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XVIII. *On some Points in Climatology. A Rejoinder to Mr. Croll. By SIMON NEWCOMB\**.

IN the Philosophical Magazine for October 1883, page 241, Mr. Croll publishes a reply to certain criticisms of mine urged seven years ago against his theory of the cause of glacial epochs (American Journal of Science, vol. xi. p. 263, May 1876). The pleasure and interest with which I have read Mr. Croll's paper induce me to reply to it, notwithstanding a want of confidence on my part in the value of anything short of a purely mathematical investigation of the subject. It will be well to begin by examining the nature of the question, and stating in a broad way what seems to me unsatisfactory in the foundation of Mr. Croll's method.

What we are concerned with is the inference that at some former epoch in geological history the mean temperature of the northern hemisphere was much lower than it is now. Assuming this as the basis of discussion, the question is, what was the cause of this "glacial epoch"? To speak more accurately, since we can only take the causes relatively, Why was the northern hemisphere any colder then than it is now? This question Mr. Croll endeavours to answer from purely astronomical causes, combined with elementary considerations respecting the motion of heat and its relation to meteorological phenomena. His conclusion is that a great eccentricity of the earth's orbit, combined with a position of the perihelion near the northern solstice, will cause a great annual fall of temperature in the northern hemisphere, which in such a case would have a short perihelion summer and a long aphelion winter.

To this my reply is, that too little is known of the laws of terrestrial radiation of heat through the atmosphere to justify the establishment of any theory of the glacial epoch, and that, taking the case up exactly as Mr. Croll does, he fails to show sound reason why the mean temperature should be different at the supposed periods. At the same time my verdict would be, not that Mr. Croll's thesis was false, but that it was not proven. I do not deny the possibility that, when the laws of climate become thoroughly known, it may be found that epochs of great eccentricity are always glacial epochs. All I claim is that if such should be proven to be the case, it will be through the action of causes different from those adduced by Mr. Croll.

In fact, without going any further, we have at hand a *vera causa* acting in this direction which has not been considered

\* Communicated by the Author.

by Mr. Croll at all. Experiments on radiation, commenced by Dulong and Petit, tend to show that Newton's theory of the proportionality between temperature and radiation is not well founded, and that, as temperature rises, radiation increases in a much higher ratio. To speak more exactly, if we take a series of temperatures in arithmetical progression, the corresponding rates of radiation of heat will not be in arithmetical progression, but in a series of which the differences continually increase. An immediate inference from this general law is that, if an isolated body receive a given amount of radiant energy per annum, its mean annual temperature will be a maximum when this radiation is uniform, and will be lower the more irregular the reception of heat.

Now it is well known that the total amount of heat received, not only by the earth as a whole, but by each hemisphere, is constant, notwithstanding the change in the earth's eccentricity; but in virtue of the law just stated, any portion of the earth's surface on which a large portion of the annual supply of heat is delivered during a short summer will have a lower mean temperature than the hemisphere on which the heat is distributed more uniformly. But Mr. Croll does not, so far as I have ever noticed, adduce this law at all. On the contrary, he assumes Newton's law of radiation proportional to temperature, under which the cause would not act in the way suggested.

One great source of inconclusiveness in Mr. Croll's results seems to me to be a lack of quantitative precision in his language. Though he may use numbers wherever it seems to him they are applicable, one can hardly fail to notice that the quantitative terms he most uses are such as "great," "very great," "small," "comparatively small," and these without any statement of the units of comparison relatively to which the expressions are used. Now I deem it not improbable that the difference between a cold and a hot epoch may be due to the very small preponderance of one or the other of several antagonistic causes; and, if so, quantitative precision is necessary to lead to any reliable conclusion.

I shall now enter into some details. Mr. Croll suggests that I may have forgotten the researches of Pouillet and Herschel into the temperature of space. I reply that I regard the conclusion that the temperature of space is  $-239^{\circ}$  as having no sound basis. To speak with greater quantitative exactness, it has precisely the same value as a photometric estimate of the intensity of starlight, founded on observations of the sky made in full day; with an attempt to eliminate the light reflected by the sky so as to find what residue comes from the stars. The fact is, that no observations of radiant heat from

stellar spaces at large can be made below the uppermost limits of the earth's atmosphere, owing to the intervention in lower regions of the radiation from the atmosphere itself. Mr. Croll concludes, using Newton's law of radiation, that the heat received from the stars is to that received from the sun as 222 to 299. I wonder that he did not see in this a *reductio ad absurdum* either of the results of Pouillet and Herschel, or of the law of radiation which he assumes. Photometry shows that the combined light from all the stars visible in the most powerful telescope is not a millionth of that received from the sun, and there is no reason for believing that the ratio of light to heat is incomparably different in the two cases.

In considering the question of the heat conveyed by aerial currents, Mr. Croll quotes from my former paper so fully and fairly that I do not see any necessity to repeat my views at length. I can only say that while I now see more plainly than before some reason why a body at the upper region of the earth's atmosphere should, on the average, be colder than at the surface, I do not see that we have data for fixing the fall of temperature at  $5^{\circ}$  or  $100^{\circ}$ . If the degree of cold is greater than that due to expansion, then Mr. Croll is right in maintaining that the aerial current would not carry to the poles *all* the heat with which it left the equator. But, even granting this condition, I see no ground for supposing the quantity of heat conveyed insignificant.

I shall now consider some of Mr. Croll's reasons why the ocean should be warmer than the land. His assumed law that a body transparent for heat-rays would become warmer under solar radiation than an opaque body, I passed over in my former criticism as too much opposed to the fundamental laws of thermodynamics to need much consideration. He now adduces, in support of his thesis, the fact that water is more transparent to the solar rays than the rays which it would itself radiate; and that the upper layers of water would act like the glass of a greenhouse, and thus allow the water to stand at a higher temperature than it would otherwise do. This addition to the *modus operandi* seems to me quite sound, and therefore to show one true cause why water might rise to a higher mean temperature than the land, though I am unable to say whether the increase would be measurable with an ordinary thermometer. But I am sorry to find that, notwithstanding his addition of a sound cause, he adheres to views so diametrically opposite to what I supposed to be the fundamental laws of thermodynamics, that I feel compelled to state the case more fully. His first reason why the ocean should be warmer than the land is in the following words:—

“First.—The ground stores up heat only by the slow process of conduction, whereas water, by the mobility of its particles and its transparency for heat-rays, especially those from the sun, becomes heated to a considerable depth rapidly. The quantity of heat stored up in the ground is thus comparatively small, while the quantity stored up in the ocean is great.”

As just remarked, Mr. Croll substitutes a sound reason for this utterly bad one, but still seems inclined to hold on to the latter. The confusion of ideas which pervades it can best be shown by making some attempt to put the statements into quantitative language, using numbers, lengths, &c., instead of the qualifying words “slow,” “considerable,” “rapidly,” “comparatively small,” and “great.” His statement would then read something in this shape:—The ground stores up heat only by the process of conduction which admits of only 10 calories per square metre being absorbed in a day, whereas water, by the mobility of its particles &c., becomes heated to a depth of thirty feet at the rate of  $1^{\circ}$  Fahr. per hour (day or week as the case might be). Thus only 1000 units of heat are stored up in a cubic metre of earth, while 5000 units per cubic meter are stored up in the ocean.

When stated in this form, the question how hot the ocean would get at the end of  $x$  days, weeks, or years under the supposed law of heating, and how the number of units of heat stored up respectively in the ground and the ocean would fix their respective temperatures, would at once have arisen in Mr. Croll's own mind and showed him the utter failure of his reasoning; but by using instead of numbers the qualifying phrases I have quoted he confuses integral quantity of heat, rate at which heat is radiated in a unit of time, heat stored up, and temperature, without destroying the apparent soundness of his argument in the mind of the uncritical reader.

The second reason is in the following words:—

“Second.—The air is probably heated more rapidly by contact with the ground than with the ocean; but, on the other hand, it is heated far more rapidly by radiation from the ocean than from the land. The aqueous vapour of the air is to a great extent diathermanous to radiation from the ground, while it absorbs the rays from water and thus becomes heated.”

Here, again, the fallacy of the reasoning will be seen by giving the respective number of degrees, or any quantitative statement of the rate at which the air was heated by radiation from the ocean and from the land respectively. The fact I suppose to be that there is no rapidity of heating in question, but that the question is simply one of stationary temperature

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to be ultimately reached. I must repeat that I know not the slightest authority for the statement in the last sentence quoted, and can gain no clear idea from what Mr. Croll says on the subject.

In considering the third reason, which I need not quote, but which is found in Mr. Croll's reply, I suggested in my former paper what I supposed to be a *reductio ad absurdum* of Mr. Croll's method of reasoning, by pointing out the apparent conclusion that two bodies could heat each other up by their mutual radiation. I supposed he would disclaim this conclusion and try to show that I had misunderstood his premises in drawing it, but he apparently accepts its possibility as a logical result of Prevost's well-known theory of exchanges.

The fourth reason may be summarily disposed of in the same way as the preceding ones. Let the reader take it up as presented by the author; let him substitute quantitative statements at pleasure for the words "more freely," "greater," "greater difficulty," "more rapidly," "of higher mean temperature," &c., and let him also bear in mind that it is stationary temperatures and not quantities of heat with which we are ultimately concerned, and the inconclusive character of the reasoning will be at once apparent.

I shall next pass to the question of the non-melting of snow during a short perihelion summer, in which, as I stated in my former review, calculating temperatures by Mr. Croll's formula, we should have a mean temperature ranging from 100° to 150° Fahr. I had to acknowledge some embarrassment from Mr. Croll's causes producing their effects through the two diametrically opposite modes of operation, to wit:—

1st. By making the air exceedingly transparent, and thus permitting radiation into space.

2ndly. By filling the air with fogs, and thus preventing the solar heat from reaching the ground.

His reply to this is that he did not suppose the fogs and the clear atmosphere to exist at the same place and at the same time, but that in either case an inability on the part of the sun's rays to melt the few inches of snow which could have fallen during winter would have resulted.

I see no use in arguing this point, for the simple reason that I do not know enough about the relations of temperature to the aqueous vapour in the atmosphere to admit of my saying any thing of value on the subject. I would merely remark that I cannot see in Mr. Croll's reasoning the slightest ground for admitting that the perihelion summer radiation would produce any other effect than it does now.

I am surprised that Mr. Croll should have been willing to

present reasoning so obviously inconclusive as that in which he endeavours to show that my objection to the reliableness of his dates for glacial epochs, on account of the insufficiency of the fundamental data for the secular variations of the planetary orbits, falls to the ground. My objection and his statement in reply I can leave to the judgment of the reader who chooses to refer to them.

I conceive that some general remarks on the nature of the problem will be of more value than a further analysis of Mr. Croll's reasoning. It is an observed fact that we now have a glacial epoch at a comparatively moderate height in the atmosphere, and on the tops of most high ranges of mountains far removed from the equator. It is evident that if, at any former epoch, the state of things at the surface of the ground was the same that it now is at the height of two or three miles in the atmosphere, there must have been a glacial epoch. To what causes are we to attribute the cold of the upper regions of the air? There are two known causes, but we cannot assign an exact quantitative effect to each.

I. The passage of air from the lower to the upper regions is accompanied by expansion, and the reverse motion by compression, which would naturally result in the upper regions being colder than the lower: the exact amount of cooling, supposing no disturbing cause to come into play, is readily computed, and has, I think, been assigned by Professor Sir William Thomson and others; but I need not now refer to the results.

II. Researches on radiant heat seem to show that the atmosphere absorbs the extreme rays of the spectrum, especially those of greatest wave-length, more powerfully than the rays of mean wave-length. The rays radiated by the earth are of longer wave-length than the great mass of those received by the sun. The natural result of this selective absorption would be to make the temperature of the earth higher than if there were no atmosphere, or if the atmosphere exercised no selective absorption on heat-rays. It seems probable that this selective absorption is due very largely, if not entirely, to aqueous vapour in the air. If this be so, an epoch of dry air would be a glacial one.

A crude test of the efficacy of the first cause might be devised. In order that it may act, it is essential that there shall be a continuous interchange of air between low and high altitudes. Now if there are any high table-lands so extended that, in their central portions, the air has not during several days an opportunity to be replenished from lower regions, such air should be warmer than that at an equal height on isolated

mountains. Probably the conditions for such an observation do not exist on the earth's surface.

In conclusion, I may be allowed to express my regret at not being able to make a contribution of positive value to the investigation of this subject. The state of the question is about this:—A well-founded theory of terrestrial temperature can be built only upon an accurate knowledge of the laws of emission and absorption of radiant energy of different wavelengths, especially in the atmosphere, and the result will appear as a numerical calculation, more or less exact, of the temperature resulting from assigned conditions, and not as the conclusion of an argument to show one thing or another.

### XIX. *Note on the White Rainbow.*

By JOHN TYNDALL, F.R.S.\*

THE highest portion of Hind Head Moorland, which is close to the spot where I now write, is about 900 feet above the sea. Over this high plateau, at the present season of the year, mist and fog frequently settle. This has given me of late frequent opportunities of observing, on dark nights, the white circular bows described in my last brief communication to the Philosophical Magazine. All fogs are not able to produce these circles. What is usually known as "Scotch mist" appears eminently suitable for this purpose.

Last Christmas Day was heralded by mist and fog of a very dense character. Both, however, became thinner as the day advanced; and finally a blue sky overarched the moorland. Accompanied by my wife I walked out upon the common. When facing the sun we noticed the air alive with small glistening specks. They were an obvious residuum of the fog, produced probably by the coalescence of its smaller particles to minute drops. My previous experience led me to infer that these specks or globules must be able to produce some kind of rainbow. The inference was immediately verified. Turning my back to the sun, and looking across the Devil's Punch Bowl, a well-defined white bow was seen spanning that remarkable basin. The sky above us was blue at the time, the fog had disappeared, and nothing which could be properly called rain existed in the atmosphere.

Circular bows are sometimes seen on looking from the edge of a precipice into an abyss filled with fog. But the precipice, though useful, is not necessary, as the following observation shows. Standing upon the common, with a perfectly level area of dark heather in front of us, and watching with the

\* Communicated by the Author.