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Hoping the few observations I have contented myself with making on the defects of the article *Observatory* in this valuable Encyclopædia will find a place in your Magazine, and produce a good effect, I remain, gentlemen,

Your obedient servant,

Sept. 1822.

A. M.

XXXVII. *A Letter to Professor MILLINGTON, of the Royal Institution, respecting some Frigorific Experiments made on the Magnetic Fluid, and on Sea-Water. By B. DE SANCTIS, M.D.**

SIR,—THE magnetic experiments of yesterday which I had spoken of to you the day before, have been successful beyond our most sanguine expectations. Mr. Cary (Strand), in whose house they were made, took a very active and intelligent part in them. But had you been present, as we expected, they would have been much more agreeable to us. Between two hollow parallelopipeds of laminar copper, four inches in length, four ditto in breadth, and one inch in depth, filled with ice and muriate of lime, and rendered air-tight by greased covers, the thermometer F being placed across them and the bulb resting on the cover of one of them, marked 40, the magnetic action of a common arrow-shaped needle, two inches long, 1-16 ditto in the greatest breadth, weighing with the brass cap $7\frac{1}{2}$ grs., was greatly paralysed in the open air, and also under the glass of the air-pump, and much more during and after exhaustion, when it stood still. The centre of the needle was at half the height of the parallelopipeds, and nearly at the distance of an inch and an half. Two other needles of the same shape, but three inches long, 1-8th in the greatest breadth, and weighing with the brass cap $11\frac{1}{2}$ grs., showed still less activity, but not so much as the other. In trying a new needle, it was necessary to allow it sufficient time to adapt itself to the lower degree of temperature before it exhibited the *paralysis* of its forces. By this word I understand its being less sensible, or even altogether insensible, to the bar at the same distance and direction, and its sluggishness in returning to its former position, if it ever does so. The extremes of the scale of temperature, the thermometer being placed as before, were from 30 deg. at the moment of the most perfect vacuum to 40 deg. in the open air. Comparative observations on a more extended and detailed scale of temperature, particularly *in vacuo*, would be an interesting acquisition, and so much the more, as sufficient attention has not been paid to the temperature, when it has been said

* Communicated by Mr. Cary.

that

that the magnetic force augments *in vacuo* through the abstraction of the resistance of the air.

Now, sir, I am thoroughly convinced of the exactness of the indication, if not of the graduation, of the little watch-needle I had the honour of showing you at your lectures at the London Institution. That needle, amongst many other interesting facts, in the course of my last magnetic researches presented to me some time ago the same effects when plunged in a bath of ice and muriate of lime; but to avoid the objection that the movements of such a needle were not to be depended upon, and that they might be paralysed by the dampness of the air at so low a temperature, I wished to repeat the experiments at large, and in a vacuum, where certainly there was no dampness till the return of the air caused the vapours of liquefied ice which penetrated through the grease to fall upon the parallelopipeds, the needles and their supports—where a small degree of humidity was afterwards sensible, but only to the touch. This objection however at present can no longer exist, since I repeated at Mr. Cary's the same experiment on a larger needle similar to the lesser one above described. It was placed in a wooden case, likewise covered with glass, and protected from any effect of dampness by spreading the ground with dry powdered muriate of lime; and it was really interesting to see how soon the needle passed from activity to sluggishness at the alternate immersions and emersions from the frigorific mixture.

Amongst the many principles of reform in the construction of magnetic needles, of which I spoke to you, as being partly tried and partly to be tried, Mr. Schmalcalder agrees with me, that it would be perhaps better to destroy the action of the magnetic forces, in order to balance the inclination needle, by intense cold than by intense heat. The steel is always damaged by a high temperature, notwithstanding every precaution; and besides, I have seen practically enough in these matters to be able to assert, that even the incandescing heat cannot entirely destroy the magnetic power of any considerable mass of steel or iron, excepting loadstone of some particular mines; and even were it to happen momentarily in thin laminar steel, as soon as it cools the power begins to reappear. I was communicating the new project to Mr. Garden, of Oxford-street, when he very intelligently remarked that it would perhaps be better to try it *in vacuo*, as the numidity necessarily arising from a too low temperature in the air, might disturb the exact effect of gravity by its unequal precipitation on the needle and the augmentation of friction. In fact, that which was scarcely observable, even after the readmission of the air

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in the vacuum, at Mr. Cary's, I afterwards easily observed in the needles in the open air, by lowering more and more the temperature of the parallelopipeds, in consequence of many alternate and thinner layers of the mixture. At all events, therefore, I would recommend the trial *in vacuo*, with layers like the last, and with their ingredients reduced to the most convenient thinness.

But, waving the discussion till some practical observations give us more insight into the matter, let us rather relate how the magnetic forces decreased under the exhaustion of the air-pump, and how they increased on the readmission of the air. The decrease was slower than the increase; but the exhaustion too was slower than the readmission; and it was the same with respect to the progressive diminution and elevation of the temperature from 40 to nearly 30, and from 30 again to 40. But, notwithstanding the greater rapidity of augmentation, the air and the temperature once restored as before, the magnetic action seemed not restored to the same degree. I do not know whether this anomaly is to be deduced from an increase of friction depending on the precipitation of aqueous vapours, or from the protracted influence of the cold. How far could these two circumstances affect the oscillatory laws of the magnetic power when referred to the two opposite agents *in vacuo*, viz. the greater or less resistance of the medium combined with less or greater degree of temperature, and tried at different intervals through their different combinations? and what is the cause of such varying paralyses in needles? It is not an easy matter to answer the first of these queries, as depending very much on the delicacy of instruments and experiments; but as to the second, it seems the cause of the magnetic paralysis is not in the condensation of the air presenting an obstacle to the magnetic currents, as the effect equally takes place *in vacuo*: it seems the cause is not in the condensation of the steel, as it is commonly known that the greater the density of the steel, the more difficult, but the more great and tenacious, the intensity of the same currents once established, and besides, it could not so rapidly increase and decrease; therefore it is only, or at least principally, the action of cold on the magnetic fluid itself which produces the paralysis. Yes: as there are ices of rougher fluids, so there are more refined ices of the ethereal ones; and Des Cartes's mechanical principles will account as well for those which we may see and touch and taste, as for those which we can but barely discover by their relations with the movement of the needles.

Encouraged by the results of yesterday, I wished to try again this morning, in the open air, the effect of a low temperature

on needles, the bars being out of the sphere of the cold; and, moreover, its effect on bars, the needles being in their turn out of the sphere of the cold; and lastly, the effect of the same on both of them at once. A more favourable stratification of the mixture allowed me to observe more decided effects at each combination, notwithstanding the general temperature of the room being lower than yesterday (both of them from 60° – 70°), and notwithstanding I had no larger apparatus for cold than the former ones. They were, however, adequate to the graduated series of the bars, lying sometimes on their sides between the two parallelopipeds joined together, and sometimes separated only by their joined depths or lengths from the needles. I have even plunged needles and little bars into ice, and used both of them after drying them. The results have been always the same: the lower the temperature and the longer the immersion of the bars and needles, each separately or both at once, in the frigorific sphere, the greater the paralysis. What influence, therefore, must not be exercised on the currents of needles, as well as sometimes on the terrestrial ones, by the low temperature of the polar regions?—I am indebted for the idea of these experiments to the simple observation of Captain Ellis, who, meeting ice mountains in Hudson's Bay, saw his needles sluggish at their approach, and says that he restored them to their former activity by warmth. Far from reasoning on the circumstances, he has the air of repeating the fact as a kind of mysterious accident. What a difference between that transient rough observation and the results we have obtained! Such is the progress of science, aided by time, zeal for experiment, and skill in observing!

This morning, too, I have completed, with Mr. Garden, the experiments happily begun yesterday at Mr. Cary's about freezing sea-water with the same frigorific mixture used in the magnetic experiments. Sea-water from near South End (Essex), sold at the Establishment of the Sea-water Baths in George-street, Adelphi, freezes when in perfect quiet at only about 18° F., or at the utmost from 18° to 20° , when in small quantity and agitated on purpose. At 22° it was impossible to obtain ice through any length of time. What credit then shall we give to what Thomson says, even in the sixth edition of his "System of Chemistry," on "Nairne's authority from the Philosophical Transactions," that sea-water freezes at $28^{\circ}.5$ Fahrenheit? By the by, the general temperature of the room was rather high, to favour as much as possible the frigorific effects of the mixtures! The ice obtained from the water of the sea may be nearly deprived of salt, particularly when obtained
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without agitation, and well washed with cold water and dried on blotting paper; but animal and vegetable substances chemically or mechanically dissolved, impart a much deeper colour to the salt which remains after the complete evaporation of the washed liquefied ice, than of an equal quantity of the sea-water from which it originates. Therefore, if they are not more accumulating in the ice, they are at least the same quantity in a less proportion of salt; and consequently the colouring must be more strong. The colour of the dry salts certainly did not depend upon iron, as their solution in distilled water was not sensible in the least, either to the gallic acid or to the prussiate of potash. It follows naturally from these trials, that fresh water for kitchen uses and washing may be obtained from sea-water better by distillation than by freezing and particularly if care be taken to purify the distilled water further by filtering it through sand and powdered charcoal, and to impregnate it afterwards with a small quantity of carbonic acid gas, which would take off its flatness, and make it more palatable. By these precautions perhaps all bad effects would be better avoided, should they even be as great as those described by some physicians of the South, who had an opportunity of seeing the effects of the long-protracted use of that water on some criminals, and which have never been observed in England upon any occasion of its use, not even when, at a large dinner of two hundred persons, South-End water was the only water used even for ices. But sea-water taken at the same depth in the southern seas might perhaps be more charged with noxious substances than the sea-water of South-End: and, besides, there is a wide difference between the use of it for one day at a great dinner, and for weeks together with frugal fare. But, be that as it may, it will always be of some interest to know that with a small quantity of ice, and muriate of lime, a great deal of ice might be produced in a vessel without any waste of fresh water or ice fit for use.

Some couples of thin lead cylinders, or perhaps better parallelopipeds, placed the one within the other, would suffice. Fill the internal one with sea-water; fill with alternate layers of pounded ice and muriate of lime, which needs not absolutely to be powdered, the little space between the two; and keep them quiet. After some time, always depending on the more or less advantageous disposition of the dimensions of the recipients, on the general temperature of the air, and the former temperature of the water (which ought to be taken as little warm and salt as is allowed by the means at hand and the depth of the sea) light clouds shall be seen floating all along the water. This is the moment of helping the crystallization by lightly moving the

water with a stick. Thus lumps of ice are obtained sufficiently large and thick to be taken out of the recipient and fit for resisting fusion. The ice, once well fixed, may serve partly for immediate use, and partly to reproduce ice without breaking in upon the ship's store. The first liquefied ice may be useful for cooling, were sea-water to be frozen in another recipient, if it is no longer fit for the same purpose, and so on.

The muriate of lime is easily collected for further use from the sweet liquefied ice by evaporation; but it is not so with the muriate of lime dissolved by sea-water ice. It mixes then with the salt of the sea-water, and after evaporation they remain united. But there is muriate of lime too in the sea-water, and muriate of magnesia; and if the muriate of soda is not so favourable to the frigorific mixtures as the others, it will always be of some use when mixed with a sufficient quantity of the muriates of lime and magnesia, as used by the confectioners, and certainly will never be unfavourable to cooling. The sweet ice ought to be kept in wooden cylinders within leaden ones, with a sufficiently deep stratum of powdered charcoal between them to preserve a constant temperature. The muriate of lime ought to be put in earthen jars well corked up. The driest and coldest place of the ship is the best for the ice; the driest and warmest for the muriate of lime. I shall not recommend to seamen to collect the salt employed in a state of crystallization; they would scarcely understand the name, and not at all the management, and not much more than the name and the management of the air-pump for obtaining ice: but I would recommend it to any chemical man on board hospital-ships, for the use of which the proceeding is intended more than for increasing the luxuries of vessels, as the effect of the first part of water taken even by the deliquescent salts would tend rather to increase the temperature. But the passage is so rapid to the contrary effect, and such is the degree of its power, that the circumstance is not worthy of notice with seamen. I said "sea-water ice might be obtained nearly deprived of salt," because it is impossible to separate it entirely from it. The cold water would carry away the very salt water adhering to its surface; but it cannot carry away the similar drops inclosed inside without destroying the ice itself. These drops are found here and there in the thickest pieces of ice; and let the ice drain as it will, let it be washed and washed again with cold water, and dried and dried again on blotting paper, the greatest part of the internal drops will always remain inside, at least in our laboratories, and particularly if the ice has been obtained with agitation.

Might not this observation shed some light on the origin of the

the great masses of sweet ice of the polar seas? I would not say that that ice is not of maritime origin on account of its sweetness, as there is reason to believe that sweet ice might be formed through seas where the salt is diminishing as they approach the poles, particularly where at a stationary low temperature the less salt or nearly sweet water might freeze without agitation; and, besides, water descending from the air might contribute to its increase. But, besides fields floating from the North, I should incline to refer to the frozen waters of rivers and lakes, dissolved by thaw, a great quantity of those pieces of ice which encumber the shores of less sweet seas. The ice of their water cannot be deprived entirely of its salt, and particularly when formed with agitation, as would be the case at any degree above 18° . Meanwhile those ices have been found every where sweet by the majority of voyagers, and so we receive them, from the shores of Norway and Iceland. I would finish here, but allow me to add that the specific gravity of the sea-water of our experiments is 1.020.

I am, with respect, sir,

Your most obedient servant,

2, Foubert's Place, Regent-street,
June 21, 1822.

B. DE SANCTIS, M.D.

To Professor Millington.

P.S.—After having written this letter, two other reflections occurred to my mind, which seem not unworthy of being mentioned in addition to the former ones.

1. Should the process of the sea-water distillation ever be rendered, by particular construction, more easy and not at all troublesome to seamen, the distilled water might perhaps undergo a further improvement under *iceing*; it is an experiment at least to be tried at large. The vegetable and animal substances which seem not to be disposed to separate from the crystals of the sea-water, might perhaps separate where they are not surrounded by any salt drops. Besides, when the sweet ice of the ship was nearly finished, a new provision might perhaps be obtained from the iceing of the distilled water more fit for resisting fusion than the salt ice. At any rate, the muriate of lime could always be obtained again clean and equally power ful from its solution in the distilled sea-water: which circumstance is rather of importance.

2. Might not the electrical discharges through the cold air of the polar regions favour the crystallization of the aqueous vapours of the atmosphere, as they favour the formation of the frost and snow and hail in our apparatuses? Such discharges must be very frequent in the polar regions, as the very rotation of the earth and of the surrounding air must necessarily develop electricity
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in the higher regions of the atmosphere, where it is equally spread, and where it must naturally be compelled towards the poles by the perpetual current of the air from the equator towards them. Such an accumulation not only might furnish the reason of the frequency of that decidedly electrical phenomenon which is called *aurora borealis* on the North, and which is not less rare towards the Southern Pole, but might perhaps render a just reason for the great part which the atmospherical water might take in the formation of the inexhaustible ices of the polar regions.

XXXVIII. *On Lithographic Printing.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,—HAVING found considerable inconvenience arise from the use of grease on the surface of the stretched leather in the tympan frame, over which the scraper passes,—on account of the dirt it creates, the injury it occasions to the leather, and the waste of paper,—I have tried various substitutes.

The most successful experiment I have yet made, has been with Castile soap rubbed over the leather with a little water. It very speedily produces, by the action of the scraper passing over it, a glossy surface; and I feel confident that the labour in working the press is even less than when grease is employed. This, in addition to the other advantages it possesses, viz. cleanliness and œconomy, strongly recommend its use to those employed in lithographic printing—particularly those who practise it as an amusement.

I am, gentlemen, your obedient servant,

The Lithographic Press,
8, Pickett-street, Strand, Sept. 23, 1822.

CHARLES M. WILlich.

XXXIX. *Short Account of the Rocks in the Neighbourhood of St. John's, Newfoundland.* By Mr. JOHN BAIRD*.

IN approaching the fishing grounds on the coast of America, the soundings were from sixty to thirty fathoms; over the great Bank of Newfoundland, generally about thirty-five. The lead brought up a fine sand, and frequently small pieces of a rough flint, together with particles of a green smooth mineral, in the form of coarse green sand. It is certainly a singular fact, that so large a portion of shallow water should exist so

* From the Memoirs of the Wernerian Natural History Society for 1821-22. Vol. iv. Part I.