | 1 | 5 | 5330 | 10,800 |
| 1902 ... ... | 12 | 2 | 1 | 3 | 6 | 4858 | 7832 |
| 1903 up to Sept. 30 | 7 | — | — | 2 | 5 | 5560 | 8770 |
| Sums... ... | 33 | 5 | 2 | 7 | 19 | — | 10,800 |
| Mean ... ... ... ... .. ... | | | | | | 5188 | |

Total number of ascents of all kinds, 851.



To the total of 851 ascents must be added 8 ascents of unmanned balloons in which no records were obtained through various accidents. Out of a total of 74 experiments of this nature, only two instruments have been lost up to the present; on one occasion an instrument remained fixed in the top of a tree in a large forest for a period of ten complete months, during which the records were perfectly preserved. In these 74 ascents are included 4 unintentional ascents of captive balloons, which broke away and which attained an altitude of at least 5,200 metres.

The number of kite ascents only slightly exceeds the number of balloon-kite ascents, 388 against 364; from this it might be concluded that the atmospheric movements over Berlin were not sufficiently strong to raise kites during about one-half the time. But on comparing the different years, we find a sensibly different ratio between the numbers of ascents of each kind; up to 1902, inclusive, the balloon-kite ascents predominate, but during 1903 the kite ascents greatly exceeded the others. From this we might conclude that the wind was stronger during the latter period but the increase was mainly the result of greater experience in the use of kites, the manipulation of which presents greater difficulties than that of balloon-kites, the same fact is apparent when we consider the heights attained.

Out of 100 kite ascents, the heights attained were:—

| — | 1000 Metres. | 2000 Metres. | 3000 Metres. | More than 3000 Metres. |
|---|---|---|---|---|
| In 1902 ... ... | 35 | 34 | 25 | 6 |
| „ 1903 ... ... | 20 | 39 | 30 | 11 |

Thus the number of ascents to low altitudes has been diminished, while that of ascents to greater heights has been increased.

Since November, 1902, the results of the morning ascents have appeared regularly in the day's issue of the official "Reichsanzeiger," and also in the evening editions of several other Berlin daily papers; since 1st January, 1903, the results have also appeared in the weather charts of the Berlin Meteorological Office, and further, since the 1st April, in the Daily Reports of the Deutsche Seewarte. These arrangements, which have been favourably received on all sides, make it necessary to terminate the ascents somewhat before noon, in order that, after a rapid inspection of the curves, the results obtained may be despatched by telephone at a sufficiently early hour for publication. These communications have gained in interest since the kite station of the Deutsche Seewarte, established at Hamburg by Professor Köppen, commenced publishing their observations in the same journals. It is obvious that, but for the necessity of early reading, it would be possible to continue a much larger number of the ascents to a great height, and in consequence, the result of 11 per cent. possible ascents to more than 3,000 metres, quoted above, must not be looked upon as the limit of what can be achieved with kites.

The importance of balloon-kites in a programme of daily ascents is brought out by the numbers quoted, although the altitudes attained by this method have been relatively lower than those reached with kites. But in the case of balloons also, we may hope for an increase in the mean height attained as more experience in their management is acquired. Nevertheless, an increase in the maximum height reached by balloons must not be expected, because the limits within which balloon-kites can be employed are much smaller than is the case with kites; the altitude of 2,000 metres can only be exceeded on rare occasions when the weather is absolutely calm.

In the matter of india-rubber "ballons-sondes," we have to note, with surprise, that there has not only been no increase in the height attained, but an actual decrease has occurred in 1903. This is to be explained by the inferior quality of the india-rubber recently used for these balloons, which has, moreover, increased in price. Whereas in 1902 it was a rare occurrence for a balloon not to burst as it ought to do, but to be brought to the ground by the gradual loss of gas, this disagreeable mischance, which robs the process of one of its main advantages, occurred frequently in 1903, so that it became necessary to inflate the balloons to a rather greater extent, and increase the velocity of ascent, giving them a greater up-thrust, and bringing about a greater ventilation of the thermometer. But, as was to be expected, the altitude attained has suffered in consequence, a fact which is indicated in the table by a diminution in the mean height of from 11,321 metres in 1902 to 7,579 metres in 1903, and in the maximum height from 19,960 metres to 13,370 metres.

The 33 ascents of manned balloons, shown in the fourth column of the table, show that the skill of the aeronauts of the observatory, especially of MM. Berson, Süring, and Elias, has not diminished, and that they still hold their former record of having attained the greatest altitudes in the international ascents. The great ascent of 31st July, 1901, in which Berson and Süring reached an altitude of 10,800 metres (exact result worked out subsequently) will probably not be easily surpassed.



Dr. Hermann von Shrötter, of Vienna, a scientist well-known as a specialist on the physiology of the respiratory organs, took part in two other ascents, in which great altitudes, viz., 7,500 metres and 8,800 metres, were reached.

No means have been taken of the results of the experiments made during the last four years and summarised in the above table.

During the last complete year, *i.e.*, from October 1st, 1902, to the same date in 1903, 279 kite ascents and 175 balloon-kite ascents were carried out, making a total of 454 *captive* experiments, not counting the ascents of manned and unmanned balloons.

The following table gives the chief results of these continuous ascents carried out during a complete year:—

TABLE II.

| — | Number. | | Above 1000 m. | | Up to 2000 m. | | Up to 3000 m. | | Above 3,000. | Mean Height. | | Maximum Height. | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Kites. | Kite Balloons. | Kites. | Kite Balloons. | Kites. | Kite Balloons. | Kites. | Kite Balloons. | Kites. | Kites. | Kite Balloons. | Kites. | Kite Balloons. |
| 1902. | | | | | | | | | | | | | |
| October ... | 15 | 33 | 2 | 14 | 6 | 19 | 5 | 0 | 2 | 1878 | 1099 | 3410 | 1856 |
| November... | 13 | 23 | 5 | 10 | 4 | 13 | 4 | 0 | 0 | 1465 | 1100 | 2697 | 1571 |
| December ... | 20 | 10 | 9 | 4 | 6 | 5 | 3 | 1 | 2 | 1473 | 1144 | 4820 | 2139 |
| 1903. | | | | | | | | | | | | | |
| January ... | 33 | 4 | 5 | 1 | 15 | 3 | 12 | 0 | 1 | 1727 | 1199 | 3486 | 1584 |
| February ... | 34 | 3 | 9 | 1 | 15 | 2 | 6 | 0 | 4 | 1596 | 1113 | 3995 | 1635 |
| March ... | 29 | 14 | 6 | 5 | 6 | 9 | 11 | 0 | 6 | 2110 | 1145 | 4520 | 1640 |
| April ... | 25 | 11 | 6 | 0 | 11 | 11 | 6 | 0 | 2 | 1751 | 1417 | 3710 | 1830 |
| May ... | 19 | 19 | 3 | 5 | 4 | 14 | 11 | 0 | 1 | 2112 | 1299 | 4565 | 1990 |
| June ... | 18 | 17 | 3 | 5 | 8 | 11 | 4 | 1 | 3 | 1900 | 1248 | 3310 | 2080 |
| July ... | 21 | 15 | 5 | 1 | 9 | 13 | 4 | 1 | 3 | 1737 | 1346 | 3925 | 2010 |
| August ... | 29 | 8 | 3 | 1 | 11 | 7 | 10 | 0 | 5 | 2047 | 1007 | 3600 | 1200 |
| September ... | 23 | 18 | 6 | 5 | 11 | 13 | 5 | 0 | 1 | 1601 | 1332 | 3025 | 1765 |
| Sums ... | 279 | 175 | 62 | 52 | 106 | 120 | 81 | 3 | 30 | Mean for the year. 1783 | 1204 | Maximum for the year. 4820 | 2139 |
| | 454 | | 114 | | 226 | | 84 | | | | | | |



## APPENDIX VII.

## REPORT ON SIMULTANEOUS SOLAR AND TERRESTRIAL CHANGES.

By Sir Norman Lockyer, K.C.B., F.R.S., Director of the Solar Physics Observatory, South Kensington.

There are very many cases recorded in the history of science in which we find that the most valuable and important applications have arisen from the study of the ideally useless. Long period weather fore-casting, which at last seems to be coming into the region of practical politics as a result of the observation of solar changes, is another example of this sequence.

The first indications of these changes on the sun, to which I have referred, are matters of very ancient history, and so also is the origin of some of the branches of observation on which the study of them depends.

I will begin by referring to these and to the conclusions arrived at in relation to simultaneous solar and terrestrial changes previously to the last 25 years.

The facts that there are sometimes spots on the sun, and that there is a magnetic force which acts upon a needle, seem to have been known to the ancient Chinese. In more modern times the enquiries, with which we are now concerned, date from the times of Galileo (1564-1642) and Kepler (1571-1630).

To Galileo, Fabricius, and Scheiner we owe the first telescopic observations of the spots on the sun; to Kepler, the basis of spectrum analysis, which has not only revealed to us the chemistry of the sun and of its spots, but enables us to study daily other phenomena, the solar prominences, which will in all probability turn out to be more important for practical purposes than the spots themselves.

It is only quite recently that the importance of the study of the prominences in this direction has been indicated, so that we have to deal, in the first instance, with a long period of years in which only the spots and their terrestrial echoes were in question.

According to Prof. Wolf (as quoted by Prof. Köppen), Riccioli, in 1651, shortly after the first discovery of sun spots, surmised that some coincidence might exist between them and terrestrial weather changes.*

In the first year of the last century, Sir Wm. Herschel drew attention to this subject.† He wrote:—

"The first thing which appears from astronomical observations of the sun is that the periods of the disappearance of spots on the sun are of much greater duration than those of their appearance.

"With regard to the contemporary severity and mildness of the seasons, it will hardly be necessary to remark that nothing decisive can be obtained. An indirect source of information, however, is opened to us by applying to the influence of sunbeams on the vegetation of wheat in this country. I do not mean to say that this a real criterion of the quantity of light and heat emitted by the sun, much less will the price of this article completely represent the scarcity or abundance of the absolute produce of the country.

"On reviewing the period 1650-1713, it seems probable, from the prevailing price of wheat, that some temporary scarcity or defect of vegetation has generally taken place when the sun has been *without* those appearances which we surmise to be symptoms of a copious emission of light and heat.

"To those acquainted with agriculture who may remark that wheat is well-known to grow in climates much colder than ours, and that a proper distribution of rain and dry weather are probably of much greater consequence than the absolute quantity of light and heat derived from the sun, I shall only suggest that those very circumstances of proper alternations of rain and dry weather and wind, &c., favourable to vegetation, may possibly depend on a certain quantity of sunbeams being supplied to them."

Herschel's suggestion was a daring one, for however perfect our national statistics may have been in relation to the price of wheat, there was nowhere kept up a continuous record of the changes observable on the sun's surface, nor had there been any serious attempt made to determine the law underlying them.

In 1825 this serious attempt was made, and by Schwabe of Dessau, who discovered a cycle of about 11 years in the solar changes. Wolf afterwards took up the question.

* Blanford, Bengal, Asiat. Soc. Journ. 65; Part II., 1875, p. 22.
† Phil. Trans., 1801, p. 265.



Herschel had associated the variation in the number of spots with that in the price of corn, the connecting link being sunshine or weather. It was to him a question of meteorology.

A year after the publication of Herschel's papers, Wollaston extended the early spectrum work of Kepler and Newton by discovering that in the solar spectrum there were many dark lines; these were for the first time mapped by Fraunhofer in 1814.

Soon after 1850 it became a question of the connexion of sun spots with terrestrial magnetism as well as with meteorology. A new idea was introduced.

Lamont, Sabine, and Allan Broun discovered that there was a well marked coincidence between the variations of magnetic effects, as observed on the surface of our planet by delicately suspended magnets, and the quantity of spotted area observed on the sun. This in later telegraphic days is not merely a pious opinion which does not interest anybody, because, when the magnetic changes are very considerable and the disturbances arrive at a maximum, it is very difficult to get a telegram from London to Brighton.

The period around the year 1860 was rendered ever memorable by a still further extension of Kepler's and Newton's work, which at once explained the dark lines observed in the solar spectrum by Wollaston and Fraunhofer.

Hitherto undreamt-of attacks on the nature of the sun became possible. The names of Kirchhoff, Bunsen, Ångström, Stokes, Balfour Stewart will go for very long down the stream of time, because they showed us that in spectrum analysis we had the power of practically conversing, chemically, with the distant worlds in space, and these distant worlds, of course, included the sun, although it is practically our neighbour.

It was now established that the solar radiation came from the incandescence of metallic vapours and gases in the sun's atmosphere, the metals and gases being for the most part those with which we are familiar on the earth. Not only was a high temperature demonstrated in this way, but it was further shown that above the sun's apparent surface there was an absorbing atmosphere, consisting of vapours cooler than those below, but yet hot enough to be composed of the steam of iron and other metals.

In 1865, De la Rue, Stewart, and others, in an attempt to get the periodicity of the solar phenomena still more accurately determined, started work at Kew; while the former observations were carried on by Schwabe and Wolf by the eye, photography, which was then being introduced into astronomical work by the labours of Warren De la Rue, was for the first time now utilized, and a picture of the sun was taken each day.

In 1866 a new method of observing solar changes, which consisted in throwing an image of the sun on the slit plate of a spectroscope, revealed the fact that the spectra of spots differed from that of the photosphere generally; certain lines were widened in the spot spectrum.*

In 1867 a connexion between changes in spotted area and in terrestrial temperatures was pointed out by Baxendell.† He noticed a distinct and very striking relation between the number of sun spots and the ratio which exists between the difference of the mean maximum temperature of solar radiation and the mean maximum air temperature on the one hand, and that of the mean temperature of the air and of evaporation on the other.

In 1868 a spectroscopic method was discovered of observing in full daylight the "prominences" or "red flames" which hitherto had only been glimpsed during eclipses, and it was established that, closely surrounding the sun ordinarily seen, there was an envelope, named the chromosphere, of incandescent gases and vapours, hydrogen, and a new substance named helium chief among them.‡

Many spectroscopic observations made on the spots and prominences about this time indicated great changes in the solar temperature in different regions, and possibly, therefore, changes in the amount of heat radiated earthwards. From the changes thus actually seen it was easy to imagine that there might be a cycle of terrestrial changes depending no longer on the sun's presentation to us in its daily and yearly rounds, but on physical changes in the sun itself, requiring, perhaps, many years to accomplish.

In 1869, Janssen showed§ that by a special arrangement of the spectroscope an image of the sun, showing the prominences both on the disc and surrounding it, might be obtained.

It was not very long before it was found that the reaction of these solar changes on the earth was not so limited as had formerly been thought. This was an idea started by Dr. Stone of the Royal Observatory at the Cape of Good Hope, Piazzi Smyth of the Royal Observatory of Edinburgh, and others, about the years 1870 and 1871, but the most striking Imperial contribution to the matter we owe to the labours of a distinguished meteorologist, Dr. Meldrum, Director of the Observatory at Mauritius, which has since become the Royal Alfred Observatory. He showed that the number of wrecks which came into the harbour of the Mauritius and the number of cyclones observed in the Indian Ocean, could enable anyone

* Lockyer, Proc. Roy. Soc., 11th October, 1866.

† Memoirs of the Manchester Lit. & Phil. Soc., Third Series, Vol. IV., pp. 128, *et seq.*

‡ Lockyer, Proc. Roy. Soc., 20th October, 1868.

§ Comptes Rendus, Vol. LXVIII. (1869), pp. 367 *et seq.*



to determine the number of spots that were on the sun about the time. The Mauritius is most admirably suited for the making of these observations, because the Tropics are really the right region in which to try and estimate the possibilities of this solar action. Meldrum found, in fact, that the maximum number of cyclones was associated with the maximum number of sun spots. He wrote*:—

"During the period 1847-72 it is found that some years have been remarkable for a frequency, and others for a comparative absence of cyclones.

"1847-51 were characterized by cyclone frequency.
"1852-57 „ „ „ comparative calm.
"1858-63 „ „ „ cyclone frequency.
"1864-68 „ „ „ decrease.
"1868-72 „ „ „ great increase.

"It will be seen that the years correspond with the maxima and minima epochs of sun spots. It appears to me that there is more than a mere coincidence as to time.

"The numbers of wrecks during these periods also show a similarly regulated frequency."

Poey, investigating shortly afterwards the cyclone condition in the West Indies,† found that the greater number of years of maxima of storms fall from six months to two years, at the most, after the years of maxima of solar spots.

Out of 12 maxima of storms, 10 coincide with maxima periods of spots. Out of five minima of storms, five coincide with minima of spots.

It will be seen that the results from both the East and West Indies are the same. Next came the question of a rainfall cycle corresponding to the solar spots.‡

When I was preparing to go to India, in 1871, to observe the eclipse, Mr. Ferguson, the editor of the *Ceylon Observer*, who happened to be in London, informed me that everybody in Ceylon recognised a cycle of about thirteen years or so, in the intensity of the monsoon—that the rainfall and cloudy weather were more intense every thirteen years or so. This, of course, set one, interested in solar matters, thinking, and I said to him:—"But are you sure the cycle recurs every thirteen years, are you sure it is not every eleven years?" adding, as my reason, that the sun spot period was one of eleven years or thereabouts, and that in the regular weather of the Tropics, if anywhere, this should come out.

* Nature, Vol. 6, p. 357, 1872.

† Comptes Rendus, 24th November, 1873, p. 1222.

‡ Solar Physics, (Lockyer 1874), p. 425.

It afterwards turned out that the period in Ceylon was really of eleven years, five or six years dry, and five or six years wet, and that a longer period of about 33 years was recognized.

Mr. Meldrum passed from cyclones to rainfall by a very obvious step, because cyclones are generally accompanied by torrential rains. A study of the rainfalls of Port Louis, Brisbane, and Adelaide led him to to the conclusion that a case had been made out for a supposed periodicity.

On my return from India I looked up the Cape and Madras records for the periods available, and found that they followed suit, hence I quite agreed with Dr. Meldrum that investigations were desirable, and I wrote as follows*:—

> "Surely in meteorology, as in astronomy, the thing to hunt down is a cycle, and if that is not to be found in the temperate zone, then go to frigid zones, or the torrid zones and look for it, and if found, then above all things, and in whatever manner, lay hold of, study it, record it, and see what it means. If there is no cycle, then despair for a time if you will, but yet plant firmly your science on a physical basis, as Dr. Balfour Stewart long ago suggested, before, to the infinite detriment of English science, he left the Meteorological Observatory at Kew; and having got such a basis as this, wait for results. In the absence of these methods, statements of what is happening to a blackened bulb in vacuo, or its companion exposed to the sky, is, for research purposes, work of the tenth order of importance."

With reference chiefly to Dr. Meldrum's paper, I added:—

> "Surely here is evidence enough, evidence which should no longer allow us to deceive ourselves as to the present state of meteorology. A most important cycle has been discovered, analogous in most respects to the Saros discovered by the astronomers of old, indeed, in more respects than one, may the eleven yearly period be called the Saros of meteorology, and as the astronomers of old were profoundly ignorant of the true cause of the Saros period, so the meteorologists of the present day are profoundly ignorant of the true nature of the connexion between the sun and the earth.

> "What, therefore, is necessary in order to discover the true nature of this nexus? Two things are necessary, and they are these. In the first place, we must obtain an accurate knowledge of the currents of the sun, and secondly, we must obtain an accurate knowledge of the currents of the earth. The former of these demands the united efforts of photography and spectrum analysis, and

* Solar Physics, pp. 424–5.



> the second of these demands the pursuit of meteorology as a physical science, and not as a mere collection of weather statistics. When these demands are met—and in spite of the Mrs. Partingtons who are endeavouring to prevent this, they will soon be met—we shall have a science of meteorology placed on a firm basis—the meteorology of the future.*"

At this time the Indian authorities were quite alive to the importance of such investigations as these. India is in the tropics, India is a child of the sun, the inhabitants depend almost entirely upon the beneficent rains which seemed, in some way or another, to depend upon solar action. India also had then the germs of one of the best equipped meteorological organisations which exist on the surface of the planet, and the meteorologists felt that there was something behind their meteorological registers which might be assisted by taking a very official step and going to headquarters, headquarters being the sun. When I was in India, in 1872, Lord Mayo, the then Viceroy, did me the honour to ask me to go to Simla with a view of choosing a site for a proposed Solar Physics Observatory. That is 30 years ago! Unfortunately, I was Secretary of the Duke of Devonshire's Commission, which was then sitting, and I could not get leave, and, therefore, could not go; the scheme, which was then before the Indian authorities—which, if I may say so, was altogether grandiose and extravagant,—fell through.

In 1873, the idea of the possible connexion of solar and magnetic changes had got so far that the magnetic and meteorological department of the Royal Observatory at Greenwich, which had been established in 1838, received an important addition. A photo-heliograph was set up in order to continue the daily photographic record of the sun's surface, began at Kew in 1865.

In the same year Köppen found that the maximum temperature occurs in the years of sunspot minima and the reverse; years with many spots are cool years.†

Of special importance for the connexion between the temperature on the earth's surface and the sun's spotted area is the fact that the temperature curve (mean number for the whole earth) and the curve representing the sun-spotted area is identical in all the irregularities.

* I very much regret that, in the article quoted, my reference to Carlyle's German "Dry as dust," as a patient enquirer who would eventually apportion credit to all meteorological workers, has been misunderstood by some of my German friends. Relying on imperfect dictionaries, which have told them that a mere "bookworm" was meant, they have missed the high compliment I intended to pay them.

† W. Köppen. Über mehrjährige Perioden der Witterung. Zeitschrift f. Meteorologie, Bd. viii., 1873, pp. 241–248 and 257–268.

In the tropics in the

Year before the sun spot *Min.*, the temperature is 0·41° higher than the mean.

Year before the sun spot *Max.*, the temperature is 0·32° lower than the mean.

The variation is thus 0·73°.

By this time spectroscopic observations of the solar changes had proved that the sun was hottest when there were most spots, thereby upsetting the old idea that the spots acted as screens and reduced the radiation at sun spot maximum. Köppen's result, therefore, was a paradox, and was thus explained by Blanford*:—

"The temperatures dealt with by Prof. Köppen are of course those of the lowest stratum of the atmosphere at land stations, and must be determined *not by the quantity of heat that falls on the exterior of the planet, but on that which penetrates to the earth's surface, chiefly to the land surface of the globe.* The greater part of the earth's surface being, however, one of water, the principal immediate effect of the increased heat must be the increase of evaporation, and, therefore, as a subsequent process, the cloud and the rainfall. Now a cloudy atmosphere intercepts the greater part of the solar heat, and the re-evaporation of the fallen rain lowers the temperature of the surface from which it evaporates and that of the stratum of air in contact with it. The heat liberated by cloud condensation doubtless raises the temperature of the air at the altitude of the cloudy stratum; but at the same time we have two causes at work, equally tending to depress that of the lowest stratum. As a consequence, an increased formation of vapour, and therefore of rain, following on an increase of radiation, might be expected to coincide with a low air-temperature on the surface of the land.†"

The next important advance had to do with atmospheric pressure. In 1875, Mr. C. Chambers, the director of the Bombay Observatory, found that—

"The variation of the yearly mean barometric pressure at Bombay shows a periodicity nearly corresponding in duration with the decennial sun-spot period."‡

The years round 1875 were rendered very important by the number of new organizations established to record and demonstrate various classes of observations with which we are concerned in this short history. Meteorological enquiries on a large scale were organized at home and in India, and observa-

* Blanford, Bengal, Asiat. Soc. Journ., 1875.

† *See also* Blanford, Nature, 23rd April, 1891, Vol. 43, p. 583.

‡ Meteorology, Bombay Presidency, August, 1875, S. 26, p. 12.



tories were established at Potsdam, Paris, and London, with the main object of studying solar changes. At the same time steps were taken to resume observations in the tropics. It is not out of place here to make a brief reference to what was done in Britain and India.

The Government took this action in consequence of a strong recommendation of the Royal Commission on Science, presided over by the late Duke of Devonshire, of the establishment by the State of an Observatory of Solar Physics in which enquiries relating to the nature of the sun and its changes should be fostered, and various investigations which were necessary should be carried on.

The Commission also proposed that similar institutions should be established in various parts of the Empire.

The ground on which the Royal Commission, and subsequently a memorial presented to the Government by the British Association, urged this new departure was that, in the opinion of a considerable number of scientific men, there was a more or less intimate connexion between the state of the sun's surface and the meteorology of the earth; and they called attention to the fact that recent independent investigations on the part of several persons had led them to the conclusion that there was a similarity between the sun spot period, periods of famine in India, and cyclones in the Indian Ocean. The memorialists concluded by saying:—

"We remind your Lordships that this important and practical scientific question cannot be set definitely at rest without the aid of some such institution as that the establishment of which we now urge."

The Lords of the Committee of Council on Education referred this memorial to a Committee, consisting of Prof. Stokes, Prof. Balfour Stewart, and General Strachey, for their opinion as to whether a commencement might not be made to give effect to the proposals of the memorialists by utilising the chemical and physical laboratories at South Kensington, as the proposed observatory must be more chemical and physical than astronomical. The following paragraph appeared in the terms of reference:—

"Although we are not at present in a position to consider the establishment of a physical observatory on a comprehensive scale, we believe that some advantage can be gained if a new class of observations can be made with the means at command, since the best method of conducting a physical observatory may thus be worked out experimentally, and an outlay eventually avoided which, without such experience, might have been considered necessary."

While the discussion as to the establishment of a solar physics observatory in this country was going on, Lord

Salisbury, who was then Secretary of State for India, permitted me to send him a memorandum on this subject. In it I pointed out that what we wanted, especially in reference to solar enquiries, was to learn, day by day, what the sun was really doing, which India and other tropical countries always could tell us, while it seemed almost impossible that we should ever get sufficiently continuous records in England.

I gave the following extracts:—

"Solar research is now being specially carried on in Europe at—

"(1.) Potsdam, in the new Sonnenwarte.

"(2.) Paris, in the new physical observatory.

"(3.) Rome and Palermo.

"(4.) South Kensington, in connexion with the Science and Art Department.

"(5.) At Greenwich, Wilna, and other places it is carried on in a less special way.

"In these European observatories, however, especially in the more northern ones, we are attempting to make bricks without straw, that is, the climate is such that the observations are often interrupted, at times for weeks together, while, in addition to this, in winter the sun's altitude is so small that fine work is impossible.

"While this state of things holds in Europe, in India, on the other hand, one has an unlimited and constant supply of the *raw material*, by which I mean that here one can, if one chooses, obtain observations of the finest quality in sufficient quantity all the year round. I may even go further, and say that, limiting my remark to English ground, we have in India a *monopoly* of the raw material."

The prayer of the memorandum was granted, and shortly afterwards I had the pleasure of sending out one of my assistants to India. Unfortunately, he died soon after the first series of daily photographs of the sun had been commenced, but eventually the Trigonometrical Survey Department took the matter up, an observatory was built at Dehra Dun, and India began its work, and I am thankful to say that it has gone on continuously ever since.

It was not till 1879, and after a letter from the Duke of Devonshire, that a sum of £500 was taken on the estimates to replace the assistance formerly obtained by myself from the Government Grant Fund administered by the Royal Society, and to allow of more research work being undertaken. At the same time, the Solar Physics Committee was appointed.



The object sought was to make trial of methods of observation, to collect and discuss results, to bring together all existing information on the subject, and to endeavour to obtain complete series of observations along the most important lines.

This State action was taken because the sun has to be studied, if studied at all, continuously, because it is ever changing, and the more we study it the longer are the cycles which we find to be involved; hence, all enquiries into its nature must be on an Imperial basis. Individuals die, nations remain. Nor is this all. Observatories are not only wanted in the centres of intellectual activity where research can be conducted in a scientific atmosphere, but there must be others to obtain the necessary observations in those favoured regions of our planet in which the maximum of sunshine can be depended upon.

The then Astronomer Royal, Sir George Airy, was most sympathetic, and as a result of this State action, the little observatory at South Kensington was shortly afterwards enlarged; it has considerably grown since then, but it is still in the experimental stage. Although, perhaps, I am not the one to say it, I am prepared to take the responsibility of stating that it is now one of the best equipped for its special work in the world. It certainly is the shabbiest to look at. Irreverent comparisons have been made even in the House of Commons; the general appearance of its wood and canvas huts having been likened to that of a more or less disreputable looking travelling menagerie, but, at all events, it is instrumentally efficient, and that for the present must be sufficient.

During the last quarter of a century a great deal of work has been going on, and the Colonies and Dependencies of Britain have also been doing yeoman service; very little has been said about it, because not all Departments are in the habit of advertising themselves, and Blue Books are not as a rule light reading. In the first place, the Indian daily photographic record, which was weak during a month or two during the S.W. monsoon, was supplemented by the erection of a duplicate instrument at the Mauritius; and I am again thankful to say that the work has gone on at the Mauritius continuously since. Thus we have now two tropical records, which, taken together, may be described as absolutely continuous, of solar changes sent to us in the most Imperial fashion by two observatories. Another appeal was made to Australia. For a time records were sent us, but I am sorry to say that after a time they ceased.

These records are sent regularly with every precaution against loss, to the Observatory at South Kensington; and for the days when no photographs have been taken at Greenwich the necessary photographs are transmitted there, where they are reduced in continuation of the record commenced in 1873 there, in succession to Kew.

What has been the result of this? The late Astronomer Royal took up this work at Greenwich in 1873. In 1874, 1875, 1876, 1877, 1878, the average number of days on which it was possible to obtain photographs in each year was a little over 160, the exact figures being 159, 161, 167, 171, 149. This was Greenwich working alone, national work.

Next, we come to the Imperial work. Selecting years at random, and dealing with 1889 to 1893, I find that we obtained photographs of the sun in 1889 for every day in the year except five, in 1890, for every day except four, in 1891, for every day except two. It is easy to understand that with such a magnificently complete record as this the study of solar physics was enormously improved.

Very fortunately for science, even before these steps were being taken to secure a continuous record of the spotted area, Prof. Respighi (1869) and Prof. Tacchini (1872) had commenced at Rome a daily record of the solar prominences and of the latitudes at which they appeared at different times.

I pass on to some of the most important work done during the last quarter of a century, only referring to the results obtained which bear upon the connection between solar and terrestrial changes.

Many important advances were made in 1878.

Mr. F. Chambers, in continuing his studies on the Indian barometer, found* a remarkable degree of resemblance in the progression of barometric pressure during summer, winter, and year, and sun spots from year to year; but he noted that the barometric curve *lags* behind the sun-spot curve, particularly in the years of maxima of sun spots. The winter curve is more regular than the summer one, probably because the weather generally in India is more settled in the winter than in the summer; but on the whole the two curves support each other in having a *low pressure* about the time of *sun-spot maximum*, and a high pressure about the time of sun-spot minimum. We may therefore conclude that the *sun is hottest* about the time when the *spots are at a maximum*. He added, that these results appear to harmonise well with the decennial variations of the rainfall in India, and to throw light upon the inverse variation (compared with the sun spots) of the winter rainfall of Northern India.

Dr. Allan Broun also, in a discussion of Indian barometric readings, found that the years of greatest and least pressure

* Nature, Vol. XVIII., p. 567.



are probably the same for all India, and that, therefore, the relation established by Mr. Chambers for Bombay holds for all India.*

I next pass to rainfall. Dr. Meldrum, returning to his rainfall studies, found that†

"There is a remarkable coincidence between the rainfall and sun-spot variation at Edinburgh, much more remarkable than that at Madras. The years of maximum and minimum rainfall, and sun spots for the mean cycles, coincide, and on the whole there is a regular gradation from minimum to maximum, and from maximum to the next minimum."

The minimum rainfall occurred, on an average, in the year immediately preceding the year of maximum sun spots.

The results of these investigations show that the rainfall of 54 stations in Great Britain from 1824-1867 was ·75 inches *below mean* when sun spots were at a minimum, and ·90 inches *above mean* when sun spots were at a maximum.

For the 34 stations in America, the corresponding numbers were ·94 inch and 1·13 inch.

In the report of the Meterological Department of the Government of India, published this year (1878), the following reference to solar action occurs:—

"The following are the main important inferences that the meteorology of India in the years 1877-1878 appears to suggest, if not to establish:—

"There is a tendency at the minimum sun-spot periods to prolonged excessive pressure over India, and to an unusual development of the winter rains, and to the occurrence of abnormally heavy snowfall over the Himalayan region . . . . This appears also to be accompanied by a weak south-west monsoon."

In 1880 the relation of Indian famines and the barometer was first fully treated by Mr. F. Chambers, the Meteorological Reporter for Western India.‡ He concluded from his enquiry that there is some intimate relation between the variations of *sun spots, barometric pressure*, and *rainfall*; and as famines in general are induced by a deficiency of rain, it is probable that they also may be added to the above list of connected phenomena.

Commencing with the daily abnormal variations observed at several stations in Western India, it was found that as the time over which an abnormal barometric fluctuation extended

* Nature, Vol. XIX., p. 6.

† Nature, Vol. XVIII., p. 565.

‡ Nature, Vol. XXIII., p. 109.

became longer and longer, the range of the fluctuation became more and more uniform at the various stations, thus leading to the conclusion that the "*abnormal variations of long duration affect a very wide area.*" For testing this, the conditions of Batavia were compared with those at Bombay, and the results showed a striking coincidence, the curves obtained for the two places being almost identical in form, but with this remarkable difference, the curve for Batavia was found *to lag* very persistently about one month *behind* the Bombay curve.

Similar results were then worked out for other stations. St. Helena, Mauritius, Madras, Calcutta, and Zi-ka-wei. On comparing the curves obtained for these various places, though a strong resemblance in form between all the curves is observed, there is also strong evidence of a want of simultaneity in the barometric movements at different stations, and that as a rule the changes take place at the *more westerly stations* several months *earlier than* at the more *easterly ones.*

Thus, on comparing the curves for St. Helena and Madras from 1841-1846, the latter sometimes lagged behind the former as much as six months, and for Bombay and Calcutta the corresponding difference was often upwards of six months.

The facts suggested to him long atmospheric waves (if such they may be called) travelling at a very slow and variable rate round the earth, from *West to East,* like the cyclones of the extra-tropical latitudes.

With special reference to famines, he remarked that, on comparing the dates of all the severe famines which have occurred in India since 1841, widespread and severe famines are generally accompanied or immediately preceded by waves of high barometric pressure. He suggested, therefore, that intimation of the approach of famines might be obtained in two ways:—

(*a*.) By regular observations of the solar spotted area, and early reductions of the observations, so as to obtain early information of current changes going on in the sun.

(*b*.) By barometric observations at stations differing widely in longitude, and the early communications of the results to stations situated to the eastward.

In the same year, Dr. H. F. Blanford discovered that*:—

"Between Russia and Western Siberia on the one hand, and the Indo-Malayan region on the other, there is a reciprocating and cyclical oscillation of barometric pressure, of such a character that the pressure is at a maximum in *Western Siberia* and *Russia* about the epoch

* Nature, Vol. XXI., p. 480.



of maximum sun spots, and in the *Indo-Malayan area* at that of minimum sun spots."

Up to 1881, the general idea had been that there was a great difference between the meteorological conditions at the maximum and minimum of the sun-spot curve, but the more numerous and more accurate series of observations available in the year in question revealed to Meldrum "extreme oscillations of weather changes in different places at the turning points of the curves representing the increase and decrease of solar activity."

This was a most important change of front. Not the maximum only, but both the maximum and minimum had to be considered.*

In relation to these pressure changes Blanford wrote as follows†:—

"Among the best established variations in terrestrial meteorology which conform to the sun-spot cycle, are those of tropical cyclones, and the general rainfall of the globe, both of which imply a corresponding variation in evaporation and the condensation of vapour. Now the variation of pressure with which we have to deal evidently has its seat in the higher (probably the cloud-forming) strata of the atmosphere. This is not only illustrated in the present instance by the observed relative excess of pressure at the hill stations as compared with the plains, but also follows as a general law from the fact established by Gautier and Köppen, viz., that the temperature of the lowest stratum varies in a manner antagonistic to the observed variation of pressure. It is then a reasonable inference that the principal agency in producing the observed reduction of pressure at the epoch of sun-spot maximum is the more copious production and ascent of vapour, which may operate in three different ways. First, by displacing air the density of which is three-eighths greater; second, by evolving latent heat in its condensation; and thirdly, by causing ascending currents, and thus reducing dynamically the pressure of the atmosphere as a whole. The first and second of these processes do not indeed directly reduce the pressure but only the density of the air stratum while they increase its volume. In order, therefore, that the observed effect may follow, a portion of the higher atmosphere must be removed, and this will necessarily flow away to regions where the production of vapour is at a minimum, viz., the polar and cooler portions of the temperature zones, and more especially those where a cold dry land surface radiates rapidly under a winter

* Weather, Health, and Forests. Mauritius, 1881.
† Nature, xxi., p. 482.

sky. Such an expanse is the great northern plain of European Russia and Western Siberia north of the Altai."

In 1886 we got the first fruits of the observations of the widened lines in sun spots, which had been obtained on a definite plan, since 1879. The changes which occurred from a spot-minimum to a spot-maximum, and some distance beyond, had therefore been recorded. The changes were most marked, showing a great change in the chemistry of the spots at these times. At minimum the lines chiefly widened were those of iron and some other metals, but at the maximum the lines widened were classed as "unknown," because they had not been recorded in the spectra of the terrestrial elements. It was reasonable to suppose, therefore, that the sun was not only hotter at maximum, but hot enough to dissociate iron vapours.*

In 1891 Janssen's suggestion of 1869 was brought into a practical shape for observatory work, by Hale and Deslandres,† and the prominences on the sun's disc, and surrounding it, were photographed in full daylight by using only the light radiated by the calcium vapour, which they always contain.

By the year 1900 we had accumulated, at South Kensington, observations of the widened lines for a period of over 20 years. There was a curious break in the regularity of the results obtained after 1894, and the Indian meteorologists reported contemporaneous irregularities in the Indian rainfall.

I determined, therefore, to make a connected enquiry into both these classes of phenomena. Thanks to the establishment of the Indian Meteorological Department in 1875 we had rainfall tables extending over a quarter of a century, and in the tropics, where the problems might be taken as of the simplest, to compare with the new solar data.

I have already stated that in the preliminary discussion of the most widened lines observed in the sun spots up to the year 1885 a most remarkable difference was observed in the lines observed at sun-spot maximum and minimum. This continued till about 1895, another ten years. As the curve of iron lines went up, the curve of "unknown" lines came down; there were therefore *crossings* of the curves which might, on the hypothesis before referred to, be taken as the times at which the temperature of the sun had a mean value. These crossings turned out to be about half-way between the maxima and minima of the spotted area which had to be considered as the times at which the sun was hotter and colder than the mean.

* Proc. Royal Soc., 1886, p. 353.

† Comptes Rendus, 17th August, 1891.



We were then brought into the presence of three well-marked stages of solar temperature—it was no longer a question merely of spots and no spots, but of heat pulses.

The next point was to study these heat pulses in relation to the Indian rainfall, and it was found that in many parts of India the plus and minus heat pulses on the sun, which, of course, occurred immediately after the time of mean temperature, when the sun was getting either hotter or colder, were accompanied by pulses of rain in the Indian Ocean and the surrounding land. It was next found, from a study of the Indian Famine Committee's Reports, that the famines which have devastated India during the last half century have occurred in the intervals between the pulses.

In 1902, with a view of getting more light on the important issues raised by the comparison of the solar heat pulses and the Indian rainfall, I determined to reduce the observations of prominences made by Tacchini at the Observatory of the Collegio Romano since 1874, and compare the Indian meteorological conditions with them. The reason for this step was that the admirable photographs of the prominences on the solar disc, published by Hale and Deslandres, showed the extensive area over which they were distributed. An argument which has been used against the possible connexion between solar and terrestrial changes was based upon the small area covered by spots. In 1877 Eliot wrote as follows:—*

> "So far as can be judged from the magnitude of the sun spots, the cyclical variation of the magnitude of the sun's face free from spots is very small compared with the surface itself; and consequently, according to mathematical principle, the effect on the elements of meteorological observations for the whole earth ought to be small."

Now the photographs, to which I have referred, exhibited broad bands of prominences extending almost across the whole disc, and if we assume two belts of prominences, N. and S., 10° wide, with their centres over latitude 16°, a sixth of the sun's hemisphere would be in a state of disturbance. Hence it followed that the prominence effect, when fully studied, might be much more striking and important than that produced by spots.

The prior work in connection with the Indian rainfall had shown not only that there was a close connection between pressure and rainfall, but that the pressure was much the more constant element over the different areas. The comparison with the prominences obtained from the discussion of Tacchini's results was in the first instance compared with the Indian pressure curve.

* Report on the Meteorology of India, 1877, p. 2.



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The result was magnificent. In addition to the well-marked prominence maximum at the maximum of the spotted area, there were others corresponding approximately with the "crossings" of the widened lines, and all were re-echoed by the Indian barometers!

The sun-spot cycle of eleven years gave way to a prominence cycle of about 3·7 years, and by this interval, as a rule, are the Indian pressures separated.

To see whether such a striking and important result as this was limited to Indian ground, the important series of pressure obtained at Cordoba in South America were studied. Here the same effect was also most marked, but with the important difference that the curves were inverted; that is, high pressure years in India were represented by low pressure years in Cordoba.

In order to extend the Indian and Cordoba areas and see how far these conditions prevailed, the pressure variations of stations as widely distributed as possible were examined. The result of this inquiry showed that the world might be divided roughly into two portions. The Indian area was found to extend to Australia, East Indies, Asiatic Russia, Mauritius, Egypt, East Africa, and Europe, while the Cordoba region might be said to include not only South and Central America, but the United States and Canada, extending further west than Honolulu.

This discovery of this barometric surge, which has been corroborated since by Prof. Bigelow, was an important advance, and will enable the investigator to connect up regions that undergo similar pressure changes.

In addition to the two periods, namely, 11 and 3·7 years, mentioned above, Brückner* has pointed out that there is a long period weather variation. His discussion of all the available data of pressure, rainfall, temperature, &c., led him to conclude that there is a periodical variation in the climates over the whole earth, the mean length of this period being about 35 years.

Since this work, a recent discussion of the sun-spot data by Dr. W. J. S. Lockyer† has brought to light a similar long period, and this has taught us that each eleven-year cycle is different from the one immediately preceding and that following it.

A further inquiry into the distribution of the solar prominences, as observed by Respighi, Secchi, Tacchini, Riccò, and Mascari,‡ has resulted in increasing our knowledge of the circulation of the solar atmosphere. The centres of prominence action, or the centres of the prominence belts, have a tendency to move from low to high latitudes, the opposite of spots; generally speaking, two belts in each hemisphere exist for some time, then they couple up and move towards the solar poles, while in the meantime a new belt begins to form in low latitudes.*

* Klimaschwankungen, Eduard Brückner (Vienna, 1890).
† Proc. Roy. Soc. Vol. 68, pp. 285–300.
‡ Memorie della Società degli Spettroscopisti Italiani.



The existence of prominences in the polar regions is coincident with great magnetic disturbances on the earth, just previous to or about the time of sun-spot maxima.† Further, these polar prominences are responsible for the existence of large coronal streamers near the solar poles, as seen during solar eclipses about the time of sun-spot maximum. In fact, recent research seems to indicate that this prominence circulation is intimately associated with all the different forms of the corona.‡

There seems little doubt, therefore, that we must look to the study of the solar prominences not only as the primary factors in the magnetic and atmospheric changes in our sun, but as the instigators of the terrestrial variations.

In dealing with solar phenomena, especially from a meteorological point of view, it is of great importance that the solar disc be treated in zones and not as a whole.

Just as it has been shown that the prominences sometimes exist in these zones in one hemisphere at one time, so is this the case with spots, but, unfortunately, it is only until very recently the phenomena occurring in each hemisphere have been treated in this manner.

It has already been pointed out that a possible connexion existed between changes in the spotted area of the sun and terrestrial temperatures. Quite recently this question has been studied by Charles Nordmann,§ who finds that—

> "The mean terrestrial temperature exhibits a period sensibly equal to that of solar spots; the effect of spots is to diminish the mean terrestrial temperature, that is to say, that the curve which represents the variations of this is parallel to the inverse curve of the frequency of solar spots."

* Proc. Roy. Soc., Vol. 71, pp. 446–452.
† *Ibid.*, pp 244–250
‡ Monthly Notices R.A.S., Vol. LXIII., 1903.
§ Comptes Rendus, No. 18, 4th May, 1903, Vol. 136

## APPENDIX VIII.

### THE USE OF RADIO-ACTIVE SUBSTANCES AS COLLECTORS OF ATMOSPHERIC ELECTRICITY.

By M. Adam Paulsen.

I have already given a brief description of the apparatus used for determining the electric potential of the air during my stay in Iceland (1899-1900), in a short paper, printed with the Reports, presented to the Meteorological Congress of Paris in 1900. A filter paper was rubbed with a radio-active powder and placed on the upper surface of a disc of copper, which formed the collector. This apparatus was unsatisfactory, as it could not be used during damp weather. An attempt was made in Iceland to cover the powder with a sheet of aluminium, but it was not possible to prevent the powder from slipping from beneath the sheet.

M. La Cour, the present head of the weather service of the Danish Meteorological Institute, has perfected the apparatus so that it can now be used under all meteorological conditions. For this purpose, the radio-active powder is mixed with a solution of india-rubber so as to form a paste which is spread on a copper disc which acts as the collector. Before the paste sets, a piece of copper gauze is pressed against it so as to sink it slightly into the paste without covering the threads of the gauze.

This is the arrangement of the electrode which M. La Cour used during his stay in Finland (1900-1901). Should the paste get moistened by rain-water, its action is retarded, but by drying it, it can be brought back to a satisfactory state of activity; it need only be slightly heated.

To test the insulation of the metal disc used as a collector, and of the electroscope with which the potential of the air was measured, the electrode was covered with another copper disc, and an electric charge communicated to the whole system.

I have the honour of presenting to you the apparatus which was in use in Finland. Insulation was secured, as in Iceland, by means of a cylinder of paraffin fixed at the bottom of an earthenware vessel. The paraffin projects into an inverted



metal vase, whose bottom rests on the upper end of the column of paraffin. The object of thus surrounding the insulator was to prevent the deposition of dust or the precipitation of water or ice on the surface of the paraffin. The experiments made in Iceland and Finland have shown that the paraffin always gives good insulation.

Such was the apparatus used during the stay of the Danish Expedition in Finland. With a powder of a radio-active power of 1,000, the potential of the air in contact with the disc is obtained in 1½ seconds. The instrument is consequently liable to show immediately any sudden and transient changes of the atmospheric potential. The apparatus might, perhaps, be made to retain its great sensitiveness during rain by replacing the disc under such circumstances by another, of which the lower surface is also covered with a paste of radio-active powder and protected from the rain by a small screen attached to the edge of the disc.

If the apparatus appears to you to be worth attention, I venture to recommend it for examination at the observatories in which measurements of atmospheric electricity are carried on. It need hardly be said that the insulator could be put in any of the rooms of the observatory.

## APPENDIX IX.

### THE APPLICATION OF RADIUM SALTS TO THE STUDY OF ATMOSPHERIC ELECTRICITY.

By M. Moureaux.

In observations of Atmospheric Electricity the potential of the air is usually obtained by means of a water-dropper. This arrangement, which works perfectly in warm countries, often becomes useless in our climate on account of the freezing of the water, as the reservoir is frequently placed on the top of a high building where there are no arrangements for warming it. Attempts have been made to protect the nozzle of the water-dropper against frost by covering it with wool, or by adding a certain amount of alcohol or glycerine to the water, but these arrangements present great difficulties in practice. As a matter of fact, the registration of variations of potential is frequently interrupted in winter, at any rate, during severe frosts.

The discovery of the salts of radium, by M. and Mme. Curie, has provided a new method which is applicable during all seasons. M. Curie has had the kindness to put at our disposal some samples of radium salts of different degrees of activity, and we have studied them successively by comparing them with the water-dropper in ordinary use. The arrangement provisionally adopted in these experiments was as follows:— In a copper disc of about 4 cms. diameter and 2 mms. thickness M. Curie had a cavity of 15 mms. diameter and of half the thickness of the disc scooped out; a thin layer of asbestos was placed in the bottom of this cavity, and on this 0·1 gr. of chloride of barium containing radium was placed. A plate of aluminium 0·1 mm. in thickness was then carefully soldered on to the copper disc so as to cover the salt and to completely seal up the capsule thus formed. A copper tube, of exactly the same size at its free end as the usual orifice of the water reservoir, was soldered on to the reverse surface of the disc; the potential of the air could then be obtained at will, either by means of the radium, or by means of the water-dropper, at two points in the atmosphere as near each other as possible.

The experiments were tried at the Observatory of Parc Saint-Maur during March and April, 1903, with the apparatus there in use. Three capsules were studied successively. In the first, one of the first samples of radium obtained by M. and Mme. Curie in 1899 was placed; its radio-activity had not

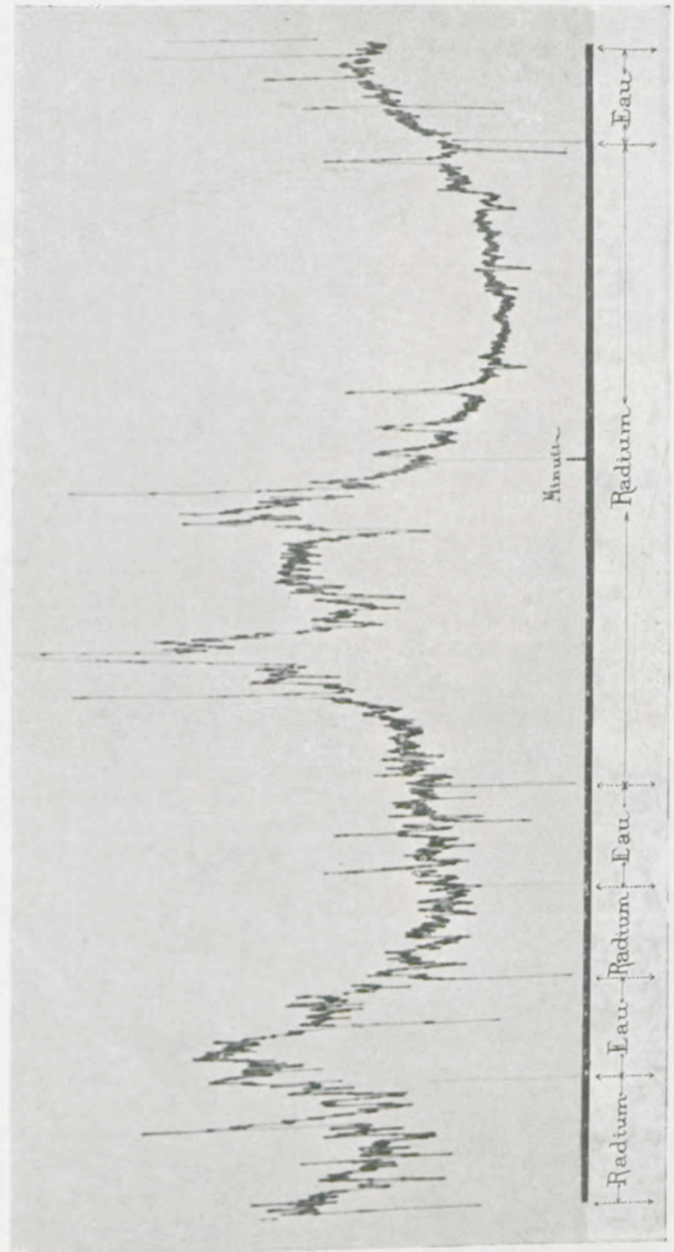
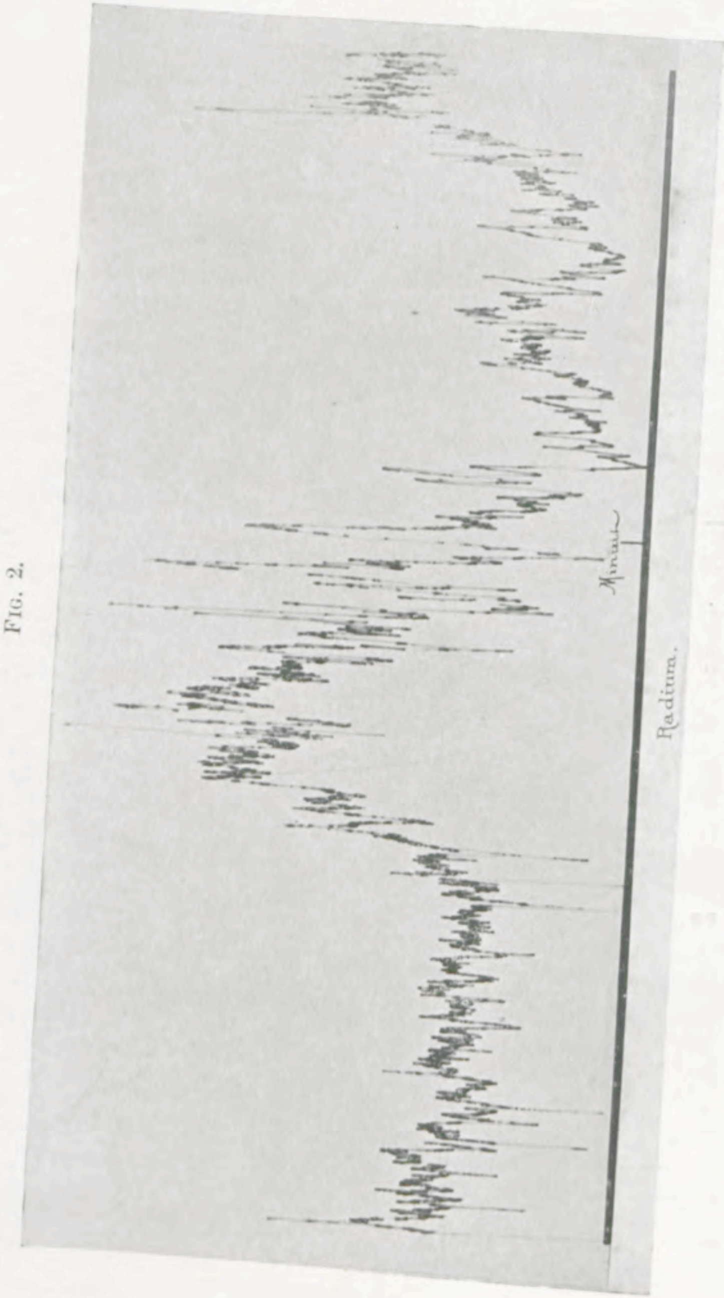



Fig. 1.

Fig. 2.






been determined. Experience showed that in every case the radio-activity of this salt was insufficient; the needle of the electrometer was brought into equilibrium with the potential of the air only very slowly. Rapid variations were considerably damped, and, moreover, the curves traced always showed a distinctly lower potential than those obtained with the water-dropper; the return to the zero was also extremely slow.

The second capsule contained some chloride of barium of radio-activity 5,000 times that of uranium. The curves obtained with this second specimen do not differ sensibly from the preceding ones; they show that in this case, also, the radio-activity is not sufficiently great to insure the values recorded being equal to the true potential of the air in the neighbourhood of the capsule, and the details of the variations are also reduced or even suppressed by reason of the slowness of the motion of the needle of the electrometer.

Finally the third capsule contained 0·1 gramme of chloride of barium containing radium of activity 30,000 times that of uranium; this degree of activity renders the sensitiveness of the apparatus comparable with that of the water-dropper. Figs. 1 and 2 are photographic reproductions of two of the daily curves traced by the recorder while this third capsule was placed at the end of the jet. In the case of the first curve, the potential was determined, at fixed intervals, alternately with the radium and with the water-dropper; it will immediately be seen that the state of equilibrium and the return to the zero are effected with equal rapidity under both conditions of the experiment. The same details are obtained with the radium as with the water-dropper, and the curve is continuous at the points where the capsule was put on or taken off.

The experiments were continued for several days and always gave the same results.

The curve of Fig. 2 has been obtained entirely with the third capsule; it differs in nothing from those obtained with the water-dropper. This capsule is now in regular use at the Observatory of Parc Saint-Maur: control experiments are carried out from time to time to make sure that the radio-active salt has lost none of its energy.*

* Since this note was prepared (August 1903) the aluminium plate was found (in November) to have been affected in course of time by the emanations from the radium; it shows a large number of perforations, and the metal, having been worn away above the cavity, no longer afforded sufficient protection. M. Curie is of opinion that a good varnish will protect the salt from changes of humidity as efficiently as a plate of aluminium; experiments have been successfully tried by spreading three coats of a special varnish, used in the coaches of the "voitures de luxe," over the whole face of the capsule. This varnish, which resists inclemencies of weather well, secures complete and permanent protection and, moreover, it presents the further advantage of diminishing the resistance which the plate of aluminium opposes to the emanations of the radium, so that by suppressing the aluminium plate it is possible to use a salt of smaller radio activity which can be obtained at a lower cost in the type of apparatus definitely adopted. The experiments have been continued in this manner on the advice of M. Curie.

The process then has the necessary efficiency, provided that the salt is endowed with sufficient radio-activity, and that it is completely protected from the effects of moisture.

The advantages of the employment of radio-active salts for the observation of atmospheric electricity are numerous; among the principal ones may be mentioned, constancy of the height above the ground of the point at which the potential is measured; the ease with which the point may be raised above buildings, trees, and other obstacles of any kind which, by modifying the course of the equi-potential surfaces, deprive the actual observations of a great deal of their value; complete avoidance of the breaks in the records caused by a stoppage of the flow due to dirt in the water or enforced by the severity of the temperature; less constant attendance; suppression of the reservoir and consequent diminution of the cost of installation and maintenance; extension of the method to the polar regions where the study of the phenomena is particularly interesting.

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## APPENDIX X.

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## ON THE METEOROLOGICAL SERVICE OF THE AZORES.

By M. F. A. Chaves, Director.

An account of the organisation of the meteorological observations of the Azores which were to be maintained by an international grant, in accordance with the project put forward by and under the distinguished patronage of His Serene Highness Prince Albert I. of Monaco, was presented to the International Meteorological Congress which met in Paris in 1900.*

Shortly after this, His Majesty the King of Portugal, Dom Carlos I, deigned to inform me of his desire to establish such a service at the sole expense of Portugal. With the consent of his Government, I was charged with the organisation of this meteorological service; the Legislature sanctioned its formation in June, 1901, and work was commenced under my direction in the following October.

Their Majesties the King and Queen, desiring to give expression to the great interest which it was their pleasure to take in the new institution in a striking manner, came to the Azores to lay the first stone of the Meteorological Observatory at Horta.

Under the distinguished patronage of a Sovereign so enlightened and so devoted to the progress of Science and Navigation, the meteorological service of the Azores has been organised as follows:—

### Staff.

A director, whose duties consist in supervising the work of all the observatories of the Azores, and whose usual place of residence is at the Island of St. Michael.

* Report on the proposed establishment of an international meteorological service at the Azores. Printed at Monaco 1900.

*At St. Michael.*

One superintendent, to undertake magnetic and seismological observations, three meteorological assistants, and one servant.

*At Terceira.*

Two meteorological assistants.

*At Fayal.*

One superintendent, one meteorological assistant, one servant.

*At Flores.*

A similar staff to that at Fayal.

The chief officers of the semaphore stations at Ferraria and Arnel, on St. Michael, and at Capellinhos, on Fayal, are paid by the meteorological service of the Azores to make meteorological observations, which are sent by telegraph every day to the observatories at Ponta Delgada and Horta respectively.

OBSERVATORIES.

Observatories have been installed on the Islands of St. Michael (at Ponta Delgada), Fayal (Horta), Terceira (Angra), and Flores (Santa Cruz).

The Meteorological Observatories of Horta and Santa Cruz carry on their work in provisional buildings; those of Ponta Delgada and Angra have been definitely installed under favourable conditions in former convents.

Buildings for the Meteorological Observatory of Horta and for the Magnetic Observatory of Saint Michael are at present in process of construction.

The site of the latter is about 6 kilometres distant from the town of Ponta Delgada, in a region exclusively devoted to the culture of cereals, and free from water-courses which could be used as sources of power for dynamos. It consists of a residence for the staff, some structures of wood to house the magnetic instruments, similar to those used at Cheltenham (near Washington), and a building for the seismographs.

In addition to the services described in the report mentioned above, I have to draw attention to the following particulars:—

1. Since 1901, the Island of Fayal has been in possession of four cables (in addition to the Horta-Ponta Delgada-Lisbon Line), which put it in direct and constant communication with the United States (landing place, New York), Canada (Canso), Germany (Emden), and England (Waterville). A new service of meteorological telegrams has consequently been established, by means of these cables, between the Observatory of Horta and the Weather Bureau of Washington, the central Meteorological Offices of Paris and London, and the Deutsche Seewarte of Hamburg.



2. Seismographs have been installed at Horta and Ponta Delgada and have been in use since December last.

3. A chronometric station has been established at the Meteorological Observatory of Ponta Delgada, and has proved itself to be of great value to navigation. The comparison of chronometers is undertaken gratuitously for all navigators on application. The time is sent by the Royal Astronomical Observatory of Lisbon, and is received on an electric chronograph so arranged as to afford a direct comparison of the Lisbon clock, not only with that of the station, but also with any other chronometer.

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## APPENDIX XI.

### THE USE OF THE HAIR HYGROMETER INSTEAD OF THE PSYCHROMETER.

By M. J. M. Pernter.

In 1899, at the Meeting of the International Meteorological Committee at St. Petersburg [the use of the psychrometer at stations of the second order (Report of the International Meteorological Committee, 1899)], I proposed a resolution to the effect that the hair hygrometer is to be preferred to the psychrometer at stations of the second order, at which the non-ventilated phychrometer is generally employed. The Committee did not feel themselves to be in a position to pronounce an opinion on this question. I had not then at my disposal the results of the experiments which have been made at the K.K. Centralanstalt at Vienna under my direction, and I therefore contented myself with stating that I would renew my proposal when I should be in a position to bring before the Committee the details of the results of our experimental researches on the hair hygrometer. That is what I propose to do to-day, by moving the following resolution: "The Committee are of opinion that the hair hygrometer is at least of equal value with the non-ventilated psychrometer, and that in cases in which it is not possible to have both instruments, and in which a choice has to be made between one or other, the hair hygrometer is to be preferred." On this occasion my proposal is based on the extensive researches of Dr. Pircher [Ueber Haar Hygrometer (Denkschriften der kais. Akad. d. Wiss. in Wien 73 Bd. Jubelband der K.K. Centralanstalt für Meteorologie, p. 267)], which are known to all. The conclusions to which his researches have led him are the following:—

(1.) The values of the humidity indicated by the hair hygrometer are independent of the temperature of the air.

(2.) These values may differ from the humidity of the surrounding air by as much as 4 per cent.

(3.) The aspirated psychrometer gives even larger differences. The aspirated psychrometer and the hair hygrometer may give differences of humidity up to 5 per cent.

(4.) The error of the non-ventilated psychrometer is at times double that given by the hair hygrometer (also non-ventilated), even though the psychrometric factor corresponding to the force of the wind be used.

(5.) The indications of the hair hygrometer are independent of the force of the wind, those of the psychrometer, on the contrary, depend greatly on the velocity of the wind.



(6.) The sensitiveness of the hair hygrometer is much greater than that of the psychrometer.

To these advantages, which the hair hygrometer possesses over the psychrometer, may be added the following:—

(7.) The hair hygrometer gives the humidity of the air by direct reading, and the tension of the aqueous vapour can be easily and rapidly calculated by means of the temperature of the air. If tables of the tension of aqueous vapour are desired, they can be easily calculated and are much shorter than psychrometric tables.

(8.) The hair hygrometer will work normally also at low temperatures, whereas the psychrometer requires a trained experimenter in order to give satisfactory results under these circumstances.

(9.) The hair hygrometer is the only type of instrument which can be employed in "ballons-sondes" experiments, and even in ascents with manned balloons the ventilated psychrometer gives incorrect results, whereas the hair hygrometer remains exact.

In discussing the advantages of the hair hygrometer, the defects of the non-ventilated psychrometer must also be considered. The formula for the psychrometer is not theoretically correct; it as a formula based on empirical co-efficients. It requires a knowledge of the constant A in the formula

$$e'' = e' - A\,h\,(t - t')$$

But A is found to vary with the velocity of the wind. Consequently three different values for A have to be used: A = 0·0012 in calm weather, 0·0008 when the wind is light, and 0·000656 when it is strong. But that is not all. The value of A also depends on whether the wet bulb is covered with water or with ice. In the latter case, the above values become respectively A = 0·00106, A = 0·000706, and A=0·000579. Nor is this all! M. Svensson has shown* that this factor changes with the humidity itself, in such a manner that A diminishes as the psychrometric difference increases. In consequence of this, it is necessary to substract from $e'$ not only the quantity $A\,h\,(t-t')$ but also a further correction $\varepsilon$ and M. Svensson writes the equation in the form

$$e'' = e' - \varepsilon - A\,h\,(t - t')$$

in which A decreases in such a manner that the results do not differ much from those given by the ordinary formula. But even if this last variation of A, which has only a small effect on the result, be neglected in practice, although its influence is always inconvenient if great accuracy is desired, the other

* A. Svensson, Zur Kenntniss des ventilirten Psychrometers (Akademische Abhandlung: Stockholm, 1898, p. 34). Meteorologische Zeitschrift, Vol. xxxi, p. 213.

variations remain and render the use of the psychrometer inexact.

But the determination of the pressure of the aqueous vapour in the air does not depend only on A, but also on the barometric pressure $h$. For this reason it becomes necessary to calculate $e''$ from different tables for each change of the barometer exceeding 40 mms. (1·6 ins.). The changes in the height of the barometer never exceed 40 mms. at any one station, and, consequently, sufficient accuracy is attained if the mean height of the barometer be used in the formula when calculating a psychrometric table. But for another station, at which the mean height of the barometer differs from the adopted value by more than 40 mms., another table must be calculated.

If we restrict ourselves to stations of which the altitude does not exceed 3,000 metres, we have already 7 different tables; but as we have seen above, we have to consider 6 different values of A, which gives $6 \times 7$ different values of A $h$, and consequently 42 psychrometric tables. In the following table I have put together these different values of A$h$, with the help of which the tables can be calculated:—

| — | Thermometer covered with | mms. 755 | mms. 715 | mms. 672 | mms. 638 | mms. 594 | mms. 558 | mms. 524 |
|---|---|---|---|---|---|---|---|---|
| Calm ... | Water ... | 0·906 | 0·858 | 0·805 | 0·758 | 0·713 | 0·669 | 0·626 |
| | Ice ... | 0·800 | 0·758 | 0·712 | 0·667 | 0·630 | 0·578 | 0·555 |
| Light wind. | Water ... | 0·604 | 0·572 | 0·538 | 0·506 | 0·475 | 0·446 | 0·420 |
| | Ice ... | 0·533 | 0·505 | 0·475 | 0·446 | 0·419 | 0·394 | 0·370 |
| Strong wind. | Water ... | 0·495 | 0·469 | 0·441 | 0·415 | 0·390 | 0·366 | 0·344 |
| | Ice ... | 0·437 | 0·414 | 0·389 | 0·366 | 0·344 | 0·323 | 0·303 |

I have, as a matter of fact, used these 42 factors to calculate the "Kurze Psychrometer Tabellen" (abridged psychrometric tables) published in the new edition of Jelinek's "Psychrometer Tabellen." As we have seen above, the hair hygrometer is independent not only of the speed of the wind and of frost, but also of the pressure of the atmosphere. In spite of these and several other advantages of the hair hygrometer, the psychrometer possesses one advantage, the importance of which cannot be denied; there is no necessity for verifying its readings from time to time; the corrections of the thermometers, once determined, remain constant. On the other hand, the hair hygrometer has to be examined from time to time, and its readings cannot be relied on unless the point 100 is frequently verified.



But this does not mean that the psychrometer requires no attention. On the contrary, constant care is needed to see that it is well moistened, and the muslin must be frequently changed, at least once a month. But this involves much more work than the verification of the hair hygrometer and the testing of the saturation point, which need only be done once every three months. This advantage which the psychrometer possesses depends of the fact, that the thermometers remain constant, or should any separation of the mercury in the column occur, it is noticed immediately. On the other hand, it is impossible to tell whether the indications of the hair hygrometer have altered without making a fresh determination of the point 100. This is especially true when the changes take place in a continuous manner, and for this reason it is necessary to test the instrument from time to time. If the changes are sudden, an observer, even one of little experience, notices the error immediately. Consequently no such great importance need be attached to this superiority of the psychrometer as to prevent us from stating that the hair hygrometer is an instrument of at least equal value.

Excellent hair hygrometers can now be obtained. We have placed an instrument of this type, invented by Dr. Schmid of Bruck-on-the-Mur, in the exhibition of meteorological instruments at Southport. This hygrometer will, we believe, satisfy all demands made on it.*

In spite of what I have said, I am no opponent of the psychrometer for temperatures above the freezing point of water. On the contrary, I think it is highly desirable—if sufficient money is available—to use both the psychrometer and the hygrometer at stations of the second order. The readings of a wet-bulb thermometer have, above all, great physiological value, as indicating the temperature of the skin of the human body. For this reason these temperatures must be regarded as a most important climatological element. I have arranged the new edition of Jelinek's tables in such a way that it will be possible to use psychrometer observations for obtaining the humidity of the air with sufficient accuracy on all occasions when the wet-bulb is not covered with ice. Below the freezing point, whether satisfactory results are obtained with this instrument, depends entirely on the efficiency, attention, and patience of the observer.

By reason of what I have stated above, I now ask the Committee to accept my proposal, and to adopt a resolution to the effect that the hair hygrometer is an instrument at least of equal value to the non-ventilated psychrometer, and that consequently the hair hygrometer ought to be preferred at stations of the second order in those cases in which both instruments are not used.

* A description of this instrument will shortly be published.

# APPENDIX XII.

## NOTE ON THE HAIR HYGROMETER.

By M. RYKATCHEW.

Richard's self-recording hair hygrometers have been used since 1895 for observing the humidity of the atmosphere at Pawlowsk, and the hourly values of that element are published in our "Annales"; a small type of instrument was used during 1895, another one was substituted for this during 1896-97, and since 1898 an instrument of a large type has been in use.

Throughout the summer months (May to September) the readings of the hygrograph were checked by means of daily observations of a ventilated psychrometer, made at 7 a.m., 1 p.m., and 9 p.m. The mean result of this checking is to show that the values given by the hygrograph are too large after a dry period and too small after a damp one. This will be seen from the following table, which gives mean values for two sets of observations, one containing the observations made after a relatively dry interval of 24 hours, during which the humidity remained below 90 per cent., the other the observations made at the close of intervals of great dampness during which the humidity remained between 99 and 100 per cent. for the preceding 24 hours:—



TABLE I.

*Differences: Richard Self-recording Hygrometer—Ventilated Psychrometer.*

| Years. | After dry weather. (Maximum < 90 per cent.) | | | | | After great humidity. (99 to 100 per cent.) | | | | | Amplitudes. | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | 7 a.m. | 1 p.m. | 9 p.m. | Mean. | No. of Obs. | 7 a.m. | 1 p.m. | 9 p.m. | Mean. | No. of Obs. | 7 a.m. | 1 p.m. | 9 p.m. | Mean. |
| | Per cent. | Per cent. | Per cent. | Per cent. | | Per cent. | Per cent. | Per cent. | Per cent. | | Per cent. | Per cent. | Per cent. | Per cent. |
| 1895 ... | 1·1 | 1·0 | 2·0 | 1·4 | 30 | — 2·3 | — 1·9 | 0·1 | — 1·4 | 77 | 3·4 | 2·9 | 1·9 | 2·7 |
| 1896–97 ... | 2·5 | 1·5 | 1·9 | 2·0 | 86 | — 1·5 | — 2·0 | 0·5 | — 1·0 | 91 | 4·0 | 3·5 | 1·4 | 3·0 |
| 1898–1900 | 2·6 | 1·7 | 2·7 | 2·3 | 102 | — 1·9 | — 1·1 | 1·7 | — 0·4 | 245 | 4·5 | 2·8 | 1·0 | 2·8 |
| Means ... | 2·1 | 1·4 | 2·2 | 1·9 | 218 | — 1·9 | — 1·7 | 0·8 | — 0·9 | 413 | 4·0 | 3·1 | 1·4 | 2·8 |

It will be seen that the result is the same for each of the instruments, and for all the years considered; the hygrograph gives values which are too high by 2 per cent. after dry periods, and too low by 2 per cent after damp periods. The mean amplitude of the divergences shown by the hygrograph is therefore 4 per cent., depending on the humidity which prevailed during the previous 24 hours.

In the following table the differences at the moment of observation, after dry and damp periods respectively, have been grouped according to the relative humidities prevailing:—

TABLE II.

*Differences: Richard Self-recording Hygrometer—Ventilated Psychrometer for Different Degrees of Humidity.*

| Humidity. | After dry weather. | | | | After great humidity. | | | | Differences. | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | 7 a.m. | 1 p.m. | 9 p.m. | Mean. | 7 a.m. | 1 p.m. | 9 p.m. | Mean. | 7 a.m. | 1 p.m. | 9 p.m. | Mean. |
| Per cent. | Per cent. | Per cent. | Per cent. | Per cent. | Per cent. | Per cent. | Per cent. | Per cent. | Per cent. | Per cent. | Per cent. | Per cent. |
| 90 | 2·7 | 3·0 | 2·7 | 2·8 | —1·0 | —1·5 | 1·6 | —0·3 | 3·7 | 4·5 | 1·1 | 3·1 |
| 80 | 2·2 | 2·5 | 3·3 | 2·7 | —3·0 | —2·8 | 0·8 | —1·7 | 5·2 | 5·3 | 2·5 | 4·3 |
| 70 | 2·6 | 1·0 | 2·3 | 2·0 | —1·7 | —1·9 | 2·1 | —0·5 | 4·3 | 2·9 | 0·2 | 2·5 |
| 60 | 2·3 | 1·9 | 1·3 | 1·8 | —2·4 | —1·0 | —0·8 | —1·4 | 4·7 | 2·9 | 2·1 | 3·2 |
| 50 | 0·0 | 1·8 | 2·3 | 1·4 | — | —1·0 | — | — | — | 2·8 | — | — |
| 40 | — | 0·6 | — | — | — | —0·7 | — | — | — | 1·3 | — | — |
| 30 | — | 2·7 | — | — | — | —1·6 | — | — | — | 4·3 | — | — |



These results show that the greatest differences occur when the humidity is 80 per cent.

The following investigations show that the correction of the hygrometer remains more nearly constant when the instrument is kept in a very moist atmosphere during the interval between the observations than when it is exposed to ordinary conditions. From July, 1900, to the end of the year 1902, simultaneous readings of a psychrometer and of two hygrometers were taken at St. Petersburg at the fixed hours of observation; both these hygrometers were placed in Russian screens, the one in its cage, the other without it; the latter remained under a bell-jar, part of which was covered internally with muslin saturated with water. The humidity under this bell-jar was about 97 per cent. during the summer months. Half an hour before each observation the bell-jar was removed. Table III., page 85, gives the mean corrections of the readings of these hygrometers at humidities of 80 per cent. and 60 per cent. of saturation referred to the psychrometer observations.

The diurnal range of the differences between the readings of the hair hygrometer and those of the psychrometer is much diminished in the case of the hygrometer, which is exposed to a high humidity during the intervals between the observations, as may be seen from the mean values given in Table IV., page 86.

Moreover, it was found that the differences of the corrections applied to each of the observations from the mean correction were greater for the hygrometer under ordinary conditions than for that kept in a very damp atmosphere, as is shown in the following table. (*See* Table V., page 87.)

The following experiments which were carried out at the Central Physical Observatory by M. Choukevitch, between 28th July and 9th August, 1903, with a large number of hygrometers, show that the differences referred to do not depend on the psychrometer but on the humidity to which the hair hygrometer has been exposed previous to the observation. Throughout the experiments two of the hygrometers remained in a room in which the humidity remained nearly constant at 65 per cent. They gave values of the relative humidity identical with those given by an Assmann psychrometer placed in the room. The other hygrometers and the Assmann psychrometer were placed inside a large glass case which was closed on all sides; two small openings, which could be closed at will, allowed of the psychrometer being moistened, and of the ventilator being wound up. The humidity of the air in the case could be varied by placing a more or less dilute solution of sulphuric acid in it; to secure great humidities, the acid was replaced by slightly warmed water. The instruments were read after the observer had satisfied himself that the humidity had remained constant for at last half an hour. After the hygrometers had been exposed to a given humidity for a certain length of time, they were taken out of the case and readings were taken in the room in which the humidity had remained almost constant. The results of these comparisons which were repeated several times show that the hygrometers always give values which are too high after exposure to a dry atmosphere, and too low after exposure to a very moist one.

For example, the hair hygrometer No. 29,557 gave the following comparisons. (*See* Table VI., page 88.)

On July 30th the humidity inside the case was successfully maintained at 90 per cent. for a period of three hours by means of a very weak solution of sulphuric acid; during the last half hour the aspirated psychrometer invariably showed 90 per cent., the hygrometer 95 per cent. On the 31st, by using pure water, the humidity was increased to 92 per cent., at which point it was kept constant; at the end of five minutes the hygrometer read 97 per cent., so that its correction had remained constant; but after five minutes it commenced to fall, and after 20 minutes the reading was 90 per cent.; it had, therefore, fallen by 7 per cent., while the humidity, as shown by the psychrometer, remained constant. Immediately after this experiment the hygrometer was placed in the room in which the humidity was, at the time, 70 per cent.; the hygrometer gave a reading which was less by 5 per cent. On the same day the hygrometer was exposed to a humidity of 98 per cent., first for a period of 15 minutes and then for 45 minutes. On both occasions the reading of the hygrometer rose to a certain extent, but less on the second occasion than on the first, and then it fell so that the final reading at the end of the experiment was 10 per cent. lower than that given by the psychrometer. When placed in the room in which the humidity remained constant at 70 per cent. the difference between its readings before the experiments began and after the last one was 6·5 per cent. A repetition of the experiments on the 4th, 7th, and 8th of August gave almost exactly the same results.

Ten other hygrometers experimented on at the same time gave similar variations. The following table gives the difference of the readings of these hygrometers at a humidity of 70 per cent. before and after the experiment, as well as the diminution of their readings during the last 20 minutes while they were exposed to a humidity of 92 per cent. (*See* Table VII., page 89.)



TABLE III.

*Corrections of Hair Hygrometers Compared with a Ventilated Psychrometer.*

| Month. | 1900. Under Ordinary Conditions. No. 2282. 80 per cent. | 60 per cent. | 1900. After Great Humidity. No. 2279. 80 per cent. | 60 per cent. | 1901. Under Ordinary Conditions. No. 2281. 80 per cent. | 60 per cent. | 1901. After Great Humidity. No. 2284. 80 per cent. | 60 per cent. | 1902. Under Ordinary Conditions. No. 2281. 80 per cent. | 60 per cent. | 1902. After Great Humidity. No. 2284. 80 per cent. | 60 per cent. |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| May ... ... ... | — | — | — | — | —6 | —7 | +4 | +2 | +4 | +1 | +6 | +5 |
| June ... ... ... | — | — | — | — | 0 | —2 | +5 | +2 | +5 | +3 | +6 | +6 |
| July ... ... ... | —5 | —4 | +4 | +2 | 0 | —2 | +5 | +2 | +6 | +4 | +7 | +6 |
| August ... ... ... | —4 | —2 | +5 | +2 | +3 | 0 | +4 | +2 | +7 | +5 | +7 | +6 |
| September ... ... | —2 | —2 | +6 | +3 | —2 | —2 | +4 | +2 | +5 | +4 | +7 | +6 |
| October ... ... | +1 | 0 | +5 | +2 | +2 | —2 | +4 | +3 | +7 | +3 | +8 | +7 |

### TABLE IV.

*Differences: Hair Hygrometers—Ventilated Psychrometer.*

| Month. | 1900. Under Ordinary Conditions. No. 2282. | | | 1900. After Great Humidity. No. 2279. | | | 1901. Under Ordinary Conditions. No. 2281. | | | 1901. After Great Humidity. No. 2284. | | | 1902. Under Ordinary Conditions. No. 2281. | | | 1902. After Great Humidity. No. 2284. | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | 7 a.m. | 1 p.m. | 9 p.m. | 7 a.m. | 1 p.m. | 9 p.m. | 7 a.m. | 1 p.m. | 9 p.m. | 7 a.m. | 1 p.m. | 9 p.m. | 7 a.m. | 1 p.m. | 9 p.m. | 7 a.m. | 1 p.m. | 9 p.m. |
| May ... | — | — | — | — | — | — | 0·0 | —0·3 | 0·1 | —0·1 | —0·1 | 0·1 | 0·8 | —0·7 | —0·8 | 0·0 | —0·4 | 0·0 |
| June ... | — | — | — | — | — | — | 0·0 | —0·9 | 0·6 | 0·0 | —0·2 | 0·1 | 0·3 | —0·3 | 0·1 | 0·0 | 0·2 | —0·1 |
| July ... | —0·9 | —0·4 | 0·8 | —0·4 | 0·3 | 0·1 | —0·2 | 0·0 | 0·3 | 0·2 | —0·1 | —0·1 | —0·7 | 0·0 | 0·6 | —0·6 | 0·5 | 0·1 |
| August ... | —1·6 | —1·3 | 1·5 | —0·5 | —0·1 | 0·5 | —0·7 | —0·1 | 1·4 | —0·4 | 0·4 | —0·2 | —0·6 | 0·2 | 0·5 | —0·7 | 0·4 | 0·0 |
| September | —0·6 | —0·6 | 1·0 | —0·2 | —0·3 | 0·2 | 0·1 | —0·2 | 1·3 | —0·2 | 1·0 | —0·1 | —0·5 | 0·0 | 0·7 | —0·5 | 0·4 | 0·2 |
| October ... | 0·6 | 0·0 | 0·7 | —0·5 | 0·0 | 0·1 | —0·9 | 0·0 | 0·5 | —0·1 | 0·4 | —0·2 | 0·0 | —0·2 | 0·6 | 0·0 | —0·6 | 0·0 |
| Mean ... | —0·6 | —0·6 | 1·0 | —0·4 | 0·0 | 0·2 | —0·3 | —0·3 | 0·7 | —0·1 | 0·2 | 0·0 | —0·1 | —0·2 | 0·3 | —0·3 | 0·1 | 0·0 |



### TABLE V.

*Mean Range of the Differences (Hair Hygrometer—Psychrometer).*

| Month. | 1900. Ordinary Conditions. No. 2282. | 1900. After Great Humidity. No. 2279. | 1901. Ordinary Conditions. No. 2281. | 1901. After Great Humidity. No. 2284. | 1902. Ordinary Conditions. No. 2281. | 1902. After Great Humidity. No. 2284. |
|---|---|---|---|---|---|---|
| May ... ... ... ... | — | — | ± 1·7 | ± 1·2 | ± 1·9 | ± 1·3 |
| June ... ... ... ... | — | — | 2·0 | 1·1 | 1·8 | 1·1 |
| July ... ... ... ... | ± 1·9 | ± 1·1 | 1·4 | 0·8 | 1·5 | 1·3 |
| August ... ... ... | 1·9 | 1·2 | 1·6 | 1·1 | 1·4 | 1·0 |
| September ... ... ... | 1·9 | 1·1 | 1·9 | 1·2 | 1·3 | 1·2 |
| October ... ... ... | 1·6 | 1·5 | 2·2 | 1·3 | 1·7 | 1·2 |
| Mean ... ... ... | ± 1·8 | ± 1·2 | ± 1·8 | ± 1·1 | ± 1·6 | ± 1·2 |

TABLE VI.

| — | Per cent. | Per cent. | Per cent. | Per cent. | Per cent. | Per cent. | Remarks. |
|---|---|---|---|---|---|---|---|
| Aspirated Psychrometer ... ... | 20 | 60 | 70 | 80 | 90 | 98 | |
| Hygrometer, July 29–30 ... ... | — | 59 | 69 | 83 | 95 | — | Before the experiment. |
| " August 3 ... ... | 22 | 63 | 74 | 86 | 96 | — | After being exposed during the preceding night to a humidity of 20 per cent. |
| " August 6 ... ... | 21 | 61 | 74 | 85 | — | — | Ditto. |
| Aspirated Psychrometer ... ... | 20 | 60 | 70 | 80 | 90 | 98 | |
| Hygrometer, July 31 ... ... | 14 | 56 | 65 | — | — | 88 | After exposure to a humidity of 98 per cent. |
| " August 4 ... ... | — | — | 63 | — | — | 88 | |
| " August 7 ... ... | — | — | 64 | — | — | 89 | |
| " August 8 ... ... | — | — | 64 | — | — | 89 | |



TABLE VII.

*Diminution of the Readings of the Hygrometers.*

| Hygrometer. | At 92 per cent. humidity. | At 70 per cent. humidity. |
|---|---|---|
| No. 29,557 ... | 7 | 6 |
| " 11,812 ... | 5 | 5 |
| " 26,959 ... | 6 | 8 |
| " 29,558 ... | 4 | 6 |
| " 29,559 ... | 4 | 8 |
| " 29,560 ... | 3 | 7 |
| " 29,561 ... | 8 | 8 |
| " 29,562 ... | 6 | 7 |
| " 29,563 ... | 10 | 10 |
| " 29,564 ... | 10 | 9 |

The variation was of the same sign in every case.

If a hygrometer, after having been exposed to a high humidity, be left for some time under ordinary conditions exposed to moderate humidity, its correction gradually returns to its first value. Experiments made by M. Choukevitch to study this point have given the following results:

TABLE VIII.

*Readings of the Hygrometers.*

| Dates. | Hygrometer. | At 98 per cent. | At 65 per cent. after having been exposed to 98 per cent. | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | | 10 mins. | 20 mins. | 1 hour. | 2 hours. | 4 hours. | 24 hours. |
| 1903. August 7... | 29,557 | 89 | 59 | 59 | 60 | 60 | 61 | 62 |
| " | 29,558 | 92 | 61 | 62 | 62 | 62 | 63 | 66 |
| " | 29,566 | 91 | 56 | 58 | 58 | 58 | 59 | 61 |
| " | 29,567 | 92 | 58 | 59 | 59 | 59 | 60 | 62 |
| " | 29,568 | 91 | 58 | 59 | 59 | 59 | 60 | 61 |
| " | 29,569 | 91 | 61 | 62 | 62 | 62 | 63 | 64 |
| August 8... | 29,557 | 89 | 59 | 59 | 61* | 64** | — | — |
| " | 29,558 | 93 | 62 | 62 | 64* | 66** | — | — |
| " | 29,566 | 91 | 56 | 56 | 60* | 62** | — | — |
| " | 29,567 | 93 | 58 | 58 | 61* | 63** | — | — |
| " | 29,568 | 91 | 58 | 58 | 61* | 63** | — | — |
| " | 29,569 | 92 | 61 | 61 | 64* | 66** | — | — |

This table shows that the effect of exposing a hygrometer to a high humidity decreases after a time, but it sometimes remains appreciable for 24 hours. On the other hand this influence can be annulled by exposing the hygrometer to a very low humidity. This experiment was tried on August 8th 20 minutes after the instruments had been taken from the case in which the humidity was great; they were exposed to a humidity of 30 per cent. for 20 minutes. After the observation, at 1 hour, had been made, this operation was repeated during the time from 1 hour to 1 hour 30 minutes. The observations made after this experiment are shown by means of the signs * and **; they show that the correction resumes its original magnitude much more rapidly after this process has been gone through.

All these researches and experiments prove incontestably that the corrections of hair hygrometers depend very largely on the degree of humidity to which they have been exposed during the time immediately preceding the observation. If we further take into consideration the fact that, in addition to this defect, the hygrometric capacity of hair changes in course of time,* it does not appear to me to be prudent to recommend the substitution of the hair hygrometer for the psychrometer. In any case, it is preferable to have both instruments, the psychrometer for the summer and the hygrometer for the winter, as we have at our stations of the second order.

* Every year a number of hygrometers at stations in our system become useless because of irregularities in the readings or want of sensitiveness, and have to be replaced.

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## APPENDIX XIII.

### REPORT ON RADIATION.

By M. J. Violle.

During the four years which have elapsed since the last Meeting of the International Meteorological Committee, work on solar radiation has followed its normal course of development.

When the Committee established a permanent Committee on Radiation, they did so with the object of showing the interest which they took in this fundamental question of Meteorology, and of correlating the efforts which were being made by different organisations so as to study it under all its varying aspects.

The question is, indeed, an extremely complex one. It demands a complete study of each of the simple radiations which go to make up the total solar radiation.

Whatever be the type of apparatus employed to isolate and measure the energy of a radiation, this energy will be known (1) on its arrival at the apparatus, and (2) on its entry into our atmosphere, if we succeed in determining (1) the loss due to the apparatus employed, (2) the loss due to the passage of the radiation through the terrestrial atmosphere. Then by taking the sum of the individual energies of all the incident radiations we can obtain the energy of the total radiation which penetrates into our atmosphere.

To obtain the energy of the radiation emanating from the sun it would further be necessary to know for each component radiation:

(3) The loss which it suffers in its passage between the sun and the earth.

It would also be useful to measure for each component radiation:

(4) The loss which it undergoes in crossing the complex atmosphere of the sun (comprising in this term everything outside the photosphere).

Only under these conditions would we know the amount of the energy radiated by the photosphere.

This programme, so easy to describe, cannot be easily realised.

(1.) Apparatus has been constructed which allows of the accurate measurement of the energy which a particular radiation gives up to it. Further we can always evaluate the loss which it experiences in its passage through the apparatus itself with sufficient accuracy. The first part of our programme, the measurement of the energy received during one minute (the unit of time usually adopted in actinometry) by one square centimetre of surface of the earth at a given point and at a given instant, can therefore be regarded as capable of satisfactory solution.

(2.) The second part presents much greater difficulties. Spectroscopic work had already given valuable information on absorption in the atmosphere, either by the help of photography or by the use of the electric thermopile, when Mr. Langley undertook his methodical study of the spectrum (particularly of the part between 0·45 $\mu$ **and** 2·5 $\mu$,) with his bolometer for different thicknesses of the atmosphere at successive hours of the day and at increasing altitudes (principally at Mt Whitney). I need not dwell on the progress which has been made in this branch of study. I will also not repeat what I said in my last report concerning the complications introduced by our atmosphere, which are due not only to the special absorption of each radiation by each of the atmospheric constituents, but also to dispersion by dust particles of every kind suspended in the atmosphere, and also by the air particles themselves. The consequences of these complications do not appear to have been fully appreciated in certain recent researches, in which it has apparently been assumed that the amount of energy received from the sun by one square centimetre of surface placed at the limit of our atmosphere in one minute, a quantity usually called the *solar constant,* could be calculated if the exact distribution of energy in the spectra obtained at the surface of the ground and at a high level observatory were determined. But even if we allow that each individual absorption conforms to Bouguer's law, is there no danger that some rays might be completely absorbed by the upper layers of the atmosphere and consequently escape detection? And is there no further danger of an appreciable and unknown portion of the radiation being deviated from its normal direction by dispersion, or even entirely reflected from our atmosphere?

(3.) and (4.) But granted that by taking account of the known effects of absorption and dispersion, we are able to trace a curve showing the probable variations of the true distribution of the solar energy which reaches the terrestrial atmosphere, we can, by tracing this curve for several years, obtain information as to the variations due to different positions of the earth in space, and to changes on the surface of the sun, more particularly in the spots, the influence of which can be otherwise discerned. This immediately leads on to a study of the influence of the solar atmosphere, which will naturally be made by comparing the energies which reach us at the same instant from different parts of the solar globe. This last part of our programme is thus directly connected with the earlier ones; and though it presents difficulties of its own, it can help to throw light on obscure points in the second part.



Mr. Langley* proposes to continue his researches at the Smithsonian Institution, and to carry out a programme similar to that which I have described. He has already published some very important results deduced from his bolographs, which he now obtains automatically in 15 minutes, for the portion of the spectrum between 0·45 $\mu$ and 2·5 $\mu$; the area of these bolographs is directly proportional to the energy received by the bolometer. The coefficient of transmission of the atmosphere was measured at noon on six very fine days for certain points on the bolographs where there are no marked absorption bands; between 0·45 $\mu$ and 2·5 $\mu$ it is represented by six very concordant traces corresponding to different seasons and to very different thicknesses of air. The bolographs taken about noon on every calm day from February to October, 1902, show that, between 0·76 $\mu$ and 2·0 $\mu$ very variable amounts of energy were received from day to day, the amounts being particularly small during the summer months. The diminution which is shown at this time of the year is due to the large quantity of water-vapour then present in the atmosphere. This is shown by a comparison of the areas of the parts of the bolographs which are known to be greatly affected by water-vapour with those of the rest of the bolographs which are not so much affected. It is interesting to note that Mr. Langley has thus divided the solar energy into two portions of very different capacity for being absorbed, a distinction which had already been drawn by Forbes.

We now come to the measurement of the solar energy given by actinometers.

The determinations made by M. Chistoni,† who has worked for several years and at several stations with a Violle actinometer and an Ångström pyrheliometer, call for special mention. M. Chistoni has shown that these two instruments give comparable results, and he has carried out an exhaustive research on the Ångström pyrheliometer.

M. Ångström‡ has made a further study of his compensation pyrheliometer. With the help of this instrument he has

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* Langley, The solar constant and related problems (Astrophysical Journal, vol. xvii. pp. 89–98, No. 2, March, 1903).

† Chistoni, Misure pireliometriche fatte a Sestola nell' estate del 1901 (Rendiconti della reale Accademia dei Lincei, t. XI, serie 5ª, 1902, 1er semestre, pp. 77–84). Misure pireliometriche fatte sul monte Cimone nell' estate del 1901 (Ibid., pp. 479–486 et 539–541). Misure pireliometriche eseguite a Corletto nell' estate del 1898 (Ibid., t. XII, pp. 53–57). Misure pireliometriche eseguite a Sestola ed al monte Cimone nell' estate 1899 (Ibid., 1903, 1er semestre, pp. 258–263).

‡ K. Ångström, Ueber absolute Bestimmungen der Wärmestrahlung mit dem elektrischen Compensations-Pyrheliometer nebst einigen Beispielen der Anwendung dieses Instrumentes (Ann. der Phys. und Chem. Neue Folge, t. LXVII, 1899, pp. 633–648). Intensité de la radiation solaire à différentes altitudes (Nova acta Reg. Soc. Sc. Upsal, 1900). Énergie dans le spectre visible de l'étalon Hefner (Ibid., 1903).

analysed the visible radiation of a Hefner element, and has shown that Wien's law represents the distribution of energy in the luminous spectrum of the Hefner lamp, and of the incandescent lamp satisfactorily.

I will not repeat what I said in my last report concerning the electric compensating pyrheliometer invented by M. Ångström. It is well known that the instrument consists essentially of two identical very thin metal bands, which are blackened on one side and provided with a thermo-electric junction on the other. These two thermo-elements are connected with a sensitive galvanoscope, which enables us to determine whether the two bands are at the same temperature. One of the bands is exposed to the radiation, while the other is protected by a screen, and an electric current of suitable strength is then passed through the screened band until the galvanoscope shows no deflection. When this is the case, we may conclude that both bands are in the same thermal state.* If $i$ be the strength of the compensating current, $r$ the resistance of the bands per unit length, $b$ their length, and $a$ the absorbing power of the blackened surface, then Q the intensity of the radiation received in 1 minute by 1 square centimetre, measured in calories, is given by the equation

$$Q = 60 \frac{r\,i^2}{4{\cdot}19\,b\,a} = 14{\cdot}32 \frac{r\,i^2}{b\,a}$$

As the temperatures of the two bands are equal, the losses by radiation convection and conduction must also be identical, and require no correction. To this important advantage the instrument adds the further one that it attains its state of equilibrium in 10 seconds, as the exposed band does not require more than 10 seconds to reach a steady state. "Pour la promptitude, cet instrument laisse donc tous les autres pyrhéliomètres en arrière."

I have already stated that the comparisons made by M. Ångström between this instrument and the one of the older type as adopted and modified by M. Chwolsen have shown a very satisfactory agreement between the readings of the two very different instruments, an agreement in favour of both giving correct values, and I have no doubt that the systematic investigations undertaken by M. Chistoni will establish more firmly the great value of M. Ångström's compensating pyrheliometer.

* It appears that the disparity due to the different methods of heating may be neglected. M. Kurlbaum has shown that the difference of temperature between the surface and the interior of a sheet of platinised platinum does not exceed 0·016° C. when the temperature of the sheet is 4° C. above that of its surroundings (Kurlbaum, Wiedemann's Annalen, Vol. LXVII., 1899, p. 846).



Work with the instrument has been further simplified by the substitution of an accurate milliammeter (the construction of which has now been accomplished) for the electro-dynamometer formerly used to measure $i$ accurately, and by the use of manganine instead of platinum in the construction of the bands; the change in the electrical resistance of manganine with change of temperature is almost zero, so that the small correction which had to be applied for this cause when platinum was used can be neglected.

The only remaining difficulty is one which it possesses in common with all instruments for measuring radiation, viz., the determination of the co-efficient of absorption, $a$. This question which has been already studied by MM. Crova* and Ångström† has formed the subject of an important research by M. Kurlbaum,‡ who has come to the same conclusion as his predecessors, viz., that platinised platinum, thoroughly smoked, is the best absorbent at present known.

But even this is no perfect absorber; MM. Rubens and Nichols have found that it absorbs the infra-red rays with difficulty. Moreover, as its properties depend on the method of preparation adopted, the co-efficient of absorption for the rays considered has to be determined specially in each case.

Among other devices, Mr. Bennett§ successfully used a sensitised paper for following the variations of the solar radiation during the solar eclipse of the 28th May, 1900.

Mr. Buchanan‖ successfully used a steam calorimeter during the same eclipse after accurately determining the loss experienced by reflexion at the mirrors which he had arranged to reflect the solar rays to the calorimeter.

The advantages of the radio-micrometer by Boys, of the bolometer by Paschen, and, above all, of the self-registering apparatus of Callendar, appeal strongly to many scientists on account of their simplicity, but the difficulties connected with the exact evaluation of the results are almost insurmountable on account of our ignorance of the true values of certain constants.¶

* Crova, Comptes Rendus, Vol. LXXI., 1875, p. 1205; Annales de Chimie et de Physique, 5th series, Vol. XI., 1877, p. 443; Comptes Rendus, Vol. CXXVI., 1898, p. 707 (jointly with M. Compan).

† K. Ångström, Öfversigt af K. Vet. Akad. Förhand., 1883, p. 385; Wiedemann's Annalen, Vol. XXXVI., 1893. p. 715.

‡ Kurlbaum, Aenderungen der Emission und Absorption von Platinschwarz und Russ mit zunehmender Schichtdicke (Wiedemann's Annalen, Vol. LXVII., 1899, p. 846).

§ S. R. Bennett, Actinometry during partial solar eclipse (Royal Dublin Society Proceed., Vol. IX., 17 October, 1900, pp. 365-376).

‖ J. Y. Buchanan, Solar calorimeter for eclipse observations (Cambridge Phil. Soc. Proc., Vol. XI., January, 1901).

¶ F. W. Very, Solar constant (Monthly Weather Review, Vol. XXIX., 1901, pp. 357-366).

The discussion of methods of experimenting and reduction has given rise to many important papers, by studying which an accurate survey of the question can be obtained. I must confine myself to mentioning them here.* The question of the part played by the atmosphere, as it presented itself recently to the authors of these memoirs, has specially occupied the attention of M. Ångström,† who has attempted to determine the exact effect of the water-vapour and carbonic acid gas contained in it. M. Ekholm‡ and M. Very§ have studied this important question with great care. For my own part, I have continued to investigate the variations in the loss due to the atmosphere up to the greatest altitudes which can be reached by "ballons-sondes."‖ M. Assmann¶ has also collected some interesting observations on radiation carried out from balloons.

By means of the spectrobolometer, M. Very** has measured the radiations emitted from different points on the solar disc; among other interesting results, he has found that the atmosphere of the sun is more transparent at its edges than near the centre. It is thus evident that progress has been made in every direction during the last four years, and the work of the Sub-Committee on Radiation has largely contributed to this result. By publishing a complete and impartial review of what has been done, and by drawing attention to the objects still to be achieved, it has rendered a valuable and timely service to Science.

It is now necessary to continue the work. The study of the distribution of energy in the spectrum remains the most important part of our work, and we are assured that the labours

* J. Scheiner, Strahlung und Temperatur der Sonne (Leipzig, W. Engelmann, 1899; Crova, Sur la constante solaire (Paris, Congrès international de Physique, 1900); F. W. Very, solar constant (Monthly Weather Review, Vol. XXIX., 1901, pp. 357 and 366); J. Y. Buchanan, Solar radiation (Nature, Vol. LXIV., 5 September, 1901); W. E. Wilson, Effective temperature of sun (Roy. Soc. Proc., Vol. LXIX., 4 January, 1902, pp. 312–320); S. P. Langley, loc. cit.

† K. Ångström, Ueber die Bedeutung des Wasserdampfes und der Kohlensäure bei der Absorption der Erdatmosphäre (Ann. d. Physik., vierte Folge, 3, 1900, pp. 720–732); Ueber die Abhängigkeit der Absorption der Gase, besonders der Kohlensäure, von der Dichte (Ibid., Vol. VI., 1901, pp. 163–173); Einige Bemerkungen zur Absorption der Erdstrahlung durch die atmosphärische Kohlensäure (Kongl. Vetenskaps-Akademiens Förhandlingar, 1901, Stockholm, n° 6, pp. 381–389).

‡ Nils Ekholm, Ueber Emission und Absorption der Wärme und deren Bedeutung für die Temperatur der Erdoberfläche (Meteorologische Zeitschrift, Wien, 1902, pp. 1–26).

§ F. W. Very, Atmospheric radiation (U.S. Department of Agriculture. Weather Bureau. Washington, Government Printing Office, 1900).

‖ J. Violle, Actinométrie en ballons-sondes (Comptes Rendus de l'Académie des Sciences, passim).

¶ R. Assmann, Die Sonnenstrahlung (in wissenschaftliche Luftfahrten, herausgegeben von R. Assmann und A. Berson, Braunschweig, F. Vieweg und Sohn; 1900).

** F. W. Very, The absorption power of the solar atmosphere (The Astrophysical Journal, Vol. XVI., 1902, pp. 73–91).



of MM. Langley and Angström and their collaborators in solar physics will be continued.

Correlatively with this, the action of our atmosphere must be more fully understood; the losses due to absorption and diffusion are capable of more accurate determination. The method of studying atmospheric absorption introduced by Mr. Langley will probably continue to yield valuable results. The part played by diffusion, which has been established theoretically by Lord Rayleigh, and experimentally by MM. Abney, Nichols, and Ångström, will be more precisely stated. Mountain observatories, balloons, and kites will all help to extend our knowledge. Photography of different parts of the spectrum at different heights will be a useful aid in this work. If the part played by the terrestrial atmosphere is known, the loss which the total amount of energy received at noon on a clear day at the surface of the earth by one square centimetre of surface placed normally to the sun's rays has undergone from this cause can be fixed with very considerable accuracy. This total energy is the quantity which an actinometer measures for us. We therefore take this opportunity of expressing a desire "that actinometric observations be made regularly and continuously at all meteorological observatories."

We should thus know the amount of energy received at each instant by one square centimetre of surface at the place of observation, and consequently the daily, monthly, and annual amounts, which are such important factors in determining climate, would also be known. From them could be deduced an approximate value of the variations of the radiation of the sun itself.

The Ångström compensating pyrheliometer is the most suitable instrument for such researches. Failing this, any of the instruments, whose trustworthiness has been established by experiment could be used. In all cases it is of the greatest importance only to work with suitably constructed instruments which have been carefully verified and standardised. As far as possible, a continuous self-recording apparatus and also a direct-reading instrument for controlling the readings of the self-recorder should be used. It would also be highly desirable to experiment with several instruments of different types, at least, at some observatories in each country, so as to ensure more efficient control and to make a more complete comparison of the different systems.

If the International Committee will support the requests which we have advanced with their high authority, Meteorology will reap great profit from the developments which will consequently take place in actinometric studies.

Fixin, 8th September, 1903.

# APPENDIX XIV.

## RESOLUTIONS ADOPTED BY THE ASSOCIATED ACADEMIES OF GÖTTINGEN, LEIPZIG, MUNICH, AND VIENNA, ON THE SUBJECT OF THE OBSERVATION OF ATMOSPHERIC ELECTRICITY.

After careful study of the question, the Associated Academies place on record that they consider observations of atmospheric electricity and allied phenomena, made on a previously arranged plan over a very large area, exhibiting great climatological variations, to be most important. To attain this object, they desire to make the following recommendations:—

A. Observations of the potential gradient ought to be made, wherever possible, with self-recording instruments especially with mechanical recorders—

(*a*) at stations on plains and inland lakes;

(*b*) at mountain stations.

It may be mentioned, as far as (*a*) is concerned, that three or four stations would apparently suffice for an area of the extent of Northern Germany.

B. The electric dispersion ought to be observed once a day, about noon, at the same stations, or at others grouped on an analogous plan. If radio-active substances be used in determining the potential gradient, the measurement of the dispersion ought naturally to be made by other methods.

As the study of the instruments used for the determination of the electric dispersion has not been sufficiently exhaustive to render results obtained with instruments of different types strictly comparable, the only instruments used should be those constructed by Messrs. O. Günther, of Brunswick. This firm is the only one authorised to construct the apparatus invented by MM. Elster and Geitel.

The Associated Academies attach special importance to the performance of these experiments in balloons, particularly on the days of international ascents. It is also desirable that the observations made in balloons be supplemented by others made at ground level.



As the above-mentioned researches have a direct bearing on the question of the existence of electric currents traversing the surface of the earth, it would also be desirable to undertake the following researches on terrestrial magnetism as soon as possible:—

A. A matter of primary importance would be the determination of the terrestrial magnetic elements along a parallel of latitude by numerous accurate observations, in order to be able to calculate the integral of the magnetic forces along this line with accuracy.

B. It is further recommended that simultaneous observations of the electric dispersion be organised in correlation with the accurate registration of the magnetic elements. The dates of particularly accurate records should be communicated to those who undertake scientific balloon ascents.

## APPENDIX XV.

The Chairman,
International Meteorological Committee.

SIR, Southport, September 14th, 1903.

As you are aware, a Committee has been appointed by the British Government to consider, from the point of view of public utility, the purposes to which the annual grant made to the Meteorological Office is applied.

At an early period of its work, it appeared to the Committee that it would be very desirable to have the views of Meteorologists outside the British Isles on some of the questions that arise, but the Committee were unable to devise a feasible plan for securing such information.

The Meeting at Southport of the International Meteorological Committee, and the presence of so many distinguished Meteorologists, seems to offer the desired opportunity, and, with the consent of the Chairman of our Committee, the Rt. Hon. Sir Herbert Maxwell, M.P., we therefore ask for your assistance.

The points on which information would be specially valued are:—

1. The desirability of continued expenditure on High Level Stations with a view to (*a*) their utility for forecasting, and (*b*) the progress of Meteorological Science.

2. The means to be adopted to render the information obtained by Meteorologists more generally useful and available.

3. In what respects it is possible either by modifying our system of observation or otherwise to *co-operate* more efficiently with other nations in promoting the progress and utility of Meteorology.

4. The feasibility of further research into questions relating to Meteorology, with a view to greater knowledge and increased certainty in forecasting, particularly for longer periods.

5. The desirability of organised connection between Meteorology and other branches of knowledge, *e.g.*, Solar Physics and Terrestrial Magnetism, and the nature of such connection.

The Committee of which we are members would, we are confident, greatly value the opinions of yourself and your distinguished colleagues on these subjects.

If, therefore, it is convenient to you to bring the matter before them and to inform us of their views, you will, we believe, thus render a very real service to British Meteorology.

We remain, Sir,
Your obedient Servants,

R. T. GLAZEBROOK,
J. LARMOR.



## APPENDIX XVI.

### REPORT ON QUESTIONS 10, 11, 12, AND 15.

By M. G. HELLMANN.

10. The resolution adopted by the Conference at Copenhagen (1882), that the occasions on which it was raining, snowing, &c., at the hour of observation should be noted in the column headed "Amount of Cloud" (Nébulosité), ought to be reconsidered and completed.

In accordance with a proposal made by M. Köppen (Zeitschrift für Meteorologie, 1880, p. 362), the Conference at Copenhagen (1882) recommended that rain or other phenomena noted at one of the fixed hours of observation should be indicated by the symbol of the phenomena, followed by a figure to indicate the epoch at which the observation was made, thus:—

●$^1$ indicates rain at 8 a.m. (or 9 a.m.)
⁎$^3$ indicates snow at 8 p.m. (or 9 p.m.)

This system was introduced by M. Mohn in the Jahrbuch des Norwegischen meteorologischen Instituts in 1880, although M. Köppen's original proposal required that the symbols of the hydrometeors observed at one of the fixed hours of observation should be put immediately after the figure, giving the amount of cloud in the column thus headed. The latter method of notation, which has many advantages for purposes of calculation, and which admits of the observations being summarised at a glance, was adopted by the German Institutes. The other systems followed suit, and in 1896 M. Mohn also adopted it, so that the system is now in use in Germany, Austria, Finland, Hungary, Norway, and Sweden.

The works of MM. Köppen, Meyer, Mohn, and Sprung have shown the great utility of these simple observations, both in climatological and in synoptic meteorological researches, and it appears to me to be desirable to introduce this method of recording the phenomena noted at the hour of observation generally, and also to extend it so as to include the phenomena of fog (≡) and of sunshine (⊙), as has already been done since 1901 in the German "Jahrbuch." The old astronomical symbol ⊙ is used to indicate the fact that the sun was shining. If it be objected that typographical difficulties prevent the general adoption of this method, we need only refer to the publications of the systems mentioned above. If the symbols ●, ⁎, ▲, △, ≡, ⊙ be cast in two sizes (large for use in the "Remarks" column, and small for the "Cloud" column) they can be placed after the figures indicating the amount of cloud, even if there is occasion to use two symbols simultaneously.

11. Instead of putting the symbol ⊛ (ground covered with snow) in the "Remarks" column, it would be preferable to measure the height of the layer of snow each day at the hour of the morning observation, and to publish it in a special column.

At the Conference in Munich (1891), a recommendation was adopted of noting the fact that more than one-half of the country surrounding a station was covered with snow at the hour of the morning observation, in the meteorological registers, by means of a new sign [⊛]. These observations are without doubt of great value if they are made regularly every day, but it appears to me to be still more important, for scientific and practical reasons, to actually measure the height of the layer of snow every morning, and to publish this height in a special column by the side of the rainfall column.

These tables would inform us not only of the fact that the ground was covered with snow, but also of the thickness of the layer and of its variations from day to day, facts which are of great interest in both meteorological and hydrological questions. The difficulties met with in measuring the thickness of the layer of snow covering the ground are not very great if a scale with one fixed and one movable index be used and if measurements are taken in several places. The publication of the height of the layer of snow in a separate column bearing the heading "Height of snow layer" ("Hauteur de la couche de neige"), as has been done in the "Deutsches Meteorologisches Jahrbuch" since 1901, requires less space than putting the figure for the height by the side of the sign.

12. The value of the height of the anemometer above the level of the ground should always be given at the head of all published tables of wind velocities.

It is well known that the value of the absolute velocity of the wind shown by an anemometer depends very largely on the height above the ground at which the instrument is fixed. Even the daily and annual periods are affected if the heights are considerable (*see* my Memoir in Meteorologische Zeitschrift, 1897, p. 323, and 1899, p. 546). It is consequently absolutely indispensable to know the height of the anemometer above the ground, in order to be in a position to appreciate the values of the wind velocity published at their correct value. Nevertheless, this note is wanting in most publications. Sometimes it can be found, not without some difficulty, in a passage in the introduction, but it is nowhere given in its proper place, *i.e.*, at the head of the tables of wind velocity published, except in the reports of the Weather Bureau of Washington.

On the analogy of the international designations—

$h_t$ = height of thermometers }
$h_r$ = height of rain gauge } above the ground.



I suggest the use of the symbol $h_a$ for the height of the anemometer (cups, pressure-plate, orifice of tube, &c.) above the ground, and the putting of a note $h_a$ = . . . . feet (metres) at the head of all tables of wind velocities.

15. The official publication of an international meteorological handbook containing all important resolutions adopted by the various Meteorological Congresses and Conferences which have met between 1872 and the present time, together with comments and explanatory notes.

It is well known that the various resolutions of the fourteen International Congresses, Conferences, and Committees which have met since 1872, have not been adopted in many of the observating systems, especially in those outside Europe. It seems to me that this is not entirely due to the character of the meetings, which were at first naturally exclusively European, and which have gradually become truly inter-continental, but, above all, to the difficulty of finding the information wanted among the various important decisions arrived at on each subject. It is true that reports of almost all the Conferences have been published in three languages (English, French, and German) but these have taken the form of a reprint of the Minutes without any detailed index of subjects, so that it is difficult to find the information desired. This is the reason why Mr. Harrington, at the Meeting at Munich (1891), expressed a desire for the Committee to arrange and publish the decisions of the different Congresses and Conferences. The President, M. Wild, himself collected these documents and published a collection of them in the "Repertorium für Meteorologie" (Vol. XVI., No. 10). This publication of 61 pages, in quarto, which covers the period from 1872 to 1891, is very valuable, but it does not fulfil all demands that can be made on it, for three reasons:—

1. It is only published in German, although editions of it in English and French would be of great value in many countries.

2. It appears to me to be too extensive and detailed, and does not bring out the important decisions sufficiently.

3. It is already somewhat out of date.

For these reasons, I suggest the official publication of an international meteorological handbook, containing all important resolutions adopted by the Meteorological Congresses and Conferences held between 1872 and the present time, together with comments and explanatory notes.

It would also be very useful to add a detailed index of authors and subjects of all Papers and Notes which have appeared as Appendices to the Minutes.

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## APPENDIX XVII.

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### ON THE PROJECTED REGULAR NIGHT SERVICE AT THE NICHOLAS CENTRAL PHYSICAL OBSERVATORY FOR FORECASTING THE WEATHER.

By M. M. RYKATCHEW.

*Question* 4.

During the last three years (1900-1902), the number of successful storm-warnings issued was, on the average, 77 per cent. of the total on the Baltic coast, and 71 per cent. of the total on the shores of the Black Sea and the Sea of Azov. On the other hand, 35 gales were experienced on the Baltic coasts and 29 on those of the Black Sea or Sea of Azov which were not forecasted, or for which the warnings were issued too late.

A summary of the warnings sent to the railways shows that during the last three winters 80 per cent. of the total proved correct, but 165 snowstorms were experienced (on the different railways) which had not been forecasted, or for which the warnings were issued too late. An important diminution in the number of failures, still less their complete elimination, cannot be attained by the introduction of any special measures in the present state of our knowledge. The system of forecasting depends for its success mainly on the three following factors:—

(1.) The necessary closeness and suitable equipment of observing stations.

(2.) Careful empirical study of synoptic charts with a view to determining the laws of the formation and propagation of gales, snowstorms, and other important phenomena.

(3.) The possibility of receiving information on the state of the atmosphere over Europe and surrounding areas with the necessary frequency and rapidity.

The last two factors are the most important for Russia. The number of stations is too small in Central Asia and Asia Minor, but in European Russia it is sufficiently large.

The work of the Daily Weather Report branch of the Nicholas Central Physical Observatory is at present so organised that 17 hours elapse between the evening service,



which deals with the synoptic chart for 1 p.m., and the morning service, at which the charts for 7 a.m. and 9 p.m. of the previous evening are drawn. This interval is, *a priori*, too long when rapid changes are in progress, and frequently proves the cause of gales, snowstorms, and other phenomena of great practical importance not being forecasted. As a matter of fact, the cyclones which cross Scandinavia and the North Sea sometimes move with a velocity of 100 kilometres per hour, so that they are often not shown on the charts for 1 p.m., while on those for following morning they already occupy the North-Western or Western parts of Russia. For example, during the night from the 7th to the 8th of February, 1903, a depression traversed a distance of 1,050 kilometres in the interval between 9 p.m. and 7 a.m. next morning, which gives a mean velocity of 105 kilometres per hour; during the night from 6th to 7th of October, 1896, a depression maintained a mean velocity of 115 kilometres per hour for the same interval of time. Cyclones sometimes move with considerable velocity over the land surface of European Russia. Particularly in Southern Russia and the regions near the Black Sea, the rapid development and brisk movement of anti-cyclones during the second half of the day and during the night are of great importance.

In the United States of America, in which the forecasting service has been most thoroughly organised, the interval between the morning and the evening observations is only 12 hours; moreover, it has been found that this interval is still too long, and to remedy this drawback the following rule has been established:—Each meteorological station is obliged to keep the Central Office informed of any sudden changes observed in the meteorological elements during the interval between the observations at fixed hours.

To pave the way for the introduction of a regular night service, the Nicholas Central Physical Observatory has for the last three years carefully examined all cases in which gales or snowstorms occurred which were not forecasted, or for which warnings were issued too late, so as to obtain an estimate of the probable value of such a night service. It was found that in 52 per cent. of these instances warnings could have been issued to the ports in the Black Sea and Sea of Azov in very good time; in the Baltic the proportion is still more favourable, it amounts to 80 per cent. As to snowstorms on the railways, it may be hoped that the number of occasions on which they are not forecasted might be reduced by from 30 to 60 per cent. in the different districts of Russia.

A further opportunity will be afforded of estimating the practical importance of a night service, by establishing a service for the special object of keeping a watch on the great risings of the waters of the Neva. A synoptic chart of the Gulf of Finland would then be prepared.

For the satisfactory organisation of a regular night service it would suffice if evening observations were received from those stations which at present send their 1 p.m. messages to the Central Physical Observatory. The following is a list of these stations:—

| | |
|---|---|
| Aberdeen. | Christiansund. |
| Paris. | Haparanda. |
| Toulon. | Stockolm. |
| Turin. | Swinemünde. |
| Rome. | Neufahrwasser. |
| Palermo. | Buda-Pesth. |
| Metz. | Hermannstadt. |
| Chemnitz. | Bukarest. |
| Hamburg. | Soulina. |
| Borkum. | Sofia. |
| Copenhagen. | Athens. |
| Skudesnaes. | Varna. |
| Bodö. | |

# LIST OF PUBLICATIONS.

## Issued under the Authority of the Meteorological Council.

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### 4. Marine Meteorology—*continued.*

CHARTS—*continued.*

*Atlantic :—*

Charts of Meteorological Data for the Nine 10° Squares of the Atlantic, which lie between 20° N. and 10° S., and extend from 10° to 40° W., with accompanying Remarks, ending with the Best Routes across the Equator. (Official, No. 27, 1876.) 24*s.*

Monthly Current Charts for the Atlantic Ocean. From Information collated and prepared in the Meteorological Office. Published by the Admiralty. (Official, No. 132, 1897.) 7*s.*

*Atlantic (North) :—*

Charts of Meteorological Data for Square 3, Lat. 0°–10° N., Long. 20°–30° W., and Remarks to accompany the Monthly Charts, which show the Best Routes across the Equator for each Month, &c. (Official, No. 20, 1874.) 20*s.*

Charts illustrating the weather in the North Atlantic Ocean in the Winter of 1898–99. (Official, No. 142, 1901.) 6*s.* 6*d.*

Currents and Surface Temperature of the North Atlantic Ocean, from the Equator to Latitude 40° N., for each Month of the Year. With a General Current Chart. (Official, No. 12, 1872.) 2*s.* 6*d.*

Discussion of the Meteorology of that Part of the Atlantic lying North of 30° N., for the Eleven Days ending 8th February, 1870. With Charts. (Official, No. 13, 1872.) 5*s.*

Meteorology of the North Atlantic during August, 1873, with 31 Synoptic Charts. (Official, No. 32, 1878.) With Book of Charts. 15*s.*

Synchronous Weather Charts of the North Atlantic and the adjacent Continents, 1st August, 1882, to 3rd September, 1883. Parts I. to IV. (33 sheets each.) (Official, No. 71, 1886.) 17*s.* each Part.

*Atlantic (South) :—*

Charts showing the Surface Temperature of the South Atlantic Ocean in each month of the year. (Official, No. 4, 1869.) 2*s.* 6*d.*

Wind Charts for the Coastal Regions of South America. From Information collated and prepared in the Meteorological Office. Published by the Admiralty. (Official, No. 159, 1902.) 7*s.*

Monthly Wind Charts of the South Atlantic. Published by the Admiralty. (Official, No. 168, 1903.) 6*d.* each.

*Atlantic, Indian, and Pacific Oceans :—*

Charts showing the Surface Temperature of the Atlantic, Indian, and Pacific Oceans. (Official, No. 59, 1884.) 21*s.*

Charts showing the Mean Barometric Pressure over the Atlantic, Indian, and Pacific Oceans. (Official, No. 76, 1887.) 10*s.* 6*d.* Supplementary Chart, 6*d.*

*Atlantic (North) and Mediterranean :—*

Monthly Pilot Charts, commencing April, 1901. (Official, No. 149.) 6*d.* each; subscription for one year, 5*s.* (exclusive of postage).

*Indian Ocean :—*

Monthly Current Charts for the Indian Ocean. From Information collated and prepared in the Meteorological Office. Published by the Admiralty. (Official, No. 124, 1896.) 7*s.*

*Indian Ocean (North) :—*

Meteorological Charts of the portion of the Indian Ocean adjacent to Cape Guardafui and Ras Hafun. (Official, No. 92, 1891.) 6*s.*

*Indian Ocean (South) :—*

Cyclone Tracks in the South Indian Ocean. From information compiled by Dr. Meldrum, C.M.G., F.R.S. (Official, No. 90, 1891.) 7*s.*

### 4. Marine Meteorology—*continued.*

CHARTS—*continued.*

*Indian Ocean (South)*—continued.

Meteorological Charts for the Ocean District adjacent to the Cape of Good Hope, with accompanying Remarks. (Official, No. 43, 1882.) Charts, 25*s.* Remarks, 7*s.*

*Pacific Ocean :—*

Quarterly Current Charts for the Pacific Ocean. From Information collated and prepared in the Meteorological Office. Published by the Admiralty. (Official, No. 134, 1897.) 5*s.*

Wind Charts for the Coastal Regions of South America. From Information collated and prepared in the Meteorological Office. Published by the Admiralty. (Official, No. 159, 1902.) 7*s.*

*Red Sea :—*

Meteorological Charts of the Red Sea (Official, No. 106, 1895.) 21*s.*

*Southern Ocean :—*

Meteorological Charts of the Southern Ocean between the Cape of Good Hope and New Zealand. (Official, No. 123, 1899.) 12*s.*

OTHER PUBLICATIONS ON MARINE METEOROLOGY :—

Contributions to our Knowledge of the Meteorology of the Antarctic Regions. (Official, No. 18, 1873.) 2*s.*

Contributions to our Knowledge of the Meteorology of the Arctic Regions. (Official, No. 34, 1885.) Vol. I.: Part I., 2*s.*; II., 10*s.*; III. and V., 6*s.* each; IV., 5*s.*

Contributions to our Knowledge of the Meteorology of Cape Horn and the West Coast of South America. (Official, No. 11, 1871.) 2*s.* 6*d.*

Notes on the Form of Cyclones in the Southern Indian Ocean.—By C. Meldrum, M.A., F.R.S. (Non-Official, No. 7, 1873.) [Out of print.]

On the Physical Geography of the part of the Atlantic which lies between 20° N. and 10° S., and extends from 10° to 40° W. A Paper read before the British Association at Bristol, in August, 1875.—By Captain H. Toynbee, F.R.A.S. (Non-Official, No. 10, 1876.) [Out of print.]

On the Winds, &c., of the North Atlantic along the Tracks of Steamers from the Channel to New York. Translated from a Paper issued by the Deutsche Seewarte, Hamburg. (Non-Official, No. 5, 1872.) 6*d.*

Report to the Committee of the Meteorological Office on the Meteorology of the North Atlantic.—By Captain H. Toynbee, F.R.A.S. (Non-Official, No. 2, 1869.) 1*s.*

Report on the Gales experienced in the Ocean District adjacent to the Cape of Good Hope, between Lat. 30° and 50° S., and Long. 10° and 40° E.—By Captain H. Toynbee, F.R.A.S. (Official, No. 44, 1882.) 7*s.* 6*d.*

Routes for Steamers from Aden to the Straits of Sunda and back. Translated from a Paper issued by the R. Meteor. Inst. of the Netherlands. (Non-Official, No. 4, 1872.) [Out of print.]

### 5. Miscellaneous Publications.

Harmonic Analysis of Hourly Observations of Air Temperature and of Pressure at British Observatories. (Official, No. 93, 1891.) 12*s.*

Report of an Inquiry into the Connexion between Strong Winds and Barometrical Differences.—By Robert H. Scott. (Non-Official, No. 1, 1868.) 6*d.*

Report on the Storm of October 13–14, 1881.—By Robert H. Scott, F.R.S. (Official, No. 46, 1882.) 1*s.* 6*d.*

Report to the Committee of the Meteorological Office on the Use of Isobaric Curves.—By Captain H. Toynbee, F.R.A.S. (Non-Official, No. 3, 1869.) [Out of print.]