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A GROUP OF HYPOTHESES BEARING ON CLIMATIC CHANGES

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A GROUP OF HYPOTHESES BEARING ON CLIMATIC CHANGES.[1]

WHILE the atmosphere is the most active of all geological agencies, it has received the least careful study from geologists. Its very activity destroys its relics almost as soon as formed and gives them peculiar evanescence. This has invited the neglect of geologists laudably prone to concentrate their attention upon agencies which have left enduring and unequivocal records. The atmospheric element in geological history bids fair to long remain obscured by elusive factors and uncertain interpretations. None the less it is an element of supreme importance and should be persistently attacked until it yields up its truths. This must be my excuse for offering a paper which, I am painfully aware, is very speculative in many of its parts.

All our attempts at the solution of climatic problems proceed on some conscious or *unconscious* assumption concerning the extent and nature of the atmosphere at the stage involved. These assumptions are too often unconscious and the conclusions reached command a confidence which might not be reposed in them if the underlying assumptions were frankly stated. It may not be unwholesome, therefore, to raise some radical doubts respecting current assumptions regarding the early stages of the

[1] Read before the British Association for the Advancement of Science at Toronto, August 20, 1897. For obvious reasons it was necessary to treat the many factors involved with extreme brevity and hence with some obscurity and much lack of adequate qualification. I have taken the liberty of adding some tables and other matter

atmosphere and to offer for trial a competitive hypothesis or group of hypotheses. To admit a competitive hypothesis to the working list is a concrete form of embodying a doubt respecting existing hypotheses and serves better than any abstract skepticism to keep alive the sources of doubt. I assume that the system of multiple working hypotheses is accepted as furnishing the most wholesome conditions for research, and that any additional hypothesis not in itself incredible will be welcomed.

If we compute the mass of the several constituents of the present atmosphere, and estimate the rate at which they are being consumed in alterations of the superficial rocks, we find that the carbon dioxide will last but a short period unless there be some source of supply. A group of careful estimates by different methods gives results ranging from five thousand to eighteen thousand years[1] with a weighted mean of about ten thousand years. Only the alteration of the crystalline terranes was admitted to the computations. The estimates assumed the degradation rates of current geological opinion. Granting these may be too high, and multiplying the results accordingly, it still appears that we are confronted by the early exhaustion of a vital factor of the atmosphere, if there be no compensating source of supply.

There is, however, an immediate source of compensation. The ocean is an atmosphere in storage. It is not improbable

and of slightly extending and modifying the treatment on some points, but it still remains merely synoptic. The treatment of the periodicity of Pleistocene atmospheric changes is especially incomplete, but this is only a particular case under a general hypothesis whose value does not necessarily hang upon this individual application.

I desire to add that most of the questions involved in the paper have been discussed with scientific friends and with the advanced graduate students of my classes during the past two years, and that I have received from them much valuable aid. Computations and quantitative estimates have been made by F. R. Moulton, H. L. Clarke, A. W. Whitney, J. P. Goode, H. F. Bain, Samuel Weidman, C. F. Tolman, Jr., N. M. Fenneman, and C. E. Siebenthal, which I desire to gratefully acknowledge. The main points of the paper were presented to the Geological Club of the University of Chicago, October 1896.

[1] Made by A. W. Whitney, H. Foster Bain, J. P. Goode, Samuel Weidman, C. F. Tolman, Jr., and the writer.

that every portion of it has once been a constituent of the atmosphere and may be again. In atmospheric studies it must be recognized as a potential atmosphere. According to the best data at command, the ocean holds in solution about eighteen times as much carbon dioxide as the atmosphere. But even this reserve supply if fully available leaves the perpetuity of atmospheric conditions congenial to life very short, viewed geologically. This threat of disaster is not, however, a scientific argument, whatever function it may have in awakening interest and neutralizing inherited prejudice.

A broad comparison between the atmosphere of Palæozoic times and that of Cenozoic times fails, I think, to give proof of any radical difference in the constitution of the two atmospheres. The magnolia flora in North Greenland in Tertiary times indicates a scarcely less wide distribution of warm climate than the life of the same region in Palæozoic times. Glaciation in northern Norway announced by Reusch and confirmed by Strahan, in times apparently just preceding the Palæozoic era, is as suggestive of atmospheric poverty as anything that introduces the Cenozoic times. The signs of glaciation at the close of the Palæozoic era in India, Australia, and South Africa, reaching within 20° of the equator, indicate a thermal depression even more marvelous than that which closed the Cenozoic era. The salt deposits of the middle latitudes in Palæozoic times, notably those of Michigan and New York, in areas where the great basins now overflow voluminously, seem to imply an aridity quite comparable to anything which has succeeded. The extensive terranes of hematite-stained rocks, contrasted with the limonite-stained terranes, while their interpretation is more problematic, make suggestions of concurrent import.

A comparison of early with later life, stripped of theoretical presumptions, does not seem to me clearly to imply any great difference in the content of carbon dioxide. Air-breathing life, to be sure, has left no certain record earlier than the middle Palæozoic, but these earliest forms afford no clear proof that they were determined by non-susceptibility to an excess of carbon dioxide. The delay in the appearance of land life is sufficiently assignable to the obstacles to its evolution to make needless a theoretical appeal to a noxious condition of the atmosphere.

But if we compute the amount of carbon which has been extracted from the atmosphere in the production of the carbonates and the carbonaceous deposits, and restore this to the atmosphere, following a time-honored custom, we are led to the time-honored conception of an exceedingly extensive, dense, warm and moist atmosphere. The amount of carbon dioxide represented by the limestones and carbonaceous deposits has been variously estimated at twelve thousand to one hundred and fifty thousand times the present content of the atmosphere. My own estimates lead me to favor figures lying between twenty thousand and thirty thousand. These estimates do not go back of the Palæozoic series and leave an unknown factor to be added for the pre-Cambrian limestones and carbonaceous deposits. The amount of carbon extracted from the atmosphere since the introduction of air-breathing life is probably not less than 8000 or 10,000 times the amount now in the air. This forces the question whether this large amount of carbon dioxide or any major part of it was ever in the atmosphere at any one time.

The alternative is to assume that the atmosphere was originally less ample and has been fed as well as robbed during all the geological ages, its history being a struggle between enrichment and depletion. In some measure this is an accepted view, but it is part of the purpose of this paper to show that the way is open to freer hypothesis in this direction.

The current view of a vast original atmosphere is derived less perhaps from the computation of material extracted from it than from theoretical views regarding the origin and early history of the earth. There has been quite general assent to the nebular theory of the origin of the earth. Even where dissent from the gaseous features of this theory has been entertained there has been acquiescence in the doctrine of the earth's early molten condition and all that it implies. If the earth were in a thoroughly molten condition, there would seem at first thought but scant ground for any dissent from the inference that the present hydrosphere was then mainly a part of the atmosphere. It does not rigorously follow, however, that this was so. Hypothesis may go so far as to attribute much of the subsequent ocean and atmosphere to vapors thrown out of the molten magma as it cooled and to vapors gathered from space since. But I venture to question the supposed original molten state. While making no claim to disprove it, I doubt whether it rests on sufficiently solid theoretical grounds to justify the assumptions so unhesitatingly built upon it. There is still some ground to doubt the nebular hypothesis and to entertain some of the various phases of the meteoroidal hypotheses. The nebular hypothesis correlates a wonderful array of remarkable facts and has gained a profound hold upon the convictions of the scientific world, yet some of its great pillars of support have recently weakened or have fallen away entirely. Of the 5000 known nebulæ to which we naturally look for analogy very few, if indeed any, strictly interpreted, exemplify in a clear and decisive manner the systematic annular evolution postulated by Laplace. The photographs of the nebula of Andromeda, that were hailed with such delight on their first appearance as exemplifying the Laplacean hypothesis, appear upon more critical study to support it only in vague and general terms, if indeed they lend it support at all. The Saturnian rings, the trite source of illustration and analogy, prove under the test of the spectroscope to be formed of discrete solid particles, and not of gas, and the investigations of Roche have put a new phase on their theory. While in their form they tally with the annular hypothesis they do not support its gaseous phase, if indeed they lend it any support at all. But our chief interest is not in the general theory, but in the special inferences drawn from it respecting the early stages of the earth. Let us assume the possible or, if you please, probable truthfulness of the nebular hypothesis so far as the separation of an earth-moon ring from the shrinking sun is concerned. Do the subsequent steps commonly postulated logically follow?

The vast radiating surface of such a ring, its attenuated nature and the extremely high temperature necessary to maintain its refractory substances in a volatile condition combine to suggest its speedy passage from the vaporous to the Saturnian or discrete solid condition from loss of heat. It seems a severe tax upon probabilities to suppose that such a ring would remain in the gaseous condition during the long period of its aggregation into a spheroidal form.

But a graver source of doubt is found in the high molecular velocities of the gases under these conditions. Dr. Johnstone Stoney[1] and others have attempted to show that the attractive power of small planets is insufficient to control gases of the higher molecular velocities, especially aqueous vapor. To this is attributed the measurable absence of atmospheres on the satellites and small planets. An endeavor to apply a similar line of reasoning to the conditions of the early earth leads to such disquieting results that I may be justified in briefly sketching it.

Each celestial body has an attractive power sufficient to control molecules shot away from it at velocities below a certain limit. At these velocities the discharged molecules pursue elliptical paths and return to the starting point. At the limit of these velocities they pursue parabolic courses and never return. Hence arises the expression "parabolic velocity" to indicate the limital speed at which particles shot away from the body will not return. The parabolic velocity of the earth at its surface is about 6.9 miles (1118127^{cm}) per second. A molecule discharged from it at that speed or a greater one will not return to it. The parabolic velocity is but an expression of effective gravity and

[1] "On the Cause of the Absence of Hydrogen from the Earth's Atmosphere and of Air and Water from the Moon." Royal Dublin Society, 1892. Since this paper was put in type I have been permitted to see an advanced copy of Dr. Stoney's later paper, "Of Atmospheres upon Planets and Satellites," Trans. Royal Dublin Society, Vol. VI, Part 13, Oct. 25, 1897, in which the author's investigations are much more fully set forth and his conclusions greatly strengthened. He takes account of the rotary speed of the outer equatorial zone and of westerly winds as projectile aids, factors which are neglected in this discussion. He also bases a very strong argument on the absence of helium from the present atmosphere, which on account of its chemical inertness would accumulate if it were not discharged.

depends not only upon the amount of the material embraced in the body, but on its distribution and other conditions. The parabolic velocity declines with height, as shown in the following table prepared for me by Mr. F. R. Moulton.[1]

I.

TABLE OF THE EARTH'S PARABOLIC VELOCITIES (V_p') FOR VARIOUS HEIGHTS ABOVE ITS CENTER (x) THE EFFECTS OF ROTATION BEING NEGLECTED.*

When x (height above center) $= 0$		$V_p' = +\infty$
When $x = r$ (earth's radius)		$V_p' = 11181.3$ meters
When $x = 7 \times 10^6$ meters		$V_p' = 10672.5$ "
When $x = 8 \times 10^6$	"	$V_p' = 9983.2$ "
When $x = 9 \times 10^6$	"	$V_p' = 9412.2$ "
When $x = 10^7$	"	$V_p' = 8914.1$ "
When $x = 12 \times 10^6$	"	$V_p' = 8151.2$ "
When $x = 14 \times 10^6$	"	$V_p' = 7546.6$ "
When $x = 17 \times 10^6$	"	$V_p' = 6848.4$ "
When $x = 20 \times 10^6$	"	$V_p' = 6313.9$ "
When $x = 25 \times 10^6$	"	$V_p' = 5647.3$ "
When $x = 30 \times 10^6$	"	$V_p' = 5155.3$ "
When $x = 40 \times 10^6$	"	$V_p' = 4464.6$ "
When $x = 60 \times 10^6$	"	$V_p' = 3645.3$ "
When $x = 10^8$	"	$V_p' = 2823.7$ "
When $x = 15 \times 10^7$	"	$V_p' = 2305.5$ "
When $x = 5 \times 10^8$	"	$V_p' = 1262.8$ "
When $x = 25 \times 10^8$	"	$V_p' = 564.7$ "

$${}^*V_p' = \frac{\sqrt{2gr^2}}{\sqrt{x}}$$ Log. $2g = 1.2923447$.

$2g = 64.32$ ft. $= 19.604$ meters. Log. $r^2 = 13.6092842$.

$r = 6{,}377{,}377$ meters. Log. $\sqrt{2gr^2} = 7.4508144$.

The parabolic velocity is also reduced by the centrifugal component of rotation. The effect of this is shown in the following tables computed by Mr. Moulton:

[1] Assistant in Astronomy at the University of Chicago.

II.

TABLE OF PARABOLIC VELOCITIES OF THE EARTH (V_p') FOR VARIOUS HEIGHTS ABOVE ITS CENTER (X) WHEN THE PERIOD OF ROTATION IS 23 HOURS, 56.067 MINUTES.*

When x (height above center) = 0		$V_p' = +\infty$	
When $x = r$ (earth's radius)		$V_p' = 11181.27$	meters
When $x = 7 \times 10^6$ meters	(4,349 miles)	$V_p' = 10672.49$	"
When $x = 8 \times 10^6$ "	(4,972 ")	$V_p' = 9983.16$	"
When $x = 9 \times 10^6$ "	(5,593 ")	$V_p' = 9412.15$	"
When $x = 10 \times 10^6$ "	(6,214 ")	$V_p' = 8914.05$	"
When $x = 12 \times 10^6$ "	(7,457 ")	$V_p' = 8151.14$	"
When $x = 14 \times 10^6$ "	(10,700 ")	$V_p' = 7546.52$	"
When $x = 17 \times 10^6$ "	(10,564 ")	$V_p' = 6848.31$	"
When $x = 20 \times 10^6$ "	(12,428 ")	$V_p' = 6313.79$	"
When $x = 25 \times 10^6$ "	(15,544 ")	$V_p' = 5647.17$	"
When $x = 30 \times 10^6$ "	(18,643 ")	$V_p' = 5155.14$	"
When $x = 40 \times 10^6$ "	(24,857 ")	$V_p' = 4464.39$	"
When $x = 60 \times 10^6$ "	(37,286 ")	$V_p' = 3644.98$	"
When $x = 100 \times 10^6$ "	(62,144 ")	$V_p' = 2823.17$	"
When $x = 150 \times 10^6$ "	(93,216 ")	$V_p' = 2304.70$	"
When $x = 500 \times 10^6$ "	(310,720 ")	$V_p' = 1260.14$	"
When $x = 2500 \times 10^6$ "	(1,553,600 ")	$V_p' = 551.40$	"
When $x = 10000 \times 10^6$ "	(6,214,400 ")	$V_p' = 229.19$	"
When $x = 30434 \times 10^6$ "	(18,902,905 ")	$V_p' = .00$	"

$$*V_p' = \frac{\sqrt{2gr^2}}{\sqrt{x}} - \frac{4\pi^2 x}{t^2} \qquad t = 23^h\ 56.067^m = 86164^s.$$

$$\text{Log. } \sqrt{2gr^2} = 7.4508144. \qquad \text{Log. } \frac{4\pi^2}{t^2} = \bar{9}.7257080.$$

III.

TABLE OF PARABOLIC VELOCITIES OF THE EARTH (V_p') FOR VARIOUS HEIGHTS ABOVE ITS CENTER WHEN THE ROTATION PERIOD IS 1 HOUR, 24 MINUTES, AT WHICH THE CENTRIFUGAL COMPONENT OF ROTATION EQUALS THE ACCELERATION OF GRAVITY AT THE EQUATORIAL SURFACE.*

When x (height above center) = 0	$V_p' = +\infty$	
When $x = r$ (earth's radius)	$V_p' = 11171.39$	meters
When $x = 7 \times 10^6$ meters	$V_p' = 10661.69$	"
When $x = 8 \times 10^6$ "	$V_p' = 9970.8$	"
When $x = 9 \times 10^6$ "	$V_p' = 9398.2$	"

III—*continued.*

When $x =$	10×10^6	"	$V_p' =$ 8898.5	"
When $x =$	12×10^6	"	$V_p' =$ 8132.5	"
When $x =$	14×10^6	"	$V_p' =$ 7524.8	"
When $x =$	17×10^6	"	$V_p' =$ 6822.0	"
When $x =$	20×10^6	"	$V_p' =$ 6282.8	"
When $x =$	25×10^6	"	$V_p' =$ 5608.4	"
When $x =$	30×10^6	"	$V_p' =$ 5108.7	"
When $x =$	40×10^6	"	$V_p' =$ 4402.4	"
When $x =$	60×10^6	"	$V_p' =$ 3552.0	"
When $x =$	100×10^6	"	$V_p' =$ 2668.3	"
When $x =$	150×10^6	"	$V_p' =$ 2072.4	"
When $x =$	500×10^6	"	$V_p' =$ 485.7	"
When $x =$	691×10^6	"	$V_p' =$ .0	"

$$*V_p' = \frac{\sqrt{2gr^2}}{\sqrt{x}} - \frac{4\pi^2 x}{t^2} \qquad \text{Log. } \sqrt{2gr^2} = 7.4508144$$

$$t = 1^h\ 24^m = 5040^s \qquad \text{Log. } \frac{4\pi^2}{t^2} = \bar{6}.1914987.$$

The molecular velocities vary with temperature. The following table computed for me by Mr. A. W. Whitney exhibits these velocities for temperatures ranging from zero to 4000° C.:

IV.

TABLE OF AVERAGE MOLECULAR VELOCITIES FOR VARYING TEMPERATURES, IN CENTIMETERS PER SECOND, STANDARD PRESSURE.

	0°	100°	1000°	1250°	1500°	2000°	3000°	4000°
H_2	169611	198257	367258	400428	432243	489410	587282	671029
H_2O	56522	66067	122054	133501	144042	163093	195707	223619
CO_2	33259	38876	71819	78556	84759	95965	115160	131580
O_2	39155	45768	84551	92482	99786	112983	135576	154907
N_2	41735	48784	90122	98574	106359	120425	144508	165115

The molecules of a gas of a given temperature have a mean velocity, but this does not express the actual velocity of the individual molecules. By their interaction upon one another the velocities of some are depressed, the limit being zero, and the velocities of others are increased, the limit being infinity.

Theoretically both these limits may be reached, but extremely high velocities are acquired at such distant intervals as to be negligible. Very considerable exaltations of velocity are however attained with sufficient frequency to be effective in discharging a large part of the gas under suitable conditions, since each molecule in succession is liable to acquire a high velocity. The following table shows the proportion of molecules that reach or exceed the designated multiples of the average velocity at any instant:[1]

V.

TABLE SHOWING THE PROPORTION OF MOLECULES WHICH HAVE A GIVEN NUMBER OF TIMES THE AVERAGE 0°C. VELOCITY (OR MORE) AT ANY INSTANT, STANDARD PRESSURE, FOR TEMPERATURES RANGING FROM 0°C. TO 4000°C.

Proportion of Molecules	Times Average 0°c. Velocity for different Temperatures					
	t = 0°c.	t = 1000°c.	t = 1500°c.	t = 2000°c.	t = 3000°c.	t = 4000°c.
4.7×10^{-1}	1	2.2	2.5	2.9	3.5	3.9
1.7×10^{-2}	2	4.3	5.1	5.8	6.9	7.9
4.2×10^{-5}	3	6.5	7.6	8.7	10.4	11.9
7.4×10^{-9}	4	8.6	10.2	11.5	13.9	15.8
9.7×10^{-14}	5	10.8	12.7	14.4	17.3	19.8
9.6×10^{-20}	6	12.9	15.3	17.3	20.8	23.7
7.2×10^{-27}	7	15.1	17.8	20.2	24.2	27.7
4.2×10^{-35}	8	17.3	20.4	23.1	27.7	31.7
1.9×10^{-44}	9	19.4	22.9	25.9	31.2	35.6
6.5×10^{-55}	10	21.6	25.5	28.9	34.6	39.6

The molecules of water vapor at 0° C. have an average velocity of 56522^{cm} per sec. The foregoing table shows the

[1] This table was computed by means of the formulæ given by Risteen ("Molecules and the Molecular Theory of Matter," pp. 24–28), which are based on Maxwell's determinations. The high velocities assigned to a part of the molecules are mathematical deductions from data not altogether perfect, and are doubtless to be held with something less of firmness than would be warranted if they were experimental demonstrations, but in the absence of an available method of experimental demonstration these deductions may be accepted as the nearest approximation at present obtainable. A brief non-mathematical statement may be found in Maxwell's "Theory of Heat," pp. 314–316. The results require some modification for mixed gases and for special conditions, but this is not thought essential in this general argument.

number of times this velocity a given proportion of molecules attain at any instant when they have certain specified temperatures. For example, the table shows that when the gas is at 0° C., 47×10^{-1}, or 47 per cent. of the molecules have a velocity greater than the average velocity at zero centegrade; when the gas is at 1000° C., 47 per cent. of the molecules have a velocity 2.2 times the average velocity at 0° C.; when at 1500° C., the same per cent. have 2.5 times the average velocity at 0°C., etc. To raise the velocity of these molecules to the parabolic velocity of the earth the multiplier must be about 19.8 (since 1118127^{cm} per sec. is the parabolic velocity of the earth at the surface and $1118127 \div 56522 = 19.8$, nearly). The table shows that the proportion of molecules attaining this velocity or over (taking the figure nearest to 19.8) is as follows:

For 1000°	1.9×10^{-44}	For 3000°	7.58×10^{-20}
For 1500°	4.18×10^{-35}	For 4000°	9.7×10^{-14}
For 2000°	7.22×10^{-27}		

It now becomes important to ascertain how frequently all the molecules, on the average, will acquire the parabolic velocity of the earth. Every time a collision occurs the velocities of the colliding particles change. The formula for the time required for complete change will therefore be $\frac{1}{NPm}$ where N is the number of collisions per second at 0° C. standard pressure, and Pm is the proportion of molecules having the parabolic velocity, given in terms of 0° C. velocity, standard pressure.

The number of collisions per second for 0° C. standard pressure is given by Maxwell as 17,750,000,000 for hydrogen, 7,646,-000,000 for oxygen, and 9,720,000,000 for carbon dioxide. For the number of collisions for water vapor I find no authentic estimate, but it probably sustains the same ratio to the collisions of hydrogen and oxygen that their velocities do to each other, increased by a certain factor representing the effect of the size of the molecules. It will here be assumed that the number of collisions of the molecules of aqueous vapor is 10,000,000,000 per second at 0° C. standard pressure. The results can easily be modified for any other figure that may be thought nearer the truth. The number of collisions also increases with the density of the gas. In the supposed case of an atmosphere containing all the water of the globe, the density would perhaps be 300 times the standard density. In the upper regions the density would be low and $\frac{1}{100}$ of the standard density may be taken as a representative of the conditions there. Making the assumption that the collisions of water vapor are 10^{10} per sec., the periods required for all the molecules, on an average, to acquire the parabolic velocity of the earth would be as follows:

Temperature	At Standard Pressure	At $\frac{1}{100}$ Standard Pressure	At 300 times Standard Pressure
1000°	1.7×10^{26} years	1.7×10^{28} years	5.7×10^{23} years
1500°	7.6×10^{16} "	7.6×10^{18} "	2.5×10^{14} "
2000°	4.3×10^{8} "	4.3×10^{10} "	1.4×10^{6} "
3000°	33 years	3300 years	40 days
4000°	1030 seconds	28.5 hours	3.4 seconds

Under the current hypothesis of a molten earth derived from a gaseous one the temperature of the atmosphere would probably exceed 4000° C. during the stages of condensation of the refractory material of the earth from the form of a gas to the form of a liquid. From this fervid stage the temperature would fall to 2000° C., or below before the crust would begin to form and the external liquid condition cease. The temperatures of a liquid earth may therefore be assumed to range from 4000° C. to 2000° C. or below, and the figures of the preceding table may be interpreted on this basis.

If the question were simply the acquisition of molecular velocities at the surface of the liquid mass greater than the parabolic velocity of the earth at that point within an available length of time, it would appear that the retention of the water vapor would be put in serious jeopardy during the hotter stages, but that it might survive the cooler ones in large part if it reached them, unless they were very prolonged. But there are other considerations to be taken into account. Under the most favorable conditions only a part of the molecules which attain a speed beyond the limital velocity of the earth's control would escape because they would not be projected away from the earth. Besides, the escape of molecules projected outwards is seriously limited by the interference of the particles above. This interference is practically prohibitory for the molecules in the base of the atmosphere. The problem, therefore, involves the extent to which the high velocities of the lower hot atmosphere would be communicated to the upper atmosphere whence escape would be possible. The interpretation of this is beset with great difficulties. The molecular velocities of the higher parts of an atmosphere surrounding a molten earth involve factors which cannot be safely estimated from the phenomena of a cold earth. It must of course be assumed that the molecular velocities of the molecules of the rising air would be lowered in proportion to the work done by them or the energy lost, but in convectional movements certain parts of the air are recipients of motion rather than generators of it, and do not lose the energy their movements might seem to imply. It is probable that the interchange of lower and upper air about a molten earth would be extremely violent. It is not unlikely that explosive convection like that of the sun would be the customary mode of action. If hot bodies of vapor were shot violently into the outer limits of the atmosphere, molecular discharge would seem to be probable if not inevitable, whatever might be true of the more quiet mode of action. Besides this the current nebular hypothesis apparently involves the passage of even the outer atmosphere through very hot stages during the early period when the refractory gases of the now solid material were condensing and separating themselves from the atmospheric gases.

The case in this form seems at present indeterminate. There is an apparent probability that a large loss would be suffered while the temperatures ranged from 3000° to 4000°. At the same time there is a possibility that a residue would remain if the period of this high temperature were not prolonged, and a probability that a large part of the atmosphere would be retained if it survived until the temperatures were near the melting point of rock.

The considerations that grow out of altitude above the surface which reduces the parabolic velocity have been neglected thus far. These are not very important in a shallow atmosphere as may be seen by reference to the tables previously given, but they might be consequential in an exceedingly extended atmosphere. While it would be hazardous to estimate the height of a superheated atmosphere embracing the whole present hydrosphere, it seems not improbable that its outer portion would be appreciably affected by the reduction of the parabolic velocity due to its high altitude.

To this is to be added also the effect of the high rotation which the earth is assumed to have had. In the supposed discharge of the moon under either the nebular or fission hypothesis the attraction of the earth in the equatorial zone must have been nearly or quite neutralized by the centrifugal effect of rotation. This must have greatly promoted the expansion of the atmosphere in that zone and correspondingly reduced the earth's power to control its outer portion, indeed it is difficult to see how the moon could have separated from the earth without carrying away the atmosphere, unless indeed the separation took place while the material of both bodies was in a perfect gaseous condition and the atmospheric constituents were diffused throughout the entire gaseous mass. But even in this case the subsequent contraction of the earth should apparently have accelerated its rotation to such an extent that the retention of the outer equatorial atmosphere would be put in jeopardy.

There is still another consideration whose importance may possibly be decisive—the dissociation of water. Dr. Stoney has maintained that even under present conditions the earth is incompetent to retain hydrogen. This conclusion is in harmony with the fact that hydrogen does not permanently exist in the atmosphere, though this absence may be otherwise explained.[1] At 1000° C. all molecules of hydrogen would acquire the parabolic velocity of the earth some hundreds of thousands of times

[1] In his last paper, referred to in a previous footnote, the weakness of the argument from the absence of hydrogen owing to the ease with which it may combine

per second. Now the temperatures of the supposed molten earth reached and probably much exceeded the temperatures of effective dissociation of water vapor. The dissociation is probably due to violent impact of molecules of high velocities. It probably takes place in some degree even at moderate temperatures.[1] The proportion of dissociated molecules greatly increases with temperature until the dissociation so far exceeds the recombination that it may be said to be nearly or quite complete. Authorities differ as to the temperature of effective dissociation. The estimates commonly given lie in the lower half of the range of temperatures above assigned to the molten stage of the earth. If, therefore, the temperatures of the molten globe ranged as high as the current hypothesis seems to require, the dissociation of the aqueous vapor would seem to be inevitable and the loss of hydrogen would be endangered notwithstanding its disposition to recombine.

If the retention of the atmosphere be put in jeopardy by the earth's temperatures in a supposed liquid state much more would it be endangered if the temperatures were those of volatilization of the refractory material of the earth, as assumed by the Laplacean hypothesis, for not only would the molecular velocities be enormously increased, but the extension of the mass would push its exterior portions out into the regions of low parabolic velocity.

If the mass be still further dispersed into the vast gaseous ring of the Laplacean hypothesis the argument from molecular velocities is immeasurably strengthened, for not only must the temperatures requisite to the retention of the refractory substances of the earth in the attenuated condition of such a gaseous ring be exceedingly high, but the parabolic velocity of the body

with the free oxygen of the air is in a large measure covered by resting the argument chiefly on the absence of helium, which is chemically very inert. As helium is given off slowly by hot springs, it is urged that in the vast lapse of the geological ages it should have accumulated to an appreciable quantity if it had not escaped. As it has twice the molecular mass of hydrogen it is held that the "minimum speed of control" at existing temperatures lies below the molecular velocities of gases which are twice as heavy atomically as hydrogen.

[1] RISTEEN, Molecules and Molecular Theory, pp. 50–51.

in such an extremely distributive form would be exceedingly low. It would seem, therefore, that unless the argument from molecular velocities is radically and grievously at fault the hypothesis of a gaseous earth-moon ring is untenable unless a degree of tenuity be assumed which separates the molecules beyond the limits of effective kinetic relations. In this case the argument from rapid cooling becomes peculiarly strong and seems to leave no alternative but the conversion of the refractory matter of the ring into the discrete solid condition.

Impressed by these considerations and following what seem to be the legitimate implications of molecular studies, I have ventured for myself to place the atmospheric inferences from the supposed gaseous and molten conditions of the primitive earth in the list of uncertain deductions and to add an alternative hypothesis to my working list.

But occasion for doubt concerning an early molten earth and its vast atmosphere is not limited to this line of approach. On other grounds we cannot fail to recognize that some form of the meteoroidal hypotheses of the origin of the earth is entitled to be reckoned among the possibilities. Whether an accretion of meteoroidal matter would give rise to a molten earth or not would depend upon the rapidity and violence of the infall. If the intervals between falls were sufficient the heat would be lost concurrently. A relatively cold earth is theoretically as possible as a hot one until it is shown that the aggregation must be rapid. Even following the general line of the nebular hypothesis a cold earth is hypothetically possible. We have found reason for thinking that the earth-moon ring, if formed, would probably become cooled to discrete solid particles, while still in the ring form. Now it does not appear that there are any conditions inherent in such a ring that tend toward sudden concentration into a spheroidal body. Quite on the other hand, the problem presented by such a ring is to find agencies which will lead to its concentration at all. Just how the concentration would take place is an unsolved question.[1] But two things seem certain; first,

[1] I have ventured to speculate a little upon this, though beyond the province of a

the process would be slow; the individual conjunctions more or less distant in time, and the heat generated by one impact so far forth lost before another took place; second, the conjunctions would not be opposing collisions but overtakes in which both bodies were moving at nearly the same speed, and the heat of conjunction hence relatively small. It would appear, therefore, that the aggregation might take place without the development *at any one time* of a general high temperature. The present accretions of the earth show us that growth is possible without notable increase of temperature. Following the general line of the nebular hypothesis, therefore, it is possible to suppose the earth to have been affected by relatively low surface temperatures at all stages of its growth. By changing our assumptions as to the rate and vigor of accretion we can correspondingly change our conclusions respecting the earth's temperature. The range

geologist, because it involves a supposed objection to meteoroïdal aggregation. In a solid rotating ring the outer part moves faster than the inner and if broken and condensed to the globular form the rotation must be direct. But in a ring of planetoids the inner members move faster than the outer and if the several concentric orbits be symmetrically drawn together so that the inner planetoids uniformly or usually collide with the inner sides of the outer planetoids *retrograde* rotation follows. But this is inconsistent with the facts of the solar system except in the case of Uranus and Neptune (Cf. Faye, sur l'Origine du Monde, 1896, pp. 165, 270–281). But it seems improbable that this would be the mode of union except in the case of the outer planets, for the mutual gravitation of minute planetoids is very slight and expresses itself chiefly in perturbations under such conditions (see On the Stability of Motion of Saturn's Rings, Scientific Papers of James Clerk Maxwell, Vol. I, pp. 288–376), while the disturbing influence of the great planets is appreciable, as the ellipticity of the orbits of the planets testify. If the orbits of the particles or planetoids of the supposed earth-moon ring were at first nearly circular and concentric the conjoined attractions of the outer planets would render them elliptical. But the line of their apsides would not be concordant and would be subject to subsequent shifting in a more or less non-concordant fashion. It is therefore conceived that they would be brought to cross each other and that this would lead to collisions. Now an outer orbit could only cross an inner one by a more or less perihelion portion of it coming into coincidence with a more or less aphelion portion of the inner one. But the perihelion movement of a body in an outer orbit is greater than the coincident aphelion movement of a body in an inner orbit. Hence on the average the outer body in collision will have the greater speed and the consequent rotation will be direct. As this reasoning applies to the inner planets and not to the outer, and as the inner planets have direct rotations while the outer probably have retrograde rotations, it has at least the merit of coincidence with the facts.

of rational hypothesis seems therefore to be wide. It is herein urged that it is wholesome at present to recognize this wide range in its fullest amplitude.

But if we question current conceptions we should present alternatives which account for the atmosphere and for internal heat. Let us therefore hastily follow the hypothetical growth of a planet built up by the slow aggregation of small bodies which join it at low velocities and develop a minimum heat. Let the case be purposely made rather extreme to develop sharply the difficulties springing from it. Let the infalling particles be small and their rate such as not to generate a high surface temperature. The growth of such a body up to the size of the moon may be taken as an hypothesis of lunar history and the phenomena of the moon may serve as a check upon it. The moon may, however, have originated by fission even though the earth were built up by accretions. In the early stages of growth the gravity being low the aggregation may be supposed to have remained uncondensed. Volcanic aggregations of bombs, cinders and ashes are perhaps the nearest terrestrial analogues. The ingathering particles obviously carried with them so much of the atmospheric material as was entrapped or occluded within them in their solidification, or was absorbed into their pores or adhered to their surfaces. Judging from meteorites the amount of this might have been large. Gaseous molecules moving as independent bodies may have joined the aggregation and become absorbed in its porous body, but they would not have been collected into an appreciable atmospheric envelope until the body passed the size of the moon if the molecular considerations urged earlier in this paper hold good, though an atmospheric envelope would not have been entirely absent. As the mass grew the central pressure increased and condensation produced heat at the center proportional to the work done. I find the explanation of internal heat chiefly in this self-condensation, it being essentially the application of the Helmholtz solar theory to a solid body. Tidal kneading and chemical action doubtless added their contributions. When the growing mass reached the size of the moon a definite problem was presented of which the present moon stands as a possible representative and invites computation. If in its loose state of aggregation the mass had a specific gravity of 2. and if it shrank by self-condensation to 3.4, the average specific gravity of the moon, the possible heat generated by the gravitative fall would have equaled 3900° C. for the whole mass, the specific heat being assumed to be .2, which is very prudent. I owe the computation to Mr. Moulton. For convenience of computation the condensation was assumed to be uniform and the distribution of heat uniform. The original distribution of internal heat would perhaps have varied with the square root of the pressure, according to Laplace's formula. As the computed temperature is more than twice the melting temperature of average rock not under pressure it seems ample for all igneous phenomena indicated on the moon with a large residue for secular loss.

Assuming that the exterior temperature remained below zero during the pre-atmospheric stages of growth, the hypothetical structure of the planet when it reached the size of the moon may be pictured as embracing (1) a dense central portion raised to a high temperature by compression, giving a potentiality of liquefaction under relief of pressure; (2) a zone of declining temperature and less compressed structure, graduating toward a porous condition, and (3) at the surface the still unconsolidated open aggregation. The low average specific gravity of the moon (3.4) encourages the belief that the outer porous zone was deep and open. The notion is entertained that the central heat and compression would lead to the expulsion of a part of the centrally entrapped gases and vapors, and that these would be driven outward into the exterior porous portion, which having a low temperature, like that of the moon today, condensed the aqueous vapor in the spaces of the open texture and the whole became bound together more or less completely with a matrix of frost and ice. It is assumed that the internal condensation would be attended by readjustments of matter of the nature of diffusions, differentiations and concentrations, and that there would be deformations and igneous extrusions as on the earth today. Perhaps the reduction of metallic oxides and the working of the slag toward the surface may have been an incident of the process. Now whenever igneous extrusions invaded the zone of congealed vapor conditions would be afforded favorable for the generation of great quantities of steam temporarily restrained by the overlying fragmental mass and facily subject to explosive discharge. The peculiar constitution of such a body invites the notion of exceptionally explosive eruptions, as do also the extraordinary pits of the moon. As a matter of fact the suggestion arose from studying the pits and not from the peculiar constitution of the body to which the speculation had led. These remarkable cavities seem to be the close analogues of the few explosive craters which the surface of the earth presents.

The pre-atmospheric stage of the evolution would obviously cease when the growing earth acquired a size sufficient to measurably control its exhaled and ingathered gases. A certain measure of control was incidental to all stages, for even a small planetoid has some power to control gases of very low initial velocity if it continues at low temperatures. At the size of the moon gases of much more than the average molecular velocity of those of the earth at 0° C. could be held if their velocities were not exalted by interaction. This exaltation would become ineffective when the gases became extremely rare and the surface very cold. The molecular argument does not therefore affirm the total absence of an atmosphere on the moon, but rather on the contrary its scanty presence. An effective control would perhaps begin to be gained by the growing planet when the size of Mercury or thereabout was attained. After this the vapors and gases of lower molecular velocity would collect upon the surface and initiate the appreciable history of the external atmosphere. Whensoever the accretions of this atmosphere acquired the power of retaining the heat of the sun to such a degree as to give a surface temperature above the freezing point, the inauguration of the hydrosphere would take place and with its progressive development the familiar phenomena due to superficial waters would appear. The surface would soon lose its extremely fragmentary condition and take on the terrestrial form; the subterranean frozen zone would disappear and the vulcanism assume the terrestrial type.

This hypothesis, it will be observed, departs radically from the familiar view in that it initiates the atmospheric history by a tenuous envelope which continued to slowly increase. By the hypothesis, as thus far sketched, the atmosphere was derived from the interior. After the earth reached the requisite size the collection of wandering gases would supplement it. The competency of this external source is almost wholly a matter of conjecture and its vague possibilities need not be discussed here. It need only be remarked that the hypothesis of molecular discharge involves the peopling of space with flying molecules.

The measure of competency of the interior to supply an atmosphere is obviously a critical question. Unfortunately we are almost entirely without specific quantitative data bearing on the subject. We know that there is not a little atmospheric material in the interior as demonstrated in volcanic action and in the content of the minute pores of the hypogene rocks, but we do not know how far this was derived from the surface. If the moon never has had an appreciable external atmosphere its explosive eruptions were not due to surface infiltration and the implications of its numerous and vast craters are very suggestive. We can also draw inferences from meteorites which sometimes contain several times their volume of gases, as well as solid matter susceptible of conversion into atmospheric constituents. But at best we can only form very vague quantitative notions. On the other hand, we are liable to overestimate the amount required. The atmosphere and the ocean combined are little more than $\frac{1}{5000}$ of the mass of the earth. To be competent, the ingathered matter need therefore only contain about $\frac{1}{50}$ of 1 per cent. of atmospheric and aqueous material, plus an added factor for what may have been lost and what still remains in the interior. This percentage does not seem large enough to render the hypothesis improbable in the present state of knowledge.

The competency of self compression to generate the internal heat of the earth is also a critical question already touched upon. Estimates made by different methods seem to give an ample supply. The safest seemingly is that of Mr. Moulton who simply computed the energy that would be required to lift the matter of a homogeneous earth of 5.6 sp. gr. against gravity alone to such a height as to give the whole a uniform specific gravity of 3.5. This is more than the present specific gravity of the moon and is obviously extremely conservative. The fall of this matter was found capable of raising the whole mass (specific heat being taken at the over-figure of .2), to 6560° C., or about four times the average melting point of rock at the surface. If the original specific gravity be taken at 2. on a gross average, which seems much more probable than 3.5, when the supposed loose state of aggregation is taken into account, the possible temperature, if all the potential energy were converted into heat and retained, would exceed 13,000°. A portion of the energy might take other forms than heat and a portion would be lost concurrently, but as the heat was generated in the interior and must have been conducted to the surface very slowly, the secular loss must have been of the conservative order. On the other hand, tidal friction and possibly chemical action would add to the interior heat and more or less offset these sources of loss. On the whole, therefore, self condensation seems a competent source of internal heat unless the rate of aggregation was excessively slow.

Although aside from my central purpose, it may be remarked that the recognition of a progressive self-condensation of the earth from a loose aggregation to a more dense one by a prolonged and still incomplete process presumes a degree and quality of shrinkage peculiarly suited to explain the inequalities of the earth's surface. An explanation must be found not only for the mountainous wrinklings of the crust in post-Cambrian times and the great crumplings and crushings of the Archæan ages, so much neglected, but also for the great continental elevations and their superposed plateaus, and the deep oceanic depressions with their abyssmal anti-plateaus — phenomena with which current hypotheses have struggled so unsatisfactorily. It is also necessary to find an explanation for the unequal distribution of densities which have been partially revealed by gravity observations, but which are more broadly suggested by the unsymmetrical aggregation of the hydrosphere. The total shrinkage of the earth from first to last, under the hypothesis here proposed, would perhaps be sufficient to reduce its volume as much as one-half or even more, this, of course, depending on the original density. While the most of this contraction would antedate known geological history, the process can scarcely be supposed to have been complete in pre-Cambrian times, or even to be complete now. A part of the condensation must, therefore, quite certainly have fallen within geological history, and a part must remain yet to be accomplished, for, in addition to the retardation of the process of condensation caused by the heat generated, by the rigidity of the outer rocks and by the rapid rotation of the sphere, the maximum condensation of the mass could only be attained by means of a general rearrangement of the heterogeneous material of the meteor-built globe through the agency of diffusion, segregation, re-combination, re-crystallization and other processes which aid in giving a maximum compactness to mixed material. This internal readjustment must necessarily have been a slow process if the globe has been solid throughout its entire history, and must doubtless be yet incomplete. This progressive rearrangement of internal material adds a special agency of contraction to loss of heat, change of rotation and similar processes now recognized and which would act under this hypothesis essentially as under the current view.

If we make the plausible assumption that a slow process of diffusion, differentiation, concentration and gravitative readjustment has been in progress throughout the whole history and is yet active, and that matter has crept up from the hot compressed center into the superficial parts where relief of pressure would cause liquidity, we seem to have an equally facile basis for the explanation of molten extravasations.

It may also be remarked that the acquisition of an atmosphere and hydrosphere at a moderate temperature when the growing earth reached a medial size introduced conditions congenial to life at a stage sufficiently anterior to the Cambrian period to satisfy the most strenuous demands of theoretical biology. Most of the restrictive arguments of Lord Kelvin and others lose their application under this hypothesis.

Returning to the atmospheric problem, it is to be remarked that the assumption of a limited early atmosphere may be entertained quite apart from the foregoing accretion hypothesis. Under the current hypotheses of the separation of the moon, whether by the annular mode of Laplace or the fission mode of George Darwin, great rotary speed and high temperature are assumed as necessary or probable conditions. We have seen that these seem to put the retention of the atmosphere in jeopardy. The balance of theoretical probabilities, as I now see them, favors the presumption that the atmosphere would have been greatly reduced under these conditions. There does not therefore seem to me any firm ground, even on current theories of the earth's origin, for insisting on the acceptance of the doctrine of a vast primitive atmosphere, as the great reservoir from which subsequent abstractions have been chiefly taken. I think we are free, therefore, to assume just such a Palæozoic atmosphere as the life and deposits of that time seem to imply, interpreted by the phenomena of today. Such an interpretation seems to me to indicate conditions not radically dissimilar to those of the recent geological ages; warm climates in high latitudes at times, colder climates in lower latitudes at times, moisture at times, aridity at times, and like oscillations. This view carries with it the necessary corollary that the atmosphere has been supplied by accessions in some near proportion to its losses. That additions have been made to the atmosphere of vital importance is a familiar doctrine, but it is here pressed to an unfamiliar degree.

If we push the doctrine thus far it is important to assign causes for the fluctuations of supply and exhaustion of the atmosphere, to give the doctrine a working form and to devise means of putting it to the test. Concerning external sources of enrichment we know so little that we can scarcely say that there is a leaning of probabilities either toward or against practical uniformity. The internal supplies were probably correlated in some measure with igneous extravasations—not that such extravasations were the sole mode of liberating gases, but that other modes probably worked concurrently with them. The escape of gases was probably also correlated with crustal movements, especially those that compromised the continuity of the surface rocks, particularly the profound crushings which mining and the microscopic study of the hypogene rocks reveal. In these phenomena therefore, may be found a rational basis for inferring the times of probable atmospheric enrichment. Formulated as a proposition, it may be postulated that special enrichment coincided with special igneous extravasation and crustal disruption, taking the earth as a whole. The supply may be assumed to have been uniform in so far as these and other means of liberation were on the average uniform.

The phases of depletion are susceptible of more satisfactory treatment. In the first place, the depletion was differential. The loss of nitrogen was doubtless slight, because of its chemical inertness, and hence, though the supply may have been small, the nitrogen grew to ultimate dominance. The depletion of oxygen through the alteration of surface rocks was notable, but less than that of carbon dioxide. As a result the latter became *the minimum factor of the atmosphere and the critical one.* The enormous reserve supplies of water rendered its consumption inconsequential.

In the second place, the depletion was conditioned upon the exposure of the surface rocks to atmospheric alteration. This in turn was conditioned upon topography. In stages of elevation the water table of the land is depressed and the zone of atmospheric penetration is deepened. At the same time the zone of effective penetration of aerated water below this is also deepened. Hence the alteration of the rocks is promoted. In stages of low elevation — stages of baseleveling, for example — the zone of atmospheric penetration is shallow and rock alteration proceeds slowly. From this may be deduced the law that during stages of depression or baseleveling, depletion proceeded slowly. The aggregate surface must, of course, be considered.

To apply this law, let us assume for the moment, a uniform supply equal to the *average* rate of exhaustion. With the inauguration of any great epoch of general uplift there would begin an era of relatively rapid atmospheric exhaustion, which would proceed continuously during such elevated stage and might result in notable atmospheric impoverishment, as the computations cited early in this paper show. As the cutting down of the surface approached baselevel, the depletion would be retarded and, the supply continuing the same by hypothesis, the rate of exhaustion would fall below that of supply and an epoch of enrichment begin. A second elevation would re-inaugurate the depletion, and so oscillations of enrichment and impoverishment would follow the general oscillations of the land surface.[1] Applying this law by itself, atmospheric poverty should follow *at some distance* the stages of general elevation, and, on the other hand, atmospheric enrichment should follow at some distance the stages of baseleveling or depression.

But the assumption of a uniform average supply needs revision. In the main the igneous extrusions and crustal disruptions that are presumed to favor gaseous emanation probably fell in with the initiation of the elevated stages that favored depletion. In a general way the curves of supply and of depletion ran together in geological history and gave a measurably

[1] More strictly, the oscillations of that part of the land surface whose rocks consumed the atmosphere by their alteration — in general terms, the crystalline areas. Periodic general elevations followed by general baselevelings or some notable approach to baselevelings, are here assumed. It would be obligatory to state the grounds for this in an ampler discussion, but the all too narrow limits of this paper make this impracticable.

constant atmosphere, but their occasional failures to run in consonance are herein assigned as possible causes of exceptional climatic episodes, for it is almost axiomatic to say that climatic changes would attend changes in the constitution of the atmosphere. I assume that atmospheric poverty, especially in the critical item of carbon dioxide, is correlated with low temperature, as urged by Tyndall and others.

It is impossible here to attempt to apply the doctrine in detail to geological history. But it may be noted in passing that the Pleistocene glaciation followed at a notable interval the formation of the great plateaus and epeirogenic uplifts of late Tertiary times. The glaciation of India, Australia and South Africa occurred about the the time of the crustal revolutions that marked the close of the Palæozoic era. The uncertainty of the homotaxis of the strata involved makes a precise correlation at present impossible. The glaciation perhaps came too early to fit the hypothesis.[1] Here, at least, is an excellent chance to put it to trial. All other hypotheses of glaciation have fared badly when brought to the supremely severe test of the ancient oriental low-latitude glaciation, and if this hypothesis shall follow them to the junk shop of broken down theories it will find an already beaten path. The glaciation of northern Norway as determined by Reusch and Strahan succeeds the pre-Cambrian stage of elevation, but in what precise relations is not known.

The great extensions of warm climate to the high north appear to be associated with baseleveling periods in a general way ; but whether in a specific connection of sufficiently declared nature to indicate the relation of cause and effect remains to be determined.

Another source of atmospheric depletion needs recognition. Dr. S. W. Johnston is responsible for the opinion that the entire carbon dioxide of the atmosphere would be removed by the present annual growth of vegetation if there were no return through decomposition and animal life provided it were continued uni-

[1] It may fall under the organic factor of the hypothesis mentioned later.

formly for one hundred years.[1] Animal life, however, makes such nearly complete returns that the permanent loss is usually regarded as negligible. Nevertheless it is something. In certain stages of the world's history it has been important, as the coal beds testify. The loss in the Carboniferous age has been held sufficient to remove a noxious excess from the early atmosphere. On the same basis it might be held to cause serious depletion in the absence of the excess. It is necessary at least to consider whether, under the theory of a limited early atmosphere, conditions which restrain the animal factor of the organic cycle may not so far impoverish the air as to seriously affect climate. But this cannot be entered upon here. The organic cycle is very sensitive and very rapid in its action. It would naturally be greatly influenced by the topographic conditions which were concerned in the supply and exhaustion of the atmosphere, and lend to them either its concurrent or its counteracting influences.

It is now a little more than fifty years since Tyndall suggested that the periods of terrestrial glaciation might be dependent upon the carbon dioxide of the atmosphere whose peculiar competence to retain solar heat he had demonstrated. The suggestion of the origin of glaciation through the depletion of this atmospheric constituent is, therefore, not at all new. It has been entertained by others than Tyndall. If it has failed to find much acceptance this has perhaps been partly from a doubt as to its adequacy and partly from the lack of any definitely assignable cause for the requisite intermittent depletion. Dr. Arrhenius has recently contributed to the subject a most important discussion bearing especially upon the former point.[2] By an elaborate mathematical analysis of data derived from Langley's experiments he has endeavored to ascertain what degree of depletion of the carbon dioxide of the present atmosphere would bring on the conditions of Pleistocene glaciation, and, on the other hand, what degree of enrich-

[1] How Crops Feed, p. 47.

[2] Svente Arrhenius, Phil. Mag. S. 5, Vol. XLI, No. 251, April, 1896, pp. 237–279.

ment would produce the warm climate of the Tertiary. He arrives at the conclusion that the removal of 38 to 45 per cent. of the present carbon dioxide would bring on glaciation and that an increase of 2.5 or 3 times its value would produce the mild temperatures of the Tertiary times. He quotes the opinion of Professor Högbom in support of the competency of earth changes to produce this depletion, and also the competency of the interior and other sources to re-supply the impoverished atmosphere. He, therefore, carries the suggestion of Tyndall and others a very notable step in advance, and, what is especially important, has given it quantitative expression on the basis of deductions from observed data. He does not, however, postulate the conditions which control the enrichment and depletion of the atmosphere which has been the essential endeavor of this paper.[1]

But we do not meet geological demands when we simply offer *general* explanations of climatic changes. Our theories must ultimately be found to fit the precise phenomena. How are we to explain the profound glacial oscillations? Here is where existing hypotheses are put to the stress and our atmospheric hypothesis seems at first thought even less adaptable to the phenomena than most others. If we could deny that the oscillations were profound, as some do, it would be convenient. But I fear we cannot. We may appeal to variations of atmospheric supply, to the precession of the equinoxes, etc., but field experience leads me to doubt whether these will fully fit the phenomena, though they must doubtless be reckoned as factors. I have endeavored to follow out the doctrine of atmospheric gain and loss on its own lines, and although the studies are incomplete, the results are at least encouraging. I seem to find a rhythmical action that may in part explain the glacial oscillations. To do it justice it should have elaborate and careful statement, but I can here only suggest its nature in bald outline

[1] I may here remark that the main features of the ideas herein advanced were entertained and expressed to my students some time before I saw Dr. Arrhenius' important paper, but I fear I might not have felt justified in giving them a more public statement but for the encouragement of his weighty opinion on the vital point of quantitative sufficiency.

and in terms that need qualification. The idea hinges (1) on the action of the ocean as a reservoir of carbon dioxide and (2) on the losses of the organic cycle under the influence of cold. Cold water absorbs more carbon dioxide than warm water. As the atmosphere becomes impoverished and the temperature declines, the capacity of the ocean to take up carbon dioxide in solution increases. Instead, therefore, of resupplying the atmosphere in the stress of its impoverishment, the ocean withholds its carbon dioxide to a certain extent, and possibly even turns robber itself by greater absorption, though the diminution of the tension of the carbon dioxide of the atmosphere as its amount is reduced tends to increase the discharge of carbon dioxide from the ocean to restore the equilibrium, and, to the degree of its efficiency which is undetermined, offsets the increased absorption of the cold water. So also, with increased cold the process of organic decay becomes less active, a greater part of the vegetal and animal matter remains undecomposed, and its carbon is thereby locked up, and hence the loss of carbon dioxide through the organic cycle is increased. The impoverishment of the atmosphere is thus hastened and the epoch of cold is precipitated.

With the spread of glaciation the main crystalline areas, whose alteration is the chief source of depletion, become covered and frozen, and the abstraction of carbon dioxide by rock alteration is checked. The supply continuing the same, by hypothesis, reënrichment begins, and when it has sufficiently advanced warmth returns. With returning warmth, the ocean gives up its carbon dioxide more freely, the accumulated organic products decay and add their contribution of carbonic acid, and the reënrichment is accelerated and interglacial mildness hastened.

With the reëxposure of the crystalline areas, alteration of the rocks is renewed and depletion reëstablished and a new cycle inaugurated. And so the process is presumed to continue until a change in the general topographic conditions determines a cessation.

The rhythmic curve which represents these oscillations should have an increasing or declining amplitude, according to the advance or decline of the topographic conditions which control the depletion of the atmosphere. This brief sketch needs much elaboration and qualification, but as the studies are still in progress, and the paper has already transgressed the limits due the occasion, it must be deferred.

T. C. CHAMBERLIN.