by the speaker under the title "Evolution of the Falls of Niagara." R. L. FARIS, *Secretary*

THE ELISHA MITCHELL SCIENTIFIC SOCIETY OF THE UNIVERSITY OF NORTH CAROLINA

THE 176th meeting was held in the main lecture room of the chemical laboratory, Wednesday, February 12, 1908, at 7:30 P.M. Professor Collier Cobb addressed the society on "The Cause of Earthquakes in the Light of Recent Earthquake Action." The lecture was fully illustrated with lantern slides.

A. S. WHEELER, *Recording Secretary*

DISCUSSION AND CORRESPONDENCE

SCHAEBERLE, BECKER AND THE COOLING EARTH

TO THE EDITOR OF SCIENCE: Professor Schaeberle is certainly a bold man when, in your current number (March 6, 1908, p. 392), basing himself on his method of observing stellar temperatures, he would upset modern astronomy with one hand, and make the sun the center of the sidereal cosmos, and with the other would upset most modern theories of geological climate! I hardly think that Borrell, in the current number of the *Journal of Geology,* Huntington, in the current volume of the Geological Society of America, or most of the speakers at the Geological Congress in Mexico, will at all agree that Manson's hypothesis is "demonstrated as a true theory"! They will agree with Chamberlin's strictures. But one can not yet go into further criticism, save to urge those of your readers who are neither geologists nor astronomers not to accept Professor Schaeberle's *ipse dixit,* but rather await the demonstration which he promises "later on."

The object of this letter is rather to call attention to the bearing which his work has on that of Becker[1] on the cooling earth. Since Becker has kindly undertaken what I had thought to do myself, when I had just a few more facts, a few comments as to the applicability of his conclusions may be ventured.

[1] SCIENCE, February 7 and March 8, Vol. 27, pp. 231, 232, 392.

Though Becker's concise method of deriving his formulæ (2) is not beyond criticism mathematically, the same may be derived from Riemann's and Byerly's[2] more general and rigid treatment. But Becker's discussion of his formula obscures a most important limit to its application, to wit, the *temperature must remain constant at the surface of the cooling body, which he assumes to be the rock surface.* Otherwise the solution applicable is that given by Byerly (*loc. cit.*) on page 88, following Riemann.

If we assume the temperature of the atmosphere at the surface to have varied appreciably, and especially if we assume that its temperature depends on that of the earth, as Schaeberle says is largely the case (that two thirds of its temperature is due to interior heat), Becker's solution is inapplicable in the form he gives it. In fact, if two thirds of its heat comes from the ground *now,* originally, at the time "hell froze over" and the waters above the earth were separated from those under, must not the temperature of the atmosphere near the earth have been much hotter and much nearer that of the freshly consolidated rock than Becker assumes? Must not the waters of the ocean have been then largely up in the air and so the blanketing effect and the atmospheric pressure much greater? If so, Becker's conclusions are utterly useless. For his fundamental formula may be thus worded:

$$\left(\frac{V = \text{original excess of surface rock temperature over atmospheric}}{\text{present geothermal gradient} - \text{original gradient}}\right)^2 = 22/7 \times \text{diffusivity} \times \text{time elapsed}.$$

Now, Becker estimates the numerator as something like 1,300° C., apparently assuming this as the fusion point of a fairly silicious rock, and the atmospheric temperature at 0° C.? But all my work with grain indicates lower consolidation temperatures for the acid than the basic rocks, the former being in the state of aqueo-igneous fusion of a sugar syrup at 150° C. Moreover, as Day and Co. have shown, quartz will not crystallize above

[2] "Fourier's Series and Spherical Harmonics," p. 84.

800° C. A fundamental crust of the earth without any original quartz would be a geologically novel conception. The atmosphere above, being vastly heavier perhaps than at present, should certainly be at over 0° C. I should guess very much over 100° C., and it would seem that Becker's 1,300° might well be cut in two, if, indeed, there were any sudden jump between the atmospheric temperature and the rock surface temperature.

Now, when we come to the denominator, he fixes the original gradient, which will still persist below the relatively thin rim of the earth that has cooled appreciably, so that the earth may remain rigid. That seems reasonable, and is practically the hypothesis of Chamberlin and Lunn. But with regard to the present geothermal gradient, Becker's work seems defective in that he has made no attempt to get his geothermal gradient and diffusivity for the same rocks, though they depend on each other. The diffusivity in dry sand and peat is very low and the gradient proportionally high. Moreover, most determined values of the geothermal gradient are in that "ragged pellicle" of detrital matter which he would leave out of consideration. On this account alone, as well as the fact that they may occupy several thousand meters, out of some 114,000 meters discussed, the omission is a serious one. Thus to "abnormally high diffusivity" as a possible cause of deceptively low gradient we may add depression under a load of sediment, downward water circulation, endothermic reactions.

It so happens that in our Michigan copper mines we have an unusually good chance to coordinate gradient and diffusivity, for:

1. There has been but little net erosion or deposition since a very early date.

2. The rocks are very uniform in character. The average of thousands of feet of diamond drilling shows only about 7 per cent. of sediments in the main range. The traps are quite uniform in character, mainly ophitic auvergnoses.

3. Sulphides are rare and reactions that generate much heat are not present.

4. The gradient is known for an unusual depth.

5. The conductivity of the three main types of rock has been determined by Professor B. O. Peirce, of Harvard.[3] The mean conductivity of the formation is about .0035, the diffusivity .0064 in c.g.s. units, or in Becker's units the meter and year about 20.

The rate of increase of temperature throwing out the first 100′ of drift[4] is 1° F. in not over 110′; 107′ seems better, *i. e.*, 1° C. in 59 to 60 meters.

Varying diffusivity, if the same depends merely on the depth, will be no great hindrance. We need merely to peel the earth up into layers like an onion, each of which shall offer the same resistance to the transmission of heat. They will be of unequal thickness really, but we shall have to call it for each of them an equal increment of z, z being a new variable in terms of which to express the flow of heat. Then we shall have to express x also in terms of z, x being the real distance from the surface or some other place of reference. We can then use all the old diagrams of Kelvin, King and Becker, but have to change the divisions on the scale corresponding to the earth's radius and make them unequal.

Becker is also quite safe in neglecting the curvature of the earth. The cooling of a sphere has been treated by Woodward,[5] and, as I have elsewhere remarked,[6] if we represent by V_m and u_m the temperatures, respectively, in an infinite slab with plane sides at a distance (c) apart and in a sphere (radius c) cooling under certain similar conditions, at points, in the one case at a distance x from the plane face such that $x/c = m$, and in the other case at a distance from the center such that $u/c = m$, then

$$V_m = mu_m + 1 - mu_{1-m}.$$

Now, for points near the surface $m = u/c$ is

[3] *Proc. Am. Acad. Sci.*, May, 1903, XXXVIII., No. 23, p. 658.

[4] See discussions in my reports, Board of Geol. Surv. of Mich., 1901, pp. 244 ff., 1903, pp. 195 ff.

[5] R. S. Woodward, *Annals of Mathematics*, Vol. III., 1887, Eq. 10.

[6] Geological Survey of Michigan, Vol. VI., Part I., p. 121.

near 1, while $1-m$ is near 0. Also, u_{1-m} is for a point near the center, which will not have cooled appreciably and its rate of change $D_t u_{1-m}$ will be 0. Thus V_m approaches u_m as $m=1$.

It would be quite a help if Becker would compute the general solution of the problem as given by Riemann and by Byerly[7] for a select lot of plausible hypotheses, including one which shall include the surface temperature, becoming from time to time 0°, when there are glacial periods.

The only way that I can at all see of using Kelvin's method which considers the surface kept at a constant temperature, is to include the atmosphere, and let the temperature be that of space, which is $-270°$ C. *If*, then, we can assume that the effect of atmosphere, ocean and pellicle of sediments amounts to that of a very narrow contact zone, a questionable yet plausible assumption, *if* we may consider the present surface gradient as uniform for an equivalent distance beneath the surface, after a short time rock and contact zone cool together as one mass and we may apply the treatment I applied to that case,[8] and we may find what kind of a jump in temperature at the surface would have produced the gradient that we have in a given number of years. But that temperature must be reckoned *from the temperature of space*. Using 100,000,000 years as the age of the earth and the Calumet gradient and diffusivity, we have

$$\frac{V^2}{(.0168+—.00675—)^2}=\tfrac{22}{7}\cdot 20\cdot 100{,}000{,}000$$

$$=(79500)^2.$$

$V=1{,}320°$ above the temperature of space $=1{,}150°$ C.

We may, therefore, if we like, assume that one hundred million years ago the earth was shrouded in an atmosphere whose temperature rather suddenly increased from that of space to a little above the critical point of water near the rock surface, and that the rock surface or a few feet below was just below the melting point of diabase. This looks quite reasonable to me.

But if we want the earth only 60 million years old we can get it if we are willing to assume a granitic crust solidifying under an atmosphere with an enormous water pressure at the modest temperature of 345° C. This also looks good.

After all, however, what reason have we to believe there was ever any such sudden jump in temperature at the bottom of the atmosphere as any application of Kelvin's method of finding the age of the earth must assume? There would be none on the planetesimal hypothesis nor on the crenitic hypothesis, nor others we might frame. Yet unless there was some such jump the gradient so far as the mere cooling of the earth is concerned *is just what it was in the beginning*,[9] subject, of course, to accidents of water circulation, volcanic activity, etc. That, in fact, the gradient was not greater than at present in very early times might be inferred from the fact that rocks seem to have been buried to as great depths without metamorphism then as now. As soon as a layer largely iron was reached the diffusivity would increase and the rate of increase of temperature decrease. If that iron layer was at different distances from the surface in different parts of the world, but everywhere at about the same temperature, the rate of increase of temperature from the surface to it would, of course, vary.

The geothermal gradient would then depend upon the diffusivity and the thickness of the crust. Rocks would, with the copper country gradient of 1° C. in 60 meters, attain a temperature of 2,000°, that of some gabbros, in 120,000 meters, which is about the thickness of the crust Becker assumes, if the rocks remained of uniform diffusivity.

Until then we can throw out these latter suppositions by showing some geological signs of a higher rate of increase of temperature in early geological times, such as that rock of the same kind was hotter, or the crust subject to folding and fracture thinner, speculations on the age of the earth based on the

[7] Fourier's Series, last equation on p. 88.

[8] Annual for 1903, equations 15 and 20, pp. 213 and 214.

[9] If the numerator $V=0$ in the equation above, the denominator must be so also, unless $\tau=0$.

geothermal gradient will be mere speculative hypotheses.

Alfred C. Lane
Lansing, Mich.

SPECIAL ARTICLES

THE REDISCOVERY OF A LOST ART AND A FEW NOTES ON THE THEORY OF THE VIOLIN

Shortly before the discovery of America they were using, on the gondolas in Venice, a perfectly transparent, lustrous, orange-red varnish. It is reasonable to assume that the great beauty of this flame-colored material, on the handsome figured wood used, prompted the subsequent lavish extravagance in their decorations, that almost ruined the owners and was so universal, that in the sixteenth century a sumptuary edict was passed by the grand council compelling the use of black only on all gondolas. The principal use, then, for this lustrous varnish having been done away with, the price fell to a point where the cabinet-makers and others could use it for certain purposes.

The historical ceremony, "The Wedding of Venice to the Adriatic," has been preserved to us on canvas by a painting of this gorgeous scene made at the time, in which can be seen the color of this varnish on the hull of the royal gondola. The varnish itself can be seen on the wood of an old figure-head of one of these boats (preserved in the museum) where the black paint has been chipped off. The cast-off varnish had not long to wait for a market, as the violin came into existence at this time, and the now cheap varnish found immediate favor with the violin makers of Italy, and was used exclusively by them until the supply at Venice was exhausted, about the year 1730.

Tradition has it that a Venetian varnish dealer, in reply to solicitations from Cremona on the subject, said: "My supply is exhausted, I know not what it is, nor where it came from."

It is possible that this inquiry came from Stradivarius himself. If so it might account for the much more sparing coats of varnish he put on his violins at this time, than earlier —he may have already begun to husband his supply.

Italian furniture of the seventeenth century, still extant, has varnish of this character upon it, but since about the time when the old Cremona violin varnish was last used, diligent investigation fails to find any article whatever with this material upon it.

By carefully comparing the physical properties of the varnish on any of these older articles, with that on a Cremona violin, a striking similarity is at once seen, and no effort is necessary to conclude that the two are identical. The materials, then, must have been imported into Venice, as no colored gums or resins of this texture are produced in Italy, they being certainly of tropical origin. The proximity of Africa to Italy naturally suggests the source of supply of these gums. (Subsequent experiments, with African gums, produced a red varnish not to be distinguished from that on a 1715 Stradivarius.)

Certain characteristics of this varnish are known, and how these affect tone has been carefully tested, and the most distinguishing feature seems to be that it damps out the upper harmonics, leaving the pure fundamental tone to be heard.

In drawing the bow across the strings of any violin, a certain fairly constant fraction of the energy is transformed into sound—a portion of this is carried by the fundamental tone, while the rest goes into the upper harmonics. Now, the preponderance of these upper harmonics gives to a violin its harshness. If a large proportion of the total energy is dissipated in these higher harmonics, the amount of fundamental tone reaching the hearer will be small; if, on the other hand, the large proportion of the total energy is forced into the fundamental, the instrument will have great carrying power.

Had it not been for this compound, known as the old Italian varnish, the world would not have heard of the town of Cremona, nor of her sons Amati, Guarnerius and Stradivarius, and in all probability the violin itself would have passed out of existence, after a very brief experimental stage, like most other musical instruments of these early times, such as lutes, lyres, gigues, crwths, etc.

The writer, after a great deal of experi-