ROYAL DUTCH METEOROLOGICAL INSTITUTE.

A SEQUEL

TO THE

SUGGESTIONS

ON A

UNIFORM SYSTEM

OF

METEOROLOGICAL OBSERVATIONS.

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UTRECHT,
Printing Office „The Industry"
(K. A. HANSSEN.)
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Meteorologists have every reason to be satisfied with the results of the preliminary congress held at Leipzig on the 14—16 August 1872. Soon after having published my suggestions on a uniform system of meteorological observations, I was informed that three distinguished meteorologists, Prof. Bruhns of Leipzig, Dr. Jelinek and Dr. J. Hann of Vienna had formed a committee for preparing a congress. Dr. G. Symons in successive numbers of his monthly magasine dwelt at considerable length on my paper and the committee above mentioned published in the Austrian Journal VII. the points they wished to be brought in discussion at the congress of Leipzig, in order that we might be better prepared for the definitive congress of Vienna. Different Savants sent in their observations on my suggestions, and their opinion on the points stated by the committee; and so 52 metereologists met at Leipzig and discussed them with an earnestness commensurate to their vast importance.

It may not be superfluous to give once more my opinion on several propositions, and to elucidate other

points, mentioned by me in the suggestions, for the discussion of which no time was left, since we had dwelt too long on questions, perhaps of no less interest; but which every observer and director must answer for himself. It is of no small value indeed, if the different methods of observation and the different instruments be mentioned, that every one may direct his attention to them and select which he thinks the best, but it must be left to our own judgment, and it must not be made incumbent on any one to observe according to this or any other method. We trust that all of us will try to give the most exact observations. It is a great question how to get exact observations of the different phenomena; but it is not the only one.

What has been stated relates principally to this question, and to the other: what phenomenas are to be observed? but there is more. We must try to have observations taken at every equidistant point of the globe, and we must deliberate on the means, how and in what form they must be published, that every one at the least expense and after the shortest lapse of time may inspect them, and may get them ready for the investigation of the laws, which regulate the disturbances and their mode of propagation.

To the care of the committee we owe the publication of an exact account made by Dr. G. Neumayer of the meeting at Leipzig and of the notes sent in by some eminent men before the meeting. For the use of the English and American readers I will now give my opinion based on these communications, especially as I said but little on that occasion, in order to

spare time for discussion. At the same time I once more direct attention to my own suggestions, which perhaps have afforded the opportunity of stating these questions, modifying my solution of them a little, after having heard the opinion of others.

In this I will proceed regularly, following exactly the order of discussion and giving the citations after the pages of the account, the notes, or of my suggestions. If I seem not to regard many valuable arguments of different philosophers present at the meeting, it is not from any want of respect for these opinions, but because I may suppose that some of them were changed during the discussion. I mention therefore only those conformable with the general resolutions, or with my own opinions given in the suggestions.

Question 1. Ought there to be introduced in all countries the same unity of measure, or is it thought sufficient to accept some rules for the reduction of the measures?

The general opinion was favourable to the desirability of a universal system of unities page 4 — Some therefore thought no result could be attained yet, but in the meanwhile prepared for; it was agreed that, besides the observations in the original, the means of months and years should be published in the metrical measure.

By this means it is easier for one, who will compare the climate of different places, to compute the normal range.

Prof. Dove objects to the introduction of the me-

trical measure for seabarometers, because most seafaring nations make use of English inches, Zus II, and reiterates his proposition to augment the Fahrenheit degrees by 100 in order to avoid the negative signs; see suggestions p. 31.

It would be in conformity with the resolution p. 30 and with the most general wish to adopt only millimeters and Celsiusgrades, but since no scale has a scientific prerogative in itself, it appears that only the same scale whatever it be, should be adopted, and so I could, say: I will observe the Fahrenheit scale if all resolve to observe it, but not so easily — as I gave my consent to publish the barometerheight in English inches if, but only if, all used that scale.

It is however to be observed that a tenth of a millimeter is nearly the quantity we may rely on; a tenth of a Parisian line is somewhat too great and so is a hundreth part of an English inch. So we want four ciphers for the English and Parisian measures, only three for the Metrical. To spare room and figures we should therefore make use of the metrical scale For the temperature, a tenth of a degree centigrade can also be given with more accuracy than a tenth of a Fahrenheit degree, and I am inclined to ask Mr. Scott if a whole degree Celsius is not sufficiently accurate for his telegraphic dispatches, since temperature has so little to do with storms. Temperature is much more the result than the foretokens of them, as I showed in my first paper on giving warnings of storms.

Since nobody has advocated Parish lines and Reaumur degrees, I should beg all observers not to

elect one of these scales for a new series of observations. I do not specially insist on a change of scale, if it is not for the millimeter and centigrade scale, before the time that we all agree on the same scale and I think with Prof. Mohn, I say: no reduction, if it is his meaning that all observations should be published in the scale according to which they are taken. It would be a source of error more, as I often see by the telegrams of the Swedish observations in the bulletins from Berlin and Paris.

Q. 2. Which is the best construction of the Barometer for stations of the second order? Is the use of aneroids to be allowed to such stations?

I do not yet feel sufficient confidence in the answer to determine by its differences with the mercurial barometer the form of the earth, and, granting to Dr. Jelinek p. 5 that perhaps for controle of barometers in inspecting those of other stations they may be thought useful, I maintain my proposition adopted at the Congress and expressed by others who sent in their opinion: The aneroid may be observed besides, not instead of, the mercurical barometer.

During a storm it may serve on a ship whose movement does not allow the reading of the mercurial barometer; but it is not to be denied that if a derangement occurs, we should not be warned. The instrument cannot indicate by itself whether, and how much it is deranged.

At every observatory there should be a syphon

barometer and a cistern one, the first to be observed daily, the other from time to time for control. I think the syphon barometer is read with the least error, and as for the deteriorations of the mecury and of the surface of the glass in the open tube, precautions can be taken. I have now observed a syphon barometer during twenty five years, nor has it suffered, since its indications present the same difference with other normal barometers, as before.

It is however necessary to have two barometers, in order that when one of them is deranged, it may be repaired and afterwards compared with the other, so there will be no occasion to divide the series of observations into two.

No corrections except those for temperature is to be put to the observations, see my suggestions p. 26. Dr. Symons in his monthly Magazine for May p. 54 was puzzled by my saying, that it was of no moment to correct the observation for instrumental errors, and he has a right to say so, for grammatically, I say it there about capacity and capillarity. I referred to the index fault of the scale and to capillarity. For these errors we can really give the quantity, once for all, and it is better to give the observations unaltered. Therefore we may dispense with that correction, and with the correction for the height at which the barometer is placed above the Sea; since the former is quite constant, and capillarity suffers so little change from temperature (the inner surface of the glass tube and the mercury must be so clear and pure that the meniscus is always well formed) that correction for this is not very safe. The more

so we may dispense with these corrections if we give departures, because these errors are common to the normal height and the individual observations, and disappear, therefore, by subtraction. I quite forgot that I had named capacity, perhaps because, as little as Dr. Symons, I make use of instruments that require reduction for capacity; but if, as at Sea, we have a correction for capacity, it ought necessarily to be joined to the observations. Therefore it is carefully determined at the Institute for every Seabarometer controlled by the evacuator, which Mr. Scott has had the kindneſs to order for our use, and to send to us.

At fixed stations we have only to look for each individual observation at temperaturecorrections. Therefore once more: at every fixed station the barometer height must be corrected for capillarity, index of scale and height above the Sea. These corrections must only be accurately determined and put at the bottom of the whole series of observations. The corrections for the height above the Sea should be sought, especially for determining them at that latitude and longitude; the normal height of mercurial column is to be taken, and we can thenĉe derive, with regard to the variation of gravity, the normal pressure of the air.

Q. 3. Which is the best mode of exposition, every where to be introduced, of the thermometer for observing the temperature of the air?

This question, no doubt of great importance, took up a considerable time. I think great attention is to

be given to the method of Prof. Lamont recommanded by Dr. C. Fritsch p. VIII, and why not to that to which Prof. Stokes has drawn the attention of physicists? Mr. Ragona strongly advocates a strong aspiration of air, as I did Sugg. 28, 30, if the method of Prof. Joule should prove insufficient. At every central observation different methods should be tried. We hope to learn much from the report of Messrs. Scott and Wild who already made interesting communications. We see at page 8 that the question has been complicated with another: how to observe at different heights, as especially Prof. Prestel has done. Naturally at every height we should take every possible precaution, but to reduce them each to a common height is not to be thought of. For departures, it is only requested, that the same observer observes always at the same height; then we have the departures for that given height and instrument. With respect to the climate of the town or country, we must choose a height two or three meters above the ground, and for studying the influence of the height, we must have more observations at the same time at different heights.

Q. 4. Which construction is to be preferred for maximum and minimum thermometers.

Prof. Ragona has a right to state, XX. that a max or min: should be observed nearly at the same place and under the same conditions as the ordinary thermometers. Where you have registering thermometers, you may use the instument itself, and at secondary stations or there, where no observations are made at

fixed hours, the choice is left between the instruments of Geissler, Walferdin, Baudin, Negretti and Zambra, and Rutherford. I think the question can be decided only by experiment. If they are consulted with great care and precaution against amalgamation of the indicator, and impurity of the inner glass surface and of the alcool, the max: and min: thermometers of Geissler may do as well as the others.

Q. 5. What instruments are to be used for the observations of the amount of the variation, and in what manner can we secure the comparability of the observations?

The spectroscope has practically demonstrated the necessity of observatories on high mountains, and prepared the way for radiation observations, the desirability of which I expressed in my paper. *Changements périodiques de température dépendant du Soleil et de la Lune déduits de* 120 *années d'observation.* Utrecht Kemink et fils 1847, and reiterated in many places, see suggestion p. 49.

Even those who give no credit to the investigation of such periods, must acknowledge that an instrument is indispensable by which we may better determine the exact amount of energy emitted by solar, lunar and stellar rays. I am sorry I did not speak at greater length and more persuasively before the congress on this point, and I hope the confidence Mr. Jelinek expressed, that English and German Physicients will compare different actinometers, and give a series of observations, will then be eagerly acted upon. Eng-

land is under circumstances fatal to this sort of inquiry; but it has so many observatories in other regions of the earth, and bestows so much on meteorology, that I have no doubt it will find observers willing to make these observations at places very well adapted to the purpose.

Most of the philosophers who sent in their opinion to the congress, prefer the maximum thermometer in vacuo IV, XXVIII or the thermolectric pile of Melloni XXXII. Prof. Ragono XX exposes his actinometer, a maximum thermometer of Negretti and Zambra the whole day, and had it directed by clockmovement to the rays of the sun, the whole day. He does not say whether it registers. Apparently it does or should do so; for we must have instantaneous indications, we must know the action during every ten minutes; because the transparency of the sky and the height of the sun above the horizon must be taken into account. Mr. Neumayer especially recommands us to investigate the influence of humidity of the air on radiation, and for this inquiry I think the pyrheliometer will do very well. I am at a loss to express a preference for any of them. The observations are so extremily delicate. If we see Mr. Soret in the Revue universelle stating that white incandescent Lime and Magnesia seen under the same angle as the diameter of the sun give only 0,49 c in the actinometer of Mr. Waterston, the sun must considerably change its emission of calorie rays, before we can be certain of that change. Perhaps it would be best to compare the difference of heat excited on two individual thermopiles in the form of a sector

each receiving the rays emitted by different sectors of the sun. Only in so doing, we must form the image of the sun, either by a mirror or by a lens, and consent to introduce a source of error from which the method of the maximum thermometers in vacuo is not quite exempt either. However difficult it be, it must be done thus or in some other manner.

Q. 6. To which Apparatus for observing the temperature of the ground preference is to be given? At what depth ought observations to be taken in order to attain a desirable comparability?

After having heard the discussion on this point p. 10 and seen the opinion of Mr. Fritsch VIII and of the French congress XXXIII, I think I have better attended to the question than I did when writing my suggestions p. 29. With Dr. Buchan and the French metereologists I now say most decidedly: such observations are especially more useful to botany than to physicists and meteorologists.

For the physiology of plants and their vegetation thermometers would suffice, at one, two three decimeters, then perhaps electrical thermometers at 5, 7, 10, 15, 20 decimeters, for trees which have their roots at these depths. For meteorology, it appears quite sufficient to observe one or two of the thermometers, to derive from their indications in what manner the range of temperature depends on the nature of the soil. As to physical geography there must be well distanced electrothermometers under the surface of invariable temperature,

Few places only are appropriate for such observations. — Many circumstances, the nature of the soil, the rain that conveys the temperature of the higher layers to the depth, disturb the observatation; and what is to be determined? The law of conductibility? The conducting power in a manner preventing a certain comparison with the method of Mr. Neumann or Angstrom, who perhaps has been brought to this method by his admirable observation of ground temperature? Shall we determine the quantity of heat that rises from the surface? In that case the temperature must be placed very deep. Only the Alps or other mountains can afford any thing valuable and when they are perforated by a tunnel, they will soon cease to do so; since the tunnel surface itself is continually cooled by the draughts of air. Nobody indeed will destroy the tunnel, in order that the thermometer may surely indicate the true temperature of the earth at those depths. Hence I despair of the telluric question for the first time to come, and I shall be very glad, if by observation of temperature in the upper layers we enable physiologists to derive — better than from the temperature of air alone — the quantity of heat needed for the growth and fructification of plants. Could we only give a measure of humidity at the same time, I think they would be grateful to us.

Q. 7. What apparatus is to be used for the determination of atmospherie humidity? Is it sufficient to observe the psychrometer, or can we use the hair hygrometer; and if so, under what conditions?

If we do not recur to the chemical method of determining the quantity of vapour contained in a given quantity of air, the only unobjectionable one, though it is troublesome and does not give the instantaneous quantity but an integral of it, I think that the indication of Prof. Mohn XVII had best be followed. The absent members, Mr. Fradesso da Silveira VIII, Mr. Hoffmeijer XIV, Dr. Symons XXVIII, the French meteorologists XXXIII were not so much against the psychrometer, with the exception of Prof. Wolff XXVIII, as those present p. 23, 24. Only the air should be introduced by an aspirator Suggestions 32. It is to be hoped that the comparisons which Mesrs. Wild, Buchan and Jelinek will make on this point, may elucidate this question. It would not be so great an obstacle, if above and under zero, different instruments were read, if they be only compared in the vicinity of this point with one another, or better, throughout the whole range with a chemical hygrometer.

Q. 8a. How can we agree on the manner of inditing the directons of the wind?

Two opinions are given, one in favour of the English letters; the other to which I strongly incline (Sugg. 44) to arches flying with the wind. I only want four different types to represent 16 directions. As to the easier interpretation of those arches, I say that arches are more natural signs and easier interpretated. Take only a sheet of paper with letters, S, WSW, ENE, NE and so on, and compare that sheet

with any page 8n of the Dutch annuary, and you will be come an advocate for the arches.

Q. 8b. Is it desirable to express the mean directions according to Lambert's formula? How are we to take into account the very weak winds, if we distribute the direction in the windrose.

If we give a mean direction of the wind during a greater or lesser space of time, it must be given by Lambert's formula. I think the strongest opponent of the application of this formula reject it, not considering the formula incorrect, but the very method of computing a mean direction of different resp. opposite winds. — For years I have adopted Prof. Prestel's manner of denoting the winds that have occurred. Only for a few places in the Netherlands, I give the result of the formula besides the number of each wind, and so for the other places I give no mean direction at all. I feel inclined with Dr. Hann p. 25 to reject the computation of a mean direction for a whole month in general, for a given lapse of time before determined; and in no case is the intensity to be disregarded. Dr. Hann himself agrees with Dr. v. Oettingen that for some inquiries the reduction of every wind to four components may be desirable, and so do I. No doubt Dr. v. Oettingen will do a good work, when he gives once more his method and his simplifications of the reduction. It takes a considerable space to give these components. I suppose with two ciphers we should indicate the wind so: 2, 3 ↑ 5, 7; then the arrow indicates one of

the sixteen directions which is nearest to the observed direction and before the arrow we have the component West or East, after the arrow the component South or North; and in this case we do not need a larger space than when we use SSW. 6. 1, which means the same and wat gives only *implicitly* the same. Moreover in giving 23 / 57 (we may omit the comma) we enable every one to take the sum of all the components for such a space during which the wind has not very much changed in direction, or even during which the wind has had one component in common, since the individual indications are likewise mentioned. We gain this, that we have expressed by one simple indication the result of such a lapse of time. Those who are not so partial to the computation of a mean even of opposite winds, may, if they like, apply the formula of Lambert in all cases, and we are all content.

Q. 9. What scale is to be followed at those places where no absolute mean is taken, but only estimation is given?

At the regular observatories measures are to be taken with anemometers and it is desirable that intensity of pressure should be compared with velocity. To the question, if the estimation should be made after 12, 10, 6 or 4 degrees, we should answer: why should we not use Beaufort's scale, which is generally adopted by seamen; and if not, but only universally, another scala.

Q. 10. Is the introduction of simple numerating apparatus for determining the velocity of the wind desirable? To which unities is the velocity to be reduced?

To the last questions the answer cannot be any other than this: the meter and secunde. We expect ampler inquiry about the comparability of the Robinson anemometer with different lengths of arms and greater or lesser cusps, since that of Mr. Frederik Stow is not yet completed. *Quarterly Journal of the meteorological Society* Vol. I. If we are so happy as to know the relation between pressure in kilograms on the square meter and velocity, we may indifferently give one of these quantities, and so the first part of the question is answered. Only we must all agree on the same quantity as to the difficulty of reading the anemometers and the defects in anemometerregistration we may consult the paper of Mr. Charles, O. T. Cator M. A. F. M. S. in the quarterly Journal of the meteorological Society Oct. 1872. Vol. I, p. 103.

So the congress dwelt rather long on the manner in which the observations are to be recorded, but did not touch the principal point Sugg. 44: what is the minimum of windobservations that ought to be printed and what moreover must be communicated? If a strong wind prevails, even then have occurred there may be a lull about the observationhour and a considerable force between two of them. It is of more interest to know the time in which a strong gale travels from one place to another than to know the velocity of an airwave. It is indispensable to afford such communications that we may state if a very strong wind has travelled from

one place to a distant one, or if they are separate gales; and it is impossible to do so, if we do not know at what time the strongest wind has arrived at the two places and from what direction. Therefore I have communicated from the first in the departures, page 4n + 2, the strongest force of wind at my four places, and the time when it is felt. We have no right to say, so inconsiderately as some have said, that the strong winds arrived first in France, then in Italy — if we have no indication that consecutively from the North of France the strong wind and the same strong wind prevailed in the middle of France, in its southern parts, in Switzerland, in Lombardy. We must compare the indications of the telegrams and the gradients of the barometer with the time which elapsed before the wind acquired great force.

Therefore I propose that we should always note in a separate column, or in the remarks: the time, at which a wind of 30 kil. on the square meter was felt. It is of much importance to know this: that we should be more inclined to omit the observation at fixed hours than to omit the time of the strongest wind. We seem tolerably well to observe the gyration of the wind all throughout the day, every one can take that into account for his private use, as he pleases; but for general utility it is infinitely more desirable to know the epochs of the feeblest wind on each day, of the wind considerably rising and exercising a given force viz. of 20 kil., and of the strongest wind, each of them with the pressure and direction of the wind prevailing at each epoch. — I say this especially respecting the observatories of

second or third order, where we wish not to embarass the observers with many observations at fixed epochs. As for the central observatories we may take from the registers just that which we need. For observing clouds I refer to Sugg: p. 46, adding only that it is sufficient to publish the directions and velocity of these by arrows, weak and strong, of two or three different sizes.

Q. 11. What is the best fom, area and exposition for rainguages? At what hour of the day must we read the quantity of the fallen water?

Mr. Buchan in conformity with my proposal, Sugg. p. 36 thinks, we should follow the indications of Dr. Symons, Prof. Galle also points to the Scottisch rainguages. As to the area surface of them Prof. Bruhns proposes $^1/_{10}$ square meter. In Holland they have only two decimeters by side: $^1/_{25}$ square meter; it may be too small, but perhaps $^1/_{10}$ square meter is also small enough to give the exact quantity. For this reason, and because it is too expensive and too troublesome to have everywhere such large rainguages, I have adhered to the proposition, that at the central observatories a comparison should be made of the quantities given by rainguages of a smaller and a larger size, as we allways did at Utrecht.

As to the time of reading the rainguages Dr. von Oettingen says well, that this may depend on the following question referring to the commencement of the meteorological day. Then, if we be strictly consistent, we must read them at midnight. But

this is a very inconvenient time, and therefore we had better read it after sunset, or at the time of the last observation of thermometer and barometer. To this point Dr. Symons, Monthly meteorological magazine June 1832 p. 73, objects assigning three reasons, or rather two, since 1 and 3 are not very different. These appear to be fatal to my proposition, since they show us the danger of losing one third of the observers on the mountain tops and far from human habitation. On these places, where insuparable difficulties present themselves, we cannot insist. There the observations cannot serve to indicate the quantity fallen during the meteorological day, but they may serve as well for the total amount. But where it is possible, for instance, to have two rainguages, one opened the other sheltered at night, it would be desirable not only for having the quantity on every meteorological day, but moreover to have the quantity fallen during the hottest and the coldest period of the day separately. Sugg, p. 40.

The second reason adduced is not of great weight. It is easy enough to read the rainguage by artificial light. The observer may gather the water it contains at night in a divided measuringglass and read it when the sun is up.

The congress did not treat the question, nor has Dr. Symons stated his opinion on it, whether we are to attend more to the variation of the wind, to the mingling of two winds, or only to the direction of the wind during the fall of the rain.

For the influence of forests Suggestions p. 38 on the quantity of rain and of evaporation Dr. Ebermayer

has made a series of observations which he will discuss in a paper he has promised the congress [1]).

Q. 12. Are the days of snow to be counted and registered separately from these of rain only?

Why should this be neglected where it is possible to do so. Mr. Wild says it is impossible to ascertain whether snow has fallen, during the night, mixed with rain. I should think this word „impossible" too strong, see Suggestions p. 39; but I agree that it will be found very difficult in many places, and that the separation is not quite accurate. Perhaps it is not very interesting to go farther as to denoting only the quantity gathered with the indicating letters: s snow and h hail, or r and h, in conformity with the proposal of the Committee.

Prof. Mohn sees a great obstacle in determining the quantity of snow XVII; however be has separated days of snow and rain. It may be that I have misunderstood his meaning, since in the account something must be amiss. See the question of the French Congres on point 11, 12 en 13 XXXIV.

Q. 13. Is it desirable to separate hail from sleet?

I think not. It will suffice to indicate by letters or remarks, whether it was a decided fall of hailstones. In this case we should determine the size of the hailstones, Sugg. 39, by taking up a certain

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1) Just now the first part of this interesting work has appeared.

quantity of them with a pair of nippers and then weighing them, and it may be worth while to attend to the hint of Prof. Prestel 23, and to give the structure of the stones.

To know in general, whether hail was mixed with the rain, may afford some indication of the temperature of the upper air.

Q. 14. In counting the days of thunder, must we register every thunderstorm or only the days on which they occurred? How are we to commemorate the days of lightning only without thunder?

Now it is clear that two thunderstorms on one day do not give two days of thunder. But to know at what hours thunderstorms are most frequent we should enregister every thunderstorm at the time of the day, on which it occurs, as we do in the Dutch annuaries. Far be it from me however to deny the due weight of Mr. Fritsche's remarks XX. I am accustomed to indicate thunder and lightning by two different signs. I do not reckon a day as a day of thunder, if thunder has not been heard.

Mr. Fradesso da Silveira, who has generally given very judicious auswers to every question, is very short in replying to these subordinate points.

Q. 14. What apparatus is to be preferred for evaporation! How is it to be exposed?

This is a very intricate point. We are all, I dare

say, desperate to find out the means of well determining the quantity of evaporated water. It is not so much the difficulty of reading the evaporators either by weighing, as we do at Utrecht, or by measuring; it is not, if we shall give preference to those of Prof. Prestel, Lamont, Viviani, Ragona XXI, Fradesso da Silveira V, Schenzel, Prettner X Marié Davy; but more so Sugg. 42, if the quantity evaporated from the evaporator is really that evaporated from the surface of water. My expressions have been quoted also by Mr. Miller in his note Monthly meteorological magazine July 1872 p. 111. Mr. Miller denies there, and in a letter addressed to me, that he finds a greater quantity evaporated than that which falls. But it is generally found so, and this seems impossible. Now M. Rogers Field has pointed out that the evaporator should be most carefully sheltered from the influence of the rays of the sun on the sides, and I have insisted on the same point in the Austrian meteorological Journal VII p. 223 referring to the observations of Mr. Elink Sterk made in the Harlemmermeer, who found a considerable difference, when he left his evaporator floating in a great quantity of water, naturally the border only a little elevated above the surface of the water. At Utrecht since that time I have also compared such an evaporator with the other, but I found no such difference, only of 0.2 of a millimeter on the daily average, perhaps because of my evaporators being already sufficiently sheltered, and still I found a quantity evaporated exceeding what had fallen.

Must then the evaporator have so very large a

surface? At the Helder it has a square meter [1]) and it agrees with that at Utrecht.

Further, for practice it is not even sufficient to know the quantity evaporated from a free surface of water, but we should know it from moist or dry ground, and from different sorts of ground, from plants in different stages of vegetation.

The interpretation of the indication of this instrument in various circumstances will cause us great trouble if the very elaborate and costly experiments XXVIII at the expense of the Royal Society of London do not, compared with those of Dr. Neumayer 29, throw sufficient light on those intricate questions. I may mention that the Provincial Society of Arts and Sciences at Utrecht has proposed a prize medal for a series of experiments on the evaporation from the surface of plants.

Q. 16. In what manner is the amount of clouds to be estimated and communicated? Is it desirable to introduce certain signs not dependent on the different languages, and therefore universally intelligible, for the amount of clouds, for the hydrometeores and other extraordinary phenomena?

The committee proposes, in conformity with the

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1) It is inconvenient to use a larger one, and nevertheless a great objection to the accuracy is taken from the smallness of many of them. So I should feel doubts on that of M. Putre recommended by M. Robinson XXXV. which seems to be of similar structure as that proposed by Mr. Marié Davy and Le Verrier in the *Bulletin hebdomadaire* and with that of Prof. Harting of Utrecht.

Suggestions p. 48, to indicate a quite clear sky by 0, a sky totally clouded by 10 [1]). The signs used in the Dutch annuary seem to be adopted everywhere, where no numerical quantities are given. Nobody has yet given so long a series of observations in the same terms; nor are other signs more easily interpreted.

Besides, the observation of cirri and their movements should be given explicitly XV.

Q. 17. Should moreover other than the meteorological elements mentioned, electricity of the air etc. not yet treated of in similar questions, be enumerated among the ordinary observations, and what are the most convenient instruments to observe them with?

So much time was spent in discussing the preceeding points, that there was none left for this question. Only ozone and atmospheric electricity were mentioned and the conclusion was that Mr. Ebermayer, Schoder and Sohnke should treat all similar questions before the Congress of 1873.

I think meteorology has more general inquiries to make, and to regulate than just these, which must be left to individual choice. In my suggestions at different places, I have mentioned these and other questions of the same kind. It may be sufficient to have drawn attention to them. Every one will sug-

1) I was well aware that in the northern countries these ciphers have opposite signification with that in Belgium, France, etc. I myself was inclined to forsake my southern method; now, seeing that M. Fradesso da Silveira XI clings to it, I propose not to make a change till we all agree on this point. See XXV.

gest some other investigations and patronize them. I should like to break a lance for electricity and magnetism. They appear to me most interesting. I speak in favour of the first topic, notwithstanding that I thought from some remarks it was not a favourite one. It is true, the meaning of the electrometer is not accepted by all in the same sense, but we must not forget, that many philosophers, more or less justly point to the electrical action. The method of observing either by the electrometer of Peltier, or, as I had the following day the opportunity to see, thanks to the kindness of Prof. W. Hankel, by his apparatus, is sure and easily followed, and only by many well regulated observations it will be possible to interpret accurately the indications and to decide, if different propositions be true or not. Prof. Ragona recommends the bifilar electrometer, which instrument I confess I am not acquainted with; and only in England, as far as I know, are there registering electrometers, most recommendable of all, if their indications may be relied on. I feel certain the Astronomer Royal or Prof. Lamont and our Nestor Quetelet would be very well pleased if the electricity of the air and the observation of earthcurrents were accepted on the program.

Then the study of electricity will throw more light on the magnetic observations, which were not even mentioned. I agree that the latter are expensive, because they must be continually photographically done, and that they can not be expected to be done at every observatory; but still it is necessary that there be indicated a few places regularly distributed over the globe, where they should be done. Only by

proceeding in this manner, can we distinguish between cosmical and local causes; only then are we able to inquire if really to changes on the sun surface correspond magnetie disturbances; only by combining the occurrence of simultaneous magnetic perturbations with changes of weather, storms, and the like, the hypothesis of P. Secchi can be tested, which, if found to be true, especially when they preceede atmospheric disturbances, may be of great practical utility, in every case may prepare scientific results.

Now we come to another set of questions which were treated before the questions 7—17. and which have more especially a direct relation to a universal plan of publication and of mutual exchange of observations.

Q. 18. Can there be introdoced synchronous times of observation for the normal observations?

Mr. Jelinek p. 72 said nearly in conformity with my Suggestions p. 17—23, that we may take from the register at the central observations, and I spoke only in referencc to these Sugg. 20, observations at the same astronomical hours viz. reckoned according to astronomical time. So does Dr. v. Oettingen p. 11, and to Prof. Bruhns I might answer that the same thing may be done for temperature observations. There is no objection to this, if we publish departures which are free from the daily range.

Why should not a central observatory add two columns, containing one for the barometer, the other for the thermometer, the means of the three or more

observations taken at synchronous hours? It would require little space, and by this every one, without being compelled to apply to the different Directors, can immediately draw up a synoptic chart of simultaneous observations or departures.

I hope Mr. Wild will excuse me for saying that I am sorry that he thinks it an argument against astronomical time, that it should include the odious question on a first meridian. I can only refer to p. 20 of my suggestions and I add, that in this respect I should like to belong to the German nation, which is foremost in meteorological researches, and of great power; for in this case it would have considerable weight if I offered, which I should be proud to do, Greenwich observatory the honour of giving the zero time of meteorological observation.

So having recommanded astronomical time for general investigation I maintain that at every place, should there be a central observatory, or only one of the second order, local time may be used. Every observation should be communicated after local time. It must give a chance of error, if one should call noon 0, another twelve, if one should name eight in the morning of the 12th what the other calls 20 of the 11th. Astronomy has no relation with the civil day, meteorology has: Astronomy teaches us the time and says, at what time the sun rises and sets; she reckons by stellar time and gives solar time; — Meteorology has solar time, respect. local time and must use it. If we had as little intercourse with unscientific persons as astronomers have, I would not object to introduce in conformity with the English, as Mr. Bruhns proposes

13 the astronomical notation, I myself incline to that *notation*, but we must avoid ambiguity, if in speaking to the people we should use another language; and in no case should we add together the observations of one civil day with those of another.

So the seafarer, as Mr. von Freeden remarks, uses the astronomical notations; he speaks only to the navigator and may continue to put down his observations as he used to do; but we, when we give a result of his observations, it we will give an account of the phenomena of a day, it must be of a civil day.

Q. 19. Can we state general rules for the verification of the instruments and for the inspection of meteorological stations?

In acknowledgement of the important consideration given by the different Directors present at the congress I readily agree to accept the conclusion: the inspection of all stations resorting under the same central observatory is to be made in the shortest possibly interlapse of time.

For the thermometer, surely, it should be made oftener than for the barometer, if we cannot trust the observer will accurately verify his zeropoint, since temperature is more local. The movements of the barometer are more general, and at a considerable distance, especially, if a place is surrounded by other places on whose observations we may rely, (suggest p. 10) we are able to see by comparison of the monthly means if a barometer has been deranged some tenths of a millimeter. We may also avoid

too frequent an inspection, always very expensive, by giving a double set of instruments to a st_tion, which we proved desirable for every station, necessary for a central observatory.

For my four telegraphing stations I have two barometers observed, by two different observers, at some distance in each place. It must be left to the judgment of the Director of the central observatory, if a recognised derangement of an instrument is of such importance, that it is allowed or not to correct the observations made by it, since under the various causes, not to be determined before, there be some, of which the influence can be estimated with exactitude.

20. After what rules, divisions of time etc. are the average means of the diverse meteorological elements to be calculated? Is it more conform to the purpose to begin the year with January or with December?

The Dutch annuaries begin the year with December. I think I did this at the recommmendation of Prof. Dove; and most of the Institutes erected after 1849 have done so. On this principle I spoke in favourable terms on the commencing with December. And indeed, for all such papers, as give only means, it is desirable to give these three months together which correspond most closely possible with the seasons. But that is all, whatever may be adduced in favour of a measure that after all gives rise to misunderstanding, and — to be as consequent as Dr. v. Oettingen, — I would now propose to begin the day at midnight,

every month on the first, and the year with January.

To every annuary should therrefore be added a table giving the means of each month having each a separate page of the Journal.

Only with this exception, we should reckon to January the 30 first days, to February the 31 Jan. till 1 March, so that it would have 31 days in the leap-years, and March from 2 till 31. The difference between the most exact calculation of Prof. Bruhn's, who for the yearly means gives the sum of the daily observations divided by 365. resp. 366, and the method of Mr. Scott taking for it the twelfth part of the sum of the twelve months is so exceedingly small, that I do not see sufficient reason not to adopt Mr. Scott's method.

Every one may add without any inconvenience the means of such groups of days, as he chooses. So in the Dutch annuaries for many years I give separetely for greater number of days in which the departures were without interruption in the same direction, of the same sign, the sum of departures for that time. We learn by this one of the forms of variability of the climate, and can easily compare by a smaller set of numbers, the occurrences at different places, and in the meanwhile we are conform to the principle: that we must never for scientific pursuits, but only for acommodation with the general civil use or for conformity, determine a priori whatever area of space and division of time, but only seek a posteriori, in what circumscription of space or time nature has given different results.

Q. 21. In what manner and for what duration of time are the meteorological value of the different elements to be deduced?

The second part of this question was treated of in the discussions on 20; for, if we give the means of epochs beginning and ending at such dates, all is prepared for computing the normals, then highly desired for the same epochs and between the same limits. Now it depends naturally on the lenght of a well continued and conducted series of years if the observer should feel himself authorised to give only the means of the year or of the seasons, or of the months, or of shorter intervals of time. It was readily accepted by the congress, that we should form at the same time greater periods of ten years each and beginning the 1 of January of the year 10n + 1.

The method of calculating the normals can scarcely be different from that which several meteorologists have already adopted. See Dr. K. Jelinek [1]) and others [2]), all perhaps preceeded by Prof. Dove. We have to choose some fundamental places where, during a century or so, reliable observations have been made. If our observation is not near to one of them we must first calculate the normals of places, which have twenty, thirty years in common with these fundamental observatories, and then seek, from

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1) Ueber den jährlichen Gang der Temperatur und des Luftdruckes in Oesterreich und an einigen benachbarten Stationen. Wien 1866.

2) Marche annuelle de la température et de la pression atmosrique en divers lieux de l'Europe. Royal Academy of Sciences of Amsterdam 1861.

the simultaneous years we can dispose of, the differences with those places. So we are able to know from fifteen years of observation the normals with tolerable accuracy. All the 70 normals from which the Dutch annuary gives the departures were deduced in this manner, I could often dispose only of five years, and I do not think that the review of them, I shall publish in the annuary 1870 II, will show great incorrectness. The name „*lustrum average*" proposed by Dr. v. Oettingen for the means of 10n + 1 till 10n + 5 and 10n + 6 till 10n, will be readily accepted.

22. Is it to be desired and is it possible to publish the meteorological observations, of a limited number of places in every country, in similar manner, and within a proportional short time after thy are made?

I think most meteorologists in the principal places of western Europe have shown, that it is possible to give the observations of a month published the following month, and they have realised it because they found it desirable. It is true Norway, Sweden, Denmark, and more so the Netherlands and Belgium have comparatively small districts and few places of observations, not to be compared with the great number of observations in Prussia, Austria, Russia, where besides the distances are so great and communication is often so defective by the inclemency of the climate; but we have to consider, that it is not for all places of observation that such a publication is asked; only for

a few well disposed, and indicated a priori. In France and Italy there are some places which give their own observations by their own monthly bulletins, and so some places in Russia and Siberia might soon send in their observations to Mr. Wild. In 1868, 69-70 we have the highly valuable Nouvelles Météorologiques by Mr. Marié Davy, and we hope earnestly that it will not last long before that publication is again taken up. We must ever try to have-as soon as possible a view of the distribution over the globe from only a few places, that we may know the great movements and in broad lines, while we have yet the impression of what happened at our places, and thus be induced to ask for further information at these points, which appeared to be in the vicinity of the perturbations, where the instruments were much in excess, as well as where they were below their usual height.

Nor need the observations be given in such detail in these monthly sheets as they are afterwards printed in the extensive annals, or in the costly graphical exhibitions of Mr. Scott. If it should be found impracticable to give the observations in full form, why should not we publish, for every month separately, a bulletin in the form of the *Comptes rendus*, of the observations at Kiel by Prof. Karsten, or in that of Triest and Fiume.

The committee brought already a scheme before the Congress of the most necessary indications, which was on the whole approved of. The committee will think of this point, after having attended to some reflexions, and then propose it to the Congress of Vienna.

There was some discussion about the manner in which the humidity of the air should be indicated.

Prof. Bruhns p. 20 said, he could find no room for the psychrometerobservations, and we must agree with him that, if possible, we must have all on one side of a sheet. So the question remains which I have treated Sugg. p. 32, whether we shall give preference to print quantity of water in a cubic meter of air, tension or relative humidity. The first I think is most closely connected with the growth of plants, more even than relative humidity, besides that it has the advantage of being a simple datum, while, both tension and relative humidity, are dependent on humidity and on temperature. The tension in millimeters, within the limits between which it occurs, bears a certain proportion to the quantity of water, so that we may deduce one from the other by mere inspection with tolerable accuracy. Therefore I have been hitherto inclined to give my preference to the printing of the tension, if one datum be given, and of the quantity of water and relative humidity, if humidity is to be given in two forms.

I do not quite well recollect what scheme the committee proposed, but I do remember, it appeared to me very appropriate. Three columns for the barometer, five for the thermometer including a column for the mininum and for the difference of the maximum above the mininum [1]), six for humidity, three for the wind two for the rain, three for the amount of clouds, one for remarks, totally twenty three, will amply suffice, even to the wishes of Prof. Ragona who desires to know

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1) I prefer this form since by it we get immediately the daily range, which depends on serenity, explicitly given. See Dutch annuairies.

the maximum, and minimum, and to those who think relative humidity alone rather defective, to give a clear view of the moisture of the air.

Q. 24. Is it desirable that in every country one or more central observatories should be organised for directing, coordinating and publishing the meteorological observations?

Happily neither any one present or absent had the least doubt about answering this question affirmatively.

Every one I think, if I judge others by myself, would wish to have the superintendency of all the meteorological observations on the whole globe with an appointement of a hundred thousand pounds a year, but that cannot be, and it would not be desirable, because even if the Director were the ablest of us all, and if he used his power in the most discreet manner possible, it would too much restrain the liberty of others. Then we must take it as it is. Every country must have one fundamental observatory at least, and one or more central places, where the observations are compiled and discussed for the surrounding places.

Relative to these centres it is to be hoped that the most amiable intercourse may exist. A few rules may be accepted to which they all conform; as for instance, to the form of publication of monthly records, see the preceeding question [1]).

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1) While this was in the press, I received the proposal of Dr. W. Köppen. *Oesterreichische Zeitschrift* VIII. n°. 2. Agreeing of course, with the

Some of the members present wished the marine observations to be directed by a separate Institute (21). I do not see why. Where there are special reasons to have the two institutes separate it may be, but it is not a thing to be wished for. Why cannot, as in Holland, two Directors placed in a certain relation to each other work at the same place, one more especially charged with the observations on land, the other with those at Sea: separation may be admitted, it can not be postulated a priori.

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Q. 26. Is the exchange of telegrams so useful as to make it desirable to practice it on a larger scale, and to organize it more in detail?

The system of exchanging telegrams is useful in the first place to seafaring nations, and we hope it will prove in the future more and more useful to agriculture also, but it is very expensive. We should like, this is the ideal prospect, to have such intercourse that every place knows at any time what is passing at every other station. We must have the barometer, the thermometer, the wind writing down their indications by means of electricity on a board at each station; but for this, we should want $\frac{n\ (n-1)}{2}$ telegraphic lines, wires, and n (n—1) messages to have this realized for n places in any part of the world. But

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general plan of distribution of the centralobservatories, and the foundation of new ones I think a Counsel of 35 Directors too numerous, and am afraid of its excercising too great an influence to it. It should only facilitate the intercourse, not direct the operations.

now, if we attend to the expense, ask France, where tidings are received from all parts of Europe, what it pays for them, or what is paid by other Governements; ask England, where also a considerable part of the money allowed is absorbed by the cost of the telegraphic signals; ask America that lavishes a hundred thousand dollars on such a system; and say if it is not time to organize that system in the most compendious way.

In my paper on the introduction and explanation of the Aeroklinoskope I have proposed a more simple plan, I may be allowed to develop it a little now.

1. Each centre chooses the places from which it receives its telegrams and to which it thinks it desirable to send them back.

2. Each centre makes its arrangements with another centre as to the number of telegrams which it desires to receive.

3. We must have such centres on the Isles in the Atlantic, or signal ships at great distances to the west.

Ad 1. Most centres, as far as they have a district not considerably larger than Belgium or Holland will find it sufficient, if the number of posts does not require a greater number of telegrams, to have intelligence from four different places twice a day. In this case it will not be found unfeasible for coming years to have registering instruments at these stations, and separate wires, which write their indications continually at the Central station, which would then be able to return back a signal to them as often as it sees a disturbance of the atmosphere arising.

Ad 2. I will only describe what I myself desire.

From France I receive Paris, Havre and Brest, from England I have every day the height of the barometer at Valentia, North Shields, Thurso, Hartlepool, Portsmouth.....

Seamen like very much to see published in their ports the state of the atmosphere and sea at different points. Moreover I receive from Paris a more extended telegram giving some indications and the prognostie formed from them, and with Mr. Scott I have the agreeable arrangement, that we warn each other, when in our district a greater disturbance occurs. I send him intelligence if one of my places in the north has a barometerheight exceeding that of a southern station by 4 millimeters or more, he informs me if in England a difference occurs greater than 17 mill. in the opposite sense, and in no other case, in order that we be certain that on any day we have not received such a telegram, there was no positive difference here, nor a negative difference there, exceeding the limits assigned.

Now I should like to have a similar arrangement with Denmark, Sweden and Norway on the one side and with Portugal on the other, and further only one telegram from every centre.

Ad 3. We all must try to have centres with registering instruments in Iceland, at the Azores, at Madeira, and to have an arrangement with America that it may warn us, if a great perturbation is observed there.

The former are stations of so great importance for all of us, that we should each contribute to a general funds, from which the expenses of these telegrams should be defrayed. To my great satisfaction I recei-

ved from Mr. Fradesso da Silveira the intelligence that a new concession has been granted for a cable between England and Brazil over the Azores and the Bermuda's.

As to the form of the telegrams, I, of course, agree with Dr. van Oettingen p. 22 that it should be that of departures. I have from the beginning (1860) sent to my ports normals of the barometer diminished by 50 mill., in order that the quantities transmitted might be all positive.

If they are above 50, the departures are positive, the barometer is so much higher than usual, and if they are under 50, the contrary occurs. So there has never been any uncertainty of sign. For temperature also every communication is intelligible in itself, without requiring him, who receives it, to know the average temperature of such a place for that time of the year, for appreciation of the degree of perturbance.

Q. 26. What measures are to be taken to realize the conclusions and plans of the Meteorological Congress?

Some of these measures have already been taken. We owe to the committee and to Dr. Neumayer that a Protocol [1]) has been published of the discussions at the Congress of Leipzig; the same triumvirat, to which we are indebted for the direction of that Congress have been unanimously appointed with acclamation to regulate also the Congress of Vienna. Every one of us will

1) Mr. Scott has translated this, and, were it not, that I had received it only some days before now, I had better referred to it in my quotations. The numbered questions however will set everyone right.

certainly transmit his speculations to the admirably conducted Austrian Journal, which is a meteorological Journal, highly appreciated, and, most of all, every meteorologist has expressed by his presence and conciliatory advices, or, if absent, by his letters, his intention to comply, as far as possible, with the general wish.

There is one point more which I discussed at some length, Suggestions 4, and which alone was overlooked, perhaps, because there is great difficulty to settle it, but which seems of great weight.

In many countries in Europe and America there are too many places paid for their regular observations whereas in many other parts of the world they are almost totally wanting. Would not it be better if every country spared some money now laid out on these superfluous stations, and therewith raised funds to pay for the establishing of some central observatories where there is a deficiency of them? We may form an idea of the distribution of the weather phenomena in Europe and a part of Asia, we could form it of America, if America published its observations every month, so of the occurrences in other parts of the world, but as in Africa and South America and the polar regions on both sides of the equator, we have not sufficient, and prompt intelligence from them. I maintain this notwithstanding the remarks Dr. Symons has given in his M. Magazine p. 37. And still we must, it is to be repeated, at every opportunity given, we must try to get a synoptic view of the phenomena over the whole world. On every occasion we must add to our sentences given, after Cato: Et ceterum censeo.

Even of many places, where observations are regularly made, we ought not to publish them (Suggestions p. 10) in full; but only insist on having their registers properly kept, to allow inspection of them when required, to send in a copy to the central observatory to which they belong, and to draw up regularly the average range of each instrument; for we must proceed economically, that we may have something to dispose of for the great purpose.

As to the observations at Sea I have something to add to what I said in my Suggestions p. 54. From merchantmen we must ask no more. If the Royal Navy will give further information by a greater number of observations and more complicated; if especially the ships of war will furnish us with an account of their inquiries on magnetism, on the depth of the ocean, and on the life beneath the surface of the water, it will be thankfully accepted; but the common seaman must not be bored with observations, he must not make inquiries which cause him to lose his time; since it is our purpose to make him gain time and to warn him against and preserve him from danger.

I have not treated then of the method of arranging these observations at sea, or the manner in which we should cooperate in this; but now, since the Congress appointed me a member of a subcommission which is to report on this subject to the Congress of 1873, I will give some indications in concert with Mr. Cornelissen the Director of the marine department of the Dutch Institute, and explain our wishes.

The observations at Sea may serve these purposes.

In the first place we look to the advancement

of science: we get fruits only by taking good care of the tree; no good tree is without fruit, no real science is without useful results, if not to those who cultivate it themselves, then at least to others. Only neither the tree grows without food nor science without observations, which the sea seems to offer to us in abundance.

The Sea covers four fifths of the surface of the earth, the phenomena at sea are less complicated than they are on land, thus we have ampler and better materials, if we have our observations from every square degree of the surface of the sea, than if we have as many from an equal surface on land; but there is a drawback. Instead of fixed observatories on land well provided with instruments and directed by persons whose sole duty is to observe, we have at sea floating observatories, moving every day to another place; there is a lack of instruments; observing the weather is a secondary question, if you go beyond barometer, temperature of the seawater, wind. All this impedes us, if we try to draw not the great general rules of oceanic circulation, but the more delicate features of the phenomena, and the relation between the range of different instruments which we succeed in detecting from landobservations. Still we do not allow ourselves to lose sight of the scientific aim, and we think we come nearer to it, if, for every spot of the ocean, we strive to determine the average indications of every instrument.

Knowing these, we may hope to discover the connexion between them and the general law, which regulates them; moreover the seaman at any spot on a

certain day, may see if all is right, or whether there is any perturbance. As long as we cannot get telegrams from the sea to know the phenomena at surrounding places, there cannot be any other information for him than the departures of his instruments and the movements of them since the foregoing reading. This could be done only in case those ships shonld meet, they could then show mutually the barometer height of the foregoing day, as they show now the longitude and latitude.

Now, I do not see any safer means to know the average indication of the instruments, for every time of the year and every place, than to note in our registers every observation on a different page according to the place in the ocean, and separately for every day of the year, that we may be able to make a graphic table of these observations, and to see, if any districts of the Sea have some phenomena in common, and what are the boundaries of these districts.

Unhappily it is not sufficient to form squares of five degrees side a priori; for it will often be the case that one half of such a square shows a digression from the mean opposite to that of the other half. For instance we seek the limits of the tradewinds; shall we find them by studying only large squares viz from 15° to 20° Lat, and from 20° to 25° Long, etc? Are they at the same latitude in different longitudes? It is impossible to determine them, if we do not know what passes at every square degree, at every square minute of a degree. Sooner or later therefore some one will come to the system of giving separately what has been observed at separate points of space and time.

This is possible, as I will show by describing at some length one of the methods to obtain it, which is followed at the Dutch Institute. We shall be happy if other Directors describe their own method, that we may compare and judge.

Every meteorological Journal, sent in to the Institute, is copied on sheets of paper, with printed lines and columns all of the same model on one side, blank on the other.

Every line of this copy bears the number of the Journal, in order that every questionable annotation may be controlled by the original; further the date and the place when and where the observations, put down in the following columns, were taken. [1])

Then by scissors the paper is separated into horizontal slips, so that each slip contains no other observation than that taken at the same degree of latitude and longitude.

These separate slips are put in different cartoons each containing the observations of a fifth or tenth degree square; they are slightly bound together in twelve different bundles corresponding to the twelve months. State B shows how we copy these abstract logs and how we divide these copies into slips.

If now we think we have a quantity sufficient to make a study of them, these slips are arranged according to time and place, and since they strictly correspond to the breadths of the columns, we can easily follow their indications, draw up a chart of large size for such a portion of the ocean as we are then treating of.

1) The heads of the Columns are fully indicated with remarks on them in the table joined to this paper, table A.

If it be objected that we might have done so from the first, we answer that so doing takes too much time and requires much more attention; that it is much more fatiguing and wearisome to annotate from the Journal, now here, then there, than to do twice the work but each time without fatigue.

Moreover by this method we get a means of control, which otherwise is possible only by doing it over once more from the beginning.

If now we have recorded the observations of one instrument on one chart, the observations of the other on another chart, we are able to publish either for a single square degree, even for smaller parts, if there is a fair number of observations, or to join several such degrees into a larger space, and to give its average value.

We are compelled to give all separately for smaller spaces, if there is any difference in the indications, if thence we infer we are near to the limits; on the contrary, we may more freely give the indications for larger spaces, if at such a larger space the indication is similar, and we shall prefer this, where it is permitted to do so, in order to make the synoptic view easier and clearer.

So, for instance, to give an illustration of what we mean, by our two latest papers, we have given the temperature of the seawater at the eastside of the northern Atlantic and for single degrees, but the storms South of the Cape we gave for smaller divisions, of one degree in longitude and 20 minutes only in latitude; on the contrary we intend to give the wind direction in some parts of the Ocean, in

the tradewinds for inst. for greater divisions, because they are nearly the same over a larger space.

From these temperatures of the seasurface, we saw immediately, that on the eastside near the coasts the water had a lower temperature than at the westside; and to make this perceptible at the first glance, we calculated the mean temperature of each parallel, and pointed out the value, higher than that, with large ciphers. If we have learned from other researches in the western parts of the Atlantic, about which our Dutch ships can give us no sufficient information, the average of each parallel will be another, and some large cipher will have to be replaced by a small one, or the contrary; we shall detect a district of cold water surrounded by warm water; so we shall be compelled in a second edition to change the limits; but till now the boundary line reaches more or less accurately in the different months from 50° N. 9 WL. tol 30° N. 33° WL., and we see it at once.

As to the storms South of the Cape Good Hope observations are scanty enough, to be sure, but nevertheless, because we desire to learn how the number of storms increases with the distance from the land, we give more minute details: on the contrary, although we have the direction and force of the tradewinds separately for each degree in our registers and charts, we shall publish only for such districts, where the result appears to be decidedly different.

For this it is indispensably necessary to have in our registers the observations separately for each square degree; for we can join the phenomena of separate degrees together to any area, but we cannot derive

from the mean of an area the phenomena observed on a part of that area. These simple arguments are irrefutable and all-prevailing.

The sea-observations and the inquiry based on them relate not only to scientific research, but also to the safety of navigation, which is a practical point of view.

For if the shortest route goes through unsafe and precarious parts of the Ocean we must warn against it. If there is in these regions greater probability of heavy hurricanes, which may be avoided by taking another way, though longer by some days, the sailor must be made aware of this danger.

So it is with the charts of icebergs. The great circle to Australia and even to Java brings us to such high southern latitudes that the risk of meeting ice is too great in some longitude. For safety's sake therefore alone we should be compelled to dissuade the seaman from it.

Therefore our Journals for merchantmen must supply us with ample information of this score. Moreover I propose for ships of the Royal Navy two columns more for the psychrometer observations, and besides these, the Journals of our Steamboats through the Suez-Canal, belonging the Steamboat company „*Stoomvaart Nederland*", have more room for the observations of the magnetic declination, and for the determination of the local deviation of the Compass, by swinging the ship in open sea. This is frequently done during the voyage, as often as from the Azimuth observations appears to the Commander to be required, to the great benefit of our study on the influence of the magnetism of the

Earth, and of the waves on the iron masses of the ship.

There is another practical purpose we aim at, the shortening of voyages. Before entering upon this subject however we venture to espress our earnest desire to see the different Institutes, co-operating in their researches on the average state and safety of different districts, in which it is more necessary and feasible to co-operate than in our inquiries as to the best tracks for ships to pursue, since every nation has in general to deal with different tracks, viz. different starting points, not so much with different places of the Ocean, and especially with ships differently built and equipped, wherefore different tracts even between the same places may be eligible. So for the two former points of the physical geography of the Sea and the dangers we are liable to encounter, we must I think assign to every one, a special subject of research.

There seem to be two ways for it; either we must each study another instrument, or each another part of the Ocean.

The material of the observation is not so well provided that, if distributed over three or four Institutes, each, England, France, Germany, even England, to which the Lion part belongs, would have a sufficient number of them, in a certain part of the Ocean, to be able to derive therefrom the details, and to be certain that these details are not only apparently true. So it might be that another nation had published charts of the same region which gave or appeared to give different details, no more true

and exact than the other, from the same cause, want of sufficient information.

Now we must conclude that even this nonconformity in some points shows us the uncertainty of these points, but it would have been better to join the two sets of observation, and to derive from the whole number a more reliabe result. Were the two charts precisely after the same model, every one could combine them afterwards; but such is not always the case, and not allways reciprocally. England can make use of the Dutch charts, and Mr. Scott has trusted so much the Dutch sailors that he has done them the honour of inserting their detailed observations into his charts. Would it not have been preferable and less expensive, if one of us had the whole material and had given only one chart!

So our chart of icebergs is, considering the time when it wo was given out, a good chart, but we are the first to complain of the scantiness of observations recorded. Therefore, when one Institute declares that it will study the physiognomy of a district in a given direction, we earnestly implore all other Institutes to supply it with all possible information they possess, gratuitously, as we are willing to do.

There is another question to be solved by a meteorological marine establishment. How can we indicate the best crossings the way for ships to follow in the shortest time between two given harbours.

If we ask the shortest way, the answer is soon given: it is the arc of the great circle, or two or more arcs of great circles, if the land or rocks do not allow ships to follow the same great circle. There was a

time, when all pointed to this way; instruments and charts have been given, by which the great circle could be set out with great facility without reckoning; and in some cases all this is not to be underrated. By this however the question is not answered; it does not mean what is the shortest way, but what way can be followed in the shortest time and without other impediments.

For instance we can not follow the great circle, if land is found on it; further it can not be followed between the Cape and Australia, because the ships would be compelled to go too far south, where they are in danger from ice, and where the seafarers suffer unnecessary inconvenience from cold, where perhaps the winds are too strong, and so forth.

Even if this is not the case, it is evident that the great circle from A to B is the same as from B to A, and that the same route cannot be followed in the shortest time both ways.

So great circle sailing alone can not be the practical solution of the question.

What then? Some say: we must conclude on the direction of wind and current and the force thereof, how we can best come from one meridian to another and in what latitude, and from one parallel to another in each meridian. Nor is this sufficient, for two reasons. First we ought to know, much better than we do, the efficiency of wind and current in every part of the ocean, to be sure of the best crossings in such a part; then secondly, even if we could derive the best crossings from our theoretical knowledge, or if we deduced from practice, where we

can best make east, north, or the opposite directions, it might and it will often happen, that the end of one partial way does not coincide with the beginning of the next partial way, and that we lose more time in coming from that end to the following starting-point than we gain by following those partial routes, instead of others which seemed less advantageous.

To make this plainer, I will suppose and it was nearly a case, which I experienced myself, two persons, A and B leave Utrecht for Edinburg at nine in the morning on Monday. A goes by Ostend and London, which is certainly not the shortest route, B goes by Havre and Peterborough, B is several hours sooner at Harwich than A is in London: A is at an earlier hour nearer to Edinburgh than B is, but B not taking the train for London directly, says: the way to Peterborough is much shorter; and nevertheless A is twelve hours later at Edinburgh than B who arrives in the afternoon of Thursday. A has taken the shortest route to the English coast and from Peterborough they have the same way; but from Harwich to Peterborough takes more time than from Ostend to Peterborough: so A loses more time from his journeys end, to the common starting point at Peterborourgh, because of there not being a direct express communication between these two points, than he has gained in taking the shortest route to the English coast.

So we may give another instance of ships from the Cape to Holland. Seventy two ships crossing in January the equator at 23° West Long and more westerly, wanted 25.6 days to the equator and 62.9

to Lezard; 63 others in the same month crossing the Line between 23° W. L. and 21° W. L. required 25.1 for the first part, and 65.2 for the whole voyage; finally a third set of 59 ships crossing easterly from 21° W. L. reached the Line in 24.5 days, and seemed to have *gained* over the first set on the first part to the Line one day, *lost* in reality 4.1 day for their whole voyage lasted 67.0 days. Their loss on the latter part of their Journey exceeded the apparent gain, they thought they had made, for though they were at the equator a day sooner, they were there at a different point, less advantageous, and to reach the better point, West from 23 W. L., it appears from our calculation that they would have lost much more than one day, because of the calms reigning there.

After this having shown, it will be evident that we must take every road as a whole, and not by parts, and we must do so not indiscriminately for the whole year, since in various seasons the best road may be different; but for single months.

We have another advantage by taking the road as a whole; for then we have always routes which have been followed by the same ships, from one end to the other, while in studying the parts separately, some may have been traversed by ships of different size, seaworthiness, laden with different burdens, than others. We thus have a result only practical, without anything theoretical mixed with it.

In this way the R. Dutch Meteorological Institute has calculated its best crossings, and I give a specimen of it in broad lines, while this method is different from the usual one, and too little known. Even in

Mr. Findlay's exellent work [1]), where notice is taken of our operations, it is not yet met with. Only the results of the ancient method are mentioned.

We compared together the routes of the ships which sailing from Holland to Java were at the same month of the year on the meridian of Greenwich in the Southern Hemisphere. So only the very slowest of them were in a different month with the best in a given parallel on the former and again in a given meridian on the latter part of their Journey, and consequently they met on the whole with the same average winds. The greater number moreover assures us that all directions of wind occurred nearly in the true proportion at each place.

Now of 100 ships, in proportion to the reality observed, 45 crossing the meridian of Greenwich in S Lat require for the whole passage 95.5 days to Java Head, while 45 others, crossing the same meridian north of that point, required 101 days. We say therefore: the best way is to cross Greenwich meridian at 40° S Lat.

Knowing now, where to cross this meridian, we examine the two parts separately. Of these 45 ships, 25 passed west of the Capverdian Islands and followed a westerly course in the easttrade-wind and have reached Greenwich meridian in 53 days, while the 20 others passed east of the Capverdians and were on the whole on the east side of the former track. They require 60 days, and lose 7 days in comparison with the former.

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1) North Atlantic Ocean by A. G. Findlay 1873, 13$^{\text{the}}$ Edition.

Further of the same 45 ships, that crossed Greenwich meridian South of 40° Lat, 20 crossed the meridian of 50 East Long South of 42° S Lat, with a southern track and require 40 days from Gr. meridian to Java Head; the others, passing north ot 42 South Lat in 50 East Long and on the whole following a more northern track, want 42 days, two days more. Therefore we say: take in November a westerly course to Greenwich meridian in 40° S Lat in 50 East Long, and you will have a gain of more than 9 days.

So we go further and we can delineate, from one end to the other, the best way to be made in the shortest time. Indeed 15 of these ships, which followed the indicated route in all parts, have only 86.5 days against 98.5, the average of all the 100 ships.

The paper: *Zeilaanwijzingen van Java naar het Kanaal* Utrecht 1868—1870. Manssen, price *f* 7 gives ample elucidations and examples. It shows how we might learn even from those ships, which did not follow the best routes in one of the parts of it, something about that best route; but here I can not enter into all particulars of this method. It may be sufficient to have shown the principal outlines of it.

Its general results are shown by coloured lanes, for each month separately, on twelve charts.

We are now getting ready the crossings from Holland to Java, and on this voyage we get a more ample profit by the new track than for the homebound ships, because it is nearly indifferent, what way ships coming from Java proceed to the parallel of 20 S. Lat, and it makes only a marked difference for the rest

of their way, especially in the part north of the equator in the Atlantic, how far they go west, before they come up to Landsend.

That even for these homebound ships the gain is not to be despised, we add this statement from the *Stoomschepen op lange lijnen* by J. E. Cornelissen, Utrecht Manssen 1870.

Before 1857 the average number of days from Java Head to Lezard for 450 ships was 110.3 days.

From 1857 till now for 70 ships it was 102.7 which gives a shortening of the voyage of 7.6 days and this is only for a part to be ascribed to a better construction etc. of the ships; for from Java Head to St. Helena, where the track is more indifferent, the gain is only 1.3 days: say it is 1.3 days more from St. Helena to Lezard; then we have a gain of 2.6 days for the construction of the ships; and the advantage gained by following a better route is of 5 days.

I stated already that as soon as we have a greater number of ships we shall give separate routes for ships of different size. We cannot give this from a limited number, as unfortunately is often too soon done. We cannot rely on the voyages of only five or ten ships, and in general we must warn against conclusions too hastely drawn from a small number, all sailing in the same year, because we have then not eliminated the different seaworthiness of the ships, and the different distribution of the winds over the Ocean.

After having given in the preceeding lines the method in which we inquire into the average distribution of the phenomena at different places of the Ocean and

the shortest routes, I point once more to this: that by the last method nothing theoretical is mixed up with the practical question, and that we have the certainty that a ship following our indicated route, made really by several ships, will have on the whole a quick passage.

This method is I think unquestionably certain, and to be followed by every one.

Utrecht, March 1873.

BUIJS BALLOT.

Abstract Log of the ship Oceaan

Date	Hour (a)	Course and Distance by the log in every watch of four hours. (b)	Latitude. by Observation.	Latitude. by Dead reckoning.	Longitude. by Observation.	Longitude. by Dead reckoning (c)	Currents. Direction.	Currents. Rate.	Magnitic Variation observed.	Winds. Direction.	Winds. Force.	Barometer. Height	Barometer. Thermomet.
IV 26	12	SSW ½ W 24′	2°12′ S	2°7′ S	29°55′ W	29°39′ W	WSW½W	17′	12°	S. E.	4	764.0	27°
	4	SSW 20′								S. E.	4	64.	26.8
	8	SSW 24′		2°53′		30°12′				S. E.	4	63.	26.1
	12	SSW ½ W 30′								S. E.	5/6	,,	26.1
	4	,, 30′		3°46′		30°40′				S. E.	6/7	64.0	25.9
	8	,, 30′								S. E.	5/6	64.5	27.6
IV 27	12	,, 30′	4°43′	4°39′	31°24′	31°9′	WSW½W			S. E.	6	64.5	28.

(a) The time for making observations is at the end of the watch; obligatory observationhours are four in
(b) We have admitted this columns, that we may have a means, to study the currents and to learn how
(c) Position by dead reckoning should be deduced from the position by the last observation.

Model how abstract

Number of the Abstract Log.	Year and month.	Date.	Hour.	Latitude by Observation.	Latitude by Dead Reckoning.	Longitude by Observation.	Longitude by Dead Reckoning.	Currents. Direction.	Currents. Rate.	Magnetic Variation observed.	Direction of the Wind.	Force of the Wind.
1462	1870 XII	12	12	14° 56	14°55′S	31° 32′	31°46′W	East	14	7°56′ NW	EbN	5
1462	1870 XII	12	8		15°48′S		31°26′W				EbN	5
1462	1870 XII	13	4		16°43′S		30°32′W				EbN	5
1462	70 XII	13	12	17° 37′	17°35′S	30° 6′	30°16′W	ESE 7/8 E	10	8°17′ NW	EbN EbS	5
1468	71 IV	30	12	25° 38′	25°29′S	53° 28′	53°15′ E	SW ¾ W	16		WSW SW	4/5
	IV	30	8		25°24′S		52°37′ E				SW	5/6
	V	1	4		25°22′S		51°57′ E				SW SSE	8/8
	71 V	1	12	25° 23′	25°28′S	51° 10′	51°19′ E	NWbw½W	10		SSE	8/7

Captain X from Y to Z 1873.

Thermometer. Air.	Thermometer. Water at the Surface.	Hours. Fog A. Rain B. Snow C. Hail D.	State of the Weather, the form and Direction of the Clouds. Proportion of the Sky Clear.	REMARKS. State of the sea, tempest, water spouts, red fogs, deep sea soundings, ele, shooting stars, Aurora borealis Halo's, Meteors, Birds, Insects, Fish, sea-weeds drift wood, etc.
27°	27.8	½ B	Cum. Str. NW 3	
26.6	27 2	3 B ¼B	Num. NW 1	
26 1	25 5	4 B	Geheel dik 0	
26.4	26.0	½ D	Cir a Cum / Nim NW / NW	
26.3	26.0	1 „	Cum / Nim NW / NNW 5	
27.4	26 8		Cum NW 6	
27.6	27.8		Cum Str. 6	

the morning, noon, and eight in the evening.
hurricanes and typhoons arise, move, and are to shunned and escaped.

Logs are to be transcribed.

Barometer Height. in Mill:	Barometer Height. in Inch.	Barometer Height. Ther. of the Barometer.	Temperature. Of the Air. Noon.	Temperature. Of the Air. 8 hour p. m.	Temperature. Of the Air. 4 hour a. m.	Temperature. Of the Water. at the Surface.	Temperature. Of the Water. at Certain depths.		Serenity of the Sky.	Hours of Fog. A. Rain B. Snow C. Hail D.	State of the Sea.	Remarks.
	30.15	74	27.2			27.1	25.5 / 100		5		Moderate	
	30.13	74		26.4		27			3		„	
	30.14	73			25.8	26.8			4		„	
	30.22	74.5	26.4			26.7			6			
763.1		25.2	25.4			24.3			4/8			
63 8		25.2		23.4		24.2	22.1 / 100		4/7			
64.3		24 6			22.8	24.2			4/3	½ B		
67.9		24.6	24.2			25.2			4		S W / High.	