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Preliminary Report on the Physical Observations Conducted on the National Antarctic Expedition, from 1902 to 1904: Discussion

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pressure reduced to 50 or 60 mm., and the observations commenced. The arrangement was as follows: twelve coincidences observed, six right and six left; then an interval of fifty coincidences allowed to pass, and twelve more coincidences observed. During this process four readings of pressure, temperature, and arc were obtained. The pendulum was then left to swing for two hours, and at the end of that time the same observation repeated. All three pendulums were swung in this manner; on the following day the case was taken off, the pendulums reversed on the agate planes, and the whole process repeated.

Thanks are due to Mr. R. W. Skelton for his valuable assistance throughout the gravity-work. He soon made himself competent in observing, and took independent sets on each occasion that the pendulums were swung.

The pendulums were swung at Melbourne and Christchurch before the departure of the expedition from New Zealand, and again at Christchurch on returning.

The following are the dates of observations made at Winter harbour :

1902, July 31. Two complete sets with all three pendulums.
 August 1.
 1903, February 1. Ditto.
 „ 2.
 „ 6.
 September 5. Ditto.
 „ 6.

The calculated acceleration for Winter harbour, found by the aid of Helmert's formula, $978.0(1 + 0.005310 \sin 2\phi)$ cms., is 982.96 cms., and a determination from a set of observations taken at Winter harbour gives 982.83 cms. However, as some of the corrections have not been applied, this value can only be regarded as very roughly approximate.

From the geologist's report on the formation of the vicinity, a determination of a correction for density of the rocks can be made.

Before the paper, the PRESIDENT said: There is no occasion to introduce Mr. Bernacchi to you, because he has already given us a very interesting paper, and he is in a position which no other human being is in, on the face of the globe, in having passed three winters in the Antarctic Regions. On the last expedition he was physicist, and we have the evidence of his commander, Captain Scott, how very assiduous and careful he was in taking these observations, and under what very great difficulties they were carried out. I will now ask Mr. Bernacchi to read his paper.

Captain CREAK: I have listened with great pleasure to the lecturer and his plain, unvarnished account of the physical observations made by the Antarctic Expedition. Naturally, being chiefly interested in questions bearing upon the magnetic observations made during the years of sojourn in those quarters, I turn to them as the subject of my remarks. Perhaps many of us who live at home at ease, or who have only made observations in comparatively temperate climates, fail to make sufficient allowance for the severe conditions of the Antarctic climate when we criticize the work done by our intrepid explorers. It is easy to ask why was not this done, or why was not that done? and generally to find fault. This I do not propose to do, but I will consider shortly the good things the *Discovery* brought home. I notice that sea observations are excluded from this paper, but I may incidentally remark that a series of ship observations, which are possibly of great value, was taken after the pack-ice was entered. If we look at the map, in

the region between winter quarters and King Edward VII. Land, a series of observations were made which are practically absolute, and are especially valuable because they give us the gradual increase of dip and force in a particular line, which is very convenient for tracing out the position of the pole. They probably will eventually have great weight when the position of the magnetic pole comes to be considered. I noticed Mr. Bernacchi talked of landing at Wood bay, or some place further north, with the object of going to the south magnetic pole. But really to fix the magnetic pole properly would require a similar expedition to Captain Amundsen's in the north, and our Antarctic explorers were not prepared for a three years' sojourn for that purpose. The site for a magnetic observatory was not by any means an ideal one, being near a volcanic mountain on a basaltic formation, but it was a case of needs must. I have reason to know that an excellent series of absolute observations were made here of the magnetic elements, and then on sledge journeys from this centre. But how about the local magnetic disturbance at such a spot? It was decidedly great, and many of the observations on land journeys connected with it were more or less vitiated. A happy thought, however, saved the situation, for in November, 1903, the tent observations of the magnetic elements were undertaken on the ice in McMurdo's strait with 220 fathoms of water below the spot. I think that is one of the most valuable observations in the whole series, because it has really enabled us to reduce all the disturbed observations and get corrections for them. These observations are not only exceptionally valuable in themselves, but they formed a key to the final reduction of the sea observations I have mentioned, as well as those on land. Respecting the observations made with the variometers, it is yet very early days to say anything, but I may remark that there seems never to have been anything like a quiet day as we know it in temperate zones. The mean daily ranges of declination, observations of which element have been successful throughout, are, judging by the comparison with those made in Discovery bay in the Arctic expedition of 1875, much as might have been expected. Taking London as 1.0, the horizontal force in Discovery bay was 0.29; at the southern winter quarters, 0.24. We find that the increase of daily range is, inversely, in about the same proportion. It is to be regretted that the force variometers did not do better. The vertical force observations are a trouble everywhere, and where a large range of temperature, as in Antarctica, prevails, you must not expect too much from them. At Cheltenham, in the United States, for example, they are so particular about the vertical-force instrument that they have built a structure which practically obviates all effects of change of temperature. So I am afraid the vertical observations will suffer accordingly, especially as the reader mentions that he had to add an extemporized weight to the instrument to balance it. It had been balanced, I understand, for 70 degrees of dip instead of 84? [Mr. BERNACCHI: Yes.] I do not agree with the lecturer with regard to the instrument being too sensitive for a polar station, but he has a well-founded objection in the confusion of curves brought about by having one photogram for all the elements and temperature curve. That seems a great pity, because they have evidently, from what I have seen of the curves, very much confused them. I think that we have a lesson to learn for the future in regard to Antarctic expeditions, that we shall not only have a larger scale, but that we shall also have a photogram for each element. Like the Arctic Expedition of 1875, little or no distinct connection was observed between auroræ and disturbances, the frequency of the latter probably masking any connection if such existed. I notice, also, that Mr. Bernacchi says he took magnetic observations on the point at Cape Adare. Were they exactly on the spots occupied by the *Southern Cross* observers? [Mr. BERNACCHI: Yes.] According to that, although there is only an interval of

two or three years to go upon, there seems to be very little secular change. [Mr. BERNACCHI : Practically none in the dip.] I suppose you have not noticed any seismic effects on the instruments? That would have been an important point to notice, because it has been one upon which Prof. Milne has made particular investigations. [Mr. BERNACCHI : In one case only, and not magnetically disturbed.] I am happy to say, from what I know, that the magnetic observations now are in exceedingly good hands. I am sure that Commander Chetwynd, of the Admiralty, will do his work well, and we may trust to Dr. Chree, who is an old hand at all these reductions of variometer observations, to obtain full value from them. I hope, when all this is combined with the work of foreign countries, we shall obtain a knowledge of terrestrial magnetism we never before possessed, not of the southern regions only, but of the whole world. I think this our southern expedition will be found to have contributed largely to this knowledge.

Captain CHETWYND : I think perhaps it would be as well if I were to start off by explaining the exact position in which the magnetic observations are now. The Admiralty undertook the reduction of the absolute observations among others. The use they would be put to was to obtain the base-line values for the curves of the magneto grams. The reductions of absolute observations is complete, and it therefore remains to consider the magneto grams on the base values obtained from them. In view of the more valuable results which will be got from considering magneto grams, I have not gone into the question, which may be gone into, of obtaining changes and diurnal variations and so on from the observations themselves. Whatever result is got in that way would not be of equal value with the result obtained from the curves. So far as we have got, I think I can say there is sufficient justification for the opinion of the extremely valuable results which will be obtained. The observations of the declinations on shore combine very well with those taken on board the *Discovery*, and putting them together I have been able to draw a fairly accurate chart of lines of equal variation for that district. I only say fairly accurate, because no diurnal change has been applied; when it comes to be applied, it may to some extent alter a few of the curves, but there will not be any very great difference. I shall not fly in the face of such a high authority as my friend Captain Creak, and jump to conclusions about the pole. Of course the observations for dip taken on shore, also those which Captain Creak mentioned, give a very near approximation, or they will when they have been fully considered, as to the position of the pole. And I may mention that I made an attempt myself to fix the position of it, and it seems to be rather more to the south-east than was previously supposed. This is also corroborated by the lines of declination, which is a very valuable corroboration, as they are two totally independent sources of finding it out. Mr. Bernacchi mentioned observations taken on the ice, and gave his opinion as to the value of them. They were used as the basis or base to which all the observations on board the *Discovery* were reduced. They were the standard set of observations taken as being most undisturbed. I am sorry I cannot answer Mr. Bernacchi's question as to the azimuth of the mark. I only had the paper a short time before I came here; but, speaking from memory and from one or two instances which I looked at, there is no reason to criticize his figures; except in one or two minor cases, they agree with my results.

The PRESIDENT : Does the magnetic pole appear to be to the south of Ross's position?

Captain CHETWYND : Yes, slightly, and it appears to be nearer Wood bay.

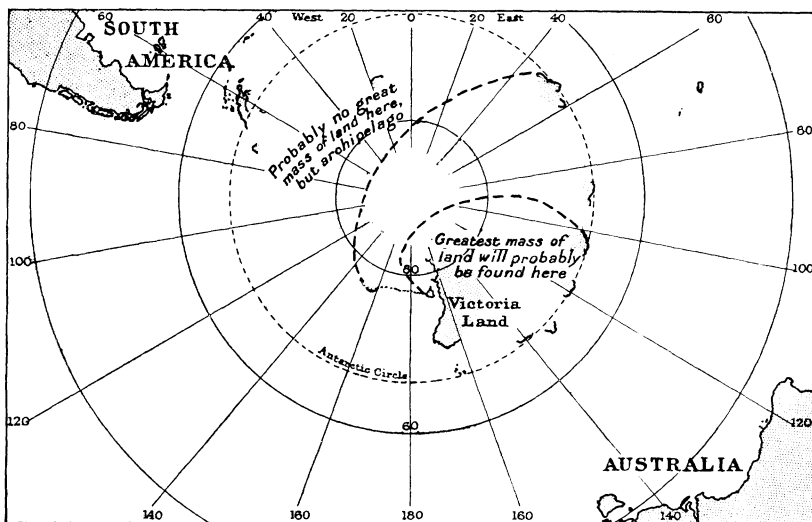
Mr. REEVES : Mr. Bernacchi has informed us that his paper can only be considered as a preliminary account of the magnetic observations taken during the time the *Discovery* was in her winter quarters, but I am sure that we must all feel

that it is a most important one, and it is certain that the results, when finally worked out, will go a long way towards throwing additional light on the magnetic conditions of the Antarctic Regions, and the subject of terrestrial magnetism generally.

I have listened to the paper with special interest, for, as some of you may remember, about nine years ago I read a paper on this subject at one of our technical meetings in this room, in which I ventured to make certain suggestions and express opinions which I had arrived at by investigations, the general outline of which I then explained. Since that time I have had many opportunities of discussing the matter with those who have made a special study of the subject, and have found, after careful consideration, that there has been on the whole a willingness to admit that the leading points brought forward appear in the main to be correct. But perhaps what is more important is, that the explorations and observations made by expeditions in recent years tend to prove the general accuracy of my conclusions.

I have naturally followed the results of recent expeditions with great interest; especially those of the National Antarctic Expedition, and I am therefore glad of the opportunity the President has now afforded me of saying a few words in connection with this subject.

When it was decided to send out the *Discovery*, working on the lines of my paper, and reducing the curves of equal magnetic inclination to one common base, which I took to be a line parallel with the Earth's geographical axis, and generalizing from the distribution of the Earth's magnetic intensity, as indicated by the lines laid down in the magnetic charts, I ventured to predict where, to be consistent with my conclusions, the greatest mass of land would be found in the Antarctic Regions. The results I indicated on a chart, which, after it had been shown to Sir Clements Markham and several others, was locked up in a drawer until the return of the expedition. The following is a rough sketch of this chart, with the indication of the distribution of land, as it is shown on the original:—



When it is remembered that before the return of the National Antarctic Expedition, and the important journeys of its various members, especially those of

2 x 2

Captain Scott and Mr. Armitage across what is evidently the continental mass of Victoria Land, we had practically no knowledge of where the principal mass of land would be found, the results as shown on my chart must be considered remarkably near the truth. For my purpose it was merely a question of the *mass* of the land, not its form, and it may be, of course, that Graham Land and the land in the neighbourhood is either a long and narrow peninsula, or an archipelago; either would answer the purpose.

There is not time now, nor is this the occasion, for me to do more than refer briefly to these points; but to my mind it is clear that, notwithstanding the necessity for actually measuring the inclination of the needle from the horizon-line of the observer's position, the curves of so-called equal inclination as *thus laid down on charts*, since they are not measured from a common base, are misleading, and, when it is attempted to generalize from them, tend to confusion. When the angles are all reduced to a common base of measurement, such as a line parallel with the axis of the Earth instead of the varying horizons of different latitudes, the curves are, as I have shown, much simplified, and become what they would be expected to be, supposing the needle to be drawn out in the direction of the Earth's great centres of magnetic force. I have in a general way worked out these curves for the northern hemisphere, and have also attempted some for the southern, where, although the information is meagre, the results are consistent with those of the northern hemisphere.

There are several points in Mr. Bernacchi's paper I should like to refer to, but it is now late, and I will only just call attention to the way in which his observations support the theory that land areas are generally more highly magnetized than the water areas. This will be seen at once by a reference to the tables given in the paper.

A great deal has been said about the South Magnetic Pole, but to me the areas of 90° dip, whether in the northern or southern regions of the Earth, are secondary matters, and I believe their location depends principally upon the distribution of the land and water areas of the polar regions. In all probability, if we had the greatest mass of land in the Antarctic Regions on the opposite side of the pole to which it is now, we should have the so-called magnetic pole on that side also. This is further indicated by the declination of the needle, the direction of which is, doubtless, determined by the surrounding magnetic force or intensity.

The PRESIDENT: I think the time has now come for thanking Mr. Bernacchi for his paper. It would seem that a great responsibility was placed upon so young a man. Not only was he given charge of all the land magnetic observations, but also the aurora observations and the observations connected with the seismographic instrument and with the pendulum. We must always remember under what very great difficulties and hardships these observations were taken, especially with regard to that very unpleasant little instrument for observing atmospheric electricity. I particularly admire the way Mr. Bernacchi and his companions went to work, after a frightfully hard day's work, to take the magnetic observations while away travelling over the Great Ice Barrier. Mr. Bernacchi in the tent, and his two comrades waiting outside for a whole hour for their suppers, not obtaining any warmth whatever after being exposed to this extreme cold for the whole day; and this day after day for many days. When we consider all these points, I think we ought to thank Mr. Bernacchi very warmly and very cordially for the way in which he stuck to his work, never shirking it day after day, through summer and through spring, continuing these very difficult observations requiring extreme care. I think we ought also to thank Captain Creak for the remarks which he has made, Captain Chetwynd for the very interesting

information he has given us, and Mr. Reeves for the great trouble he has taken in explaining to us his views respecting magnetism. In adjourning the meeting, I ask you to pass a vote of thanks to Mr. Bernacchi and those who have joined in the discussion.

Mr. BERNACCHI: I thank you most heartily for your vote of thanks, and for your attention this afternoon. There is one thing I should like to say: That although the observations were only too frequently difficult to carry out, the work was very greatly facilitated by the assistance and interest taken in it by other members of the expedition, especially the assistance rendered by Captain Scott on every possible occasion, and the personal assistance in taking observations by Engineer-Lieut. Skelton. There is one point I would speak about, and that is with regard to observations taken on the sea-ice close to the shore. It would be interesting to know whether the daily and monthly changes on the sea-ice away from the land masses are exactly the same as the changes observed on shore. I think possibly they are not. A dip observation taken on the sea-ice before starting on the Barrier sledge-journey, and another taken a month afterwards on our return, show practically no difference between the two sets, whilst there is a considerable difference between two sets taken on shore; that is to say, one set before the expedition set out, and another after it returned, a difference amounting to something like ten minutes of arc. It was at the time when the dip was changing rapidly—at the end of January and the beginning of February. Thus, whilst there is practically no change on the ice, there is a very large change on land. At Cape Adare, in 1898, there was practically no monthly difference or only a slight monthly difference in the inclination, whilst at Winter harbour there is an annual difference of something like forty minutes of arc. I think it is rather difficult to account for this difference in two places where the rocks are of much the same nature.

Captain CREAK: In the Arctic Expedition, when they had the instruments on ice they got a different result to what they had when they were placed on land. You may remember I made a point of asking your expedition to inquire into that question. This question of disturbances on land is the reason Americans are setting to work to survey the whole ocean on board ship. Land observations alone do not give satisfactory results, and they want the oceans to give their quota, so as to get more definite conclusions as to terrestrial magnetism.

REVIEWS.

AFRICA.

SOUTH AFRICAN RACES.

'The Native Races of South Africa.' By George W. Stow, F.G.S., F.R.G.S. Edited by George McCall Theal, LITT. D., LL.D. London: Sonnenschein. 1905. *Map and Illustrations.* Price 21s. net.

THIS is a very important work, which in some portions will probably rank as a classic in African ethnology. It was compiled by the late Mr. George Stow, who arrived in Cape Colony in 1843, and who died (apparently) in the "eighties" of the last century, leaving behind him a book which was nearly ready for publication, and which was to be dedicated to the late Sir H. Bartle Frere. The work seems to have remained for a long time in the possession of Mr. Stow's widow, and might never have seen the light had it not been purchased from that lady by Miss Lucy