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ART. XXIX.—*On some controverted points in Geological Climatology; a reply to Professor Newcomb, Mr. Hill and others;* by JAMES CROLL, LL.D., F.R.S.

NINETEEN years ago the theory was advanced that the Glacial epoch was the result of a combination of physical agents brought into operation by an increase in the eccentricity of the earth's orbit. Few or no objections have been urged against what may be called the astronomical part of the theory. But the portions relating to these physical agencies, which is by far the most important part, have from time to time met with considerable opposition. Considering the newness of the subject, and the complex nature of many of these combinations of physical agencies, it would not be surprising if some of the original deductions in regard to them proved erroneous. But after long and careful re-consideration of the whole matter I have not found reason to abandon any of them or alter them to any material extent.

The only class of objections urged against the theory which I have as yet considered at length are those relating to the cause of ocean currents, and their influence on the distribution of heat over the globe; and I think, it will be admitted that the views which I have advocated on these points are now being pretty generally accepted.

But it is in reference to the influence of aqueous vapor, fogs and clouds on the production and preservation of snow that the greatest diversity of opinion has prevailed. The object of the present article is to examine at some length the principal

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objections which have been advanced in regard to this part of the inquiry. I shall also take the present opportunity of discussing more fully some points on which I have been sometimes misunderstood, and which appear to have been treated rather too briefly on former occasions.

In the *American Journal of Science* for April, 1876, Professor Newcomb has done me the honor to review at some length my work, 'Climate and Time.' And as his article is mainly devoted to a criticism of my reasoning in regard to those very points to which I refer, I shall begin with an examination of his objections. One reason for entering at some length into an examination of Professor Newcomb's objections is the fact that they embrace to a large extent those which have been urged by reviewers in Great Britain. Some of his objections, however, as will be seen, are based upon a misapprehension of my reasoning.

Temperature of Space.—One of the most important factors in the theory of geological climate resulting from changes in the eccentricity of the earth's orbit is obviously the temperature of stellar space. Unless we have, at least, some rough idea of the proportion which the heat derived from the stars bears to that derived from the sun, we can not form any estimate of how much the temperature of our earth would be lowered or raised by a given decrease or increase of the sun's distance.

The question of the temperature of space has been investigated in different ways by Pouillet and Herschel; and the result arrived at was that space has a temperature of -239° F., or an absolute temperature of 222° . The mean absolute temperature of our earth is about 521° . Consequently, according to these results, the heat received from the stars is to that received from the sun as 222 to 299. All my determinations of the change of temperature due to changes in the sun's distance were computed on these data, although I believe, for reasons stated, that space must have a much lower temperature. Recent observations of Professor Langley made during the Mount Whitney Expedition confirm the correctness of my belief.

Professor Newcomb, however, wholly ignores all that has been done on that subject, for he commences his review by the statement that "practically there is but one source from which the surface of the earth receives heat—the sun, since the quantity received from all other sources is quite insignificant in comparison."

Surely Professor Newcomb must have forgotten all about the researches of Pouillet and Herschel into what has been

termed the 'Temperature of Space,' or he could not have affirmed so positively that "practically there is but one source from which the earth receives heat, and that all other sources are quite insignificant" without, at least, giving some reason for the assertion.

I am pleased to find that he agrees, in the main, with what has been advanced in 'Climate and Time,' in reference to the heating power of ocean currents, and also as to their existence being due to the impulse of the winds. But he differs widely from me in regard to the heat conveyed by aerial currents.

On the Heat conveyed by Aerial Currents.—I stated that the quantity of heat conveyed from equatorial to high temperate and polar regions is trifling in comparison with that conveyed by ocean currents. For the heated air rising off the hot ground of the equator, after ascending a few miles becomes exposed to the intense cold of the upper regions, and having to travel polewards for thousands of miles in those regions, it loses nearly all the heat which it brought from the equator before it can possibly reach high latitudes. To this Professor Newcomb objects as follows: "He (Mr. Croll), speaks of the hot air rising from the earth and becoming exposed to the intense cold of the upper regions of the atmosphere. But what can this cold be *but the coldness of the very air itself which has been rising up?* If the warm air rises up into the cold air, and becomes *cooled by contact* with the latter, the latter must become warm by the very heat which the former loses, and if there is a continuous rising current the whole region must take the natural temperature of the rising air. This temperature is, indeed, much below that which maintains at the surface *for the simple reason that air becomes cold by expansion* according to a definite and well-known law. Having thus got his rising current constantly cooled off *by contact with the cold air* of the upper regions, it has to pass on its journey towards the poles," etc., p. 267.*

Here the cooling of the ascending air is attributed to two causes (1) the heat lost by expansion as the air rises; (2) the heat lost by contact with the colder air through which the ascending air passes and with which it mixes in the upper regions. But the two may be resolved into one, viz: the heat lost by expansion; for the cold air to which the ascending air communicates its heat by contact, is assumed to have originally derived its cold, in like manner from expansion. This is evident, for although he recognizes the effect of radiation into space, he assumes that this loss is compensated by counter radiation. The upper regions are, he says, exposed to the

* The italics are mine.

radiation of the sun on the one side, and of the earth's lower atmosphere on the other, and there is no proof that these do not equal the surface temperature. And again, when the air descends in high latitudes to the earth's surface an amount of heat will be evolved by compression equal to that which it lost when it rose from the equator.

Professor Newcomb has misapprehended not only my meaning, but also the chief reason why the air in the upper regions is so intensely cold. Any one who has read what I have stated in pp. 35-40, 'Climate and Time,' regarding the temperature of space will readily understand what I mean by the temperature of the upper regions. By the temperature of stellar space, it is not meant that space itself is a something possessed of a given temperature, say -239°F . It simply means the temperature to which a body would fall were it exposed to no other source of heat than that of radiation from the stars. By the temperature of the upper regions, I mean the temperature to which air in those regions sinks in consequence of loss from radiation into space. It is mainly to this cause, and not to the loss from expansion, as Professor Newcomb assumes, that the intense cold of the upper air is due. The air in that region has got beyond the screen which protected it when at the earth's surface, and it then throws off its heat into space during twelve hours of night, getting no return from without except from the radiation of the stars. And even at noon-day, as I have endeavored to show in Appendix, p. 551, the rays of a burning sun over head would not be sufficient to raise the temperature of the air up to the freezing point. But the recent observations of Professor Langley prove that the loss of heat from radiation is in reality far greater than I had anticipated. He says: "The original observations, which will be given at length, lead to the conclusion that in the absence of an atmosphere the earth's temperature of insolation would at any rate fall below -50°F ., by which it is meant that, for instance, mercury would remain a solid under the vertical rays of a tropical sun were radiation into space wholly unchecked, or even if, the atmosphere existing, it let radiations of all wave-lengths pass out as easily as they come in."—'*Nature*,' Aug. 3d, 1882.

The temperature of the upper atmosphere, even after making allowance for heat received from below, must in this case be at least nearly 80 degrees below the freezing point. The quantity of heat lost by expansion must, therefore, be trifling compared with that lost by radiation; and although the heat lost by expansion is fully restored by compression, yet the air would reach the earth deprived almost entirely of the heat with

which it left the equator. All that it could possibly give back would simply be the heat of compression, and this would hardly be sufficient to raise air at -50° F. to the freezing point. How then can the polar regions be greatly the better of air from the equatorial regions? Professor Newcomb says: "If the upper current be as great as is commonly supposed, it must be as powerful as ocean currents in tending to equalize the temperature of the globe." How can this be?

Why the mean temperature of the Ocean should be greater than that of the Land.—"Another proposition," he says, "which the author attempts to prove, reasoning which seems equally inconclusive, is that the mean temperature of the ocean is greater than that of the land over the entire globe." I certainly never attempted to prove that the mean temperature of the ocean is *greater* than that of the land over the entire globe. The very chapter to which he here refers, and which he is about to criticise, was written to explain why the mean temperature of the southern or water hemisphere is less than that of the northern or land hemisphere. What I attempted to prove was, not that the mean temperature of the ocean is greater than that of the land, but that were it not for certain causes the mean temperature of the ocean *ought* to be greater than that of the land in equatorial regions as well as in temperate and arctic regions. In other words, the object of the chapter is to prove that the mean temperature of the southern or water hemisphere is less than that of the northern or land hemisphere, not, as is generally supposed, because the former is mainly water and the latter land, but because of the enormous amount of heat transferred from the former to the latter hemisphere by means of ocean currents; and that were it not for this transference the temperature of the water would exceed that of the land hemisphere. And it is in order to prove this that the "four *à priori* reasons" which Professor Newcomb criticises were adduced. The first of these is as follows—

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."*

These sentences are considered unworthy of criticism. Are they really so unworthy? Let us examine them a little more closely. It is in consequence of the sun's rays being able to

* 'Climate and Time,' p. 90.

penetrate to a great depth that the amount of heat stored up by the ocean is so great, and it is to this store that its warmth during winter is mainly due. The water is diathermanous for the rays of the sun, but it is not so, for reasons well known, for the rays of water itself. The upper layers of the ocean will allow a larger portion of the radiation from the sun to pass freely downward, but they will not allow radiation from the layers underneath to pass freely upwards. These upper layers, like the glass of a green-house, act as a trap to the sun's rays, and thus allow the water of the ocean to stand at a higher temperature than it would otherwise do. Again, the slowness with which the ocean thus parts with its heat enables it to maintain that comparatively high temperature during the long winter months. And again, it is to the mobility of the particles of water, the depth to which the heat penetrates, and the rapidity with which it is absorbed, that those great currents of warm water become possible. Were the waters of the ocean like the land not mobile, and were only a few inches at the surface reached by heat from the sun, there could be no Gulf Stream, or any great transference of heat from the Southern to the Northern hemisphere, or from equatorial to temperate and polar regions, by means of oceanic circulation.

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 vapor 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.’

To this Professor Newcomb objects as follows: “If then the air is really heated by contact with the ground more rapidly than by contact with the ocean, it can only be because the ground is hotter than the ocean, which is directly contrary to the theory Mr. Croll is maintaining.” What I maintained was that were it not for certain causes the *mean annual* temperature of the ocean would be higher than that of the land. During the day and also during the summer the surface of the ground is hotter than that of the ocean; and the air, of course, will be heated more rapidly by *contact* with the former than with the latter. But this does not prove that the air is not more rapidly heated by *radiation* from the ocean than from the land. Professor Newcomb says: “The statement that the aqueous vapor of the air is diathermanous to radiation from land, but not to that from water, is quite new to us, and very surprising.” I am surprised that he is not acquainted with the fact, and also with its physical explanation. This will

help to account for his inability to perceive how radiation from the ocean may heat the air more rapidly than radiation from the land, even though the surface of the latter may be at a higher temperature than that of the former.

He says: "The rapidity with which the heating process goes on depends on the difference of temperature, no matter whether the heat passes by conduction or by radiation." This statement will hardly harmonize with recent researches into radiant heat. It is found that the rapidity with which a body is heated by radiation depends upon the absorbing power of the body; and the absorbing power again depends upon the quality of the heat rays. Professor Tyndall, for example, found that in the case of vapors, as a rule, absorption *diminishes* as the temperature rises. With a platinum spiral heated till it was barely visible, the absorption of the vapor of bisulphide of carbon was 6.5, but when the spiral was raised to a white heat, the absorption was reduced to 2.9. A similar result took place in the case of chloroform, formic ether, acetic ether, and other vapors. The physical cause of this is well known.

If the aqueous vapor of the air, he says, be more diathermanous to radiation from land than from water, as I have stated, then I assigned directly contrary effects to the same cause. For, "reasoning as in (1) he, Mr. Croll, would have said that the air over the land, owing to its transparency for the heat rays from the land, becomes heated to a greater height rapidly, while the air over the ocean not being transparent, can acquire heat from the ocean only by the slow process of convection." I would have said no such thing. Radiation from the surface of the land will, no doubt, penetrate more freely through the aqueous vapor than radiation from the ocean, but the aqueous vapor will not absorb the radiation of the land so rapidly as that of the ocean, for the ocean gives off that quality of rays which aqueous vapor absorbs most rapidly.

This is not in opposition to what I have stated in reason (1), for if the ground were transparent to the sun's rays like water, evidently the total quantity of heat absorbed by it would be greater than that by the ocean. But radiation from the sun heats only the surface of the ground, all below the surface depends for its supply on the slow process of conduction, whereas the ocean is heated by direct radiation to great depths. Consequently the total quantity of heat absorbed by the ocean, say per square mile, in a given time, is greater than that by the land.

Third.—‘The air radiates back a considerable portion of its heat, and the ocean absorbs this radiation from the air more readily than the ground does. The ocean will not reflect the heat from the aqueous vapor of the air, but absorbs it, while the ground does the opposite. Radiation from the air, therefore, tends more readily to heat the ocean than it does the land.’

“Here we have,” he says, “the air giving back to the ocean the same heat which it absorbs from it, and thus heating it.” If Professor Newcomb means by this same heat the same amount of heat, then I believe in no such thing. But if his meaning be that here we have the air giving back to the ocean a quantity of the heat which it absorbed from it, then he is certainly correct in supposing that this is affirmed by me. But this is a conclusion which no physicist could for a moment doubt. To deny this would be to contradict Prevost’s well-known theory of exchanges. Did the air throw back to the ocean none of the heat which it derives from it the entire waters of the ocean would soon become solid ice. In fact, as we have seen, mercury would not remain fluid and every living thing on the face of the globe would perish.

He states that reason fourth seems to be little more than a repetition of reason second in a different form. It is, however, much more than that. It is a demonstration that were it not for the causes to which I have alluded the mean temperature of the water hemisphere ought to be higher than that of the land hemisphere, and for this reason I shall here give the section in full.

Fourth.—‘The aqueous vapor of the air acts as a screen to prevent the loss by radiation from water, while it allows radiation from the ground to pass more freely into space; the atmosphere over the ocean consequently throws back a greater amount of heat than is thrown back by the atmosphere over the land. The sea in this case has a much greater difficulty than the land has in getting quit of the heat received from the sun; in other words, the land tends to lose its heat more rapidly than the sea. The consequence of all these circumstances is that the ocean must stand at a higher mean temperature than the land. A state of equilibrium is never gained until the rate at which a body is receiving heat is equal to the rate at which it is losing it; but as equal surfaces of sea and land receive from the sun the same amount of heat, it therefore follows that in order that the sea may get quit of its heat as rapidly as the land, it *must stand at a higher temperature* than the land. The temperature of the sea must continue to rise till the amount of heat thrown off into space equals that received from the sun; when this point is reached equilibrium is established and the temperature remains stationary. But, owing to the greater difficulty that the sea has

in getting rid of its heat, the mean temperature of equilibrium of the ocean must be higher than that of the land; consequently the mean temperature of the ocean, and also of the air immediately over it, in tropical regions should be higher than the mean temperature of the land and the air over it.'

Since the publication of 'Climate and Time,' the accuracy of this conclusion has been confirmed in a remarkable manner from recent researches on the actual mean temperature of the two hemispheres, the details of which have been given by Mr. Ferrel in his 'Meteorological Researches,' Washington, 1877. It is found that the mean temperature of the northern or land hemisphere is higher than that of the southern or water hemisphere up only to about latitude 35°, and that beyond this latitude the mean temperature of the water hemisphere is the greater of the two. At latitude 40° the mean temperature of the southern hemisphere is 1°·4 higher than that of the same parallel on the northern hemisphere. At latitude 50° the difference amounts to 4°·4, while at latitude 60° the mean temperature of the southern hemisphere is actually 6° higher than that of the northern on the same parallel. The mean temperatures of the two hemispheres are as follows:

Lat.,	0°	10°	20°	30°	40°	50°	60°	70°	80°
Northern,	80°·1	81°·0	77°·6	67°·6	56°·5	43°·4	29°·3	14°·4	4°·5
Southern,	80°·1	78°·7	74°·7	66°·7	57°·9	47°·8	35°·3	----	----

From the above table we see that it is only in that area lying between the equator and latitude 35° that the southern hemisphere has a lower mean temperature than the northern. But it is from this area that the enormous amount of heat transferred to the northern hemisphere is mainly derived. Were the transference of heat to cease the temperature of this area would be very considerably raised, and that of the corresponding area on the northern hemisphere lowered. The result would doubtless be that the southern hemisphere, down to the equator, would then be warmer than the northern. But even as things are, as Mr. Ferrel remarks, "the mean temperature of the southern hemisphere is the greater of the two," the mean temperature of the southern being 60°·89, and that of the northern 59°·54.

Heat cut off by the Atmosphere.—Professor Newcomb says further: "Another idea of the author which calls for explanation is that solar heat absorbed by the atmosphere is entirely lost, so far as warming any region of the globe is concerned." This is no idea of mine. My idea is not that the heat cut off is entirely lost but merely that the *greater part* is lost. A large portion of the heat is reflected, and of that absorbed one-

half, perhaps, is radiated back into space and lost, in so far as the earth is concerned.

Tables of Eccentricity.—Referring to my tables of eccentricity of the earth's orbit he says: "That there are from time to time such periods of great eccentricity is a well-established result of the mutual gravitation of the planets, but whether the particular epochs of great and small eccentricity computed by Mr. Croll are reliable, is a different question." I may here mention that Professor McFarland, of the Ohio State University, Columbus, a few years ago, undertook the task of re-computing every one of the 150 periods given in my tables, and he states that, except in one instance, he did not find an error to the amount of .001.*

"The data for this computation," continues Professor Newcomb, "are the formulæ of Le Verrier, worked out about 1845,† without any correction either for the later corrections to the masses of the planets or for the terms of the third order, subsequently discussed by Le Verrier himself. The probable magnitude of these corrections is such that reliance cannot be placed upon the values of eccentricity computed without reference to them for epochs distant by merely a million of years."

In regard to this objection I may mention that the whole subject of the secular variations of the elements of the planetary orbits has been re-investigated by Mr. Stockwell, taking into account the disturbing influence of the planet Neptune, the existence of which was not known at the time Le Verrier's investigations were made. Professor McFarland, with the aid of Mr. Stockwell's formulæ, has computed all the periods in the tables referred to above, and on comparing the results found by both formulæ, he states that "the two curves exhibit a general conformity throughout their whole extent." And his computations, I may state, extend from 3,260,000 years before 1850 to 1,260,000 years after that date; or, in other words, over a period of no fewer than 4,520,000 years,‡ thus showing that Professor Newcomb's objection falls to the ground.

Influence of Winter in Aphelion.—I have maintained that at a time when the eccentricity is high and the winter occurs in aphelion, the great increase in the sun's distance and in the length of the winter would have the effect of causing a large

* American Journal of Science, vol. xi, p. 456 (1876).

† Le Verrier's formulæ were worked out several years before 1845.

‡ In this laborious undertaking Professor McFarland computed, by means of both formulæ, the eccentricity of the earth's orbit and the longitude of the perihelion for no fewer than 485 separate epochs. See American Journal of Science, vol. xx, p. 105 (1880).

increase in the quantity of snow falling during that season. This very obvious result follows as a necessary consequence from the fact that the moisture which now falls in the form of rain would then fall as snow. But Professor Newcomb actually states that he cannot accept the conclusion that this would lead to more snow.

Influence of a Snow-covered Surface.—I have argued that this accumulation of snow would lower the summer temperature, and tend to prevent the disappearance of the snow, and have assigned three reasons for this conclusion:—

First.—Direct radiation. The snow, for physical reasons well known, will cool the air more rapidly than the sun's rays will heat it. This is shown from the fact that in Greenland, a snow and ice-covered country, a thermometer exposed to the direct radiation of the sun has been observed to stand above 100° , while the air surrounding the instrument was actually 12° below the freezing point. Professor Newcomb and also Mr. Hill * regard the idea that this could in any way favor the accumulation of snow as absurd. They think that in fact it would have directly the opposite effect. They have perceived only one half of the result. It is quite true, as they affirm, that the cooling of the air by the snow will not prevent the melting of the snow, but the reverse. There is, however, another and far more important result overlooked in their objection. If the snow and ice-covered surface keeps the temperature of the air, in summer, below the freezing point, which it evidently does in Greenland and in the Antarctic continent, the moisture of the air will fall as snow and not as rain. No doubt this is the chief reason why in those regions, even in the middle of summer, rain seldom falls, the precipitation being almost always in the form of snow, although at that very season the direct heat of the sun is often as great as in India. Were the snow and icy mantle removed, a snow shower in summer would be as rare a phenomenon in those regions as it would be in the south of England.

Second.—‘The rays which fall on snow and ice are to a great extent reflected back into space. But those that are not reflected, but absorbed, do not raise the temperature, for they disappear in the mechanical work of melting the ice.’

This reason is also regarded as absurd. The heat of the sun during the perihelion summer would, he says, suffice to melt the whole accumulation of winter snow in three or four days. “The reader,” he continues, “can easily make a computation of the incredible reflecting power of the snow and of the unex-

*Geological Magazine for January, 1880, p. 12.

amplified transparency of the air required to keep the snow unmelted for three or four months." Incredible as it may appear to Professor Newcomb, I shall shortly show that a less amount of snow than the equivalent of the two feet of ice which he assumes does actually, in some places, defy the melting power of a tropical sun. But he misapprehends my reasoning here also, by overlooking the more important factor in the affair, namely, the keeping of the air in the summer below the freezing point. The direct effect that this has in preventing the sun from melting the snow and ice will be discussed shortly, but the point to which I wish at present to direct special attention is the fact that if the air is kept below, or even at the freezing point, snow will fall and not rain. Snow is a good reflector of heat, consequently a large portion of the sun's rays falling on the snow and icy surface is reflected back to space. The aqueous vapor of the air, on the other hand, as the vibrations of its molecules agree in *period* with those of the snow and ice, cuts off a large portion of the heat *radiated* by the snow surface; but here in the case of reflection under consideration the rays are not cut off; for the *reflected* rays are of the same character as the incident rays which pass so freely through the aqueous vapor. And in respect to the remaining rays which are not reflected but absorbed by the snow, they do not manage to raise the temperature of the snow above the freezing point. Consequently the air is kept in the condition most favorable for the production of snow.

Third.—'Snow and ice lower the temperature by chilling the air and condensing the vapor into thick fogs. The great strength of the sun's rays during summer, due to his nearness at that season, would, in the first place, tend to produce an increased amount of evaporation. But the presence of snow-clad mountains and an icy sea would chill the atmosphere and condense the vapor into thick fogs. The thick fogs and cloudy sky would effectually prevent the sun's rays from reaching the earth, and the snow in consequence, would remain unmelted during the entire summer.'

On this Professor Newcomb's criticism is as follows: "Here he, Mr. Croll, says nothing about the latent heat set free by the condensation, nor does he say where the heat goes to which the air must lose in order to be chilled. The task of arguing with a disputant who in one breath maintains that the transparency of the air is such that the rays reflected from the snow pass freely into space, and in the next breath that thick fogs effectually prevent the rays ever reaching the snow at all, is not free from embarrassment."

If he really supposes my meaning to be that the air is so transparent as to allow the incident and reflected rays of the

sun to pass freely without interruption, while at the same time and in the same place the air is not transparent but filled with dense fogs which effectually cut off the sun's rays and prevent them from reaching the earth, then I do not wonder that he should feel embarrassed in arguing with me. But if he supposes my meaning to be, as it of course is, that those two opposite conditions, existing at totally different times, or in totally different places at the same time, should lead to similar results, namely, the cooling of the air and consequent conservation of snow, then there is no ground whatever for any embarrassment about the matter.

"We might therefore show," he states, "that if the snow, air, fog, or whatever throws back the rays of the sun into space is so excellent a reflector of heat, it is a correspondingly poor radiator, and the same fog which will not be dissipated by the summer heat will not be affected by the winter's cold, and will therefore serve as a screen to prevent the radiation of heat from the earth during the winter."

There are few points in connection with terrestrial physics which appear to be so much misunderstood as that of the influence of fogs on climate. One chief cause of these misapprehensions is the somewhat complex nature of the subject arising from the fact that aqueous vapor acts so very differently under different conditions. When the vapor exists in the air as an invisible gas, we have often an intensely clear and transparent sky, allowing the sun's rays to pass to the ground with little or no interruption; and if the surface of the ground be covered with snow, a large portion of the incident rays are reflected back into space without heating either the snow or the air. The general effect of this loss of heat is, of course, to lower the general temperature. But when this vapor condenses into thick fogs it acts in a totally different manner. The transparency to a great extent disappears, and the fog then cuts off the sun's rays and prevents them from reaching the ground. This it does in two different ways. 1st. Its watery particles, like the crystals of the snow, are good reflectors, and the upper surface of the mass of fog on which the rays fall acts as a reflector, throwing back a large portion of the rays into stellar space. The rest of the rays which are not reflected enter the fog and the larger portion of them are absorbed by it. But it will be observed that by far the greater part of the absorption, if not nearly all of it, will take place in the upper half of the mass. This is a necessary result of a recognized principle in radiant heat known as the "sifting" of the rays. The deeper the rays penetrate into the fog the less will be the amount of heat absorbed. If the depth of the mass be great,

absorption will probably entirely disappear before the surface of the ground is reached. The fog will begin, of course, to radiate off the heat thus absorbed, but as it is the upper half of the mass which has received the principal part of the heat, the most of this heat will be radiated upward into stellar space, and like the reflected heat entirely lost in so far as heating the earth is concerned. A portion will also be radiated downward, some of which may reach the ground, but the greater portion will be re-absorbed in its passage through the mass. We have no means of estimating the amount of heat which would thus be thrown off into space by reflection and radiation ; but it is certainly great. I think we may safely conclude that in places like South Georgia and Sandwich Land where fogs prevail to such an extent during summer, one half at least of the heat from the sun never reaches the ground. A deprivation of sun heat of a much less extent than this would certainly lower the summer temperature of these places far below the freezing point, were it not for a compensating cause to which I shall now refer, viz : the heat "trapped" by the fog. The fog, although it prevents a large portion of the sun's heat from ever reaching a place, at the same time prevents to a great extent that place from losing the little heat which it does receive. In other words, it acts as a screen preventing the loss of heat by radiation into space. But the heat thus "trapped" never fully compensates for that not received, and a lowering of temperature is always the result.

Had all these considerations been taken into account by Professor Newcomb, Mr. Hill, Mr. Searles Wood and others, they would have seen that I had by no means over-estimated the powerful influence of fogs in lowering the summer temperature.

The influence of fogs on the summer temperature is a fact so well established by observation that it seems strange that any one should be found arguing against it.

Heat evolved by Freezing.—There is one objection to which I may here refer and which has been urged by nearly all my critics. It is said, correctly enough, that as water in freezing evolves just as much heat as is required to melt it, there is on the whole no actual loss of heat ; that whatever heat may be absorbed in the mechanical work of melting the snow just as much was evolved in the formation of the snow. Consequently it is inferred, in so far as climate is concerned, the one effect completely counterbalances the other. This inference, sound as it may at first sight appear, has been so well proved to be incorrect by Mr. Wallace that I cannot do better than quote his words :

"In the act of freezing, no doubt water gives up some of its heat to the surrounding air, but *that air still remains below the freezing point* or freezing would not take place. The heat liberated by freezing is, therefore, what may be termed low-grade heat—heat incapable of melting snow or ice; while the heat absorbed while ice or snow is melting is high-grade heat, such as is capable of melting snow and supporting vegetable growth. Moreover, the low-grade heat liberated in the formation of snow is usually liberated high up in the atmosphere, where it may be carried off by winds to more southern latitudes, while the heat absorbed in melting the surface of snow and ice is absorbed close to the earth and is thus prevented from warming the lower atmosphere, which is in contact with vegetation. The two phenomena, therefore, by no means counterbalance or counteract each other, as it is so constantly and superficially asserted that they do."—*Island Life*, p. 140.

The Fundamental misconception.—I come now to a misapprehension which more than any other has tended to prevent a proper understanding of the causes which lead to the conservation by snow. Whatever the eccentricity of the earth's orbit may be the heat received from the sun during summer is more than sufficient to melt the snow of winter. Consequently, it is assumed, no permanent accumulation of snow can take place. This objection, as expressed by Mr. Hill, is as follows: "We have no reason to suppose that at present in the northern hemisphere more snow or ice is anywhere formed in winter than is melted in summer. With greater eccentricity less heat than now would be received in winter, but exactly as much more in summer. More snow would therefore be formed in the one half of the year, but exactly as much more be melted in the other half. The colder winter and the warmer summer would exactly neutralize each other's effects, and on the average of years no accumulation could begin. *Prima facie*, therefore, high eccentricity will not account for glacial periods."* In the language of Professor Newcomb it is as follows: "During this perihelion summer the amount of heat received from the sun by every part of the northern hemisphere would suffice to melt from four to six inches of ice per day over its entire surface; that is, it would suffice to melt the whole probable accumulation in three or four days. The reader can easily make a computation of the incredible reflecting power of the snow and of the unexampled transparency of the air required to keep the snow unmelted for three or four months."

* Geological Magazine. January, 1880, p. 12.

It is assumed, in this objection, that because the heat received from the sun by an area is more than sufficient to melt all the snow that falls on it, no permanent accumulation of snow and ice can take place. It is assumed that the quantity of snow and ice melted must be proportional to the heat received. Suppose that on a certain area a given amount of snow falls annually. The amount of heat received from the sun per annum is computed, and after the usual deduction for that cut off by the atmosphere has been made, if it be found that the quantity remaining is far more than sufficient to melt the snow, it is then assumed that the snow must be melted, and that no accumulation of snow and ice year by year in this area is possible. To one approaching this perplexing subject for the first time such an assumption looks very plausible, but a little reflection will show that it is most superficial. The assumption is at the very outset totally opposed to known facts. Take the lofty peaks of the Himalayas and Andes as an example. Few, I suppose, would admit that at these great elevations as much as fifty per cent of the sun's heat could be cut off. But if fifty per cent reaches the snow this would be sufficient to melt fifty feet of ice, and this no doubt is more than ten times the quantity which actually requires to be melted. Notwithstanding all this the snow is never melted but remains permanent. Take as another example South Georgia in the latitude of England. Suppose we assume that one-half of the sun's heat is cut off by the clouds and fogs which prevail to such an extent in that place, still the remaining half would be sufficient to melt upwards of thirty feet of ice, which is certainly more than the equivalent of all the snow which falls; yet this island is covered with snow and ice down almost to the seashore during the whole year. Take still another example, that of Greenland. The quantity of heat received between latitudes 60° and 80° , which is that of Greenland, is, according to Meech, one-half that received at the equator; and were none cut off, it would be sufficient to melt fifty feet of ice. The annual precipitation on Greenland in the form of snow and rain, according to Dr. Rink, amounts to only twelve inches, and two inches of this he considers is never melted but is carried away in the form of icebergs. Mr. Hill maintains* that owing to the great thickness of the air traversed by the sun's rays and the loss resulting from the great obliquity of reflection, the amount of heat reaching the ground would be insufficient to melt more than sixteen feet of ice. Supposing we admit this estimate to be correct still this is nineteen times more than is actually melted. The sun melts

* *Geological Magazine*, April, 1880.

only ten inches notwithstanding the fact that it has the power to melt sixteen feet.

In short, there is not a place on the face of the globe where the amount of heat received from the sun is not far more than sufficient to melt all the snow which falls upon it. If it were true, as the objection assumes, that the amount of snow melted is proportional to the amount of heat received by the snow then there could be no such thing as perpetual snow.

The reason why the amount of snow and ice melted is not necessarily proportional to the amount of heat received is not far to seek. Before snow or ice will melt its temperature must be raised to the melting point. No amount of heat, however great, will induce melting to begin unless the intensity of the heat be sufficient to raise the temperature to the melting point. Keep the temperature of the snow below that point and though the sun may shine upon it for countless ages it will still remain unmelted. It is easy to understand how the snow on the lofty summits of the Himalayas and the Andes never melts. According to the observations made at Mount Whitney, to which reference has already been made, the heat of even a vertical sun would not be sufficient at these altitudes to raise the temperature of the snow to near the melting point; and thus melting, under these conditions, is impossible. The snow will evaporate but it cannot melt. But owing to the frozen condition of the snow even evaporation will take place with extreme difficulty. If the sun could manage to soften the snow crystals and bring them into a semi-fluid condition, evaporation would, no doubt, go on rapidly; but this the rays of the sun are unable to do. Consequently, we have only the evaporation of a solid which, of course, is necessarily small.

It may here be observed that at low elevations, where the snow fall is probably greater, and the amount of heat received even less than at the summits, the snow melts and disappears. Here, again, the influence of that potent agent, aqueous vapor, comes into play. At high elevations the air is dry and allows the heat radiated from the snow to pass into space; but at low elevations a very considerable amount of the heat radiated from the snow is absorbed by the aqueous vapor which it encounters in passing through the atmosphere. A considerable portion of the heat thus absorbed by the vapor is radiated back on the snow; but the heat thus radiated being of the same quality as that which the snow itself radiates is on this account absorbed by the snow. Little or none of it is reflected like that received from the sun. The consequence is that the heat thus absorbed accumulates in the snow till melting takes place. Were the amount of aqueous vapor possessed by the

atmosphere sufficiently diminished, perpetual snow would cover our globe down to the sea shore. It is true that the air is warmer at the lower than at the higher levels, and by contact with the snow must tend to melt it more at the former than at the latter position. But we must remember that the air is warmer mainly in consequence of the influence of aqueous vapor, and that were the quantity of vapor reduced to the amount in question, the difference of temperature at the two positions would not be great.

But it may be urged as a further objection to the foregoing conclusion that, as a matter of fact, on great mountain chains the snow line reaches to a lower level on the side where the air is moist than on the opposite side where it is dry and arid: as, for example, on the southern side of the Himalayas, and on the eastern side of the Andes, where the snow line descends 2,000 or 3,000 feet below that of the opposite or dry side.

But this is owing to the fact that it is on the moist side that by far the greatest amount of snow is precipitated. The moist winds of the southwest monsoon deposit their snow almost wholly on the southern side of the Himalayas, and the southeast trades on the east side of the Andes. Were the conditions in every respect the same on both sides of these mountain ranges, with the exception only that the air on one side was perfectly dry, allowing radiation from the snow to pass without interruption into stellar space, while on the other side the air was moist and full of aqueous vapor absorbing the heat radiated from the snow, the snow line would in this case undoubtedly descend to a lower level on the dry than on the moist side. Melting would certainly take place at a greater elevation on the moist than on the dry side, and this is what would mainly determine the position of the snow line.

The annual precipitation on Greenland, as we have seen, is very small; scarcely one-half that of the driest parts of Great Britain. This region is covered with snow and ice, not because the quantity of snow falling on it is great, but because the quantity melted is small; and the reason why the snow does not melt is not that the amount of heat received during the year is unequal to the work of melting the ice, but that, mainly through the dryness of the air, the snow is prevented from rising to the melting point. The very cause which prevents a heavy snowfall protects the little which does fall from disappearing. The same remarks apply to the antarctic regions.

In South Georgia and Fuegia, where clouds and dense fogs prevail during nearly the whole year, the permanent snow and

ice are due to a different cause. Here the snowfall is great and the amount of heat cut off enormous ; but this alone would not account for the non-disappearance of the snow and ice. For, notwithstanding this, the heat received is certainly more than sufficient to melt all the snow which falls, great as that amount may be. The real cause is that the heat received is not sufficiently intense to raise the temperature to the melting point. More heat is actually received by the snow than is required to melt it, but it is dissipated and lost before it can manage to raise the temperature of the snow to the melting point ; consequently the snow is not melted. Here snow falls in the very middle of summer, but snow would not fall unless the temperature were near the freezing point.

Foregoing principles applied to the case of the Glacial epoch.—Let us now apply the foregoing principles to the case of the Glacial epoch. As winter then occurred in aphelion during a high state of eccentricity, that season would be much longer and colder than at present. Snow in temperate regions would then fall in place of rain, and although the snowfall during the winter might not be great, yet as the temperature would be far below the freezing point, what fell would not melt. As heat, which produces *evaporation*, is just as essential to the accumulation of snow and ice as is cold, which produces *condensation*, after the sun had passed the vernal equinox and summer was approaching the consequent rise of temperature would be accompanied by an increase in the snowfall. A melting of the snow would also begin, but it would be a very considerable time before the amount melted would equal the daily amount of snow falling. Rain, alternating with snow showers, would probably result ; and, for some time before mid-summer, snow would cease and give place entirely to rain. Melting would then go on rapidly, and by the end of the summer the snow would all disappear except on high mountain summits such as those of Scotland, Wales and Scandinavia. Before the end of autumn, however, it would again begin to fall. Next year would bring a repetition of the same process, with this difference, however, that the snow line would descend to a lower level than on the previous year. Year by year the snow line would continue to descend till all the high grounds became covered with permanent snow.

It would not require a very great amount of change from the present condition of things to bring about such a result. A simple lowering of the temperature, which would secure that snow instead of rain, should fall for six or eight months in the year would suffice ; and this would follow as a neces-

sary result from an increase of eccentricity. Now if all our mountain summits were covered with permanent snow down to a considerable distance, the valleys would soon become filled with local glaciers. In such a case we should then have more than one-half of Scotland, a large part of the north of England and Wales, with nearly the whole of Norway covered with snow and ice. Here a new and powerful agent would come into operation which would greatly hasten on a glacial condition of things. This large snow and ice-covered surface would tend to condense the vapor into snow. It would during summer chill the air and produce dense and continued fogs, cutting off the sun's rays and leading to a state of things approaching to that of South Georgia, which would much retard the melting of the snow.

It is a great mistake, as I have repeatedly shown, to suppose that the perihelion summers of the Glacial epoch could be hot. No snow and ice-covered continent can enjoy a hot summer. This is clearly shown by the present condition of Greenland. Were it not for the ice the summers of North Greenland, owing to the continuance of the sun above the horizon, would be as warm as those of England; but, instead of this, the Greenland summers are colder than our winters, and snow during that season falls more or less nine days out of ten. But were the ice covering removed a snow shower during summer would be as great a rarity as it would be with us. On the other hand, cover India with an ice sheet, and the summers of that place would be colder than those of England.

When the high grounds of Scotland and Scandinavia, with those of the northern parts of America, became covered with snow and ice, and the eccentricity went on increasing, a diminution of the Gulf Stream and a host of other physical agencies, all tending toward a glacial condition of things, would be brought into operation. This would ultimately and inevitably lead to a general state of glaciation without the aid of any of those additional geographical changes of land and water which some have supposed. This will be shown more fully when we come to examine Mr. Alfred R. Wallace's theory.

The Mutual Reaction of the Physical Agents.—Those who think that the agencies to which I refer would not by themselves bring about a glacial condition appear to overlook a most important and remarkable circumstance regarding their mode of operation, to which I have frequently alluded in 'Climate and Time' (pp. 74-77) and other places. The circumstance is this. The physical agencies in question not only all lead to one result, viz: an accumulation of snow and ice, but their efficiency in bringing about this result is actually

strengthened by their mutual reaction on one another. In physics the effect reacts on the cause. In electricity and magnetism, for example, cause and effect in almost every case mutually act and react upon each other, but the reaction of the effect tends to weaken the cause. Those physical agents to which I have referred, no doubt, in their mutual actions and reactions obey the same law ; but, in reference to *one particular result*, viz: the accumulation and conservation of snow, those mutual reactions strengthen one another. This is not reasoning in a circle, as Mr. Searles Wood supposes; for the reaction of an effect may on the whole weaken the cause, and yet in regard to a particular result it may strengthen it. In the case under consideration the agents not only act in one direction, but their efficiency in acting in that one direction is strengthened by their mutual reactions. This curious circumstance throws a flood of light on the causes which tended to bring about the Glacial epoch.

To begin with we have a high state of eccentricity. This leads to long and cold winters. The cold leads to snow, and although heat is given out in the formation of the snow, yet the final result is that the snow intensifies the cold: it cools the air and leads to still more snow. The cold and snow bring a third agent into play—*fogs*—which act still in the same direction. The fogs intercept the sun's rays, this interception of the rays diminishes the melting power of the sun, and so increases the accumulation. As the snow and ice continue to accumulate, more and more of the rays are cut off; and on the other hand, as the rays continue to be cut off, the *rate* of accumulation increases, because the quantity of snow and ice melted becomes thus annually less and less. In addition, the loss of the rays cut off by the fogs lowers the temperature of the air and leads to more snow being formed, while again the snow thus formed chills the air still more and increases the fogs. Again, during the winters of a Glacial epoch, the earth would be radiating its heat into space. Had this loss of heat simply lowered the temperature, the lowering of the temperature would have tended to diminish the rate of loss; but the result is the formation of snow rather than the lowering of the temperature.

Further, as snow and ice accumulate on the one hemisphere they diminish on the other. This increases the strength of the trade winds on the cold hemisphere and weakens those on the warm. The effect of this is to impel the warm water of the tropics more to the warm hemisphere than to the cold. Suppose the northern hemisphere to be the cold one, then as the snow and ice begin gradually to accumulate, the ocean currents

of that hemisphere, more particularly the Gulf Stream, begin to decrease in volume, while those on the southern or warm hemisphere begin *pari passu* to increase.* This withdrawal of heat from the northern hemisphere favors the accumulation of snow and ice, and as the snow and ice accumulate the ocean currents decrease. On the other hand, as the ocean currents diminish, the snow and ice still more accumulate. Thus the two effects in so far as the accumulation of snow and ice is concerned mutually strengthen each other.

The same process of mutual action and reaction takes place among the agencies in operation on the warm hemisphere, only the result produced is diametrically opposite to that produced in the cold hemisphere. On this warm hemisphere action and reaction tend to raise the mean temperature and diminish the quantity of snow and ice existing in temperate and polar regions.

The primary cause of all these physical agencies being set in operation is a high state of eccentricity of the earth's orbit, and with a continuance of that state a glacial epoch becomes inevitable.

The Explanation begins with Winter.—Mr. Hill asks why I always begin in my explanation with the aphelion winter rather than with the perihelion summer. The reason is that the character of the summer is determined by that of the winter and not the winter by that of the summer. It is true that to a certain extent the influence is mutual, but the effect of the summer on the winter is trifling in comparison with that of the winter on the summer. To begin our explanation with the summer would be like beginning at the end of a story and telling it backward.

Herr Woeikof on the cause of Glaciation.—In an article by A. Woeikof on 'Glaciers and Glacial Periods in their relations to Climate' (Nature, March 2d, 1882), it is maintained that the chief cause which leads to the formation of snow, and consequently to a glacial condition, is a low surface-temperature of the sea surrounding or adjoining the land. When the surface temperature of the water much exceeds the freezing-

* Professor Dana has shown that in North America those areas which at present have the greatest rain-fall are, as a rule, the areas which were most glaciated during the Glacial epoch. Mr. Searles V. Wood maintains that this fact is inconsistent with the theory that the Glacial period was due to the cause to which I attribute it. I am totally unable to comprehend how he arrives at this conclusion. Supposing the Gulf Stream, as I have maintained, were greatly diminished during the Glacial period, still I think it would follow, other things being equal, that the areas which now have the greatest rain-fall would during that period probably have the greatest snow-fall, and consequently the greatest accumulation of ice. The amount of precipitation might be less than at present, but this would not prevent the areas which had the greatest snow-fall from being most covered with ice.

point the vapor, he says, evaporated from the sea and condensed on the land will be rain and not snow, but when the temperature of the water is near the freezing-point, snow will be the result. A diminution, for example, in the heat brought by the Gulf Stream that would very greatly lower the surface temperature of the sea surrounding Great Britain would, he says, bring about a heavy snow-fall and lead to permanent snow and ice. Again he maintains "as there is no reason to suppose that the surface-temperature of the sea would be lower during winter in aphelion and high eccentricity, it follows that there will not be more snow than now in countries where rain is the rule, even in winter, all other things equal."

There is surely a fallacy lurking under this theory of M. Woeikof. Snow instead of rain is not, as he supposes, owing to the low temperature of the water from which the vapor is derived, but to the low temperature of the air where the vapor is precipitated. Of course, when the surface of the sea is near the freezing-point, the air over the sea and the adjoining land is usually also not far from the freezing-point, and consequently the precipitation is more likely to be snow than rain. If the air be cold as it generally is over a snow and ice-covered country, a high temperature of the adjoining seas, were this possible, would greatly increase the snow-fall because it would greatly augment the quantity of vapor which would be available for snow.

Edinburgh, Scotland, May 1st, 1883.