PROCEEDINGS

OF THE

American Philosophical Society

HELD AT PHILADELPHIA

FOR

PROMOTING USEFUL KNOWLEDGE

VOLUME XLIX

1910

PHILADELPHIA
THE AMERICAN PHILOSOPHICAL SOCIETY
1910

PRESS OF
THE NEW ERA PRINTING COMPANY
LANCASTER, PA.

PROCEEDINGS

OF THE

American Philosophical Society

HELD AT PHILADELPHIA

FOR PROMOTING USEFUL KNOWLEDGE

VOL. XLIX. JANUARY–APRIL, 1910. NO. 194.

CONTENTS.

PHILADELPHIA
THE AMERICAN PHILOSOPHICAL SOCIETY
104 SOUTH FIFTH STREET
1910

Members who have not as yet sent their photographs to the Society will confer a favor by so doing; cabinet size preferred.

It is requested that all correspondence be addressed

To the Secretaries of the

AMERICAN PHILOSOPHICAL SOCIETY

104 South Fifth Street

Philadelphia, U. S. A.

PROCEEDINGS

OF THE

AMERICAN PHILOSOPHICAL SOCIETY

HELD AT PHILADELPHIA

FOR PROMOTING USEFUL KNOWLEDGE

VOL. XLIX | JANUARY–JUNE, 1910 | NO. 194

PHOTOGRAPHIC OBSERVATIONS OF DANIEL'S COMET.

(PLATES I–XXV.)

BY E. E. BARNARD.

(*Read April 25, 1908.*)

It is such a long time since one has had the opportunity of seeing a large comet that the sight of this beautiful object suspended in the quiet summer morning skies with its slender graceful tail streaming upwards into the night, was something long to be remembered. It was a very impressive picture and those who were fortunate enough to see it at its best must have been struck with its quiet and majestic beauty. This was specially the case for a few mornings in the middle of August when the moon was absent, and as late as the first week in September when, though very low in the east and visible only for a few minutes before dawn killed it, the tail could be traced for a distance of fifteen degrees or more.

This comet was discovered by Mr. Zaccheus Daniel at Princeton, N. J., on 1907, June 9. Though it proved to be one of the brightest comets that have appeared in the past twenty-five years, it was in some respects a disappointing object—disappointing only, however, in the want of new phenomena. It was visible to the naked eye for two full months. At one time its tail attained a length of twenty-five degrees. Shortly after perihelion passage—when last seen in the morning sky—the nucleus was as bright as a first magnitude star. Singularly enough, the comet developed its most interesting changes a month or more before perihelion passage. When near perihelion, which occurred September 3, there were few changes in its appearance from morning to morning. At that time there seemed to be a uniform unbroken flow of the tail-forming particles, so that what streams there were, were not individually prominent or striking.

In the second half of July separate streams of matter were frequent and formed a most interesting feature of the tail. These were specially beautiful on July 17 and 19. On the first of these dates the tail, where it joined the head, was made up of some five broad, diverging streams, which gave it a splendid and symmetrical appearance. This is really the handsomest photograph I have ever seen of a comet.

Comparatively few observatories obtained photographs of this comet, which was a great pity, for it was worthy of far more attention from a photographic standpoint than it received. Several, however, succeeded in getting results that are important. Excellent photographs were obtained by Mr. W. A. Cogshall at the Kirkwood Observatory at the State University, Bloomington, Indiana, with a small reflecting telescope made by himself. Though these, from the limitations of the reflector, do not show a great length of tail, they are specially beautiful and valuable for the structural details. A good series was also obtained at Greenwich. Dr. Max Wolf secured some specially valuable photographs with the 30-inch reflector, whose large scale showed the tail near the head, on several dates, to be made up of a great number of thin rays.

An excellent series of photographs of the comet was made by Mr. Duncan at the Lick Observatory. Though the time interval between these last and those of the Yerkes Observatory is roughly only two hours, there are decided changes shown in the tail when these pictures are compared with those made at the Yerkes Observatory. Unfortunately the changes in the comet are such that there are no definite markings that can be measured on the photographs to determine the motion of the tail-producing particles, with perhaps one exception—that of July 11. There are twelve dates that are common to the Lick and the Yerkes plates. Several of the Yerkes photographs show very little on account of clouds and thick sky on the dates in question.

Another fine series of photographs of the comet was made by M. F. Quénisset of M. Flammarion's observatory at Juvisy, France. The interval of some six hours makes this series specially valuable for comparison with plates taken in this country. I am greatly indebted to M. Quénisset for enlarged prints from eleven of these pictures. Out of these, there are eight dates which were duplicated at the Yerkes Observatory. A comparison of these photographs is of extremely great interest, and though there is but little material from which to accurately determine the amount of motion, progressive outward displacement, especially in the streamers, is strongly shown. A study of these photographs clearly shows how uncertain it is to connect the details of any two dates. Of course a disturbance may extend over several days and the matter from it still be visible, but any particular detail would not probably live through from one date to another. In some of M. Quénisset's photographs the change has been so great that it is almost impossible to be sure of the same features six hours after. What is quite evident, however, in the comparison, is that the structure of the tail (the streamers) has a decided outward motion as a whole; at the same time there is a diffusion effect that constantly tends to destroy the details.

Some of these comparisons follow:

July 19, Juvisy Plate.—There is a principal narrow ray that separates into two rays some distance out. A dark space intervenes between it and a broad streamer south, whose north edge is very definite. There is a very decided change shown in the Yerkes photograph. Two new short rays have appeared on the south side. The north ray has become broad and diffused and irregular. The changes are so striking that one can hardly be sure of the same features, though there is a general resemblance.

August 11.—In the Juvisy picture there are four distinct rays. The two middle ones diverge from a point close to the head. These two are clean cut in the Yerkes plate. The south one has become much brighter and more definite. Their junction has bodily moved outward for quite a distance. The north ray of the four has closed in on the one close south of it. A broad light region on the south edge of the northern of the two middle rays has drifted outwards and is less marked.

August 14.—There is one broad widening stream in the Juvisy photograph, with two lesser ones symmetrically placed on each side. In the Yerkes plate there is a general resemblance to the other; though the tail is made up of three broad streamers, they are much further out. It looks as if the three had drifted out and fused together more or less. The whole system of tails has bodily receded from the comet.

August 19.—In the Yerkes plate the head has become relatively smaller. The tail has spread out very greatly, especially on the south side. There is less structure than in the Juvisy plate.

August 20.—In the Juvisy plate a principal ray divides to the north and joins a dark space behind the head. In the Yerkes plate this ray and dark space have both moved outwards. The head and neck are also narrower.

On July 11 (which date we will treat specially) a bright condensation $1\frac{1}{4}°$ back from the head is strongly shown on both the Yerkes and Lick plates and can be seen on the Juvisy plate, but it is faint and cannot be located with very great accuracy on this last picture. A plate made at the Lick on July 10 seems to show this same object somewhat nearer to the comet or about $\frac{3}{4}$ as far out as on the eleventh. It is noticeable on all three photographs of July 11 that this condensation was receding from the comet, at the same time that it was following slowly towards the sun. From the appearance I am inclined to think that it is the same object which is visible on the plate of July 10. If so, then it must have left the comet on or about July 7. Between this condensation and the head of the comet on July 11 the tail is very faint but continuous. In reality this mass is the near end of a bright strip of the tail about 3° long. The object on the Lick plate of July 10 is joined to the head by a bright, strongly defined connection, of which the condensation is only an inconspicuous part. In the interval between July 10 and

11 the mass (if the same) had increased its distance from the head by about 20′. In the meantime it had drifted sunward 1° 10′—following in the direction of the comet's motion. It is probable that this was due to its original motion when a part of the comet, and that if its existence had been permanent enough, the motion would have become one of recession from the sun, but it rapidly dissipated before other photographs could be made of it.

With the aid of the BD charts I have taken off the following positions on the photographs of July 11:

	Position of the Head 1855.0.	Position of the Condensation 1855.0.
Juvisy,	$1^h\ 48^m.05 + 8°\ 12'$	$1^h\ 44^m.0 + 7°\ 54'$
Yerkes,	$1^h\ 49^m.50 + 8°\ 19'$	$1^h\ 45^m.0 + 7°\ 53'$
Lick,	$1^h\ 50^m.05 + 8°\ 18'$	$1^h\ 45^m.0 + 7°\ 54'$

The position angles of the mean axis of the tail on this date are:

Juvisy,	P. A.	251°.5
Yerkes,		251°.5
Lick,		251°.0

If one should take the brighter, long part of the tail, independent of the head, the axis of it would pass a little north of the head.

The following positions were taken off by the aid of the BD charts on the plates of July 19.

Juvisy.—Position of the head 1855.0 $2^h\ 40^m.50 + 11°\ 29'$. Position angle of the middle long, bright branch of the tail 249°.2. The main or central branch separates at 37′.7 back of the head. The south branch of the tail is 5° less in position angle than the middle one.

Yerkes.—Position of head 1855.0 $2^h\ 42^m.67 + 11°\ 33'$. Position angle of main branch of the tail (n. of 2) 253°.5. The south one was in P.A. 250°, but was irregularly curved.

Lick.—Position of head 1855.0 $2^h\ 42^m.50 + 11°\ 32'$. Position angle of main and largest branch 252°.9.

The following are the positions on August 11, derived from the charts.

Juvisy.—Position of head 1855.0 $6^h\ 4^m.70 + 17°\ 23'$. Position angle of south ray 257°.0. Position angle of north ray 270°.0; not extreme north ray.

Yerkes.—Position of head 1855.0 6^h $7^m.10 + 17°$ 23′. Position angle of south ray 259°.0; assuming center of head as origin. Position angle of north ray 269°.0; assuming center of head as origin.

On a number of mornings I carefully examined the comet with the 40-inch telescope and its 4-inch finder. In the great telescope the view was not satisfactory because of the very small field — 5½′ of arc. It showed the nucleus, however, and part of the head very well. The view in the finder was very much more satisfactory, but even this was a disappointment. The nucleus and head and part of the tail were very beautiful. The soft nebulous light of the comet with the bright yellowish star-like nucleus imbedded in the head made a very striking picture. But there were no details visible in either the head or the tail. The streamers which were shown on the photographs at about the same time could not be seen. Viewing the comet thus and then afterwards seeing the photograph of it, impressed one greatly with the value of photography in dealing with these objects. I think most of the phenomena of this comet would have passed away unknown had it not been for the photographic plate.

Notes on the Appearance of the Comet with the Naked Eye, with the 5-inch Guiding Telescope and with the 40-inch and its 4-inch Finder.

July 15.—The comet was visible to the naked eye as a hazy star of the fourth magnitude. It was decidedly brighter than the Andromeda nebula, but much smaller. It was ½ magnitude brighter than the star 3° east of it, BD + 9° 316 (1855.0 2^h 17^m $3^s.0 + 9°$ 57′.9 $5^m.7$). While guiding it seemed to fade for short intervals—perhaps this was due to thin patches of clouds, though I could not see any clouds.

July 17.—Bright to naked eye. It was 3½ magnitude. Very much like a considerable hazy star. Could faintly see a very slender tail for 5° ± which passed several faint stars 4° from the head. The comet was ½ magnitude or more brighter than the fourth magnitude star, BD $+7^\circ$ 388 (1855.0 $2^h\ 20^m\ 27^s.7\ +7^\circ\ 48'.4\ 4^m.5$), $4^\circ\pm$ south of it. It was brighter than any of the stars near.

July 18.—To the naked eye, when best seen—visible only through gaps in clouds—the head was third magnitude. It seemed to be brighter than on the seventeenth.

July 19.—I am sure there were frequent fluctuations of the comet's light to the extent of about one magnitude. To the naked eye the comet was $3\frac{1}{2}$ magnitude. At best it was $\frac{1}{2}$ magnitude brighter than the naked eye star, BD $+9^\circ$ 359 (1855.0 $2^h\ 37^m\ 6^s.0\ +9^\circ\ 29'.9\ 4^m.0$), 3° s.w. of it. Could see faint suggestions of a tail. Good sky.

July 28.—$15^h\ 45^m$. The head was conspicuous, like a hazy star, notwithstanding a gibbous moon. In the finder of the 40-inch I could trace the tail faintly across the field (2°). There was a bright stellar nucleus of about the sixth magnitude. In the 40-inch the nucleus was very bright, but not stellar. The head filled the field of view ($5\frac{1}{2}'$ power 460). There seemed to be a shadow effect behind the nucleus—away from the sun.

July 31.—In spite of the presence of a half moon the comet was conspicuous, like a hazy star, 2° west of Aldebaran. I could see it with the naked eye as late as $16^h\ 3^m$.

August 1.—The comet was conspicuous, like a bright hazy star. It was the same brightness as δ^1 or δ^2 Tauri. Could not be certain of any tail. In the guiding telescope the nucleus was not so distinct as it was on July 31.

August 3.—It was conspicuous like a small 3 or $3\frac{1}{2}$ magnitude star. There were faint suggestions of a tail to the naked eye. It was not decidedly brighter [than on the first]. There was, of course, less moonlight than on the other morning. In the 5-inch the nucleus was not definite—only a central condensation. There seemed to be fluctuations in its light with the 5-inch and I think they were verified with the naked eye.

August 5.—To the naked eye the head of the comet was equal to ζ Tauri. The tail was about 15° in length and stretched out to within a degree or two of Aldebaran. At times I thought I could see it as far as Aldebaran. It would have passed south of that star and was fairly distinct. The comet was a conspicuous object to the naked eye.

August 6.—With the naked eye the head was as bright as ζ Tauri. Could trace the tail, which was conspicuous but not bright, as far as Aldebaran, where it passed south of that star; it was neither slender nor broad—and seemed to be straight. With the 40-inch the nucleus was not stellar but was bright and yellowish. It was blurred or ill defined in the direction of the sun—apparently spread out—while on the opposite side (away from the sun) it was quite definite wtih a darker space in the nebulosity. The head was much larger than the field of view. In the finder, the tail stretched away across the field. There was a sixth magnitude yellowish star in it, about $\frac{1}{2}°$ back from the head. There was no detail or structure in the comet as seen in the 4-inch finder. The nucleus was about the fifth magnitude and almost stellar. The tail was very slender. The edges were soft and roundish—like a cylindrical or conical body. It was very beautiful in the finder.

August 8.—To the naked eye the tail seemed to be almost the same as on the sixth and was not sensibly longer, but the head was brighter. The comet was $\frac{1}{2}$ magnitude brighter than ζ Tauri and about equal to θ Aurigæ. The sky was good. With the 40-inch the measured diameter of the nucleus at $16^h\ 7^m$ was $2''.49$. This gives a diameter of 2,580 miles. It was slightly yellow. There was a sharp outline several minutes long nearly straight, which passed the preceding edge of the nucleus and which bounded a much denser nebulosity following, in which the nucleus was immersed. The position angle of this definitely bounded nebulosity was 160°.6 (1) at $16^h\ 8^m$. In the finder the nucleus was stellar and bright. The comet was still faintly visible with the naked eye at $16^h\ 19^m$, but at the limit of vision on the dawn-lit sky.

August 9.—Sky not very transparent. The tail was not so conspicuous as on the eighth; the head seemed brighter, however. It could be faintly traced to a distance as great as that from ζ Tauri to Aldebaran (16°). The head was somewhat less bright than λ Orionis.

August 10.—Sky very good. To the naked eye the comet was bright for some 3° or 4° back from the head. The nucleus was visible to the eye as a star of about the third magnitude. The head was midway in brightness between that of ζ Tauri and λ Orionis. The tail could be traced faintly for at least 15°. It was pretty faint but when looked at with averted vision it could be seen fairly well for a distance equal to that from ζ Tauri to Aldebaran. The end of the tail just reached to BD + 15° 732 (1855.0 4^h 56^m $18^s.0$ + 15°12′.3).

August 11.—The tail, with the naked eye, could be traced to BD + 15° 732. The south edge would pass through that star. It was 1° or $1\frac{1}{2}$° wide at that point. Though faint, it could be seen quite well. It was straight and somewhat narrow. The nucleus was conspicuous as a star-like body in the head. The head itself was narrow. The tail was bright for 2° or more and then it faded out rapidly towards the end. The head was as bright as η Geminorum. In the 40-inch the nucleus was ill defined, and blurred into the brightness following. It was distinct at the preceding edge.

August 12.—Sky first-class. With the naked eye the nucleus was bright and stellar. It was about as bright as η Geminorum. The tail was perhaps a little brighter than before but rather feeble except near the head. I could trace it faintly nearly to BD + 15° 732. The head was as conspicuous as γ Geminorum, near and to the east, but the nucleus was much less bright than that star.

August 13.—The sky was very thick, and part of the time at first the comet was behind clouds.

August 14.—Clouds at first covered the comet. It then came out and was conspicuous about 2° east of γ Geminorum. When the comet and star came out of the clouds they were very much alike, but as they rose higher the stellar condition of the nucleus was much inferior to the star—say 1 magnitude less bright. The tail was straight and rather slender. For 5° back of the head it was pretty bright, then for the rest of its length it was faint. It could, however, be readily traced to 126 Tauri (Proctor's chart).

August 19.—It was bright to the eye—perhaps brighter than before. The tail could not be traced far—perhaps nearly to γ Geminorum. Sky very poor.

August 20.—With the naked eye the comet was a very graceful and beautiful object. The tail could be faintly traced to about γ Geminorum. The nucleus was star-like and bright.

August 21.—After the nearly full moon set, the sky was still affected by moonlight when dawn began. At $15^h\ 30^m$ or $15^h\ 40^m$ the comet was bright. The nucleus was bright to the eye and was perhaps of $2\frac{1}{2}$ magnitude. The head was not as conspicuous as γ Geminorum—but not much inferior to it.

August 22.—Full moon. The head and nucleus of the comet were conspicuous in spite of the moonlight. The nucleus was about $2\frac{1}{2}$ magnitude. The tail was noticeable or conspicuous for 3° or 4°. In looking in its direction one would have been impressed with its distinctness.

August 24.—Nearly full moon. Sky clear. The comet was conspicuous. Even in the bright moonlight I could see the tail for 4° or 5°.

August 25.—In clouds and haze.

August 31.—Sky good and clear. Crescent moon. The comet was fairly noticeable to the naked eye when its place was known. Could feebly trace the tail for 4° or 5°.

September 2.—It was conspicuous to the eye with a tail 4° or 5° long even in the strong moonlight. The nucleus was about 2 or $2\frac{1}{2}$ magnitude.

September 5.—The comet was very low but the head was bright. The tail, though not bright, could be traced for 14° as drawn on a star chart. It was long and straight and gradually faded out to the end. The sky was fairly good, but as dawn came up some masses of haze were visible in the east. It was estimated that, to the eye, the head and nucleus were about third magnitude. Very slender crescent moon near horizon.

September 8.—To the naked eye the nucleus was as bright as a first magnitude star. The tail could be traced 5° or 6° but partly hidden by clouds.

September 11.—The comet was very low. The nucleus was fairly distinct to the naked eye, but there was only a suggestion of a tail. It had faded very much since the eighth [due to its low position?]. Sky lit with dawn.

September 12.—I could not see it with the naked eye though I tried hard. Not bright in guiding telescope. Sky not pure. Strong dawn.

The photographs taken here of this comet were made with the 10-inch, the 6.2-inch and the 3.4-inch portrait lenses of the Bruce telescope of the Yerkes Observatory. The plates used were Seed 27 Gilt Edge. They were backed with a dark red paste made of burnt sienna and caramels.

Much trouble was experienced from cloudy weather and bad skies. Every opportunity was taken advantage of, however, to secure photographs of the comet. I am greatly obliged to my friend Dr. S. A. Mitchell, who guided for me on several mornings that work with the large telescope would otherwise have prevented photographs being secured. On a few mornings Dr. Mitchell attached his small camera with a Goerz double anistigmat lens of $1\frac{1}{4}$-inch aperture and 6-inch focus $\therefore a/f = 1/4.8$, on to the Bruce mounting, and secured some negatives which showed a greater length of tail than was possible with the other lenses.

Following is a list of the photographs made with the Bruce telescope:

In the column marked "Lenses," *a* is the 10-inch Brashear doublet, *b* the 6-inch, *c* the 3.4-inch and *d* the $1\frac{1}{4}$-inch Goerz lens.

In conclusion it would seem that we have to deal with several different kinds of physical phenomena in the study of comets. These are doubtless closely related and are probably the same phenomena acting under different conditions.

There is the regular production of the tail through the repellent action of the sun's light. The tail forming particles in this case will be very small. They may go out from the comet as a broad stream or they may produce several streams more or less narrow. The direction of these various rays are dependent, to some extent, on an exciting and directing force in the comet itself, but the general direction will be more or less influenced by light pressure. These streams, or rays, will be more or less uniformly straight or curved—almost always straight or nearly so. They may be broken or abruptly deflected but this will be due to some influence encountered in their progressive motion in the general direction of the comet's flight. Such streams will more nearly represent the true emissive velocity of the particles. I have shown in the *Astrophysical Journal*, Vol. XVIII., p. 214, in the case of Borrelley's comet, that the tail of a comet actually moves forward bodily as a whole both outward from the sun and progressively in the direction of the comet's mo-

List of Photographs of Daniel's Comet Made with the Bruce Telescope.

Approximate Position 1855.0.

	α	δ	Cent. Stan. Time.	Duration of Exposure.	Lenses.	
	h. m.	° ′	h. m.	h. m.		
June 20	0 18	+1 20	14 23	0 45	a b	
July 3	1 8	+5 15	14 6	1 2	a b	
11	1 49.5	+8 20	13 58	2 12	a b c	
13	2 1.5	+9 6	14 7	1 55	a b c	
15	2 14	+10 0	13 52	2 20	a b c	
17	2 27.5	+10 40	13 59	2 10	a c	
17	2 27.5	+10 40	14 27	1 14	b	
18	2 35	+11 10	14 7	1 50	a b c	
19	2 42	+11 32	13 54	2 18	a b c	
20	2 50±	+12 0±			a b c	
29	4 6	+15 30	14 41	1 40	a b c	
31	4 24	+16 0	15 2	1 17	a b c	
Aug. 1	4 33	+16 9	14 46	1 45	a b c	
3	4 53	+16 40	14 53	1 40	a b c	
5	5 11	+17 5	15 8	1 6	a b c	
6	5 21	+17 10	15 3	1 21	a b c	
8	5 39	+17 22	15 30	0 33	a b c	
9	5 50	+17 20	15 1	1 32	a b c	
10	5 58	+17 25	14 57	1 43	a b c	
11	6 7	+17 25	14 59	1 42	a b c d	
12	6 16	+17 25	15 7	1 43	a b c d	
13	6 25	+17 20	15 8	1 11	a b c d	
14	6 34	+17 20	15 11	1 20	a b c d	
17	7 0	+16 55	15 10	1 30	a b c d	
19	7 16	+16 30	15 40	0 40	a b c d	
20	7 24	+16 30	15 27	1 5	a b c d	
21	7 33	+16 30	15 36	0 50	a b d	
22	7 39	+16 5	15 40	0 44	a b c d	
23	7 47	+16 0	15 45	0 40	a b c d	
24	7 54	+15 40	15 48	0 34	a b c d	
25			15 23	0 5	a b	
29			15 45±		a b c d	Few min. only, clouds
30			15 55		a b c d	" " " "
31	8 45	+14 20	15 59	0 39	a b c d	
Sept. 2	8 58	+13 10	15 51	0 18	a b c	
5	9 18	+12 30±	16 0	0 37	a b c	
8	9 34	+11 20	16 17±	0 5±	a b	
11			16 19	0 24	a b c	
12			16 25	0 20	a b c	

tion, and that some of the particles must move outward from the sun very much faster than others of the same stream. This was shown in the formation of the new tail of the above-named comet on July 24. In the tail on that date the later photographs showed that the end of the new tail was increasing its distance from the head much faster than the end of the receding disconnected tail. But in this case the conditions were different; the supply of matter forming the outgoing stream had suddenly been stopped and the stream itself continued to move out bodily into space until it was dissipated. The apparent velocity was then the velocity of the stream of particles. In the case of Daniel's comet a denser mass of particles differing from the general streams that formed the tail was separated from the main body. This would naturally leave the comet slowly and continue to partake of the original motion. Still another case was that of Brook's comet of 1889 (comet V., 1889) where the masses thrown off were so dense that they traveled with the parent comet for months as individual companions before finally disappearing.[1] And yet another case, that of Biela's comet which separated into two masses that remained individually distinct for some years and then entirely disintegrated. The motion of a dense mass thrown off from a comet would not therefore be a criterion for the determination of the velocity in general of the particles of the tail of such a comet.

The plate of September 8 is introduced, not from any scientific value it may have, but from an artistic standpoint and from its unique character. So far as I know this is the only comet, or star photograph, on which clouds are actually shown. The exposure was very short, for the comet was visible for only a few minutes in a break. The clouds stand out black and distinct on the dawn-lit sky. To the eye it was a beautiful and striking scene—the comet in pale but clear relief on the dawn-whitening sky, the dark clouds, through a break in which the comet shone, and the solemn stillness of the morning, made it a picture not soon to be forgotten. The photograph rather faithfully records the appearance of the comet and clouds and dawn-lit sky, but the reproduction cannot do justice to the

[1] See *Astronomische Nachrichten*, nos. 2914, 2919, 2988 and 2998.

reality. Quite a number of stars appear upon the original which heighten the artistic effect, but they have disappeared in the reproduction.

The great delay in the appearance of this paper has been due entirely to the difficulty of getting good half-tone reproductions. (E. E. B.)

NOTES ON SOME PSEUDOMORPHS, PETRIFACTIONS AND ALTERATIONS.

By AUSTIN F. ROGERS,

Stanford University, Cal.

(*Read February 4, 1910.*)

The writer wishes to place on record some interesting cases of pseudomorphs, petrifactions and alterations observed by him in the last few years. Some of these are recorded for the first time, some are American occurrences of minerals known abroad, while others are good examples of commonly occurring pseudomorphs. While many examples of such pseudomorphs and alterations are of mineralogical interest only, some of them have a possible bearing on the origin of ores. My thanks are due to the gentlemen named in the several items who have kindly furnished me with the specimens which make this paper possible.

Pseudomorphs.

1. *Copper* after *Cuprite*.—Calumet-Arizona Mine, Bisbee, Arizona. Collected by Mr. E. W. Rice. Cubes of 4 mm. diameter, modified by faces of the octahedron and dodecahedron occur in cavities of a limonite gangue. The copper consists of dense aggregates of small imperfect crystals with smooth cube surfaces. No cuprite was observed in the specimen.

2. *Copper* after *Chalcanthite* (?)—Carlisle, Arizona. Collected by Mr. Harry Robertson. The specimen is a coarsely fibrous seam of native copper 2 cm. wide. There are no associated minerals to give a clue as to its origin and the mode of occurrence is unknown, but as copper is practically always a secondary mineral, and as the structure is exactly similar to well-known seams of chalcanthite from Arizona, it is believed to be a pseudomorph after chalcanthite.

Pseudomorphs of copper after cuprite from Cornwall have been described by Miers;[1] copper after azurite from New Mexico by Yeates;[2] and copper after aragonite from Bolivia by Forbes.[3]

3. *Chalcedony* after *Calcite*.—Guanajuato, Mexico. Obtained from the Foote Mineral Company. An excellent specimen of this pseudomorph consists of pale brown chalcedony in the form of hollow doubly terminated scalenohedrons (2131) of calcite about 1 cm. in length.

4. *Hematite* after *Marcasite*.—Lake Co., California. Collected by Mr. H. E. Kramm from the Baker mine, six miles from Lower Lake on the road to Knoxville. The specimen consists of small encrusting crystals giving a red streak. They have the same form as unaltered marcasite crystals from the same mine.

5. *Limonite* after *Chalcopyrite*.—Granby, Missouri. Small tetrahedra (2 mm.) of dark brown limonite on a specimen of dolomite, calamine, and smithsonite have been produced by the alteration of chalcopyrite. The author found similar pseudomorphs at Galena, Kansas, but it is a rare kind of pseudomorph.

6. *Limonite* after *Cerussite*.—Burke, Idaho. Collected by Mr. H. F. Humphrey at the Bunker Hill mine. At this mine cerussite is a prominent gossan mineral. Several specimens show prismatic crystal aggregates of cerussite with a coating of limonite. Other specimens show limonite of a form exactly similar to the cerussite and are undoubtedly pseudomorphs.

7. *Wad* after *Calcite*.—Echo Mine near Mojave, California. Collected by Mr. H. W. Young. Cavities in a quartz matrix with the shape of calcite scalenohedrons are occupied by a soft black mineral answering the tests of wad. These are not direct substitution pseudomorphs but probably represent quartz encrustation pseudomorphs after calcite in which the calcite was dissolved out and then the cavities filled with wad.

8. *Calcite paramorph* after *Aragonite*.—Patterson Pass, east of Livermore, California. A travertine deposit in buff Miocene sandstone consists of a banded, coarsely fibrous aragonite of an amber

[1] *Min. Mag.*, Vol. II., p. 266, 1897.

[2] *Am. Jour. Sci.*, Vol. 38, p. 405, 1889.

[3] *Quar. Jour. Geol. Soc.*, Vol. 17, p. 45, 1861.

color, often variegated. The material has been quarried out in large blocks and on the exterior these are often altered to calcite. The aragonite is compact columnar massive, while the calcite is porous, though crystalline and shows the cleavage faintly. As the calcite retains to some extent the columnar structure of the aragonite the specimens are paramorphs. Occasional calcite crystals are found in cavities. Sicily furnishes excellent specimens of calcite paramorphs after aragonite, but this is the first example found in this country, I believe.

9. *Smithsonite* after *Calcite*.—Granby, Missouri. This is one of our best known pseudomorphs. Steep rhombohedrons of the —2R form (*02$\bar{2}$1*) implanted on dolomite have been replaced by a spongy mass of smithsonite, but the surfaces of the crystals are smooth.

10. *Smithsonite* after *Dolomite*.—Granby, Missouri. A massive cleavable dolomite specimen 2 cm. thick with warped rhombohedral crystals on one surface have been completely changed to smithsonite of a pale brown color.

11. *Cerussite* after *Calcite*.—Granby, Missouri. Scalenohedral (*21$\bar{3}$1*) calcite crystals of 1 cm. diameter are completely changed to colorless cerussite with adamantine luster.

12. *Pyromorphite* after *Galena*.—Granby, Missouri. Crystals of ½ cm. square cross section on chert matrix consist of a little unaltered galena in the center, then cerussite and finally a border of green earthy pyromorphite.

13. *Calamine* after *Calcite*.—Granby, Missouri. Scalenohedral (*21$\bar{3}$1*) calcite crystals 1 cm. in diameter on chert matrix have been replaced by calamine. When broken the crystals are found to be hollow and calamine crystals project into the hollow center.

14. *Muscovite* after *Tourmaline*.—Pala, California. A specimen 7 cm. long and 1.5 cm. in diameter represents an original tourmaline crystal, roughly trigonal in cross-section. It is now mostly white scaly muscovite in which is set a number of small black tourmaline crystals in parallel position with the large crystal.

15. *Talc* after *Actinolite*.—Apperson Creek, southeast of Sunol, Alameda County, California. Gray columnar, subradiating talc is

probably pseudomorphous after actinolite as it has the exact structure of the actinolite common in the schists of the Coast Ranges. The mineral has a greasy feel and is scratched by the finger nail. It is practically infusible and gives a little water in the closed tube. Cleavage flakes give a negative biaxial interference figure with a small axial angle. The axial plane is in the direction of the length of the columnar crystals.

16. *Chrysocolla* after *Cuprite*.—(*a*) Santa Margarita Mine, New Almaden, California (*b*) near Mammoth, Utah (Tintic district). At both of these localities chrysocolla is pseudomorphous after the chalcotrichite variety of cuprite, a variety that consists of crystal aggregates of elongated cubes crossing and branching at right angles. With polarized light, the chrysocolla exhibits an aggregate structure and the outside surface occasionally consists of concentric layers somewhat radiating. At New Almaden the cuprite occurs in seams in serpentine but it is believed to have its origin in copper-bearing pyrite which occurs in a nearby prospect shaft.

17. *Chrysocolla* after *Calcite*.—(*a*) Arlington, N. J. (*b*) Reward Gold Mine, Inyo County, California. Collected by Mr. C. E. Gilman.

(*a*) At Arlington chalcocite and secondary copper minerals occur at the contact between diabase and triassic sandstone. The pseudomorphs were found in cavities of the sandstone. They consist of small scalenohedrons ($21\bar{3}1$) completely replaced by chrysocolla.

(*b*) The Inyo County specimens are large prismatic quartz crystals coated with a crust of chrysocolla. Some of the chrysocolla is the form of acute rhombohedrons (—$2R$ or $02\bar{2}1$) with rounded edges which represents, no doubt, original calcite. The chrysocolla is made up of concentric layers, the inside ones of which are deeper greenish blue than the outside. Under the microscope the fine aggregate structure is in evidence. Associated cuprite is the source of the copper.

Petrifactions.

18. *Sphalerite* replacing *coral*.—Galena, Kansas. A conical coral, probably a *Zaphrentis*, 2 cm. in diameter, is replaced by dark granular sphalerite. Most of the fossils from the chert of this

district are molds or cavities from which the calcareous matter has been dissolved out. The specimen described is a cast probably formed by filling of the mold and not by direct replacement of the organism.

19. *Pyrite* replacing *Aviculopecten.*—Leavenworth, Kansas. Some excellent pyrite petrifactions were obtained from the Cherokee shales on coal mine dumps at this locality. The pyrite is bright brassy with purplish tarnish. The fossil is *Aviculopecten rectilaterarius,* which, in Kansas, is limited to this horizon.

20. *Limonite* replacing *gasteropod.*—Carnegie, Corral Hollow, California. Fossils of fresh-water gasteropods (probably a new species of *Melanea* according to Mr. Harold Hannibal) occur in Eocene sandstone exposed along the railroad track near Carnegie, San Joaquin County, California. The sandstone is composed of quartz grains with limonite cement. The shells are completely replaced by dense limonite .5 mm. thick.

21. *Limonite* replacing *twigs.*—Bingham, Utah. In Upper Bingham Canyon along the creek bed are found specimens of a porous mass of soft, earthy limonite. These evidently represent former plants as hollow, flattened stems are plainly visible.

22. *Malachite* replacing *cedar wood.*—Bingham, Utah. At the locality mentioned above, malachite with structure of the cedar wood common at the same place occurs. The mineral is porous and often has a mammillary surface in free spaces. The cell structure of the wood is visible with a hand lens. Selected pieces are completely soluble in hydrochloric acid showing complete replacement but in other cases there is simply a thin green coating of malachite.

23. *Barite* replacing *Productus.*—Elmont, Kansas. The writer is indebted to Dr. J. W. Beede for this specimen. A specimen of the brachiopod shell, *Productus punctatus,* has been completely replaced by pink barite and the surface markings characteristic of this species are preserved. A visit to the locality which is a limestone ledge two miles northwest of Elmont, Jackson County, Kansas, revealed a number of fossil pelecypods and gasteropods partially replaced by pink barite, but none so complete as the one described.

Alterations.

24. *Sulfur* from *Sphalerite*.—Galena, Kansas. Specimens of sphalerite and pyrite in chert breccia from the Templar ground, two miles southwest of Galena, Kan., show in an excellent manner the alteration of a sulfid to sulfur. The sphalerite is corroded and covered with pale yellow sulfur while the pyrite is fresh and free from sulfur. Evidently the sphalerite has furnished the sulfur.

25. *Strontianite* from *Celestite*.—Five miles from Austin, Texas, on the road between Mts. Barker and Bonnell. Collected by Mr. F. L. Hess. Massive cleavable celestite of a pale blue tint is much corroded and the cavities are occupied by tufts of small acicular crystals of strontianite. The crystals are flattened, tapering, six-sided crystals, the forms being a steep rhombic bipyramid (*hhl*) and a steep rhombic prism (*okl*). The crystals are often curved and in consequence give a wavy extinction. The elongation is parallel to the faster ray. The mineral contains some calcium in addition to strontium as the microchemical gypsum test shows. This is the best test for calcium in such a compound.

26. *Barite* from *Witherite*.—Northumberland, England. Colorless tabular barite crystals (*001, 110, 102*) are found in cavities of massive gray witherite. The barite is evidently a secondary product, formed from the witherite.

27. *Copiapite* from *Pyrite* and *Limonite* from *Copiapite*.—Five miles n. w. of San Jose, California. Altered pyrite crystals from chlorite-glaucophane schist boulders showed the following cross-section: The interior is hollow with small projecting bits of bright pyrite. The exterior is limonite while between these two is a soft compact yellow material which answers the blowpipe tests for copiapite. Under the microscope the copiapite is seen to consist of minute pseudo-hexagonal crystals. Here the pyrite has evidently altered to copiapite and then the latter to limonite.

28. *Hornblende* from *Hypersthene*.—Arroyo Bayo, twelve miles southwest of Livermore, California. These specimens were found in a large outcrop of norite or hypersthene gabbro. Grayish green hypersthene with faint cleavage has been altered around the borders to black hornblende with good cleavage. In fragments the hyper-

sthene has parallel extinction and characteristic pleochroism from pale reddish to greenish white while the hornblende has has an extinction angle[4] of about 14° and faint pleochroism from bluish green to yellowish green. The mineral occurs in large anhedra associated with light gray plagioclase.

29. *Sericite* from *Feldspars*.—New York City. Specimens of orthoclase and oligoclase from the pegmatites and pegmatite lenses of the schists in the upper part of New York City are often accompanied by secondary muscovite or sericite in the form of thin silvery scales. The scales occupy the cleavage planes but more especially planes of parting parallel to the unit prism faces (*110*).

[4] Determined on cleavage fragments.

MAGIC OBSERVANCES IN THE HINDU EPIC.

By E. WASHBURN HOPKINS.

(*Read April 21, 1910.*)

Various works in Hindu literature provide us with a storehouse of magical observances, from the time of Vedic Hymns onward. The Sūtras of one sort or another recognize and inculcate magical arts. But in epic literature, what is formally taught elsewhere is found in active operation. There is lacking, of course, any systematic treatment of these magical rites; they must be culled piecemeal from epic narration. Further, the relative importance of magical rites is lost, because to the heroes of the epic some magical observances were much more important than others. Finally, it must be said that at the time of the epic there was no sharp distinction felt between the regular sacrifice and the irregular magical sacrifice. All sacrifice was to win power, often from deities opposed to the sacrificer; but they were constrained to grant his wishes by the (magical) power of the rite. The same is true of the practice of austerities and ascetic observances, which, when persisted in, made the gods uneasy, because by means of such observances the ascetic won power over the gods themselves. Hence the religious devotion of a saint appalled the gods and they tempted him in various ways to fall from his asceticism, not because they disliked him or what he did, but merely as a means of self-defence.

On the other hand, magic *per se* was strictly divided into good and bad magic. The difference lay not in the rite itself so much as in the application of the rite. If one's adversary was a demon, who naturally employed magic, then a good man himself might use the same means. Injurious magic was justified against an injurer. All the gods as well as the demons use such magic and men may do as the gods do, but with the same restriction, that is, that their magic be Aryan or "noble," not for base purposes. Hence we are told that when a king and his priest perform a magical ceremony involving murder they are sent to hell (see below).

A great deal of magical lore lies in the wonder stories of the holy watering-places called Tīrthas. We read that at such and such a Tīrtha the footprints of the gods are still visible, that "fishes of gold" are to be found, that animals change their shapes, etc. Primarily, the Tīrthas are in the interest of the accepted religious cult and the reward offered for a journey to a Tīrtha and a bath in the sacred pool is forgiveness of sins, 3.47.29, etc. But besides this the pilgrim gets "all his wishes" or more specifically "regains his youth," or gets "beauty and fortune," as in 3.82.43f., *ib.* 111f.; 85.32. That diseases are cured in the Tīrthas, 3.83.50f., may be due rather to a belief in medicinal waters and is not necessarily a magical trait. But it is a trifle more magical when we read that if one eats once at the Tīrtha called Maṇināga he will never thereafter be poisoned by snake-bites, 3.84.109f. In the same section it is said that the tracks of the magical cow Kapilā "and of her calf" are preserved till now at the Tīrtha named for her, *ib.* 88.

The Tīrtha is, in short, the place where marvels are to be seen, and these marvels are of more or less magical nature, like the "marvel visible even today at Piṇḍāraka" (in Gujarat), viz., "impressions having the mark of the lotus and lotuses stamped with (Śiva's) trident," 3.82.66.

Like the Tīrthas, the trees and mountains show many magical touches, but these require separate treatment.[1]

Magic in Sacrifice.

It must be assumed at the outset that all the paraphernalia of the Vedic cult, with its fire sacrifice, *havyam* and *kavyam*, 7.59.16, sacrificial sessions, "four-month" sacrifice, the "five sacrifices," 5.134.12 (cf. 19); the "six *sādyaskas*," the *sarvamedha*, the "seven *soma-saṁsthas*," 12.24.7; 29.38, etc., were perfectly well known to the writers of the epic. Thus the horse-sacrifice and human sacrifice are referred to, *e. g.*, 5.29.18, and the countless

[1] Compare my forthcoming paper on "Mythological Aspects of Woods and Mountains in the Hindu Epic," JAOS. 1910.

cattle sacrificed by the various kings who are extolled by the poets are the same as in earlier ages, only that the numbers (cf. *e. g.*, the *jārūthyas,* 3.291.70), like the gold employed, 7.61.6, etc., exceed all probability. These need not be described. Indeed the epic does not describe them. It merely mentions them, as it does the "divine and woodland rule" of performing *rājarṣiyajñas,* 3.240.16. Only in the case of the horse of victory and its subsequent sacrifice do we get a real picture of epic sacrifices of the old sort. It is rather the new features that are instructive, such as the actual presence, in sight of man, of the gods (at sacrifice), 7.67.19; the later insistence on make-believe sacrifices, seeds representing animals, etc., as in 12.338.4 due to the doctrine of non-injury, and the sacrifices not so orthodox as those just mentioned, which smack of magic, though even the regular sacrifices are performed as mere magical rites, by which, for example, India wins the lordship of gods, 12.20.11 (cf. 5.140.14, the girls' pots and plants in sympathetic magic).

One of the most interesting of these is the human sacrifice described in 3.127.2f.; 128.5. Jantu was the only son of Somaka and his father feared that he would die and leave him childless. He therefore exhorted his domestic chaplain to devise some means by which he might secure more sons. The sinful means devised was the sacrifice of Jantu. The various wives of the king stood about the cauldron where the wretched child was being cooked and sniffed the steam and smoke. This evil sacrifice had two results. The child was reborn as the eldest of a hundred sons conceived by the wives in this process of sniffing; but on the other hand the wicked priest had to go to hell.[2]

That a domestic priest has occult power over the king is generally admitted. He is able "to sacrifice the strength of the king"

[2] The artificial "sacrifice" of life (5.141.29f.) in battle is taken seriously by the epic poets. Compare 5.58.12 (to Yama): Śiva "sacrificed himself in the *sarvayajña,* and so became god of gods," 12.20.12. "In battle a warrior makes a sacrifice (as if making an oblation into fire, *hutvā*) of his body," 3.300.36; so 12.24.27, etc., Śibi sacrificed himself, in a pretty tale, by cutting off his limbs to save a fugitive; but the demon Rāvaṇa cut off his own heads and offered them in fire, which pleased Brahmā so that he let them grow again, 3.275.20.

or of the enemy, "on the sacrificial fire" *medhāgni*, that is, it is a magical fire-ceremony, 5.126.2; 9.41.12. In the horse-sacrifice, the head is cut off and set on the fire-altar, 7.143.71, that the chief part of the rivals may be destroyed. Even the demons fade away when the domestic priest of the gods "puts meat on the fire with a view to their abolition," 9.41.30.

The sacrifice of a hundred conquered and captured kings to Śiva (Rudra) intended by the victor, king Jarāsandha, calls forth the remark in 2.22.11 that "no one ever saw the slaughter of men"; but the entirely casual statement that "there is just as much merit in going to the holy well of Nandinī as there is in sacrificing human beings," 3.84.155, seems to show (since the speaker's object is merely to exploit this Tīrtha) that human sacrifice was regarded as something actual, and rarely beneficial. In 10.7.56 a man "sacrifices himself as an offering," and being accepted by the god comes out alive with divine power.

Many of the sacrifices made by the epic heroes, however, are simple offerings of "words, water, and fruit," 3.36.45; 41.47. Sacrifice is a means of purification: "By various sacrifices cleansing off the sin committed let us go to heaven" (*pāpam avadhūya*), 3.52.20. The usual sacrifices offered by those dwelling in the woods are *iṣṭi, pitṛyāṇi,* and *kriyās,* 3.25.3. A royal list may be illustrated by those offered by Yudhiṣṭhira, to wit, "*vāiśvadeva, iṣṭi, paśubandhu, kāmyanāimittika, pākayajña, aśvamedha, rājasūya,* and *gosavas,*" 3.30.14f.[3] There is also a *quasi* sacrifice of feeding a white bull till it can eat no more, by making offerings to it as to something sacred, *anaḍuhe sādhave sādhuvāhine . . . sāuhityadānāt* (to satiety), 3.35.34.

Only a king may offer a royal sacrifice, but there is another "just as good" (as efficacious) which an ambitious prince may offer. Its present interest lies in the fact that it is something quite new. "You cannot have the Rājasūya while the king lives," says the priest to the ambitious prince, "but there is another great sacrificial

[3] In 7.68.10, *ukthyas, aśvamedha, agniṣṭoma, atirātra, viśvajit vājapeya; ib.* 66.7, *darśapūrṇamāsāu, āgrayaṇa, cāturmāsyas,* etc. Compare also 7.63.1 f. which adds *puṇḍarīkas,* and remarks that the king gave "all the property of those not Brahmans to the Brahmanas."

session equal to the Rājasūya." Out of the gold collected as tribute is made a golden plough and with it is ploughed the earth for the altar, *yajñavāṭasya bhūmiḥ,* and a great ceremony called Vāiṣṇava, "very well prepared," *susaṁskṛta,* is performed, rivalling the Rājasūya. No one except Viṣṇu ever performed this sacrifice, and the priest says it seems to him even better than the Rājasūya: *etena ne'ṣṭavān kaścid ṛte Viṣṇum purātanam, rājasūyam kratuśreṣṭhaṁ spardhaty eṣa mahākratuḥ; asmākam rocate cāi 'va śreyaś ca tava,* etc., 3.255.13–21. This rite closes with scattering corn and anointing with sandal-paste; but "some said it was not equal to one sixteenth of the other" (the Rājasūya).[4]

Sacrifice.

"Sacrifice arose in the eastern country," says the epic, 5.108.5 and 9. This is more important as showing that the eastern country (district) was no longer as of old regarded as foreign.

Like other things, sacrifice is personified. The Great Father lives in the north with Sacrifice, 5.111.15. The Great Father, by the way, brings a sacrifice, as much as do the other gods, *iṣṭīkṛtam nāma (satraṁ varṣasahasrikam),* 3.129.1.

"A sacrifice without gifts (to the priest) is dead," is another saying of the epic, *mṛto yajñas tv adakṣiṇaḥ,* 3.313.84. Cf. 5.106.22, etc.

The merit of a sacrifice pertains to the giver; but he may give that merit away to another, 5.122.13, etc.

Most of the gods sacrifice as they accept sacrifice. They are "perfectors of the sacrifice," *sviṣṭakṛtaḥ,* 5.42.40 (rare epithet of gods in general; usually of the Fire-god alone). The gods are established through sacrifices, and sacrifices are produced through

[4] This is a standing expression of depreciation, as in 3.34.22; 39.23; 174.3; 254.27; (above) 257.4; 4.39.14; 5.49.34; 7.36.7; 111.30 (31, *nā'lam Pārthasya!*); 7.197.17; 8.15.28. Compare 12.174.46 = 177.51 = 277.6. The fraction is scarcely used otherwise save in the late geographical section of Bhīṣma, where it is said that Kubera gives to man only one sixteenth of the quarter of Meru's wealth, which (quarter) he in turn receives (from Śiva), 6.6.23. In 10.12.17, "one hundredth part" and in 12.155.6, "one eighteenth part" are used in the same way as one sixteenth. But "one and one-half times" (better) is found in 7.72.34 and 11.20.1.

the Vedas, 3.150.28. Gods who "steal the sacrifice" are begotten by Tapas, and are fifteen in number (3 × 5), one group having Mitra-names, 3.220.10f. (Subhīma, Atibhīma, Bhīma, Bhīmabala, Abala; Sumitra, Mitravat, °jña, °vardhana, °dharman; Surapravīra, Vīra, Sureśa, Suvarcas, Surahantar).[5] A peculiar way of dotting earth with sacrifices is often alluded to in the epic. It is by casting a Śamī stick as far as it will go, and building an altar where it falls, over and over again, *śamyākṣepeṇa (ayajat)*, 3.90.5, etc.

The general distinction made by the epic between the worship of gods and Manes is that the gods are honored with flowers and water and the Manes with roots and fruits, the former being *arcitāḥ*, revered, and the latter being *tarpitāḥ*, pleased, 3.156.6. Every sacrifice is identified with Prajāpati, as food, and the year, 3.200.38.

But besides the regular sacrifices and the substitutions for them[6] there is evidence (cf. 8.40.33, the Mantra of the Atharva to offset scorpion poison) that the use of the "Atharva Veda crammed full of wizardry" was familiar enough. The application of the art of magic was according to circumstances. Against one who used bad magic the use of bad magic was permissible; otherwise not. The difference between good and bad ("straight and crooked") magic was recognized and practiced both in the use of legitimate and illegitimate sacrifices and in the application of magic weapons and the like (magic clouds of dust, showers of blood, frightful shapes and noises) to defeat a foe. The Mantra sufficiently potent converted the ordinary weapon into a magical dart, a boomerang or thunderbolt, with which a good and true Aryan might fight the powers of darkness and any sinful knights who relied on such powers. Ethically, the rule was "magic is sinful; but if employed against the good the good may in turn use it." The same rule, in short, as obtained in the matter of curses. If cursed not it was sinful to curse; if cursed, it was silly not to curse back and the worse the curse the better the curser. Cf. 12.100.5; 103.27f.

[5] Foreign influence may be suspected in the Mitra-named "sacrifice-stealers." The others are native devils, to whom one offers sacrifice "outside the altar." The idea is Vedic. The last name in the text is paraphrased, *surāṇāmapi hantāram*, "*slayer of gods.*" Sureśa (Agni) is a proper enough name of a (good) god!

[6] Compare also *ātharvaṇa arivināśana*, "foe-slaying," 8.90.4; *Krityā atharvāngirasī 'vogrā*, "like an Atharvan rite, horrible," 8.91.48; 9.17.44.

The use of charmed weapons was facilitated by a special ceremony called Lohābhisāra. Thus in 5.160.92, *lohābhisāro nirvṛttaḥ* means "That ceremony has been performed (for you) which forces the deities named by the Mantras you have used to preside over and govern your ordinary weapons." So all the deities of water, fire, etc., have their names given to the weapons thus inspired, and when a warrior is said to use the *Vāruṇa astra* it is merely an arrow inspired by the god named, who is temporarily at the disposal of the knight. This is a perfect parallel to the "singing of a weapon" on the part of an Australian savage and the Mantra is felt as nothing more than a magic formula. It is equally efficacious when said over a blade of grass, 10.13.19f. In 8.40.7, an arrow is "preserved for years in sandal-dust and religiously worshipped," *pūjitaḥ,* that it may be effective when needed.[7]

When Arjuna makes a lake spring up where there was no water, on the field of battle, he performs a similar magic trick, 7.99.62f., for it is done by piercing the earth with a magic arrow.

It must be observed that all these practices are in good repute if not exercised for a sinful purpose. The priest who knows magic is the king's domestic chaplain. The king himself is a magical being when as in the case of king Śāntanu he has the "healing touch," 1.95.46; "Whomsoever he touches with his hands, if worn out he becomes vigorous again." Certain priests are Brahmans of high character and yet have the honorary distinction of being *vidyājambhakavārtikāḥ*, that is, "conversant with wizardry and magic" (cf. *ib. jambha-sādhakāḥ*), 5.64.16,20.

The ordinary means of resisting disease was threefold, drugs, Mantras (holy verses), and "ceremonies," *kriyās,* as is succinctly expressed in the simile, "Karṇa attacked Yuddiṣṭhira like a fearful disease which has passed Mantras, drugs, and *kriyās,*" 8.49.8. The *kriyā* is often identical with *māyā* (magic, illusion). In 9.24.54f.,

[7] The weapon is treated like an idol. One such magic weapon is a dart made by the great Artificer (Tvaṣṭar). It is kept for years and worshipped, "with perfumes, garlands, a seat, drink and food"! 9.17.44. Two magical weapons are described in 8.53.24f. One of them encircles the foes' legs with snakes and the other invokes birds of prey which eat the snakes (*pādabandha* or *nāga*, and *sāuparṇa*). When one's fated day arrives, however, the magical weapon refuses to act, 7.191.8.

the hero solidifies a lake and lies within it hidden from his foes. This is *māyā* but also *dāivayoga*, "divine power," *ib.* 30.56. The opposing hero is then exhorted to kill the fugitive by *māyā*, because "one who uses magic should be slain by magic," 9.31.7. The divine adviser, Kṛṣṇa, then says "for by means of *kriyā* (*i. e.*, magical ceremony) the demons were killed by the gods. Thus Indra slew Bali (etc.). This fellow here has used his divine *māyā* to hide in the water (31.4) and so you should kill him by *kriyā*-means, just as Indra slew Vṛtra, and Rāma slew Rāvaṇa, and I myself of old slew the two ancient demons Tāraka and Vipracitti. So other demons were slain by *kriyā* and it is by *kriyā* that Indra enjoys heaven" (31.14). Here the "ceremony" *kriyā* is synonymous with *māyā*, illustive magic, even deceit, as clearly in 5.35.42 where it is said that the use of Atharva Veda formulas, *chandāṅsi*, "do not save one who uses *māyā*, but desert him," as a sinner, parallel to one who drinks, quarrels, etc. Compare also 9.58.5f., where *māyā* is deception.

Hair.

The idea that strength resides in the hair may be indicated by the ascription of very long thick hair to ogres (Rakṣasas). These creatures have hair as thick and long as an elephant's trunk and a *trijaṭā* female, that is a female ogre with her hair in three braids, is especially fearful. Śiva himself bears the epithet Trijaṭa. On the other hand, the sign of defeat in battle is that one stands *patitamūrdhaja*, "his head-growth fallen," that is, with loosened hair, as in 3.280.66.[8] Suppliants have "loose hair," 10.8.107, and ghosts are bald, *ib.* 71. The hair of the Yogi droops, 12.46.5, as his intelligence wanes.

The type of weakness, the eunuch, had his hair done up in a braid, *veṇīkṛtaśiras*, 4.2.27; that is, like a woman. The one braid was also a sign of mourning. In R.2.108.8, "The city wears but one braid" means that the whole city mourns. Dishevelled hair

[8] The likeness to an elephant's trunk is made to show strength. On the contrary, Sītā's, the heroine's, hair is "like a black snake," being done up in a long braid (*kālī vyālīva mūrdhani*) *dīrghaveṇī*, 3.281.25f. Hair-dressing *keśakarma*, was the occupation of a Sāirandhri, 4.3.18 (*jāti*), a special caste.

also indicates mourning (compare the funeral procession, when all the mourners go *pracṛttaśikhāḥ*, "with loosened braids," AGS.4.2.9), that is, by analogy with the one-braid sign, weakness. It makes no difference for what one mourns, whether for the death of a loved relative, or the loss of a kingdom, or, it may be, for the loss of dignity. Thus in 5.40.15: "Men bring from the house the dead son and cast him on the pyre ('fire-heap') like a log, and weep with loosened hair; while another enjoys his wealth, and birds and fire enjoy the constituents of his body." But also, in mourning for the kingdom they have gambled away: "The prince went on, covering his face with his garment, and one brother threw dust upon his limbs and another greased his face, and the women wept with dishevelled hair and covered their faces with their hair," although the explanation given is that they did this in order not to be recognized, 2.80.4f. But, though this may be true of the grease and dust, in regard to the hair it is artificial, as may be seen from other accounts. Thus in 3.173.62f.: (when their men have been killed) "The women rushed out of the town with dishevelled hair, in excitement, distressed, like ospreys;[9] and with dishevelled hair they fell on the ground, mourning for their sons, fathers, and brothers; weeping and wailing and beating their breasts; devoid of wreath and ornament." Again, at 4.16.46, the insulted heroine "loosens her hair," or, in the fuller description of the South Indian recension, 4.20.59 and 77: "She bathes not, she eats not, she wipes not off the dust. . . . All her limbs are covered with dust like those of a female elephant . . . and she has her hair loose"; and she does not braid her hair again till the insult is avenged after twelve years, *keśapāśasya padavīm gato'si*, 12.16.28.

As a sign of disgrace, a conquered foe has to proclaim in public, "I am the slave of the Paṇḍus," and wear his hair like an ascetic in five little tufts. His conqueror "with his crescent-shaped knife made five tufts." He is then *pañca-śikha-* (SI. *pañca-saṭa-*) *kṛtaḥ*, 3.272.9 and 18. In 7.202.58 this designates Śiva (as ascetic).

[9] So in 9.29.68f. "the women beat their heads with their nails and hands, wailing like ospreys, and tore their hair and beat their breasts, shrieking 'alas, alas!'" "Covering the face with her garment" (in mourning) occurs in 9.63.68.

To be bald is to be disgraced anyway, and only hermits (*parivrajanti dānārthaṁ muṇḍāḥ kāṣāyvāsasaḥ,* 12.18.32) and barbarians shave their heads. The hermit is *muṇḍa* or *vikaca,* "shaved on the head," 3.260.12 (of a Śivaite ascetic), and the poet, in describing the heads of the barbarians on the battle field, says that they had "beards but no hair on their heads," which made them look like cocoanuts. Probably ethnic characteristics lie back of the "tufted hair" of Buddha and the standing epithets applied to Kṛṣṇa and to his chief disciple, Arjuna, who are called, respectively, Hṛṣīkeśa and Guḍākeśa, *e. g.,* at the beginning of the Bhagavad Gītā 6.25.24. The obvious etymology would make the first "stiff-haired" and the second "ball-haired" (cf. *hṛṣita,* of hair standing on end; but native piety divides the word into *hṛṣīka-īśa,* "lord of senses"). Hair with curly ends is praised. Kṛṣṇā, the heroine of the great epic, has blue-black hair with its own perfume and glow like that of a snake, "with twisted (curly) ends," *vṛjināgrā,* 5.82.33.[10]

The squaws of some of our American Indians were accustomed to mal-e a hair-parting through the middle of the crown and daub it with red paint, presumably to keep evil spirits out, as they used red paint for that purpose very generally. The same practice obtained in epic times (as it does today) among the ladies of India, though, like the squaws, they regarded it as merely ornamental to decorate themselves thus.[11]

The casting of hair into the fire exhibits all the trait of a magical ceremony. In 3.136.9f., Rāibhya, a saint, cast into the fire two locks of his hair, and out of the fire came a woman and a male ogre, "with horrible eyes and terrible to see." This male ogre then pursued the enemy of the saint; who could not escape

[10] The signs of excellence in horses include (at 3.71.14) ten twists or curls called āvartas, which show good qualities. Compare Caland, *Over het Bijgeloof der Haarvervels op het paard.* In man, *tūbaraka* (hornless) "beardless" is equivalent to eunuch. "Bhīma would kill anyone who should say to him 'O thou *tūbaraka*'," 8.69.73. "Curly red hair" characterizes the foreign van-guard of a model army, 12.101.16.

[11] Catlin says that no one knows why the Indians so decorate themselves, and he himself cannot think of any reason.

The middle parting was customary: *susaṁyatāś cā 'pi jaṭā vibhaktā, dvāidhīkṛtā bhāti lalāṭadeśe,* Sl.3.113.9 (*viṣaktā. . nātisamā lalāṭe,* B.112.9).

into water, as the water dried up at his approach because he had become impure—another magical touch.[12]

Among the baths and sacred watering-places, there is one which goes by the name of Śvāvil-lomāpanayana in the Bombay text and Śvāna-lomāpanayana in the South Indian recension, at 3.83.63 (also *-lomāpaha*). This should mean the "removal of hair of porcupine" (or "of dog"), and the place probably commemorates some legend now lost; but the remark added to the name is "there (nowadays) priests pluck out their hair," and "being thus purified attain the blessed state." This is preceded by the mention of another watering-place, apparently near by (perhaps a rival Kurhaus), which "purifies merely by going to it; and one becomes purified (from sin) by drenching his hair in it" (the holy water). B. calls this Sītavana and the SI. text again differs slightly in making the name Śītavana. This last too is the reading in C. and it is probably correct as it has meaning ("the cool grove"), whereas the Bombay text is either meaningless or a corruption of "Sītā's grove," which is unlikely. In the description of the act leading to purification, *keśān abhyukṣya vāi tasmin pūto bhavati,* the "drenching of the hair of the head in this (water)" may imply casting the hair into the water, but that is not certain; while at the other watering-place the hair is certainly plucked out and (inferentially) sacrificed in the water.[13] No religious force lies in "arrange thy hair" (in preparation for battle) in 9.32.60.

The Evil Eye.

The "eye of wisdom," 3.209.50, etc., is a mental power. It is with a glance of the eye that Sagara's sons are burned by Kapila,

[12] It is curious that this hair-born ogre does not disappear or perish when he has completed the task for which he has been created. On the contrary, he gets as his reward the woman created from the other lock of hair. Her part in the matter was to make the man impure through tempting him, which she easily did as she was very fair and he was not very virtuous.

[13] Naturally, "grasping by the hair" is insulting (M. 4.83) and when the heroine of the epic is basely treated this only adds to the insult. There may, however, be a relic of superstition in it. At least Droṇa's son feels more distress at the fact that his father's foe "seized him by the hair" than on any other point in his manner of death, 7.195.8f., *keśagraha(ṇa)*. Proverbially "up to hair-seizing" means to the limit, as in R.3.46.2 ("I must strive") "as hard as I can," to the last.

according to one version of the story, as they were digging out ocean on earth's surface.[14]

That serpents have poison in their look may be inferred even from the common word for such poisonous reptiles, *āśīviṣa,* the black "poison-tooth" cobra, as compared with its synonym (*dṛgviṣa*), *dṛṣṭiviṣa* or *dṛṣṭīviṣa,* "poison glance," which, be it observed, is applied indifferently to snakes and to human beings (another word meaning "eye-poison," *netraviṣa* is used of the *āśīviṣa* serpent only, as "possessed of poison from the eye").

Many incidental remarks testify to the belief that a look may injure. Rāma's "mere look" killed the dragon-worm, 12.3.14. In 3.138.13, it is said; "Let not the one who slays a priest see thy sacrifice; by even a glance he could injure thee," *brahmahā prekṣitēnā 'pi pīḍayet tvām.* The inference here, that the evil eye is associated with an evil nature, is obvious; yet it does not follow that the injury to be done is voluntary. On the contrary the idea of envy, invidia, being at the root of the evil eye is probably not the primitive idea but a secondary notion. The evil eye works without its owner's will, though the will to cast the evil eye may on any occasion be present: "Beware lest the eyes of the weak consume thee" (they are compared with the eyes of a snake and a saint in power), 12.91.14f. Yet the action may be as effective without the wish to injure, and this is why the wedding-ceremony from the beginning associates *aghoracaksur apatighnī,* "(be) a wife without the evil eye, not a husband-slayer."

This word *ghora* is indeed in the epic associated with the "look of poison." In 5.16.26 it is said: "Never look thou upon Nahuṣa, who takes away energy; who has the poison-look; who is very terrible, *tejoharaṁ dṛṣṭiviṣaṁ sughoram.* In a following verse: "Let us overcome Nahuṣa who has the terrible glance, *ghoradṛṣṭim,* our enemy," 5.16.32. The two verses show that *dṛṣṭiviṣa* and *ghoradṛṣṭi* are practically synonymous.[15]

[14] 3.47.9–19, *darśanād eva.* The account in 3.204.27 says that they were destroyed by fire which came from the mouth of Kapila.

[15] The meter of the last verse is noticeable, *Ripum jayāma tam Nahuṣam ghoradṛṣṭim.* It should be added to the list given in my Great Epic, p. 299; also 5.29.16c: *tathā nakṣatrāṇi karmaṇā'mutra bhānti,* and the verse (not in B) C.797, *asatyām āpadi karmaṇi vartamānaḥ.*

Nahuṣa himself says of his power: "Whatever living creature I look at with my eye, his energy I quickly take away; that is the power of my sight," *dṛṣṭer balam mama,* 3.181.35. There is no implication that he wishes to do ill. The queen's eye made a sore on her nephew's finger; but with her look she might have consumed him, 9.63.65; 11.15.30.

The expression used in 3.151.6, "fruitful has my eye become," that is my sight has been blessed (through seeing thee), *mamā 'pi saphalam cakṣuḥ,* is due rather to picturesque language than to any peculiar view of the eye.

Minor Magical Traits.

The reverence accorded to the *catuṣpathas* or places where two roads meet, making "four paths," is probably due to the belief that evil spirits haunt such places, 6.192.58. To enter a place "Not by the door" is consonant only with evil designs, 2.21.53; 10.8.10.

Another relic of superstition due to magic is the aversion to leaping over another person. The feeling is so strong with savages that in some parts of the world houses are not built with two stories because of the avowed objection to the presence of another human person above the lower inmate of the dwelling. But in the epic the idea of the world-soul has blotted out this older view and the reason given for not leaping over another is explained by this later belief. Thus Bhīma refuses to vault over his brother Hanūmat, saying: "If I did not know that Being who has been manifested in all beings through tradition, *āgama,* I should vault thee, as Hanūmat did the sea; but as it is, I will not insult the Supreme Being (in thee) and will not leap over thee," 3.147.8f.

Conception by sniffing or inhaling sacred fumes, of sacrifice, etc., is sometimes varied by smelling or tasting a brew prepared with Mantras (holy formulas), and sometimes combined with tree-marriages (q. v.). But a more purely magical touch is furnished by the ceremony alluded to in 4.3.12: "I know the bulls having good signs, by smelling the urine of which even a barren woman conceives." Water alone may be doctored by a priest so as to have the same effect, 3.126.20. A god revives the dead with a handful of water, 12.153.113 (SI.v.l.); though one may also be revived by divine fiat, or by a magical plant, 10.16.16..

Water-magic is inherent in a good many observances of the epic. Water dries up before a sinner, as in the case of the wicked priest who ran away and tried to escape through water, 3.136.9f. Water is a divine witness of wrong, 1.74.30. It is probably for this reason, as being also the most easily available witness of wrong, that in making a promise, an oath, or a curse (another form of promise), the one who promises or curses touches water. In the same way he may touch earth, as another divine witness. In the gift of a bride, not only "fire and hand-taking" but water are necessary, 7.55.15. So, in accepting a gift, one touches water, and hence "having wet hands" means having accepted a gift or bribe, 12.83.7, *ārdrapāṇi;* cf. *klinnapāṇi,* "one who has had his hand wetted," i. e. been bribed, 12.139.30. An example of touching water in uttering a curse is furnished by 3.10.32, *vāry upaspṛśya.* In the same way when the young knight's armor is bound upon him, the veteran instructor "touches water and murmurs a Mantra," 7.94.39. Almost every solemn act, such as committing suicide, 3.251.19; or installing a commander of an army, 8.10.45, in the *abhiṣeka,* 48; or using a fire (divine) weapon, 7.201.15, is thus introduced by touching water. For example, in 7.79.1–3, when Kṛṣṇa and Arjuna go to bed, they both touch water, and whenever they speak or think of Śiva, from whom they hope to get a favor, they touch water, *ib.* 80.17 and 22; and a little further, *ib.* 81.12, on touching the snake which is the apparent form of the Pāśupata (magic) weapon of Śiva, water is touched. On the death of a relative or on touching the corpse one must bathe at once; hence in 8.94.30, "when Karṇa was killed in battle the sun which had touched his bleeding body sank swiftly as if to take a bath (of purification) in the western ocean." Specially prepared, medicated, water is given to kings, but this is perhaps regarded as really medicinal, *kaṣāya* and *sādhivāsa* (*jala*), 7.82.10, as in 12.332.32.

Water is however magical on occasions. In 3.289.9f., it is related that a Guhyaka came from the White Mountain at the command of Kubera, bringing with him "water by means of which you shall see all invisible things," *anena mṛṣṭanayano bhūtāny antarhitāny uta bhavān drakṣyati*, and, it is added, "he to whom you shall give it," that is, if he uses it as an eye-wash, he too shall see the invisible.

It is difficult to decide whether the power of water rests in every case on magic or on the feeling that water "of which the world is made," 1.180.18, and which is one of the three "purities" ("purity of speech, of deed, and purity consisting in water," 3.200.82) is a natural purifier. Water touched by a priest purifies from sin, 3.193.36, and this is the secret of most of the Tīrthas or sacred watering-places. They have been in contact before with some great saint or god and so won exceptional virtue.

But even plain water refreshes weary horses better if a Mantra (spell) has been said over it, 7.2.26.[16]

Water is especially associated with truth because truth is verbal purity. Consequently a very good man may walk over water or even drive his battle-car over water without sinking into it, as was the case with Pṛthu Vāinya, 7.69.9 and with Dilīpa, 7.61.9 and 10 (who was "a speaker of truth"). So too Yudhiṣṭhira's car did not sink upon earth till he told a lie, an analogous case with the divine earth instead of water as witness. The perjurer is cast out by the water in ordeals (passim).[17]

Magic Rites With Images.

The rite called *Chāyā-upasevana*, "shadow-cult," is explained by the commentator to be the well-known practice of sticking thorns or needles into the clay (wax) image of an enemy and thus inflicting pain or death upon the object of dislike. It is a clear case of "sympathetic magic." The commentor says it is explained in the

[16] Bathing "in different waters" at the end of a battle is of doubtful bearing; the water may be medicated, 6.86.54.

[17] Defilement of the water leads to the divine water's rejection of the sinner. So in Manu and other law-books. The defilement of water by casting into it excreta, saliva, etc., leads to the sinner's going to undesirable worlds, 7.73.31f. It is the fear of defiled water which causes the prohibition against living "in a village which has its water from only one well," 7.73.40. The crematory fire, when a corpse is burned, is extinguished with water, 8.20.50 (to keep off evil spirits).

Kāulika-Śāstra, 3.32.4. The South Indian recension has for this word a varied reading, the verse as there given being *ā mātṛstanyapānāc ca yāvac chayyopasarpaṇam* for the reading in B: *yāvad gostanapānāc ca yāvac chāyopasevanāt (janatavaḥ karmaṇā vṛttim āpnuvanti)*. The adoration of the image of a teacher who has become estranged from his pupil is made in the hope that the image itself will act as the teacher, relent and give the desired instruction, and such proves to be the result in the one recorded epic case of Ekalavya who "made a teacher of earth, *mahīmaya*, and by cultivating it with the adoration due to a (real) teacher attained, through faith and devotion, to great skill in weapons" (which he sought by worshipping the earthen image), 1.132.33.

Authority for Magical Observances.

Apart from the magical practices mentioned above there is little which can be classed as magic in the Great Epic. Ceremonies to raise spirits are known and the use of a jewel, "which, when bound upon one, preserves from danger of all sorts" (weapons, sickness, hunger, gods, demons, serpents, etc.) is recognized, 10.15.29. But there is no essential difference between Mantras to make demons serve one and Mantras to control the gods, except that the latter are employed without ceremony by a woman and the former by a priest with the full paraphernalia of sacrificial ritual, *karma vāitānasambhavam*, 3.251.23 and 305.20. In both cases the rite is according to the Atharva Veda, as declared by Bṛhaspati and Uśanas, the teachers, respectively, of the gods and demons, that is, this Veda is the authority in all magical observances.

Of these two, it is the gods' teacher who has most to do with the magical practices recognized in the epic, which, so to speak, sets the seal of orthodoxy on the cult. From the epic texts hitherto known we may gather considerable information in regard to him. Bṛhaspati was the son of Añgiras, and the younger brother of Utathya; also the husband of Tārakā, and the brother of "that excellent lady who was the wife of Prabhāsa and the mother of the gods' great Artificer." He is reckoned among the Adityas and is identified with the fire-god, though he is also called a divine seer. His knowledge in respect of raising the dead is inferior to that of Uśanas; but otherwise he seems to be more important than his rival. But the epic is too catholic to contrast the fourth Veda (of Bṛhaspati) with the others to their disadvantage except in one passage not hitherto known.

This passage is of great importance not only in what it says but in showing how the epic was composed. It is found only in the so-called Southern recension, and like most of the long interpolations in that recension is a late addition to the epic.[1]

In still another passage of this recension Bṛhaspati is represented as inculcating such extreme liberality as to say that a good Mleccha (barbarian) is better than a sinful Brahman; but in this particular addition he declaims against the other Vedas, which are all inferior to the Atharva as aids to the king. For the office of Purohit, king's priest, only an Atharvan priest should be chosen. The other Vedas have nothing to do with "pacificatory, auspicious, and evil-averting matters." These Vedas were "cursed by Yājñavalkya." Moreover: "The priest of the Rig-Veda is destructive to the realm; the Sāma-Veda priest is destructive to the king; and the Vājasaneyaka is destructive to the army." Here *abhicaraṇa* or sorcery is expressly mentioned as one of the objects to which the king's priest devotes himself. This passage, interpolated, together with an extract from Uśanas, into the seventy-third chapter of the twelfth book (thirty-seven verses between B.2a-b and c-d), goes far beyond the general rule of the epic, such as is given in 12.165.22, "Let one skilled in the Vāitāna be the *hotṛ*." It mentions eighteen kinds of pacificatory ceremonies and calls the Yajurveda priest the Vājin and Caraka, giving the preference to the former as the holder of the office of king's priest. After them are admissible the Rig-Veda and Sāma-Veda priests (as Purohits), provided they are duly conversant with the Atharva-Veda. All this means that the priest of the spell (*brahma*) must be preferred to all other priests for the ceremonies of magic and that the especial patron of this priest is the great "lord of the spell," Bṛhaspati, whose Veda is authoritative.

[1] On this point see a special article by the writer soon to be published on the Southern recension. The recension is a strong witness against the theory that the epic was composed "in one stream."

PHYSICAL NOTES ON METEOR CRATER, ARIZONA.

By WILLIAM FRANCIS MAGIE.

(Read April 22, 1910.)

1. Meteor Crater is situated near the center of the northern half of Arizona, six miles south of the line of the Santa Fe Road, about thirty miles east of Flagstaff. It is an immense hole or crater-like excavation in the otherwise level plain, nearly circular in shape, about 4,000 ft. across at the top, and surrounded by a rim of elevated strata and ejected material. This rim is from 120 ft. to 150 ft. high, and the floor of the crater is 570 ft. below its edge. From the edge there slopes down a very steep inclined talus, ending in the level floor.

The ejected material is mostly composed of broken and irregular limestone boulders from the higher stratum, some sandstone boulders from the next stratum, and an immense quantity of sandstone, which has been pulverized so that each grain of it has been broken into fragments so small that most of them will pass through a 200-mesh. The borings that have been made by the Standard Iron Company have shown that the interior of the crater is filled to a depth of 600 ft. or more with similar fragments of rock and with this pulverized sandstone.

The crater is the center of the area in which the Canyon Diablo meteorites have been found. These are of iron, carrying about 6 to 8 per cent. of nickel, and about three quarters of an ounce of platinum and iridium to the ton. They also contain microscopic diamonds, in large numbers, so that it is a work of great labor and difficulty to cut out a specimen for testing. Another variety of iron is also present, and judging from the residue of iron oxide which it has left, was originally present in much larger quantities than the Canyon Diablo iron. This differs from the Canyon Diablo iron in containing chlorine. It generally is found in oval or globular masses. It oxidizes readily, and forms sheets or plates of iron oxide, resembling fragments of shale, so that it has been called by Mr. D. M. Barringer and Mr. B. C. Tilghman, who first recognized its meteoric origin, shale ball iron.

Through the kindness of the Standard Iron Company the author was given an opportunity to visit and study Meteor Crater. In this paper he desires to present the results of some of his observations, which have to do with physical problems presented by the crater. The description of the crater from the more general point of view, as a geological or cosmical phenomenon, has been given by Dr. G. K. Gilbert, of the U. S. Geological Survey, and by others. The demonstration that it owes its origin to the impact of a meteorite or group of meteorites was made by Mr. Barringer and Mr. Tilghman in papers presented to the Academy of Natural Sciences of Philadelphia, December, 1905, and with additional evidence by Mr. Barringer in a paper presented to the National Academy of Sciences, November, 1909. Professor H. L. Fairchild, of Rochester, and Mr. George P. Merrill, of the U. S. National Museum, have also studied the crater from this point of view.

2. *Magnetism.*—A cylinder 9 cm. long, 1.8 cm. in diameter was cut out of a Canyon Diablo iron, and tested for its magnetic permeability with a permeameter. The method is rough and unsatisfactory, but the only one which can be employed without either wasting too much work on shaping the specimen or possibly altering its condition by heating and working it. The values of H, the strength of field, and of μ, the magnetic permeability, are given in the following table, taken from the curve which best represents the observations.

H.	μ.	H.	μ.
4	400	12	530
6	455	14	505
8	500	16	472
10	533	18	442
10.5	540	20	405

Tests with a similar cylinder of Norway iron gave values for μ about double these, so far as the observations could be carried.

The pieces of shale ball iron at the author's disposal could not be prepared for the test with the permeameter. All that could be done with them was to examine their behavior in the earth's magnetic field by the aid of a small magnet. They generally behaved like pieces of soft iron, or like pieces of the Canyon Diablo iron of about the same size.

A shale ball perhaps 9 in. in diameter, and which was entirely oxidized, was examined in position, soon after its discovery. It was found by excavating in the pulverized sandstone on the outer rim of the crater. It showed strong local poles scattered over its surface. In general, the polarity of the top was south, that of the bottom, north, but at many points the opposite polarity was found.

A piece of shale ball iron about 3 in. in diameter and 1 in. thick, when tested at the crater, showed north polarity all over the outer rim, and south polarity at two nearly opposite points on the two faces. This specimen was sent on to Princeton in a box with other specimens, and when tested again, this peculiar disposition of polarity no longer existed, and it now behaves like soft iron. This fact makes it improbable that the magnetic condition first observed was due to a superficial coating of magnetic oxide, and indicates that it was rather a real magnetic state of the iron. The shale formed from a shale ball by oxidation often shows very peculiar radial structure, and one is tempted to believe that this structure exists in the shale ball iron, and that it may be accompanied by a radial intrinsic magnetization. This view is borne out by some peculiarities of the magnetism of pieces of the shale, but the specimens of shale ball iron found are too few, and have been handled too much since they were found, to make it possible to test this view at present. Much of the shale is so feebly magnetic as hardly to affect a magnetic needle, even when close to it. Occasional pieces are strongly magnetic with well developed poles.

In 1891 Mr. Marcus Baker made a careful magnetic survey within the crater, and along lines running out on the plain. He found no evidence of any local magnetic field. The author ran some straight lines across the floor of the crater with a sensitive surveyor's compass, which could be read by estimation to about 2′ of arc, and found no variation in the compass deviation at different points on these lines. He also made a number of determinations of the magnetic dip, using a Kew Dip Circle. The mean value of the dip found by fifteen observations was 62° 7.7′, with maximum variations at different points from the mean of —5.1′, and +4.2′. These variations may have been due to errors of observation, but are probably to some extent real and due to local conditions. There are several drill holes in the floor of the crater in which the iron pipes used in sinking the holes were abandoned, and these pipes could easily modify the general field to such an extent as to account for the different values of the dip which were found.

If the size of the meteor by which the crater was made is estimated by the old rules of artillery practice, we should conclude that it is equivalent to a sphere of about 750 ft. in diameter. A sphere of iron of this size at the appropriate depth below the floor of the crater would seriously affect the magnetic field. Even on the more moderate estimates of Mr. Barringer and Mr. Tilghman that it is equivalent to a sphere 250 ft. in diameter, the values of the dip at the extreme stations, at which the dip was observed, should differ by 30′. That no such difference was found argues that the meteor was broken and scattered by the impact, or more probably, as Mr. Barringer strongly argues in his latest paper, was a cluster or swarm of small masses of iron, mostly of the shale ball variety. The possible intrinsic magnetism of these masses, coupled with the possibility that they have gradually oxidized in the depths of the crater, would account for the absence of any observed magnetic field.

3. *Mechanical Effect of the Impact.*—When the map of the crater, showing the distribution of the ejected material, is studied, a remarkable symmetry of distribution is immediately apparent. A line drawn through the center of the crater, 13° west of north, can be taken as an axis of symmetry. This line on the north rim of the crater passes through or near the lowest part of the rim, and the region where the least ejected material is found. Its other end on the south rim passes through the middle of the greatest bulk of ejected material, which is furthermore found there in small fragments or largely in the form of pulverized sandstone, or "silica." Just to the east of it, where it has been driven by the

prevailing winds, is an area of brown sand, which the borings prove has come from the depths of the crater. The central line at right angles to this axis crosses the rim at or near the middle of two opposite areas on which the ejected material is deposited in large boulders, mostly of limestone, coming from the upper stratum of the formation. In two lines from the center 33° west and 42° east of this axis, toward the south, there lie out on the plain rows of limestone boulders, marked on the map as the furthest thrown limestone boulders.

The map showing the effect of the disturbance on the original strata exhibits this symmetry in another way. Starting where the axis crosses the northern rim, where the strata are inclined at only 5°, and proceeding around the rim in either direction, the strata gradually tilt more and more, reaching an inclination on one side of 50°, on the other of 80°. At about 135° around the rim on either side we find a fault and a short stretch in which the strata stand vertically. These two narrow regions are ended by faults which separate them from the remaining part of the rim, through the middle of which the south end of the axis passes. This portion of the rim is simply lifted about 100 ft. and the inclination of the strata is 0°. The lines of the furthest thrown limestone boulders are nearly over the two regions in which the strata are vertical.

The experiment was tried of shooting a half inch spherical lead ball from a high-power rifle into a level floor of smooth densely packed silica. The inclination of the shot was about 30° from the vertical. The tilting of strata, of course, could not be observed; but the distribution of ejected matter on either side of the plane of incidence was remarkably like that described in the preceding account of the crater. The greatest amount of finely powdered material formed a rim to the shallow hole, ahead of the bullet. The edge over which the bullet passed had little or no matter piled up on it. The edges diametrically opposite, across the line of flight, were lined with powder and many lumps of silica, still forming definite masses, though the material is so friable that it was hard to pick up one of these lumps with the fingers without crushing it to powder. On either side of the plane of incidence, making angles with it of about 40°, were many fine marks or scratches in the smooth floor, showing where small particles had been driven ahead with great violence. The hole had sloping walls and an inner level floor. A sketch map made from the bullet hole and a map of the crater in which only those details are preserved which the bullet hole can show, are remarkably similar.

The theory of the strains which would end in such a distribution of the broken material ejected by an impact cannot be given; but the observations direct attention to some interesting peculiarities. The piling up of most of the ejecta ahead of the projectile is what might be expected, but it is less obvious that the stresses should be so distributed as to break up the material on either side of the line of flight into separate blocks and arrange them on either side of the hole along the rim. The two spurts of small fragments, thrown out forward diagonally from the line of flight, are also remarkable.

The lead bullet used was torn to fragments, and much of it was flung out of the hole. The other bullets tried, of other material, were generally badly deformed and torn; and were thrown backwards out of the hole, either whole or in fragments. The steel bullets were the only ones which retained their shape, and they remained buried in the holes made by them. It does not seem probable that much, if any, of the meteor which made the crater was thrown out of it in a similar way.

4. *Energy.*—The data from which to determine the energy with which the meteor struck the earth are not precise; but an estimate can be made of the energy with some degree of plausibility.

The work done in excavating the crater is insignificant in comparison with that done in crushing the rock. The mass ejected may be estimated at 330 million tons (of 2,000 lbs.). This is considerably more than the mass excavated in the construction of the Panama Canal. The bottom of this mass was 500 ft. below the original surface. To lift the mass up and clear of the hole would probably use 16×10^{10} ft. tons. Something more must be added for the work of tilting back the strata and lifting the unbroken rock masses all around the rim to a height of 100 ft. Perhaps 20×10^{10} foot tons spent in mechanical lifting would not be an overestimate.

Most of the energy was spent in breaking up the rock, and especially in shattering the grains of sandstone into the very finely pulverized silica. Only a rough guess can be made of the amount of this silica, but large parts of the ejecta consist of little else, and the borings have shown that the crater also contains great quantities of it. From the probable size and shape of the whole cavity it appears that over 500 million tons of rock were broken up. Of these, perhaps one fifth, or 100 million tons, are in the form of pulverized silica. The work done when this silica was formed was expended in separating the particles and in rubbing them over each other. The work done against friction must have been retained as heat. The temperature generally did not rise to the melting temperature of quartz, for the grains of silica rarely show evidence of fusion. A small quantity of melted quartz is found, which is full of blow holes, as if, when melted, the mass had been pervaded with an expansible vapor. One way of explaining this formation is to suppose that it represents the fusion of the shattered quartz in the presence of water, which is known to be present occasionally in pockets in the generally dry sandstone, the superheated steam formed at the same time accounting for the porous state of the material. If this explanation is correct, it indicates that the temperature of much of the silica was nearly that at which quartz melts when dry, and above the temperature at which it melts in the presence of water. If we set, as an outside limit of temperature, 2500° C., and suppose all the silica heated to that extent, the heat developed is equivalent to 9.25×10^{13} ft. tons. It is possible to ascribe the melting of the quartz to the heat more directly developed in the neighborhood of the advancing meteor. In this case it is more difficult to fix a lower limit of temperature for the silica. The iron found outside the crater in the silica shows no evidence of melting, and the Widmanstatten figures are preserved. If we assume the general temperature of 625°, to keep it not only below the melting point of iron, but also below that temperature at which the Widmanstatten figures disappear, the heat developed in the silica would then be equivalent to 2.3×10^{13} ft. tons.

The work done in the silica is surely only a fraction of the whole. A layer of hard limestone, 300 ft. thick, was also broken up, and much of it must have been pulverized also. After all, we can do no better than guess; but taking all the work done into account we may, in my opinion, estimate it without exaggeration, at 60×10^{12} ft. tons.

Such a projectile as would have made the crater would have reached the earth without retardation by the atmosphere. If it were moving, as a comet does, with the parabolic velocity of 25 miles a second, and were to encounter the earth head on, it might fall with a velocity something over 43 miles a second. We may set the outside limits of velocity at 3 and 48 miles a second. With the lowest velocity the mass required to bring in the estimated energy is 15×10^6 tons; with the highest velocity only 60,000 tons.

The mass of the meteor may otherwise be estimated from the size and shape of the crater. Experiments with cannon shot, quoted by Mr. Tilghman, show that with velocities of 1,800 ft. a second, the depth of the hole made in limestone rock is about twice the diameter of the shot, and its width at the top about five times the diameter. These proportions were well borne out by observations with spherical rifle balls shot into sandstone. If these proportions held for the crater they would indicate a projectile 750 ft. in diameter, with a mass of about 50×10^6 tons. This is evidently an over-estimate. The mass indicated in this way is however of the same order of magnitude as the greatest mass indicated by the estimates of the energy and velocity. Manifestly the extreme velocities estimated are not probable, so that the mass is probably neither so large nor so small as the extreme values obtained. A mass of 400,000 tons moving with a velocity of from 18 to 20 miles a second would bring in the estimated amount of energy. In the absence of other evidence this seems a reasonable mass to assign to the buried meteor.

THE CONVERSION OF THE ENERGY OF CARBON INTO ELECTRICAL ENERGY ON SOLUTION IN IRON.

By PAUL R. HEYL.

(*Read April 22, 1910.*)

In molten iron carbon dissolves with a liberation of energy, which, by providing a suitable negative electrode, may be obtained in the form of an electric current. The resulting electromotive force is quite small, but is clearly to be distinguished from the accompanying thermo-electric effect.

The experiment about to be described was, in outline, to set up a voltaic cell consisting of electrodes of wrought iron and carbon immersed in a bath of molten iron or steel of a low carbon content. Such an experiment is beyond the equipment of an ordinary laboratory, and for its execution I am indebted to the courtesy of Dr. Geo. W. Sargent, of the Carpenter Steel Company, Reading, Pa., who placed the necessary facilities at my disposal.

The ideal liquid for such a cell would be a carbonless iron, just as fresh acid in an ordinary cell is more effective than one which already contains some zinc in solution; but the melting point of such iron is too high to be reached in a steel furnace. It was, therefore, necessary to employ a low carbon steel. On account of its higher melting point wrought iron furnishes a convenient material for the negative element.

The positive element was built up of three carbon rods, such as are used in arc lights, wired firmly to a copper rod at their upper ends. Two such electrodes were prepared in case one should crack in the melted metal. To minimize as much as possible the danger of cracking, the carbon was gradually heated by placing it at first near, and finally in, an empty red-hot crucible. In this way the tips of the rod were made red hot.

Having no milli-voltmeter at my disposal it was necessary to use a milliammeter, which of course is equivalent to the other if the resistance of the circuit is known. Connections were made to the ammeter on the supposition that the current would leave the cell by the wrought iron electrode and return by the carbon. Care was taken, however, to insert a reversing key in the circuit.

About ninety pounds of a low carbon steel (0.10 per cent.) were placed in a crucible and heated for about four hours. Perfect fluidity could not be obtained, but the mass finally became pasty and viscous, allowing a rod of iron to be plunged into it without great difficulty. A small amount of aluminium was then added to free the mass from gas bubbles, and the crucible was then withdrawn from the furnace, placed on the floor and banked about with coal, which immediately took fire. As quickly as possible, for the metal rapidly hardened, the electrodes were plunged in while a watch was kept at the ammeter.

Almost immediately the carbon electrode snapped off at the surface of the metal, and so rapid was the cooling that by the time the spare carbon electrode could be brought into action the metal was too hard to allow it to be plunged in; but the small fraction of a second available to the watcher at the ammeter was sufficient.

Immediately on plunging in the carbon electrode the pointer of the ammeter started to move in the expected direction. When the carbon cracked the reading was 0.015 ampere. The pointer then fell back and exhibited a tendency to a reverse deflection. Turning the reversing key, the pointer again moved forward and remained stationary at about one third of its previous reading. On looking at the crucible it was found that the upper portion of the electrode was lying loosely upon the surface of the now solid, though still red-hot metal. On lifting the electrode the deflection disappeared, but reappeared as often as the carbon was touched to the hot metal or even to the (now hot) iron electrode. This deflection was plainly of a thermo-electric origin, while the earlier deflection in the opposite direction is to be explained only as the result of galvanic action.

How far the pointer would have moved had the carbon not cracked it is, of course, impossible to say. Neither is it possible to be certain as to the voltage corresponding to the observed reading of 0.015 ampere. The resistance of the ammeter and lead wires together was less than one ohm, and the uncertainty lies in the resistance of the hot metal and the contacts with it. The electrodes were about 15 cm. apart, and the crucible had a diameter of some 25 cm., so that, although the temperature was high, the resistance must have been very small. One ohm would probably be a liberal allowance. Assuming a total resistance of two ohms in circuit, the voltage would have been 0.03.

On general principles, we should expect that any such voltage would be very small. That there is any liberation of energy at all when carbon dissolves in melted iron indicates that the act of solution is, at least in part, chemical, and points to the existence of a new carbide of iron, stable at high temperatures, and of a simpler molecular formula and greater energy content than the well known carbide Fe_3C, which is stable only below a red heat, just as CO stands between CO_2 and free carbon and oxygen. Now the heat of formation of Fe_3C is not known, but its energy content must be but little less than that of its constituents in a free state; and consequently the margin of liberated energy at the formation of the hypothetical simpler carbide must be extremely small.

Further experiments on this matter are in progress.

THE ETHER DRIFT.

By AUGUSTUS TROWBRIDGE.

(*Read April 22, 1910.*)

At one time in the course of the development of physical science there were almost as many ethers postulated as there were phenomena to be explained. "Ethers were invented for the planets to swim in, to constitute electric atmospheres and magnetic effluvia, to convey sensations from one part of the body to another, till all space was filled several times over with ethers" (J. C. Maxwell). Of all these ethers the only one which has survived to our day is that of Huygens which is, so to speak, working over-time in that it has to serve the electrician as well as the optician and it is rather disheartening to think that after more than one hundred years of unremitting labor all that we really know of its properties is this—it transmits any electro-magnetic disturbance with the speed of three hundred million meters per second.

When we admit the hypothesis of the ether with this property the question arises as to the nature of the mechanical bonds between ether and matter—there are of course three possibilities—when matter is in motion either the ether moves with it completely, or it is partially entrained or it is not entrained at all.

The simplest of these three possibilities is perhaps the first—that the ether is completely entrained by the moving matter—it is the simplest hypothesis because it would mean that matter, once associated with a given quantity of ether, remained so forever and if this were the case we might hope to explain matter in terms of the ether indissolubly associated with it. Unfortunately the celebrated experiment of Fizeau on the change of velocity of light with and against a stream of moving transparent matter rules out this simplest hypothesis of complete entrainment and leaves us the choice of an ether which is partially entrained by moving matter or one which is a rest absolutely and not entrained at all.

The reason why we are left with these *two* possibilities is that the increased velocity of propagation of light in the direction of the moving matter which Fizeau clearly demonstrated by direct experiment may be due to the ether being dragged by the moving matter or again it may be that the ether is at rest while the matter moving through it in some way affects the speed of propagation of light.

It has been found impossible to develop a theory based on the first of these possibilities as an hypothesis without introducing very arbitrary assumptions as to the relation existing between the ether and moving matter but if the second possibility be adopted as an hypothesis, viz: the ether at rest in space; it has been found possible, by the Dutch physicist, Lorentz, to develop a theory without further arbitrary assumptions which is in accord with results of a considerable number of optical experiments.

If the ether be at rest it will be in motion relative to the earth as it moves in its orbit and one might expect to find a different velocity of propagation of light according as it is measured with or against the supposed drift of the ether. The effect to be expected will depend on the ratio of the velocity of the moving earth to the velocity of light—this ratio in round numbers is one ten-thousandth. There are many ways of detecting a variation in the velocity of light by this amount and a number of physicists have attempted to detect such a charge according as the velocity was measured along or across the direction of the supposed ether drift. All these experiments have given negative results and it is in favor of the theory of Lorentz that it explains the absence of this so-called first order effect when the light source and the observer are in motion together, but even on the Lorentz theory there should be an effect observable under these conditions which is proportional to the square of the ratio of the earth's velocity to the velocity of light—that is, proportional to one one-hundred millionth. This is called a second order effect.

Messrs. Michelson and Morley devised an experimental arrangement of sufficient sensitiveness to detect a second order effect due to relative motion of the earth and the ether, and they concluded that the earth must drag the ether along with it in its motion.

The results of this justly celebrated experiment of Michelson and Morley cannot be reconciled with the one theory which is in accord with the other optical phenomena such as stellar aberration and the Fizeau experiment, and which we have seen postulates an ether at rest in space and so it became necessary either to abandon the theory or to explain why the second order effect predicted by the theory should not have been detected in the Michelson-Morley experiment.

This explanation, due to Lorentz and Fitzgerald, was based on an assumed shortening of the linear dimensions of matter resulting from its motion through the ether. It is true that effects necessarily attendant on this hypothetical shortening have been sought in vain, but nevertheless this so-called Lorentz-Fitzgerald objection has tended to discredit the conclusions of Messrs. Michelson and Morley.

This, then, was the state of the question as to the relative motion of matter and ether when Professor C. E. Mendenhall and I undertook, in 1905, the work on which I am reporting at this time. (1) A well-developed theory based on the assumption of a stagnant ether which predicts a second order effect when the source of light and observer are in motion with respect to the ether. (2) Failure to detect any such effect by Michelson and Morley and the conclusion by them that the ether is at rest relatively to the moving earth and hence not stagnant. (3) The theory rehabilitated by an assumption that the linear dimension of matter is shortened by an amount of the second order when it moves through the ether. (To support this assumption good theoretical reasons were later adduced.)

In further experimentation it was obviously necessary to devise apparatus which should give indications which were independent of any hypothetical change of dimensions such as that suggested by Lorentz and Fitzgerald and be nevertheless sufficiently sensitive to detect the optical second order effect due to its motion relative to the ether.

The device adopted by us consisted of a source of light placed midway between two delicate electrical thermometers. Suppose the line joining the three points to lie in the direction of the motion through space of the point of the earth's surface where the apparatus is set up, and suppose the relative positions of the three points be such that each thermometer receives the same amount of radiation from the source of light which lies between them. Now, if the ether be drifting across the apparatus in the direction joining the three points as the theory of Lorentz would have it drift, and if thereby the dimensions of the apparatus in this direction be shortened as Lorentz and Fitzgerald have supposed it to be shortened, *then* if the whole apparatus be rotated 180° about a vertical axis the distance between the three points cannot have altered so that if any change in the amount of radiation received by the two thermometers were to be noticed on rotating the apparatus through 180° it might be taken as proof positive that the ether was in motion relative to the apparatus. On the other hand, if the apparatus were sufficiently sensitive to detect a second order optical effect and no change took place on rotation, the objection could not be made that the effect was there but had been masked by shortening, since no hypothetical shortening could be conceived to alter the relative positions of three points on a straight line.

Thus the problem which confronted us was to devise apparatus capable of detecting a change in radiation received by its two parts so small as one one-hundred millionth part of this radiation—expressed differently it was to devise a pair of electrical thermometers capable of detecting a difference of temperature of less than the ten-millionth part of a degree and yet to so protect them that in spite of the fact that they must stand several feet apart that they should be subject to no irregular fluctuation in temperature of this order of magnitude. Also they must be mounted on a support so rigid that it may be rotated without introducing irregularities due to change of shape. After considerable trouble we have succeeded in satisfying both these conditions, but there remains a third more difficult condition which we have as yet not been able to wholly satisfy—this is that the two conditions just mentioned must remain satisfied when the thermometers are subjected to radiation from a light source standing between them which is at a white heat and getting rid of energy at the rate of about one half a horse power.

In conclusion I would say that we confidently expect to be able to reduce the irregularities due to, as yet, uncontrolled temperature fluctuations—these now amount to about 8 times the effect we seek —two years ago they amounted to 1,000 times this effect.

CHARACTERISTICS OF THE INLAND-ICE OF THE ARCTIC REGIONS.

(PLATES XXVI-XXX.)

By WM. HERBERT HOBBS.

(Read April 22, 1910.)

CONTENTS

The Arctic Glacier Type.

Introduction.—As elsewhere pointed out, continental glaciers are in other than dimensional respects sharply differentiated from those types which have been described as mountain glaciers.[1] The ice-cap glacier, while of smaller dimensions than the true inland-ice or the continental glacier, is yet distinctly allied with this type, and has few affinities with mountain glaciers. The sharpness of the distinction has often been overlooked for the reason that true mountain glaciers frequently exist within a fringe surrounding the larger areas of inland-ice both in the Arctic and Antarctic regions. The distinguishing difference between mountain glaciers and continental glaciers is one primarily dependent upon the proportion of the land surface which is left uncovered by the ice, and the position of this surface relative to the margins of the snow-ice mass. *With true mountain glaciers land remains uncovered above the highest surfaces of the glacier, where, in consequence, a special erosional process—cirque recession—becomes operative.* The smaller ice-caps take their characteristic carapace form and cover the surface of the land within their margins, because that surface is relatively level. Had it been otherwise, the same conditions of precipitation would have yielded mountain glaciers in their place. The law above stated is none the less applicable, since because of this flat basement no land projects above their higher levels.[2]

[1] Wm. Herbert Hobbs, "The Cycle of Mountain Glaciation," *Geogr. Jour.*, Vol. 35, 1910, pp. 147, 148.

[2] With a keenness of insight which has rendered the descriptions of his travels particularly valuable, Sir Martin Conway has pointed out the main distinction here expressed. His explanation of cirque formation, which he

There are, as we shall see, other attributes which strikingly differentiate the large continental glaciers from all other bodies of land ice. These relate particularly to the nature of the snow which feeds them, to changes which that snow undergoes after its fall, to the manner of its transportation, etc. Most of these differences are of such recent discovery, or at least of such recent introduction into the channels of dissemination of science, that they have not yet found their way to the student of glacial geology. We shall profitably begin our description of continental glaciers with the intermediate ice-cap type, so as to establish connection with mountain glaciers in the important condition of alimentation. Before doing so, it will be well to call attention to some contrasts which exist between the north and south polar regions in those conditions upon which glaciation depends.

North and South Polar Areas Contrasted.—A glance at a globe, which sets forth the land and water areas of the earth, is sufficient to show that the disposition of land and water about the opposite ends of the earth's axis is essentially reciprocal. About the north pole we find a polar sea, the Arctic Ocean, surrounded by an irregular chain of land masses which is broken at two points, nearly diametrically opposite. In the Antarctic region, on the contrary, it is a high continent which surrounds the pole, and this is bounded on all sides by a sea in which are joined all the great oceans of the globe. This polar continent is deeply indented on two nearly opposite margins, but to what extent is not yet known. The margins of the continent are extended beneath the sea in a wide continental shelf or platform. The broad encircling ocean, while to some extent invaded by the southern land tongues of South America, Africa, Australia and New Zealand, is yet so little occupied by land masses that wind and ocean currents are alike but slightly affected by them. The Antarctic conditions are, therefore, oceanic—characterized by uniformity and symmetry to a much larger extent than is true of the northern polar region.

clearly recognized to be a result of sub-aerial disintegration rather than erosion, is however, practically that advanced by Richter, since it takes no account of the bergschrund. (W. M. Conway, "An Exploration in 1897 of some of the Glaciers of Spitzbergen," *Geogr. Jour.*, Vol. 12, 1898, pp. 142–147.)

Within the northern hemisphere a large quantity of heat from the tropics finds its way northward to the breaks in the northern land chain, through the medium of great ocean currents—the Gulf Stream in the Atlantic, and the Japanese Current in the Pacific. Cold return currents from the Arctic region, and the widely different specific heats of land and water, coöperating with the effect of the northward flowing warmer currents, result in a marked diversity in temperature, winds and precipitation at different longitudes within the same latitudes. Lack of symmetry in distribution and wide variations in climatic conditions are, therefore, characteristic of the north polar region; and it follows that the present glaciation of the northern hemisphere is localized within a few scattered areas where the land projects farthest toward the pole and near where there are sea areas of excessive evaporation to supply the necessary moisture.

The Fixed Areas of Atmospheric Depression.—Examination of Fig. 1 will show that the areas of existing heavy glaciation in the northern hemisphere lie close to the so-called fixed areas of low barometric pressure, each of which is a long, curved trough, concave to the northward, one central over the Aleutian Islands' Arc at the northern bay of the Pacific Ocean, the other extending from the southeastern extremity of Baffin Land past Cape Farewell, and northeastward across Iceland, so as to occupy similarly the northern bay of the Atlantic Ocean. For such northern latitudes these areas of fixed low barometric pressure are in consequence characterized by abnormally large evaporation. Wherever the moisture-laden winds proceeding from these areas are forced to rise by upland barriers, or to come in contact with cold rock or snow surfaces, condensation and precipitation must inevitably take place.

The prevailing westerly winds from the Pacific, when they encounter the high backbone of the Cordilleran System of mountains in Alaska nourish the great mountain glaciers of that region. The Cordilleras of Alaska are, however, competent to arrest but a small portion of these moisture-laden clouds, for it is only in moderate latitudes that they bar the way, and no highlands lie beyond them to the eastward until the vicinity of Baffin Bay has been reached.

On the border of the Atlantic low pressure area are found Greenland, Iceland, Spitzbergen, Norway, Franz Josef Land and Nova Zembla, each with its upland areas and its existing glaciation. In Norway, Iceland, and Franz Josef Land we find ice-caps; in Spitzbergen, Nova Zembla, Baffin Land, Grinnell Land and Ellsmere Land, the mantle of snow and ice is best described by the

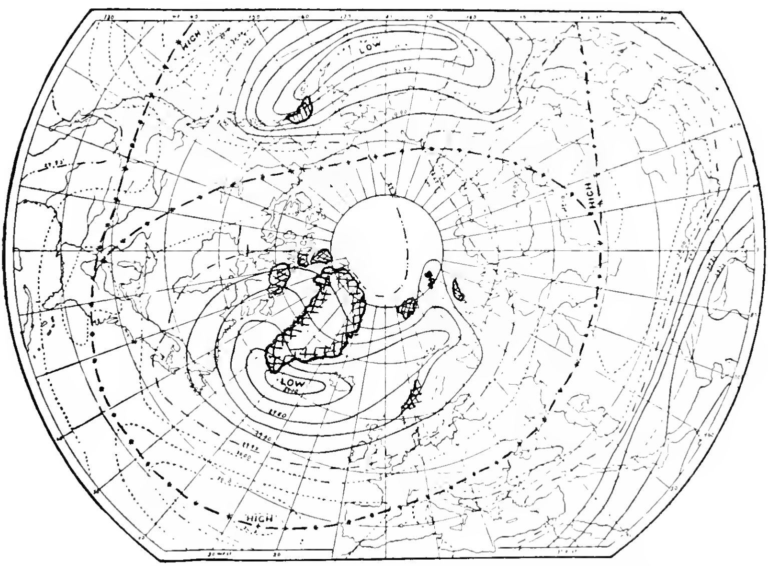


FIG. 1. Map showing the areas of fixed low barometric pressure in the northern hemisphere (after Buchan). The areas of heavy glaciation have been added.

name inland-ice, while upon the continent of Greenland the inland-ice has continental dimensions and forms one of the two continental glaciers that still exist.

Of all the northern ice sheets, with the exception of the Archipelago of Franz Josef Land, the rule holds that they are smaller than the land masses upon which they rest, and this in part expresses the difference between the northern and southern types of inland-ice.

Ice-caps of Norway.—In contrast with all save the Piedmont type of mountain glaciers, the snow fields of ice-caps are much the larger. Speaking broadly, high and relatively level plateaus, light winds, and low temperatures are favorable to the existence of ice-caps. Today they are not to be found in latitudes lower than 60°. In Norway, within the zone of heavy precipitation along the western coast, and upon the remnants of the plateau separated by the fjords, are still to be found a number of small ice-caps. These caps consist of a central carapace of snow and ice from the borders of which narrow tongues descend into the fjords. The largest of these ice-caps is the Jostedalsbraen, having an area of 1,076 square kilometers. Whereas with mountain glaciers the névé is contained within a basin, the cirque, we here find the so-called "fjeld" nearly level and resting upon the surface of the plateau. Of this fjeld broadly lobate extensions lie upon its margin separated by deep valleys or fjord heads. Much narrower extensions of the central carapace often descend the steep slopes at the upper end of these valleys and may continue down the valley floor. Their narrowness is largely explained by their more rapid motion upon the steeper slope and by the reflected heat from the rock walls on either side. See Fig. 2.[3]

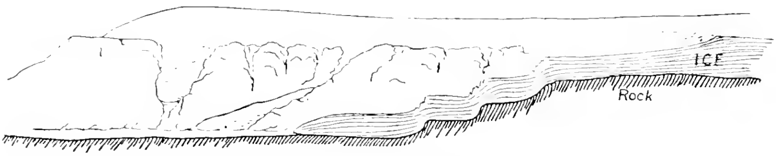


FIG. 2. Idealized section showing the form of "fjeld" and "brae" in Norwegian ice-cap.

Ice-caps of Iceland.—In Iceland are to be seen some of the finest examples of ice-caps that are known, and, fortunately, these have been carefully studied by Thoroddsen.[4] These ice-caps form gently domed crests or undulating ice fields situated upon the

[3] H. Hess, "Die Gletscher," 1904, pp. 66, 99–92.

[4] Th. Thoroddsen, "Island, Grundriss der Geographie und Geologie," *Pet. Mitt.* (Ergänzungshefts 152, 153), 1906, V., "Die Gletscher Islands," pp. 163–208.

highest plateaus which rise above the general table-land of the country. Projecting mountain peaks appear with few exceptions only near the thinnest margins of the ice, where they form either comb ridges or sharp peaks (see Figs. 3 and 4). White and altogether free from surface rock débris except in the vicinity of their margins, these ice-caps offer in this respect additional contrast to mountain glaciers. The largest of the Iceland ice-caps is the Vatna Jökull, which has an area of 8,500 square kilometers,

Fig. 3. Maps of the Hofs Jökull and the Lang Jökull (after Thoroddsen).

while the surfaces of the Hofs Jökull, Lang Jökull and Myrsdals Jökull each exceed a thousand square kilometers. The shield-like boss of the Vatna Jökull is brought out in the section of Fig. 5.[5]

Those borders of this ice mass which lie upon the plateau, the northern and western areas, are broadly lobate; but upon the southern and eastern margins, where the ice mass descends to lower levels and approaches the sea, its tongues sometimes end a few meters only above sea level. It is noteworthy, however, that where deeply incised valleys invade the plateau upon this margin, the lobes of ice push out mainly upon the upland remnants between

[5] Hans Spethmann, "Der Nordrand des isländischen Inlandeises Vatnajökull," *Zeitsch. f. Gletscherk.*, Vol. 3, 1909, pp. 36–43.

the valleys, though they send narrow tongues down the valleys themselves. This, as we shall see, is a peculiarity which ice-caps and the northern inland-ice as well have in common to distinguish

Fig. 4. Map of the Vatna Jökull (after Thoroddsen).

them further from mountain glaciers. As was found true of the Norwegian glaciers, so here the tongue which follows the valley bottom and which partakes of many of the properties of a mountain glacier, is much the narrower.[6]

From the north or plateau margin of the Vatnajökull flow mighty but sluggish streams which near the glacier are braided into constantly shifting channels within a broad zone of quicksand. In this sand, horse and rider if once entangled are quickly lost. Upon the south margin, on the other hand, the streams from the melt-

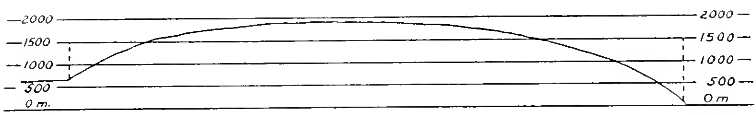


Fig. 5. Cross section of the Vatna Jökull from north to south (after Thoroddsen and Spethmann).

ing of the ice flow as fast rushing rivers, sometimes so broad as not to be bridged, and in these cases setting up impassable barriers between districts.

Icelandic ice-caps differ from all well-known glaciers at least in this, that nowhere else are large ice masses in such direct association with so active volcanoes. The *jökulhlaup,* which is the Icelandic name applied to one of the characteristic catastrophies of the island, occurs whenever a volcano, either visible in the neighborhood of the glacier or hidden beneath it, breaks suddenly into eruption. The first intimation that such an event is transpiring, is often the drying up of a stream which flows from the affected region. Sometimes the people are permitted to see great masses of lava and volcanic ash issue together from the glacier. All at once, the stream which had first dried up comes rushing down its valley as a foaming flood of water, spreading out for miles and having a depth sometimes as great as 100 feet. The entire plain is then spread with mud and sown with great rocks and also with ice blocks, some of which are as large as the native houses. These ice blocks are

[6] Carl Sapper, "Bemerkungen über einige südisländische Gletscher," *Zeitsch. f. Gletsch.*, Vol. 3, 1909, pp. 297–305, two maps and three figures. See especially Fig. 3.

often buried in the mud, and later, when they have melted, they leave deep pits in the plain similar to though smaller than the depressions in a "pitted plain" from the continental glaciers of Pleistocene time.

During such an eruption, water has been seen to shoot up from the glacier in great jets, and it has sometimes happened that the entire ice mass of the jökul has been shattered, and a chaotic mass of ice miles in width has slipped resistlessly down the slopes. With the conclusion of the disturbance, the aspect of the entire district is sometimes found to be utterly changed. All vegetation has been destroyed, and ridges which had lent to the landscape its character have vanished, so that streams have lost their old channels and entered upon wholly different courses.[7]

Ice-covered Archipelago of Franz Josef Land.—The islands of Franz Josef Land in the high latitude of 80° and over, with altitudes of 2,000 to 4,000 feet and situated as they are on the borders of an open sea, are the most Arctic in their aspect of all the smaller northern land masses. As a consequence, they are with unimportant exceptions completely snow capped, the snow-ice covering sloping regularly into the sea upon all sides. The Jackson-Harmsworth[8] and Ziegler[9] expeditions, following those of Nordenskiöld, Nansen, the Duke of the Abruzzi, and others, have now supplied us with fairly accurate maps of all islands in the archipelago. One or two of the western islands alone show a narrow strip of low shore land, but with these exceptions all are covered save for small projecting peaks or plateau edges near the ice margins (see Fig. 6). They present, therefore, a unique exception to the law which otherwise obtains, that within the northern hemisphere glacial caps are smaller than the land areas upon which they rest. The appearance of the island covers is here, however, that of névé of low density, rather than of compact glacier ice.

Prince Rudolph Island, which was the winter station of the Italian polar expedition, is no doubt typical of most islands in the

[7] Otto Nordenskiold, "Die Polarwelt," 1909, pp. 42–43.

[8] F. G. Jackson, "A Thousand Days in the Arctic," 1899, map 5.

[9] Anthony Fiala, "The Ziegler Polar-Expedition of 1803–5," 1907, map C.

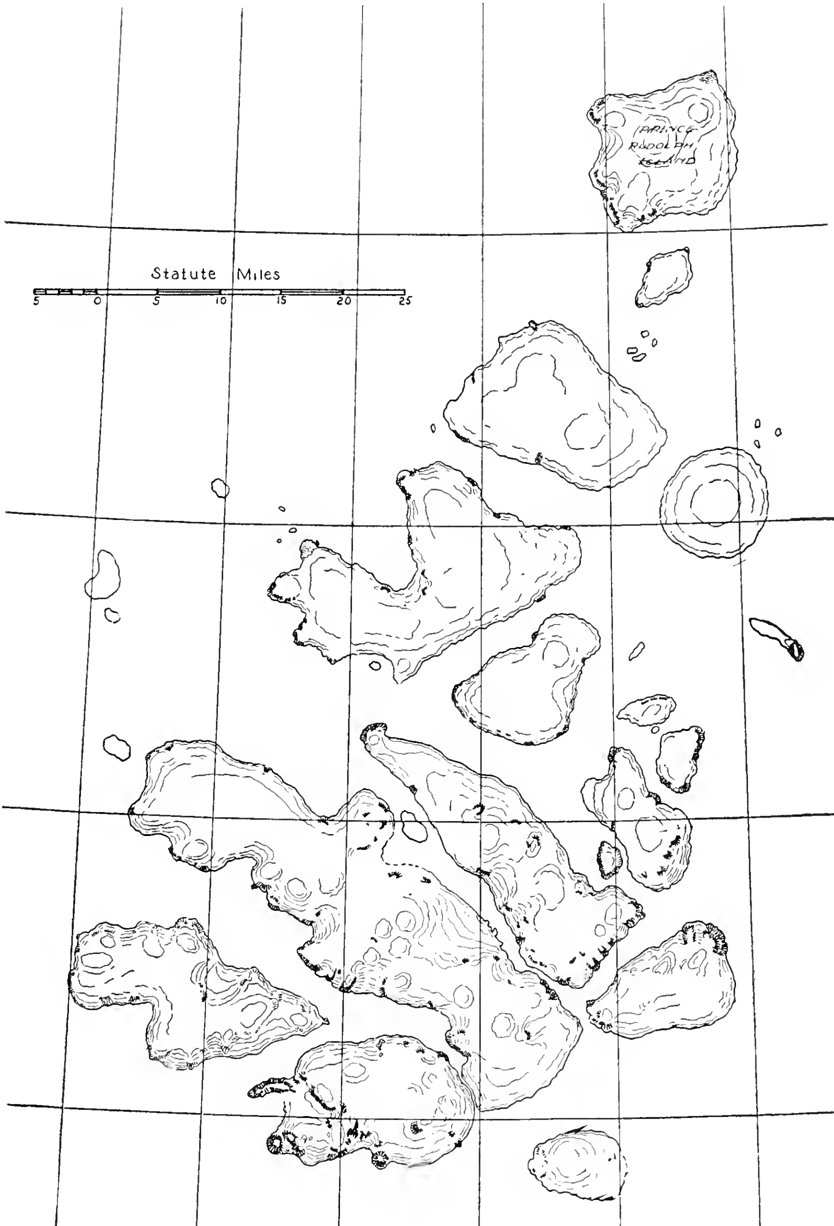


FIG. 6. Map of the ice-capped islands in the eastern part of the Franz Josef Archipelago (after Fiala).

archipelago. This island is described by Duc d'Abruzzi[10] as "completely buried under one immense glacier, which descends to the sea in every direction except at a few points, such as Cape Germania, Cape Säulen, Cape Fligely, Cape Brorok, Cape Habermann and Cape Auk. At some of these points, . . . the coast is almost perpendicular, which prevents the ice from descending to the sea. At others, . . ., the ice, stopped by a hollow, falls into the sea on each side of the headland, which thus remains uncovered. Moreover, wherever the snow can rest, there are glaciers which end at the sea in an ice-cliff, like that formed by the main glacier, so that it can be said that the entire coast, with the exception of a short extent of strand near Teplitz Bay, is formed by a vertical ice-cliff" (see Fig. 7).

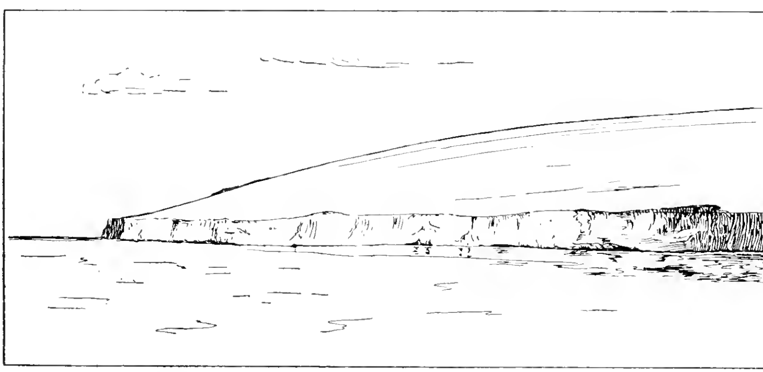


FIG. 7. Typical ice cliff of the coast of Prince Rudolph Island, Franz Josef Land (after the Duke of the Abruzzi).

The movement of the ice is so slow that though a line of posts was established for the purpose of measuring during a period of four months, no movement could be detected. Except near the outermost margin, there were few crevasses, and these were covered by snow. In summer, on days when the temperature was above the freezing point, the snow thawed rapidly so that torrents of water flowed from the glacier to the sea, hollowing out channels many feet in width.

During the stay of the "Polar Star" near the island, it was

[10] On the "Polar Star" in the Arctic Sea, 1903, Vol. 1, pp. 116–118.

noteworthy that thaw and evaporation upon the island exceeded the precipitation. Doubtless because of the slow movement of the ice, no icebergs were seen to form during the entire stay.

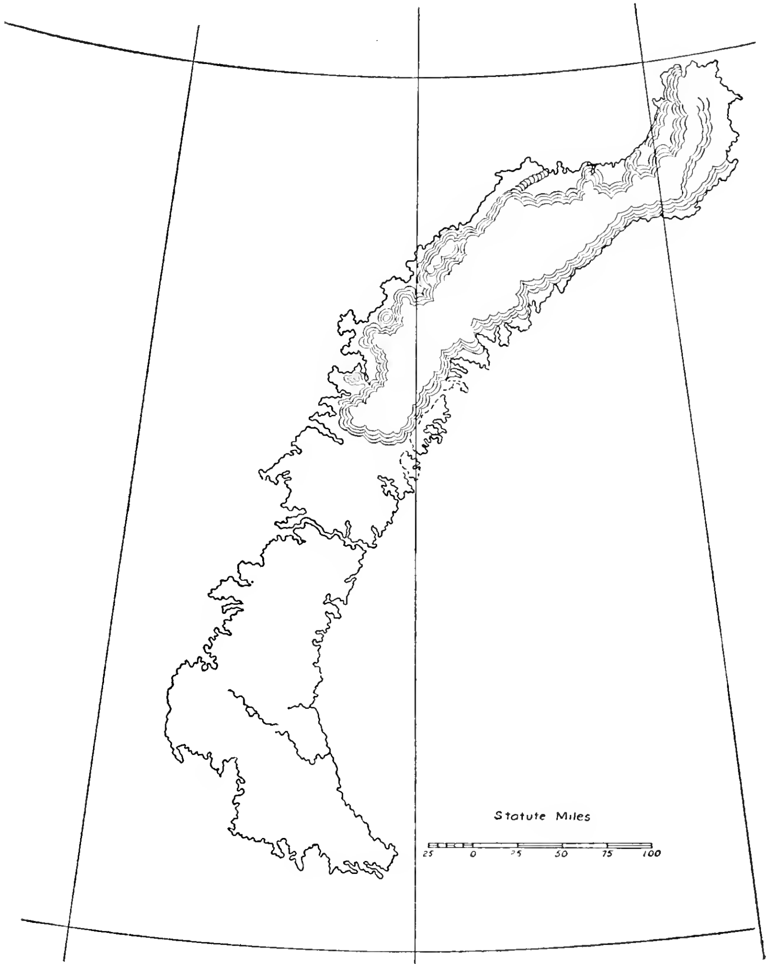


Fig. 8. Map of Nova Zembla, showing the supposed area covered by inland-ice (from Andree's "Handatlas").

The Smaller Areas of Inland-ice Within the Arctic Regions.—The ice-cap of the Vatna Jökull in Iceland, which is the largest to which this name has been applied, covers an area of 8,500 square kilometers. Ice carapaces which are better described as inland-ice, since they cover considerable proportions of the interiors of the land areas upon which they rest, occur to the northward of Europe in Nova Zembla and Spitzbergen, and in the lands to the west of Baffin's Bay, known as Baffin, Ellesmere, and Grinnell lands.

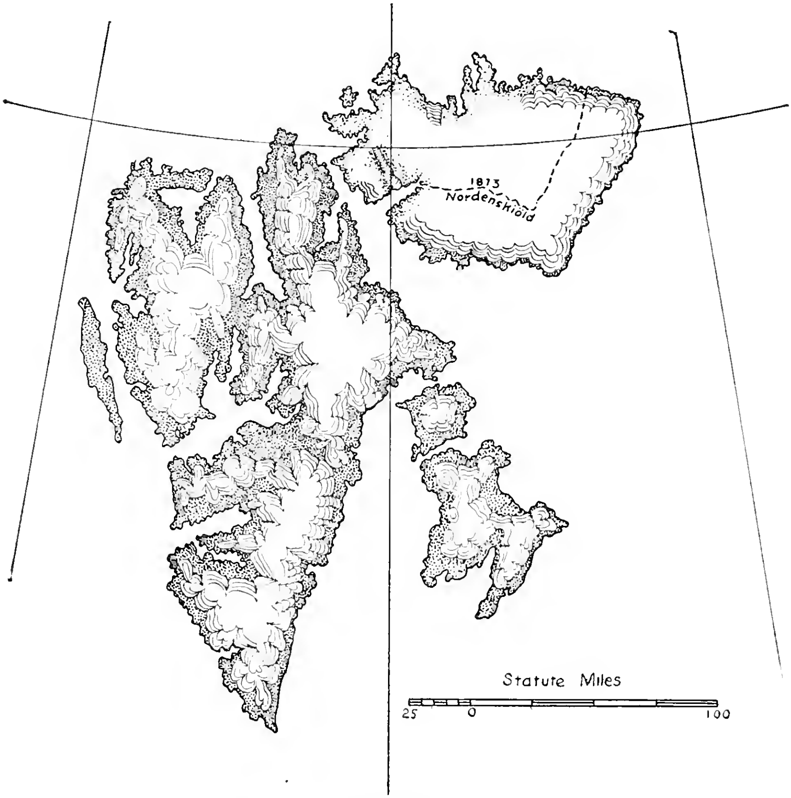


FIG. 9. Map of Spitzbergen, showing the supposed glaciated areas (from Andree's "Handatlas").

Nova Zembla is a long, narrow island, stretching between 70° and 84° of north latitude (see Fig. 8). It is in reality two islands separated by a narrow strait near the latitude of 76°. The northern island, which to the north is a plateau attaining an altitude of 1,800 feet, appears to be in large part covered by inland-ice, though it has been as yet but little explored.

The Inland-ice of Spitzbergen.—The group of islands to which the name Spitzbergen has been applied, lies between the parallels of 76° and 81° of north latitude. The surface is generally mountainous, the highest peaks rising to an elevation of about 5,000 feet, though the greater number range from 2,000 to 4,000 feet in altitude. The large northeastern land mass is called North East Land and is covered with inland-ice which was crossed by Nordenskiöld and Palander in 1873[11] (see Fig. 9). New Friesland, or the northeastern portion of the main island, is also covered by inland-ice[12] (see Fig. 10). The southwestern margin of this inland-ice was

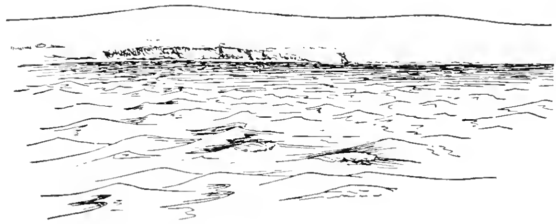


FIG. 10. Inland-ice of New Friesland as viewed from Hinloopen Strait (after Conway).

somewhat carefully mapped by Conway and Gregory in 1896,[13] and as this presents some interesting general features, it is reproduced in part in Fig. 11.

In addition to the lobes which push out upon the crest of the plateau, there is here an expansion laterally beyond the main cap and at lower levels in the form of an apron which is called the Ivory Gate (compare the Frederikshaab Glacier in Fig. 38, p. 119). Surrounding the inland-ice to the westward are small ice-caps resembling the fjelds and braes of Norway, and also true mountain glaciers whose cirques have shaped the mountains into the sharp pinnacles of comb ridges. It is to these sharp peaks that Spitzbergen owes its name.

[11] A. E. Nordenskiold, "Grönland" (authorized German edition), map on p. 141.

[12] W. Martin Conway, "An Exploration in 1897 of some of the Glaciers of Spitzbergen," *Geogr. Jour.*, Vol. 12, 1898, pp. 137–158.

[13] Sir Wm. Martin Conway, "The First Crossing of Spitzbergen," London, 1897, pp. 371, 2 maps.

In the year 1890 Gustav Nordenskiöld made a journey between Horn Sound and Bell Sound on the west coast, and found behind the sharp peaks bordering the coast an ice surface almost without crevasses or nunataks.[14] Upon the northwest coast no sharp peaks or comb-ridges are found, but there is a low plateau with deep, narrow valleys similar to the west coast of Norway where it

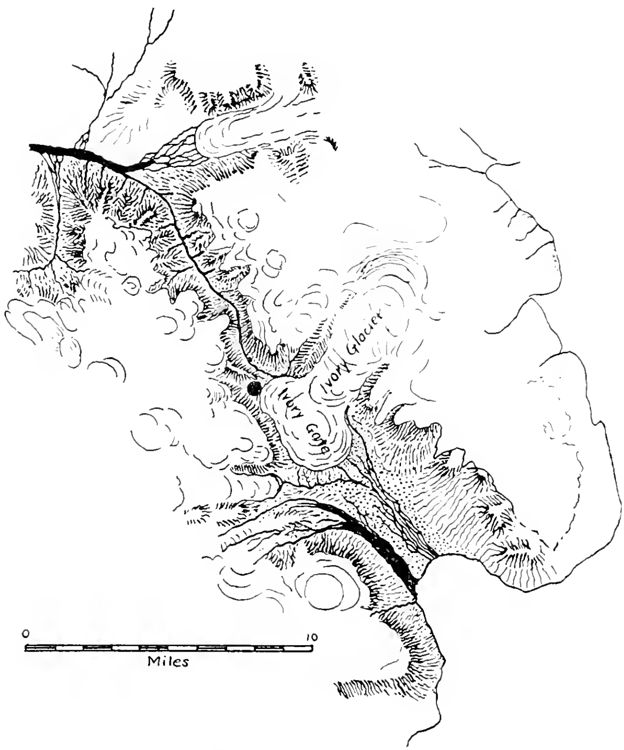


FIG. 11. Map of the southwestern margin of an extension of the inland-ice of New Friesland (after Conway).

reaches the sea near the North Cape. All the rock surfaces are glaciated.

The inland-ice of North East Land reaches the sea upon the southern and eastern coasts, but is surrounded by a hem of land upon the north and west. Over the surface of this ice Nordenskiöld

[14] O. Nordenskiöld, *l. c.*, p. 52.

observed that the snow does not melt even in midsummer, and that hence there were developed no lines of surface drainage. Of especial note were the great crevasses which ran generally in straight lines for long distances in parallel series, sometimes two interesting series being observed. More remarkable than these, however, were the so-called "canals," which also for the most part ran parallel to each other and in some cases were only 300 feet apart. These

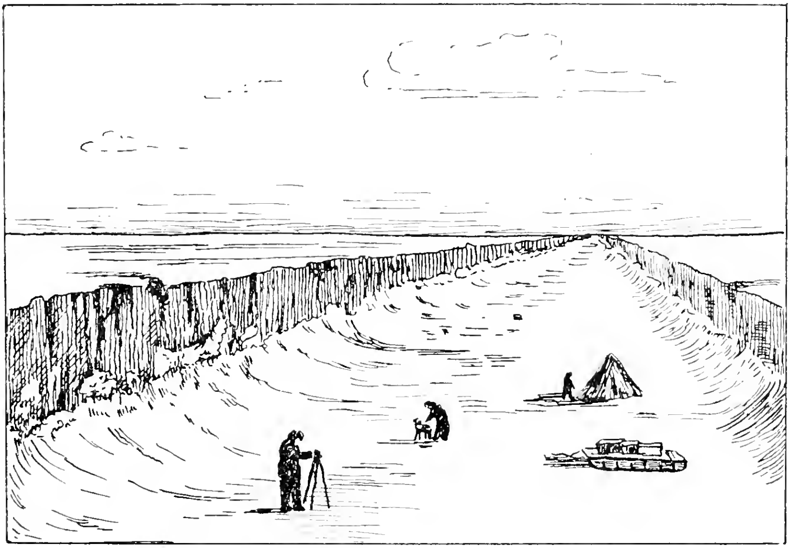
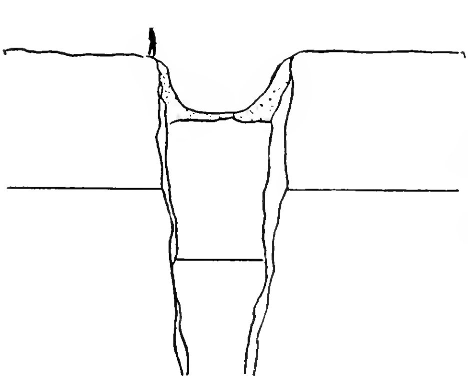


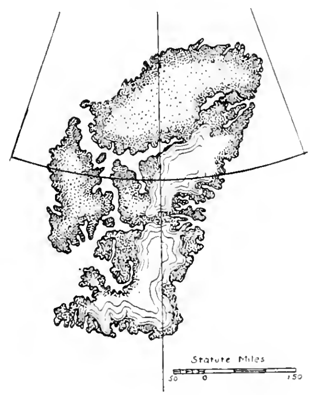
FIG. 12. Camping place in one of the "canals" upon the surface of the inland-ice of North East Land, Spitzbergen (after Nordenskiöld).

canals, which were found in the southeastern part of the area near Cape Mohn, were in reality deep, flat-bottomed troughs within the ice, bounded on either side by parallel and rectilinear ice cliffs, and were in places partially filled by the indrifted snow. Stretching for long distances over the snow plain, and set so deeply that they could be entered only where fortuitous drifting of the snow supplied an incline, they were utilized for camping places (see Fig. 12).

Nordenskiöld has explained these canals as trough faults within the ice, and has assumed that this deformation was due to changes of volume incidental to extreme temperature range (see Fig. 13). This explanation in temperature changes would leave the absence of such structures in other places wholly unaccounted for, and we venture to believe that a recent trough faulting within the rock basement below the ice and communicated upward through it, would furnish a more reasonable assumption, particularly in view of our later knowledge of dislocations connected with earthquake disturbances.



13



14

FIG. 13. Hypothetical cross section of a glacial canal upon the inland-ice of North East Land (after Nordenskiöld).

FIG. 14. Map showing the supposed area of inland-ice upon Grinnell and Ellesmere Lands (from Andree's "Handatlas").

Still deeper in-breaks of the ice were encountered within the same region. These, though deeper, were generally of less extent, and were designated by the sailors of the party "docks" or "glacier docks."

The Inland-ice of Grinnell, Ellesmere and Baffin Lands.—Something has been learned of the inland-ice of Grinnell Land (see Fig. 14) from the report of Lieutenant Lockwood upon his crossing of Grinnell Land in 1883.[15] Of especial interest is his description of the ice front or face as it was observed for long distances

[15] A. W. Greely, "Report on the Proceedings of the United States Expedition to Lady Franklin Bay, Grinnell Land," Vol. 1, especially Appendix No. 86, pp. 274–279, pls. 1–4. See also Salisbury, Jour. Geol., Vol. 3, p. 890.

in the form of a perpendicular wall which he described under the name "Chinese Wall." Over upland and plain this wall extended with little apparent change in its character. At one place by pacing and sextant angle its height was estimated at 143 feet (see Fig. 15).

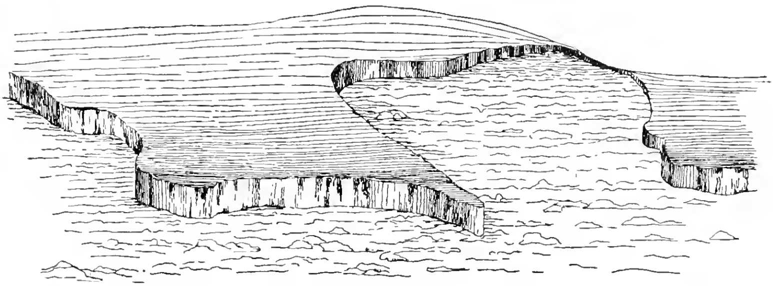


Fig. 15. View of the "Chinese Wall" surrounding the Agassiz Mer de Glace on Grinnell Land (after Greely).

The inland-ice of Ellesmere Land (see Fig 14) has been to some extent explored along its borders by members of the Sverdrup expedition. The maps of the margin in the vicinity of Buchanan Bay display much the same characters as may be observed along the margins of the better known ice-caps and inland-ice masses of the northern hemisphere.[16]

Of the inland-ice of Baffin Land little is known (see Fig. 16). There are some indications that a small ice-cap exists upon the neighboring island of North Devon.

Physiography of the Continental Glacier of Greenland.

General Form and Outlines.—The inland-ice of Greenland, we have now good reason to believe, has the form of a flat dome, the highest surfaces of which lie somewhat to the eastward of the medial line of the continent,[17] and which envelops all but a relatively narrow marginal rim. This marginal ribbon of land is usually from five to twenty-five miles in width, may decrease to noth-

[16] Otto Sverdrup, "New Land," 2 vols., London, 1904, pp. 496–504.

[17] F. Nansen, "The First Crossing of Greenland," Vol. 2, p. 404; R. E. Peary, "Journeys in North Greenland," *Geogr. Jour.*, Vol. II., 1898, p. 232.

ing, but in two nearly opposite stretches of shore it widens to from sixty to one hundred miles (see Fig. 17).

At the heads of many deep fjords long and narrow marginal tongues pushing out from the central mass reach to below sea level; and within three limited stretches of shore the ice mantle overlaps the borders of the continent and reaches the sea in a broad front.

FIG. 16. Map showing the supposed area of inland-ice upon Baffin Land (from Andree's "Handatlas").

The longest of these begins near the Devil's Thumb on the west coast at about latitude 74′ 30″, and extends with some interruptions for about one hundred and fifty miles along the coast of Melville Bay.[18] Where crossed by Nansen near the parallel of 64°, and hence near the southern margin, and also where traversed by Peary near its northwestern borders, the inland-ice has revealed much

[18] T. C. Chamberlin, "Glacial Studies in Greenland," III., *Jour. Geol.*, Vol. 3, 1895, pp. 62–63.

the same features. The great central area has never been entered, although Baron Nordenskiöld and Lieutenant Peary, as well as Nansen, have each passed somewhat within the margin near the

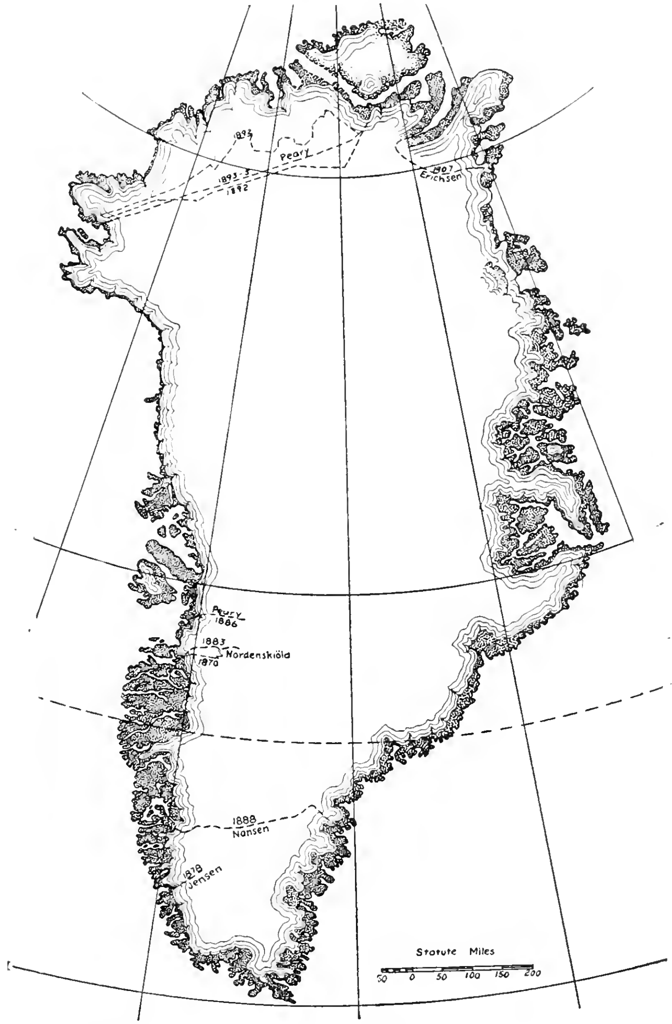


FIG. 17. Map of Greenland, showing the outlines of the inland-ice (from Andree's "Handatlas," but corrected for the northeast shore from data of the Danish expedition of 1908). The routes of the various expeditions on the inland-ice have also been added.

latitude of 68°. More recently (1907) Mylius Ericksen met his tragic death in crossing the inland-ice in northeast Greenland, but his results, most fortunately recovered, are not yet published. Yet such is the monotony of the surface thus far revealed, and such the uniformity of conditions encountered, that there is little reason to think future explorations in the interior will disclose anything but a desert of snow with such small variations from a horizontal surface as are not strikingly apparent to the traveller at any one observing point.

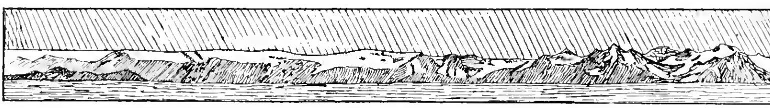


FIG. 18. Sketch of the east coast of Greenland near Cape Dan. Shows the inland-ice and the work of marginal mountain glaciers (after Nansen).

Nansen has laid stress upon the close adherence of the curve of his section to that of a circle, and has attempted to apply this interpretation to the sections of both Nordenskiöld and Peary made near the latitude of Disco Bay.[19] If the marginal portions of the sections be disregarded, this interpretation is possible for Nansen's own profile, since it is in any case very flat; but inasmuch as the margins only were traversed in the other sections, the conclusions drawn from them are likely to be misleading when extended into the unknown interior.

Hess,[20] correcting Nansen's data so as to take account of the curvature of the earth, finds the radius of this circle of the section to be approximately 3,700 km. (instead of 10,380 km., as given by Nansen). This radial distance being considerably less than the average for the earth's surface, the curvature of the ice surface where crossed by Nansen is considerably more convex than an average continental section.

We are absolutely without knowledge concerning either the thickness of the ice shield or the elevation of the rock basement

[19] H. Mohn und Fridtjof Nansen, "Wissenschaftlichen Ergebnisse von Dr. F. Nansens Durchquerung von Grönland, 1888," *Pet. Mitt. Ergänzungsh.*, 105, 1892, pp. 1–111, 6 pls., 10 figs. Especially Plate 5.

[20] "Die Gletscher," pp. 105–106.

beneath it; though a height of the snow surface of approximately 9,000 feet was reached by Nansen at a point where it could hardly be expected to be a maximum. The snow surface to the north of his section was everywhere rising, and it is likely that it attains an altitude to the northeastward well above 10,000 feet.

Though doubtless almost flat within its central portions, and only gently sloping outward at distances of from seventy-five to one hundred miles within its margin, the snow surface falls away so abruptly where it approaches its borders as to be often quite difficult of ascent (see Fig. 19).[21] The monotony of the flatly arched cen-

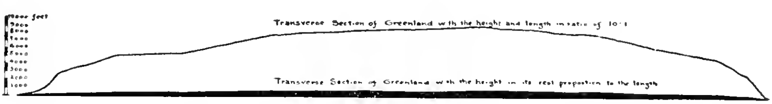


FIG. 19. The section across the inland-ice of Greenland, near the 64th parallel of latitude in natural proportions and with vertical scale ten times the horizontal (after Nansen).

tral portion of the isblink gives place to wholly different characters as the margins are approached. The ice descends in broad terraces or steps, which have treads of gentle inclination but whose risers are of greater steepness, and this steepness is rapidly accelerated as the margin is neared. In Fig. 20 have been placed together for comparison the profiles of Peary, Nordenskiöld and Nansen on the different routes which they travelled toward the interior from the coast.

The margins of the Greenland continent where uncovered by the ice, are generally mountainous, with heights reaching in many cases to between 5,000 and 8,000 feet on the east shore[22] and between 5,000 and 6,000 feet on the west shore. The bordering ice-caps within these areas are developed in special perfection on the islands of the archipelago about King Oscars fjord and Kaiser

[21] R. E. Peary, "A Reconnoissance of the Greenland Inland-ice," *Jour. Am. Geogr. Soc.*, Vol. 19, 1887, pp. 261–289.

[22] Petermann Peak near Franz Josef fjord on the east coast, which according to Nansen has an estimated height of 11,000–14,000 feet, has recently been shown to be not more than half that height (A. G. Nathorst, *Pet. Mitt.*, Vol. 45, 1899, p. 242).

Franz Josef fjord on the east coast near latitude 75° N., as these have been mapped by the Swedish Greenland Expedition of 1899 (see Fig. 21).[23] The work of mountain glaciers about King Oscars fjord is clearly displayed by Nathorst's photograph reproduced in Plate XXVI, *A*. Essentially the same features are shown also to the right in Fig. 18 (p. 78).

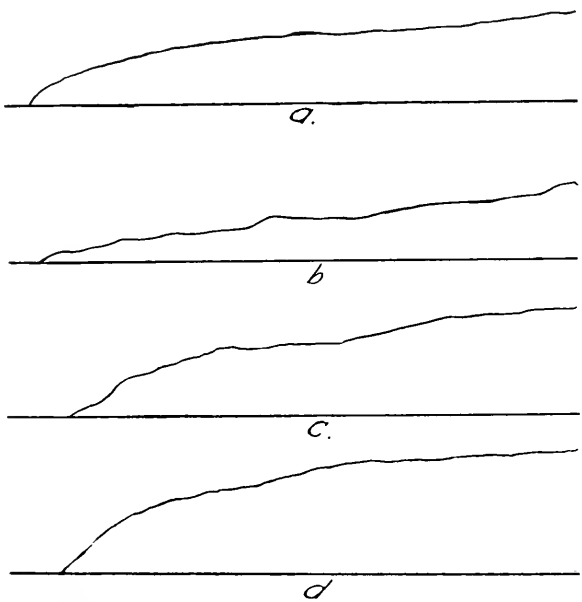


FIG. 20. Comparison of the several profiles across the margin of the inland-ice (*a*) at latitude 69 1/2° on the west coast (Peary); (*b*) at latitude 68 1/2° on the west coast (Nordenskiöld); (*c*) at latitude 64° on the west coast (Nansen); and (*d*) at latitude 64 1/2° on the east coast (Nansen).

While we are without absolute knowledge of the relief of the land beneath most of the inland-ice, we know that the mountainous upland of the coast extends well within the ice margins, since the peaks project through the surface as ice-bounded rock islands or *nunataks*. The irregularities of this basement and the submergence and consequent drowning of the valleys to form deep fjords within the marginal zones, largely account for the markedly lobate out-

[23] A. G. Nathorst, "Den svenska expeditionen till nordöstra Grönland." 1899, *Ymer*, Vol. 20, 1900, map 11.

lines of the so-called isblink[24] or inland-ice, as well as for the ice-caps and mountain glaciers, which originating in the outlying plateaus and mountains, form a fringe about the central ice mass.

It has been shown to be characteristic of the ice-caps and smaller inland-ice areas of the Arctic region outside of Greenland, that their lobate margins are in part accounted for by extensions of the

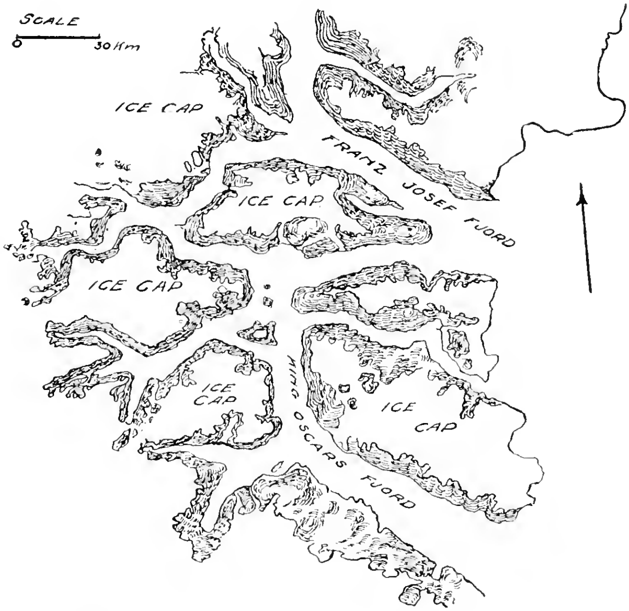


FIG. 21. Map of the region about King Oscars and Kaiser Franz Josef fjords, Eastern Greenland, showing the areas of the numerous ice-caps (after P. Dusen).

cap upon the plateau between intersecting valleys or fjords, as well as by extensions down these valleys. These latter extensions of the ice sheets are, however, much the narrower. Identically the same features are found to characterize the Greenland inland-ice as well. The manner in which this occurs in Greenland has been

[24] "Iceblink," which has been suggested by some writers, is a term generally applied among navigators to describe the appearance of ice on the horizon, and is contrasted with "land blink," which describes the peculiar loom of the land. In order to apply the term to the inland-ice without confusion, it is, therefore, better to retain the Danish form of the word.

well brought out in a map and section by Helland[25] of the Kangerdlugsuak fjord and glacier, but even better by recent maps of the

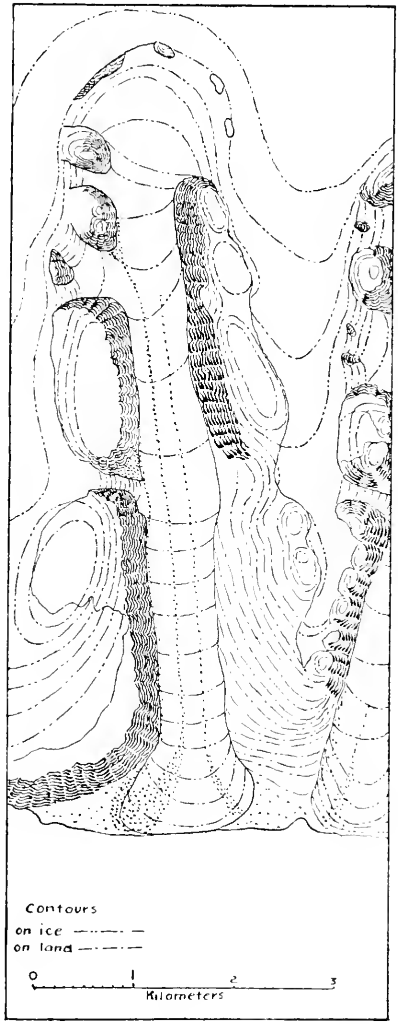


Fig. 22. Map of a glacier tongue, which extends from the inland-ice down the Umanak fjord (after von Drygalski).

[25] A. Helland, "On the Ice Fjords of North Greenland and on the Formation of Fjords, Lakes, and Cirques in Norway and Greenland," *Quart. Jour. Geol. Soc.*, Vol. 33, 1877, pp. 142–176.

Petermann Fjord by Peary (Fig. 25, p. 89) and the Umanak fjord by von Drygalski[26] (see Fig. 22). The manner in which the ice sometimes descends from the higher levels over the steep walls of the fjords has been strikingly brought out in a photograph of the Foetal glacier (see Fig. 23).[27]

As already stated, within one limited stretch upon the west coast the ice mantle overlaps the borders of the continent and reaches

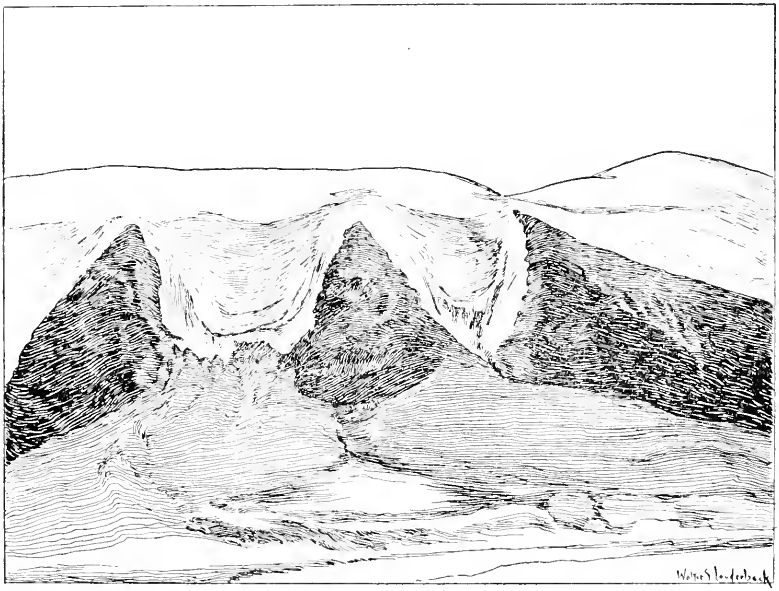


FIG. 23. Tongues of ice descending from the Foetal glacier, McCormick Bay (after Peary).

the sea in a broad front. This stretch of coast begins near the Devil's Thumb at about latitude 74° 30′ and extends, with some interruption, for about 150 miles along the coast of Melville Bay.[28] On the northeast coast the recent explorations of the Danes indicate that there are two stretches of 20 and 60 miles, respectively,

[26] E. von Drygalski, "Grönland-Expedition," Vol. 1, 1897, Map 7.

[27] R. E. Peary, "Journey in North Greenland," *Geogr. Jour.*, Vol. 11, 1898, pp. 213–240.

[28] T. C. Chamberlin, "Glacial Studies in Greenland, III.," *Jour. Geol.*, Vol. 3, 1895, p. 61.

within which the ice in like manner reaches the sea. These occur on Jökull Bay and on the north shore of the North East Foreland (see Fig. 24).[29]

The Ice Face or Front.—Concerning the form of the front of the inland-ice where it lies upon the land, widely different descriptions have been furnished from different districts. It is necessary

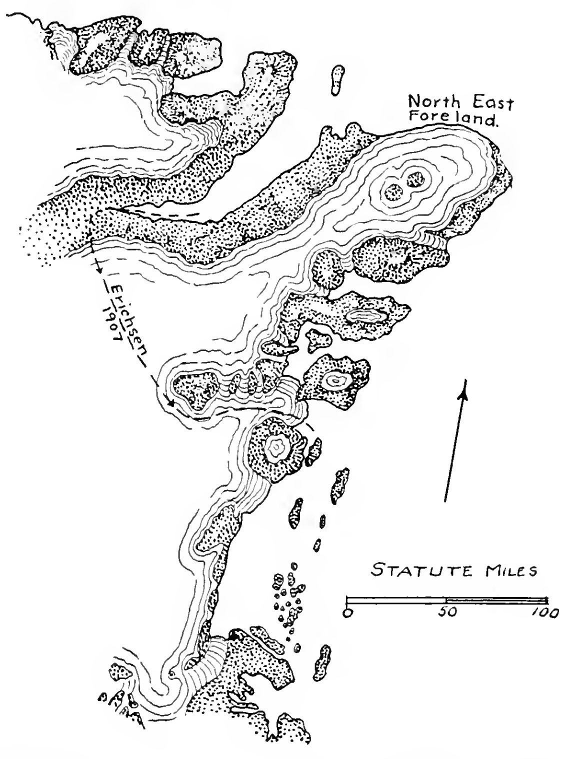


Fig. 24. Map of the Greenland shore in the vicinity of the North East Foreland (after Trolle).

to remember that the continent of Greenland stretches northward through nearly 24° of latitude, and after due regard is had to this consideration, the differences in configuration may perhaps be found to be but expressions of climatic variation. Those who have

[29] Lieut. A. Trolle, "The Danish Northeast Greenland Expeditio ·, *Scot. Geogr. Mag.*, Vol. 25, 1909, pp. 57–70 (map and illustrations).

studied the land margin of the isblink in North Greenland, all call attention to the precipitous and generally vertical wall which forms the ice face (see Plate XXVI, *B*). As a result of shearing and overthrusting movements within the ice near its margin, as well as to the effect of greater melting about the rock fragments imbedded in the lower layers of the ice, the face sometimes even overhangs in a massive ice cornice at the summit of the wall (see Plate XXVII, *A*).[30]

That to this remarkable steepness of the ice face as observed north of Cape York there are exceptions, has been mentioned by both Chamberlin and Salisbury, but Peary has also emphasized the vertical face as a widely characteristic feature of North Greenland. The recent Danish expedition to the northeast coast of Greenland has likewise furnished examples of such vertical walls. An instance where the ice face appears as a beautifully jointed surface somewhat resembling the rectangular joint-walls in the quarry faces of certain compact limestones, is reproduced from the report of the expedition in Plate XXVII, *B*.[31]

Attention has already been called to the precipitous front, the so-called "Chinese Wall," which Lieutenant Lockwood found to form the land face of the inland-ice of Ellesmere Land—a face which was followed up and down over irregularities of the land surface, and whose height in one place was roughly measured as 143 feet (see Fig. 15, p. 75).

From central and southern Greenland, on the other hand, we hear little of such ice cliffs as have been described, and Tarr in studies about the margin of the Cornell extension of the isblink[32] has shown that here the vertical face is the exception.[33] The normal sloping face as there seen is represented in Plate XXVIII, *A*. In following the ice face for fifteen miles, its slopes were here found to be sufficiently moderate to permit of frequent and easy ascent

[30] Chamberlin, *Jour. Geol.*, Vol. 3, 1895, p. 566. Salisbury, *ibid.*, Vol. 4, 1896, p. 778.

[31] Gunnar Andersson, "Danmarks expeditionen till Grönlands nordostkust," *Ymer*, Vol. 28, 1908, pp. 225–239, maps and 7 figures.

[32] To the south of the upper Nugsuak Peninsula in latitude 70° 10′ N.

[33] R. S. Tarr, "The Margin of the Cornell Glacier," *Am. Geologist*, Vol. 20, 1897, pp. 139–156, pls. 6–12.

and descent. Inasmuch as these sloping forms are characteristic of the ice front in the warmer zones, and further correspond to that generally characteristic of mountain glaciers in lower latitudes, it seems likely that its occurrence in Greenland is limited to districts where surface ablation plays a larger rôle.

In northeast Greenland (lat. 77°–82°) according to the Danes, "the frontier of the inland ice is in some places quite steep, in other places you might have mounted the inland ice without knowing it."

Features Within the Marginal Zone.—The larger terraces upon the ice slope, Nansen has ascribed to peculiarities of the rock floor on which the ice rests. Where the slopes become still more accelerated toward the margin of the ice, deep crevasses appear upon these steps running parallel to their extension, and hence parallel to the margins of the ice. Nansen found, however, that such crevasses were restricted to the outer seven or eight miles on the eastern side of his section, and to the outer twenty-five miles on its western margin. Peary in his reconnoissance across the ice border in latitude 69½°, saw such crevasses while they were opening and the surface snow was sinking into the cleft thus formed. The visible opening of the cleft was accompanied by peculiar muffled reports which rumbled away beneath the crust in every direction.[34] Of the terraced slope and its fading into the plateau above he says:

> The surface of the "ice-blink" near the margin is a succession of rounded hummocks, steepest and highest on their landward sides, which are sometimes precipitous. Farther in these hummocks merge into long, flat swells, which in turn decrease in height towards the interior, until at last a flat gently rising plain is revealed which doubtless becomes ultimately level.[35]

In sketching the general form of the Greenland continental glacier, it has been stated that the highest portion of the shield lies to the eastward of the medial line of the continent. This is shown by Nansen's section, and is emphasized by Peary, who says:

> That the crest of the Greenland continental ice divide is east of the country's median line there can be no doubt.[36]

[34] *Jour. Am. Geogr. Soc.*, Vol. 19, 1887, p. 277.
[35] *Geogr. Jour.*, Vol. 11, 1898, pp. 217, 218.
[36] *Geogr. Jour.*, *l. c.*, p. 232.

By von Drygalski[37] this lack of symmetry of the ice material has been ascribed to excessive nourishment upon the east, whereas the losses from melting and from the discharge of bergs occur mainly upon the west. The mountains of the east are, he states, completely surrounded by ice so that peaks alone project, while the mountains of the west stand isolated from the ice. In attempting to make the eccentric position of the boss in the ice shield depend upon the configuration of the underlying rock surface, von Drygalski has been less convincing, for we know that the Scandinavian continental glacier of Pleistocene times moved northwestward from the highest surface of the ice-shield up the grade of the rock floor, and pushed out through portals in the mountain barrier which lies along the common boundary of Sweden and Norway. Still there would appear to be a clear parallel between the marginal terraces of the inland-ice with their crevassed steep surfaces, and the plateaus and ice falls which alternate upon the slopes of every mountain glacier which descends rapidly in its valley.

Superimposed upon the flats of the larger ice terraces, there are undulations of a secondary order of magnitude, and these Nansen ascribed to the drifting of snow by the wind. To the important action of wind in moulding the surface of the inland-ice we shall refer again. There are in addition many other irregularities of the surface due to differential melting, and while of very great interest, their consideration may profitably be deferred until the meteorological conditions of the region have been discussed. There are, however, other features which like the broader terraces are clearly independent of meteorological conditions, and which are, therefore, best considered in this connection.

Dimples or Basins of Exudation Above the Marginal Tongues.—Seen from the sea in Melville Bay on the northwest coast, the inland-ice offers special advantages for observing its contours in sections parallel to its front, that is to say, in front elevation. Here only upon the west coast the ice extends beyond the borders of the land and is cut back by the sea to form cliffs. These ice cliffs are

[37] E. von Drygalski, "Die Eisbewegung, ihre physikalischen Ursachen und ihre geographischen Wirkungen," *Pet. Mitt.*, Vol. 44, 1898, pp. 55–64.

interrupted by rocky promontories which are surrounded on all sides but the front by ice, and hence in reality the cliff furnishes us with sections through nunataks and inland-ice alike. Says Chamberlin:[38]

> Only a few of the promontories of the coast rise high enough to be projected across this sky line and interrupt the otherwise continuous stretch of the glacial horizon. The ice does not meet the sky in a simple straight line. It undulates gently, indicating some notable departure of the upper surface of the ice tract from a plane. As the ice-field slopes down from the interior to the border of the bay, it takes on a still more pronounced undulatory surface. It is not unlike some of our gracefully rolling prairies as they descend from uplands to valleys, when near their middle-life development.

The two 1,200-mile sledge journeys of Peary in the years 1891–92, and 1893–95 across the northern margin of the "Great Ice" of Greenland, have added much to our knowledge of the physiography of the inland-ice. These journeys were made on nearly parallel lines at different distances from the ice border, and so, if studied in relation to each other, they display to advantage the configuration of the ice surface near its margin (see Fig. 25). The routes were for the most part nearly straight and ran at nearly uniform elevations which ranged from 5,000 to 8,000 feet above the sea.[39] In the sections nearest the coast, however, the route at first ascended a gentle rise to a flatly domed crest upon the ice only to descend subsequently into a broad swale of the surface, the bottom of which might be described as a plateau, and which was continued in the direction of the coast by a tongue-like extension of the ice, such as the tongue in Petermann Fjord between Hall Land and Washington Land (Fig. 25). On the further side of this basin-like depression, the surface again rose until another domed crest had been reached, after which a descent began into a swale similar to the first. On the return journey by keeping farther from the ice margin these elongated dimples upon the ice surface were avoided. The broad domed surfaces which separate the dimples clearly lie over the land ridges between the valleys down which the glacier tongues descend toward the sea.

[38] "Glacial Studies in Greenland, III.," *Jour. Geol.*, Vol. 3, 1895, p. 63.

[39] *Geogr. Jour.*, Vol. 11, 1898, p. 215. See also his map, *Bull. Am. Geogr. Soc.*, Vol. 35, 1903, p. 496.

FIG. 25. Map showing routes of sledge journeys in North Greenland in their relation to the margin of the ice (after Peary).

Peary has referred to these dimples on the surface of the inland-ice as "*basins of exudation,*" and has compared the cross profile in its ups and downs to that of a railroad located along the foot-hills of a mountain system.[40] In his earlier reconnaissance of the isblink from near Disco Bay, Peary describes such a dimple above the Jakobshavn ice tongue "stretching eastward into the 'ice-blink,' like a great bay," as a *feeder basin.*[41] The exact form of such dimples upon the ice surface is well brought out in von Drygalski's map of the Asakak glacier tongue on the Umanak Fjord (see Fig. 22, p. 82).[42]

We may easily account for the existence of these dimples by drawing a parallel from the behavior of water as it is being discharged from a lake through a narrow and steeply inclined channel. Under these circumstances the surface is depressed through the indrawing of the water on all sides to supply the demands of the outflowing current. That within the upper portions of the glacier tongues of the Greenland isblink the ice flows with a quite extraordinary velocity has long been known. Values as high as 100 feet per day have been determined upon the Upernavik glacier.[42a] By more accurate methods, von Drygalski has obtained on one of the ice tongues which descends to a fjord, a rate of about 18 meters or 59 feet in twenty-four hours.[43] Upon the inland-ice some distance back from the head of the fjord, on the other hand, a rate was measured of only one to two centimeters per day.

Scape Colks and Surface Moraines.—The velocities of ice movement which obtain within and about the heads of the glacial tongues are, there is thus every reason to believe, as different as possible from the ordinary general outward movement of the inland-ice. Within this marginal zone areas of exceptional velocity of the inland-ice are likely to be found wherever its progress is interfered with by the projecting nunataks. Just as jetties by constricting

[40] *Geogr. Jour.*, Vol. 11, p. 232.

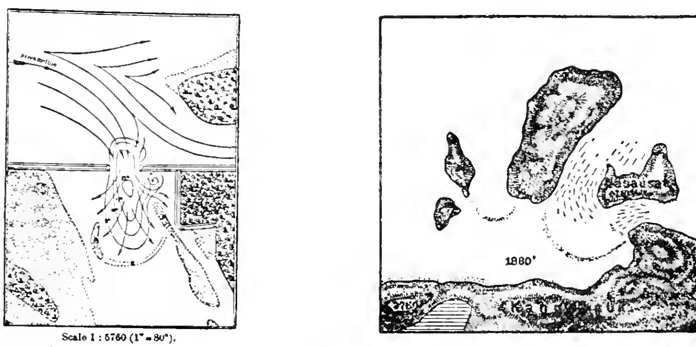
[41] *Jour. Am. Geogr. Soc.*, Vol. 19, 1887, p. 269.

[42] "Grönland-Expedition," *l. c.*, map 7.

[42a] Lieut. C. Ryder in 1886. Helland on a glacier of the Jakobshavnfjord found a rate of 64 feet daily.

[43] E. von Drygalski, "Die Bewegung des antarktischen Inlandeises," *Zeitsch. f. Gletscherk*, Vol. 1, 1906–7, pp. 61–65.

the channels greatly accelerate the velocity of stream flow within those channels, so here within the space between neighboring nunataks a local high rate of flow in the ice is developed. An inevitable and quite important consequence of this constriction was long ago pointed out by Suess and illustrated by the area between Dalager's Nunataks near the southwestern border of the isblink.[44] Here again the conduct of water which is being discharged through narrow outlets has supplied both the illustration and the explanation. In the regulation of the flow of the Danube below Vienna, the river was partially closed by a dam, the Neu-Haufen dyke, and the floor in the channel below the dike was paved with heavy stone



a *b*

FIG. 26. *a*, Closure of the Neu-Hauen dyke, Schüttau in the regulation of the Danube below Vienna (after Taussig); *b*, scape colks near Dalager's Nunataks (after Jensen and Kornerup).

blocks. The effect of thus narrowing the channel of the river was to raise the level of the water above the dike by almost a meter, and under this increased head the current tore out the heavy stone paving of the floor of the channel and dug a depression above as well as below the outlet. This excavation by the current represented a hole dug to a depth of about fifteen meters. The blocks which had been torn out from the pavement were left in a crescent-shaped border to the depression upon its downstream side (see Fig. 26, *a*).

[44] Ed. Suess, "Face of the Earth," Vol. 2, 1888 (translation, 1906), pp. 342–344.

The position of a surface moraine which stretches in a sweeping arc from the lower edge of one of Dalager's Nunataks to a similar point upon its neighbor, indicates a complete parallel between the motion of the ice and the water at the Neu-Haufen dyke, the rock débris of the deeper ice layers being here brought up to the surface. Study of the Scandinavian inland-ice of late Pleistocene times throws additional light upon the nature of this process. Flowing from a central boss near the head of the Gulf of Bothnia, the ice pushed westward and escaped through narrow portals in the escarpment which now follows the international boundary of Sweden and Norway. This constriction of its current has been appealed to by Suess to account for the interesting glint lakes which to-day lie across this barrier and extend both above and below the former outlets of the ice.[45] Lakes which have this origin he has described under the term scape colks. Perhaps if examined more carefully we should find that the bringing up of the englacial débris to the surface of the ice, is only partially due to the inertia of motion in the ice. With the more rapid flow of the ice within the constricted portion, the basic portions, shod as they are with rock fragments, accomplish excessive abrasion upon the rock bed. This is in accord with Penck's law of adjusted cross sections in glacial erosion. Where the ice channel broadens below the nunataks, the abrasion again becomes normal so that a wall develops at this place in the course of the stream.

Here, therefore, a new process comes into play due to the peculiar properties of the plastic ice, a process which has been illustrated in the formation of drumlins beneath former continental glaciers, and has been given an experimental verification. Case has shown that paraffin mixed with proper proportions of refined petroleum, and maintained at suitable temperatures, can be forced by means of plungers[46] through narrow boxes open at both ends. It was shown in the experiments that an obstruction interposed at the bottom and in the path of the moving paraffin, forced the bottom layers upward, and this upward movement continued beyond

[45] Suess, *l. c.*, pp. 337–346.

[46] E. C. Case, "Experiments in Ice Motion," *Jour. Geol.*, Vol. 3, 1895, pp. 918–934.

the position of the obstruction. The experiments of Hess[47] give results which are consistent with those of Case. Hess employed in his experiments parallel wax disks of alternating red and white colors, and these were forced under hydraulic pressure through a small opening. It was found that the layers turn up to the surface in this "model glacier" apparently as a result of the friction upon the bottom and at only moderate distances from the opening where the energy of the active moving substance pressing from the rear has to some extent been dissipated.

In Chamberlin's studies of certain Greenland glaciers, he was permitted to observe the effect upon the motion of the glacier of low prominences in its bed. These observations are confirmatory of the experiments described.[48]

The swirl colks or eddies which Suess has suggested as occurring below nunataks, in order to account for certain lakes in Norway, seemed to be much less clear, and it is a little difficult to assume an eddy in the ice which is in any way comparable to the eddies of water.

Marginal Moraines.—Inasmuch as the rock appears above the surface of the ice of the Greenland continental glacier only in the vicinity of its margins, and here only as small islands or nunataks, the rock débris carried by the Greenland ice must be derived almost solely from its basement. As described in detail by Chamberlin, it is the lower 100 feet of ice to which englacial débris is largely restricted.[49] Medial moraines, if the term may be properly applied to those ridges of rock débris which upon the surface of the ice go out from the lower angles of nunataks, have been frequently described by Nansen and others. They seem to differ but little from certain of the medial moraines which have been described in connection with the larger mountain glaciers.

Nansen has mentioned heavy terminal moraines in the Austmann Valley where he came down from the inland-ice after crossing the

[47] "Die Gletscher," 1904, p. 171, fig. 28.

[48] "Recent Glacial Studies in Greenland." Annual address of the President of the Geological Society of America, *Bull. Geol. Soc. Am.*, Vol. 6, 1895, pp. 199–220, pls. 3–10.

[49] Chamberlin, *l. c.*, p. 205.

continent. The material of these moraines consisted mainly of rounded and polished rock fragments, and is obviously englacial material.[50] Along the land margin of the Cornell ice tongue Tarr found a nearly continuous morainic ridge parallel to the ice front. This ridge usually rests at the base of the ice foot, and is sometimes a part of this foot, wherever débris has accumulated and protected the ice beneath from the warmth of the sun. Such an accumulation causes this part of the glacier to rise as a ridge. In other cases the ridge is, however, separated from the ice margin, and sometimes there are several parallel ridges from which the ice front has successively withdrawn[51] (see Plate XXVIII, *B*).

According to von Drygalski the marginal moraines of the Greenland ice sheet as regards their occurrence, form, and composition, are in every way like those remaining in northern Europe from the time of the Pleistocene glaciation, and this is true of those which run along the present border of the inland-ice as well as of those still mightier ancient moraines which follow at certain distances.[52] These moraines are generally closely packed blocks with relatively slight admixture of finer material. They are the largest where the ice border enters the plains, or pushes out upon a gentle slope, and they are smallest where the ice passes steep rocky angles.

It is worthy of note that the marginal moraines of Greenland become locally so compact and resistant that they oppose a firm obstruction to the ice movement. Then the ice pushes out laterally into the marginal lakes which develop there or pushes up upon the moraines. It thus comes to arrange its layers parallel to the slope of the morainic surface or, in other words, so that they dip toward the ice.[53]

Another type of marginal moraine which was mentioned by Mohn and Nansen from south Greenland, and later fully described by Chamberlin from north Greenland, is explained by the upturn-

[50] Mohn u. Nansen, "Wissenschaftlichen Ergebnisse von Dr. F. Nansen's Durchquerung von Grönland, 1888," *Pet. Mitt. Ergänzungsb.*, 105, 1892, p. 91.

[51] R. S. Tarr, "The Margin of the Cornell Glacier," *Am. Geol.*, Vol. 20, 1897, p. 148.

[52] Grönland-Expedition, *l. c.*

[53] von Drygalski, *l. c.*, p. 529.

ing effect of obstructions in the bed, and by the shearing and overthrusting movements which are found to exist in inland-ice near its margin[54] (see Figs. 27 and 28). This process has much in

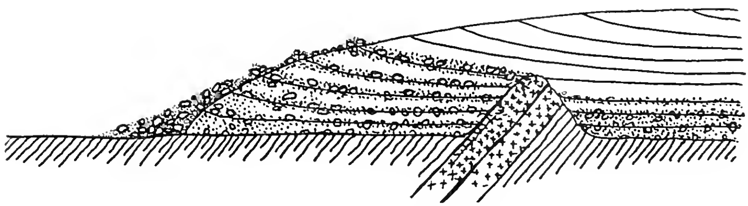


FIG. 27. Diagram to show the effect of a basal obstruction in the path of the ice near its margin (after Chamberlin).

common with that which we have already described in connection with scape colks.

Fluvio-glacial Deposits.—Where studied by Chamberlin near Inglefield Gulf, there appears to be little or no gushing of water from beneath the inland-ice. Small streamlets only appeared be-

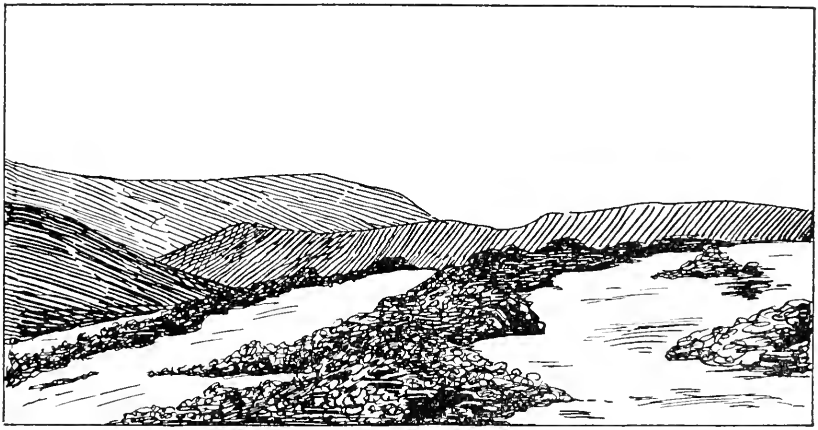


FIG. 28. Surface marginal moraine of the inland ice of Greenland.

neath the ice border, bringing gravel and sand which they distributed among the coarser morainic material. So far as land has been recently uncovered by the ice in north Greenland, and so far as differentiated from the topography of the underlying rock, it was

[54] Chamberlin, *l. c.*, p. 92.

found to be nearly plane. So far as known, no eskers have been observed about the border of the inland-ice of Greenland and only a few irregular kames near Olrik's Bay.[55]

Nourishment of the Greenland Inland-ice.

Few and Inexact Data.—The problems involving the gains and the losses of the inland-ice of Greenland require for their satisfactory solution a much larger body of exact data than we now possess. Barring a few scattered and not always exact or reliable observations, we are practically without knowledge of the amount or the variations of atmospheric pressure, or of snowfall away from the coastal areas of the continent. Even within these marginal zones, the losses from ablation and through the calving of bergs have been estimated by crude methods only. Again, the great height of the ice surface within the central plateau, and the lack of any knowledge of the elevation of the land surface in those regions, has raised questions concerning the conditions of flow and of fusion upon the bottom, which will probably long remain subjects of controversy.

An international coöperative undertaking with one or more stations established in the interior at points where altitude has been determined by other than barometric methods, and with coast stations maintained contemporaneously and for a period of at least a year, particularly if they could be supplemented by balloon or kite observations, would yield results of the very greatest importance.[56] The Greenland ice having shrunk greatly since the Pleistocene period, it is almost certain that its alimentation to-day does not equal the losses which it suffers along its margins—which in but slightly altered form applies to the Antarctic continental glacier as well.

Snowfall in the Interior of Greenland.—Almost the only data upon this subject are derived from a rough section of the surface layers of snow, as this was determined by Nansen with the use of

[55] Salisbury, *l. c.*, p. 809.

[56] Robert Stein, "Suggestion of a Scientific Expedition to the center of Greenland," Congrés Intern. pour l'Étude des Regions Polaires, Brussels, 1906, pp. 1–4 (separate).

a staff near the highest point in his journey across the inland-ice along the 64th parallel. At elevations in excess of 2,270 meters Nansen found the surface snow "soft" and freshly fallen, but of dust-like fineness. Beneath the surface layer, a few inches in thickness only, there was a crust less than an inch in thickness which was ascribed to the slight melting of the surface in mid-summer,[56a] and below this crust other layers of the fine "frost snow" more and more compact in the lower portions, but reaching a thickness of fifteen inches or thereabouts before another crust and layer was encountered.[57] Other sections made in like manner by pushing down a staff, revealed similar stratification of the surface snow with individual layers never exceeding in thickness a few feet. From these observations Nansen has drawn the conclusion that the layers of his sections correspond to seasonal snowfalls, the thin crust upon the surface of each being due to surface melting in the few warm days of midsummer. He cites Nordenskiöld as believing that the moist winds which reach the continent of Greenland deposit most of their moisture near the margin.[58]

The sky during almost the entire time of the journey is described by Nansen as so very clear that the sun could be seen, and there were few days in which the sky was completely overcast. Even when snow was falling, which often happened, the falling snow was not thick enough to prevent the sun showing through. This clearly indicates that the snow falls from layers of air very near the snow surface below. The particles which fell were always fine, like frozen mist—what in certain parts of Norway is known as "frost snow"; that is, snow which falls without the moisture first passing through the cloud stage.[59]

The air temperatures even in August and September, when the crossing was made, were on the highest levels seldom much above the zero of the Fahrenheit scale, and at night they sank by over 40° F. (in one case to — 50° F.).

[56a] In the light of later studies this may as satisfactorily be explained through hardening by the wind.

[57] Mohn u. Nansen, *l. c.*, p. 86.

[58] Nansen, *l. c.*, Vol. 1, p. 495.

[59] Nansen, *l. c.*, Vol. 2, p. 56.

Peary, while on the inland-ice in north Greenland in the month of March, 1894, registered on his thermograph a temperature of — 66° F. and several of his dogs were frozen as they slept.[60] The high altitudes and the general absence of thick clouds over the inland-ice, permit rapid radiation, so that cold snow wastes and hot sand deserts have in common the property of wide diurnal ranges of temperature. The poverty of the air over the inland-ice in its content of carbon dioxide, as shown by the analysis of samples collected by Nansen, must greatly facilitate this daily temperature change.[61]

From studies in the Antarctic it is now known that most of the snow falls there in the summer season, and that little, if any, moisture can reach the interior from surface winds. The same is probably true also of the interior of Greenland.

Though the absolute humidity of the air upon the ice plateau of Greenland is always low, the relative humidity is large, and never below 73 per cent. of saturation in the levels above 1,000 meters. Evaporation occurs chiefly when the sun is relatively high, and when the air is again chilled the abstracted moisture is returned to the surface in the form of the almost daily snow mists or frost snow. The observations went to show that only in the warmest days of summer do the sun's rays succeed in melting a very thin surface layer of the snow. Of the thirty days that Nansen's party was at altitudes in excess of 1,000 meters, on only six is a definite snowfall reported. Within the interior of Greenland it appears that *no snow whatever is permanently lost from the surface by melting.*[62]

While the relative humidity of the air over the central plateau is so high, the absolute humidity is extremely low, being measured from 1.4 mm. to 4 mm., though generally much below the maximum value. The average absolute humidity was 2.5 mm. while the average relative humidity was 92 per cent.[63]

It has been claimed by v. Drygalski that the eastern portion of

[60] *Geogr. Jour.*, Vol. 11, 1898, p. 228.
[61] Mohn u. Nansen, *l. c.*, pp. 109–111.
[62] Nansen, *l. c.*, Vol. 2, p. 491. See also Peary, *Geogr. Journ.*, *l. c.*, p. 214.
[63] Mohn u. Nansen, *l. c.*, pp. 44–45.

the Greenland ice sheet is a great nourishing region, while the western slope, on the other hand, is the locus of excessive melting and discharge. In support of this view he adduces chiefly the admitted lack of symmetry of the ice mass.[64] So far as alimentation is concerned, the view does not seem to be as yet supported by any observations, and it can hardly be regarded as a tenable hypothesis.

The Circulation of Air over the Isblink.—No exact data upon atmospheric pressures are as yet available except from stations near the sea level, mainly along the western and northern coasts. Until stations have been maintained for a more or less protracted period within the interior of Greenland, none can be expected. None the less, upon the basis of the observed winds in those portions of Greenland which have been traversed, it may be safely asserted that a fixed area of high atmospheric pressure is centered over the Greenland isblink, and that the cold surface of this mass of ice is directly responsible for its location there. Nansen, as early as 1890, announced this fact, having observed "that the winds which prevail on the coasts have an especial tendency to blow outwards at all points."[65] After many years of experience in different portions of Greenland, Peary stated the law of air circulation above the continent in clear and forceful language:[66]

> Except during atmospheric disturbances of exceptional magnitude, which cause storms to sweep across the country against all ordinary rules, the direction of the wind of the "Great Ice" of Greenland is invariably radial from the center outward, normal to the nearest part of the coast-land ribbon. So steady is this wind and so closely does it adhere to this normal course, that I can liken it only to the flow of a sheet of water descending the slopes from the central interior to the coast. The direction of the nearest land is always easily determinable in this way. The neighborhood of great fjords is always indicated by a change in the wind's direction; and the crossing of a divide, by an area of calm or variable winds, followed by winds in the opposite direction, independent of any indications of the barometer.

Except for light sea breezes blowing on to the land in February,

[64] E. v. Drygalski, "Die Eisbewegung, ihre physikalischen Ursachen und ihre geographischen Wirkungen," *Pet. Mitt.*, Vol. 44, 1898, pp. 55–64. See also by the same author, "Grönland-Expedition, etc.," pp. 533–539.

[65] Nansen, *l. c.*, Vol. 2, p. 496. Also Mohn and Nansen, *l. c.*, pp. 44–47.

[66] "Journeys in North Greenland," *Geogr. Jour.*, Vol. 11, 1898, pp. 233–234. See also "Northward over the 'Great Ice,'" Vol. 1, pp. lxix–lxx.

the Danish northeast Greenland expedition found "the wind was constantly from the northwest, this being the result of the high pressure of air which is found over the inland ice."[67]

These conditions of circulation are schematically represented in Fig. 29. In March, 1894, Peary encountered on the north slope

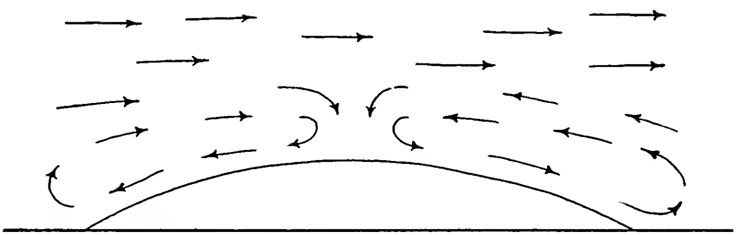


FIG. 29. Diagram to illustrate the air circulation over the isblink of Greenland.

of the inland-ice a series of blizzards before unprecedented in Arctic work, one lasting for three days, during which for a period of 34 hours the average wind velocity, as recorded by anemometer, was 48 miles per hour. Viewed in the light of violent southerly blizzards which Shackelton found to prevail upon the ice plateau in the Antarctic, these winds must be considered as belonging to the same Greenland or isblink system which has been described as of such general prevalence.

After comparing the meteorological data from his journey with contemporaneous observations on the shores of Baffin's Bay, Nansen believed that he was able to make out faintly the influence of general cyclonic movements. He says.[68]

> The plateau seems to be too high and the air too cold to allow depressions or storm centres to pass across, though, nevertheless, our observations show that in several instances the depressions of Baffin's Bay, Davis' Strait and Denmark Strait can make themselves felt in the interior.

Commenting upon Peary's conclusions above quoted, Chamberlin[69] ascribes the wind which flows downward and outward from

[67] Lieut. A. Trolle, R. D. N., "The Danish Northeast Greenland Expedition," *Scot. Geogr. Mag.*, Vol. 25, 1909, pp. 57–70 (map and illustrations).

[68] Nansen, *l. c.*, Vol. 2, p. 496.

[69] *Jour. Geol.*, Vol. 3, 1895, pp. 578–579.

the isblink to a notable increase of its specific gravity through contact with and consequent cooling by the snow surface.[70]

Foehn Winds Within the Coastal Belt.—The sliding down of masses of heavy air upon the snow surface of the Greenland ice must bring about adiabatic heating of the air and a consequent elevation of the dew point. The increase of temperature being about 1° C. for every 100 meters of descent, a rise of temperature of as much as 20° C. or 36° F. will result in a descent from the summit of the plateau, assuming this to have an elevation of 10,000 feet. Some reduction in the amount of this change of temperature will, of course, result from the contact of the air with the cold snow surface during its descent, this modification being obviously dependent upon the velocity of the current. The warm, dry winds which in different districts have been described under the names *foehn* and *chinook* are the inevitable consequence of such conditions, and are, moreover particularly characteristic of steep mountain slopes more or less covered by glaciers. Such foehn winds have long been recognized as especially characteristic of western Greenland. Dr. Henry Rink, who was a pioneer in the scientific study of Greenland, wrote in 1877:[71]

> Among the *prevailing winds* in Greenland the *warm land wind* is the most remarkable. Its direction varies according to locality from true E.S.E. to E.N.E. always proceeding though warm from the ice-covered interior, and generally following the direction of the fjord. It blows as frequently and as violently in the north as in the south, but more especially at the fjord heads, while at the same time in certain localities it is scarcely perceptible. It often turns into a sudden gale; the squalls in some fjords rushing down between the high rocks, in certain spots often sweep the surface of the water with the force of a hurricane, raising columns of fog, while the surrounding surface of the sea remains smooth.

[70] Professor v. Drygalski has shown that in the Great Karajak glacier near the coast in central western Greenland, the temperature of the snow and ice down to a depth of 60 feet or more undergoes a fall of temperature in response to the severity of the winter's cold, but in time this fall in temperature lags behind the period of maximum cold. Below that depth, however, it approximates in temperature to the zero of the centigrade scale. Temperatures of the snow measured just below the surface, varied from —11° to —26° C. (E. von Drygalski, "Grönland-Expedition der Gesellschaft für Erdkunde zu Berlin," 1891–1893, Vol. 1, 1897, pp. 470–472.)

[71] Henry Rink, "Danish Greenland, Its People and Its Products," London, 1877, p. 468.

Nansen encountered one of these foehn winds on his descent, and Peary mentions their occurrence in the north. In Scoresby's Land on the east coast, a foehn wind in the winter season has been known in a single hour to change the temperature by 24° C. (or 43° F.), and the maximum change during such a wind is far greater. It would not appear from observations that the winds of the Greenland system extend to any great distance above the surface, where the broad cyclonic areas in the atmosphere may be presumed to continue their courses with but slight modification. The anti-cyclone of the continent is, however, none the less clear and constant and is centered over the high interior. Nansen has remarked the calms over the divide of his section.[72]

There is some evidence that in adopting the important modern laws of adiabatic cooling of the air, we have allowed the pendulum to swing too far and have given too little weight to the effect of cooling through contact of air with either rock or snow. The latest results of Antarctic expeditions furnish the most striking proof of this, if other than Greenland examples were needed, and the Antarctic studies throw much light upon the conditions of snow distribution which are observed in Greenland.

Wind Transportation of Snow Over the Desert of Inland-ice.—Whymper and Nordenskiöld called Greenland a "Northern Sahara." In different ways Nansen and Peary have each instituted comparisons between the wastes of snow in the interior of Greenland and the desert of sand of the Sahara. The Norwegian explorer has emphasized especially the wide daily ranges of temperature, which because of generally cloudless atmospheres, both deserts have in common. Of the monotonous and elemental simplicity of the snow vistas back from the ice margin in North Greenland, Peary says:[73]

> It is an Arctic Sahara, in comparison with which the African Sahara is insignificant. For on this frozen Sahara of inner Greenland occurs no form of life, animal or vegetable; no fragment of rock, no grain of sand is visible. The traveller across its frozen wastes, travelling as I have week after week, sees outside himself and his own party but three things in all the world, namely, the infinite expanse of the frozen plain, the infinite dome of the cold blue sky, and the cold white sun—nothing but these (see Fig. 30).

[72] Nansen, *l. c.*, Vol. 2, pp. 487–488, 496.

[73] *Geogr. Journ.*, *l. c.*, pp. 214, 215.

There is, however, yet another marked parallel between the snow waste and the sand desert. It is the importance of wind as a transporting agent. In his shorter acquaintance with southern Greenland Nansen was less impressed with this, but he has explained the secondary snow ridges upon the marginal terraces of the inland-ice as wind accumulations.[74] These long parallel ranges of snow drift thus correspond to the similar ranges of sand dunes

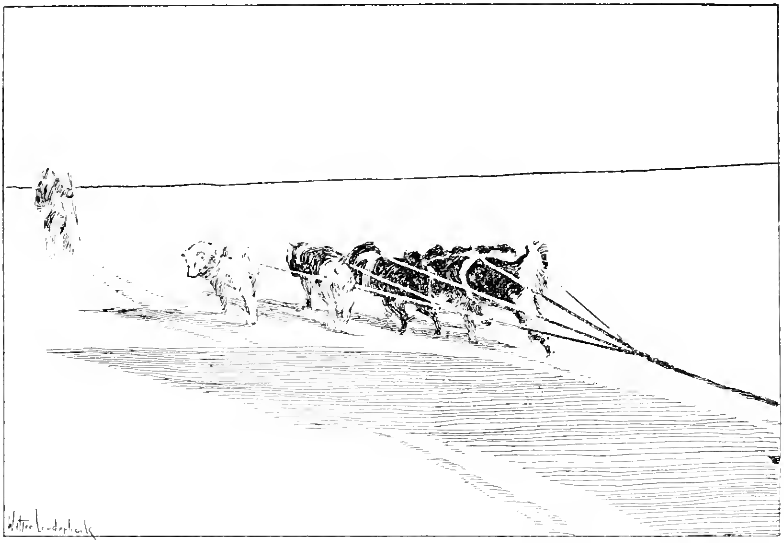


FIG. 30. On the Sahara of Snow (after Peary).

which sometimes throughout a width of many miles hem in the deserts of lower latitudes. In northern Greenland Peary's observations have a special value. He says:[75]

> There is one thing of especial interest to the glacialist—the transportation of snow on the ice-cap by the wind. . . .
>
> The opinion has been forced upon me that the wind, with its transporting effect upon the loose snow of the ice-cap, must be counted as one of the most potent factors in preventing the increase in height of the ice-cap—a factor equal perhaps to the combined effects of evaporation, littoral and subglacial melting, and glacial discharge. I have walked for days in an incessant sibilant drift of flying snow, rising to the height of the knees,

[74] Mohn u. Nansen, *l. c.*, p. 78.
[75] *Geogr. Jour.*, *l. c.*, pp. 233–234.

sometimes to the height of the head. If the wind becomes a gale, the air will be thick with the blinding drift to the height of 100 feet or more. I have seen in the autumn storms in this region round an amphitheatre of some 15 miles, snow pouring down in a way that reminds one of Niagara. When it is remembered that this flow of the atmosphere from the cold heights of the interior ice-cap to the lower land of the coast is going on throughout the year with greater or less intensity, and that a fine sheet of snow is being thus carried beyond the ice-cap, to the ice-free land at every foot of the periphery of the ice-cap, it will perhaps be seen that the above assumption is not excessive. I feel confident that an investigation of the actual amount of this transfer of snow by the wind is well worth the attention of all glacialists.

Fringing Glaciers Formed from Wind Drift.—In the vicinity of Inglefield Gulf in northwest Greenland, the inland-ice ends in a steep, snowy slope rising to a height of about 100 feet, where is a terminal moraine, above which moraine rises the great dome of the inland-ice. The whiteness and freshness of a portion of the snow of the outer border, when examined by Chamberlin,[76] showed it to be wind drift of recent accumulation. Locally, however, older and discolored snow appeared beneath the whiter surface snow, and in a few places stratified granular ice with some included rock débris. This snow and ice becomes augmented from year to year and is, in Chamberlin's opinion, a species of *fringing glacier*. Such fringes were from a few rods to a half mile in breadth, and where a favorable depression existed, one was observed extending for a mile or more down the valley. Commander Peary has found this a dominant feature on the north Greenland coast. Fringing glaciers of this type have also been described by Salisbury from the vicinity of Melville Bay. Their movement was clearly evinced by their structure and by the débris which they carried.[76a]

Nature of the Surface Snow of the Inland-ice.—The surface snow from the marginal zones of the inland-ice has the granular form characteristic of névés, as has been shown with exceptional clearness in elaborate studies by von Drygalski.[77] Such grains, grown by accretions from a single crystal nucleus and at the ex-

[76] T. C. Chamberlin, "Glacial Studies in Greenland, VI.," *Jour. Geol.*, Vol. 3, 1895, pp. 580–581.

[76a] Salisbury, *Jour. Geol.*, Vol. 3, p. 886.

[77] "Grönland-Expedition," etc., Vol. 1, 1897.

pense of neighboring crystals, must require either fusion from temporary elevation of temperature, or from pressure. The observations of von Drygalski were made on the ice of the marginal tongues and on the blue layers of the inland-ice; but as the samples taken farthest from the margins were found at a height of only 500 meters, the results throw little light upon the conditions of surface snow within the interior, where melting does not take place. In view of recent studies in Antarctica it is unlikely that firn or névé snow will be found within the interior except at considerable depths below the surface.

Nansen has described the fine "frost snow" which falls almost daily from an air layer near the snow surface, from which its moisture has been derived. Melting does not occur there, as already stated, except perhaps for a few days in the height of summer when a thin crust develops upon the surface.[78] Peary has referred to the snow at the highest altitudes which he reached in north Greenland as "unchanging and incoherent." This dry hard snow chased by the wind, has the cutting effect of sand in a blast, and thus is offered still another parallel with deserts and their wind blown sand. Each new storm, we are told by Stein,[79] piles up a snowbank on the lee sides of nunataks, but the next storm, coming from a somewhat different direction and laden with fine hard snow, cuts away the earlier deposit as would a sand blast. Peary discovered one of his earlier snow huts partly cut away by this process.

Snow Drift Forms of Deposition and Erosion—sastrugi.—The minor inequalities of the snow surface as determined by the wind blowing over the inland-ice, have been mentioned more or less persistently by all Arctic travellers, since upon the character of this surface has so largely depended the celerity of movement in sledge journeys. It is unfortunate that no one has discussed the subject from a scientific standpoint, for it has great significance in connec-

[78] "Thus it will be seen that at no great distance from the east coast the surface of dry snow begins, on which the sun has no other effect than to form a thin crust of ice. The whole of the surface of the interior is entirely the same." (Nansen, *l. c.*, Vol. 2, p. 478.)

[79] Robt. Stein, Congrés international pour l'étude des regions polaires. Brussels, 1906, pp. 1–4 (separate).

tion with the study of the strength and direction of the wind over the snow surface. All minor hummocks and ridges of this nature are included under the general term *sastrugi* (see Fig. 31).

The student may learn much concerning their form within the Antarctic regions from examination of the many beautiful photographs recently published by the Royal Society in connection with

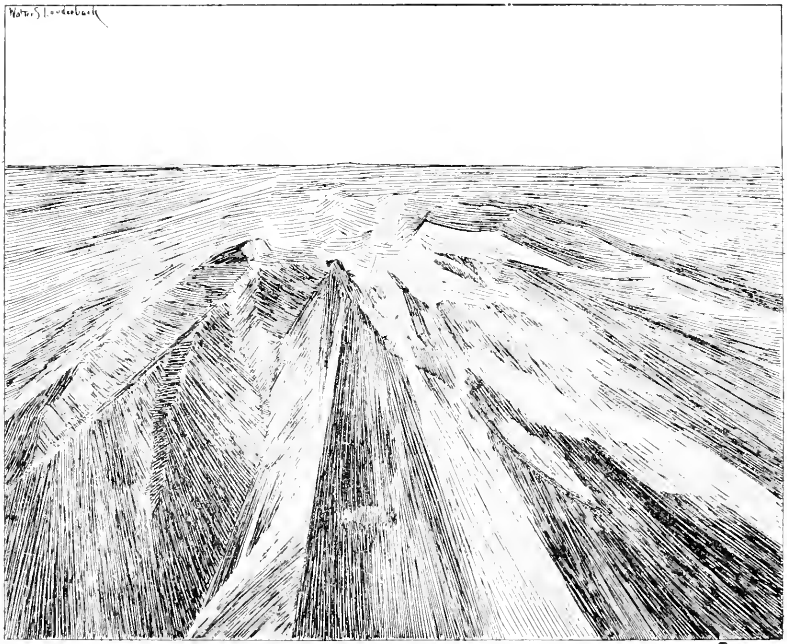


FIG. 31. Sastrugi on the inland-ice of North Greenland (after Peary).

the British Antarctic Expedition.[80] On Plate 92 of this collection, sastrugi are shown which were originally laid down in "elongated domes" and "crescent hollows," but which on account of change in the wind direction the drifting snow granules have cut away both on the soft surface and in the harder deep layers. As a result of this erosion cross flutings have been superimposed upon the original forms.

[80] National Antarctic Expedition, 1901–4. Album of photographs and sketches (with brief descriptions, Ed.), London, 1908.

Our best study of snow drift forms has been made by Dr. Vaughan Cornish, who, after a series of monographs dealing with waves of drifted sand, has spent a winter in Canada in order to study the phenomena connected with the drifting of snow.[81] It is found that snow which falls at temperatures near 32° F. is wet and sticky, and behaves quite differently from that which falls near or below the zero of the same scale; which, on the contrary, is dry and slippery. Subsequent modifications of either of these forms of snow depend chiefly upon pressure, temperature, radiation and wind. It is the cold, dry and granular snow only which makes so-called *normal* waves, and it must be this form which plays the major rôle in producing the surface irregularities of the inland-ice of Greenland.

Ripples and larger waves alike, when formed from granular snow and when shaped by wind accumulation, *have the steep side always to leeward,* in which respect the snow behaves like drifted sand. In order to produce waves or ripples the wind must have a velocity sufficient to be thrown into undulations by the irregularities of the surface over which it blows. The most perfectly moulded forms are naturally produced upon a relatively plane surface, such as is realized on the inland-ice of Greenland—the "imperial highway" of Commander Peary.

Apparently the direction of the greatest extension of the sastrugi will depend upon the strength of the wind and upon the amount of snow which is being transported, much as has been found to be the case with drifted sand.[82] Thus, with small amounts of snow and moderate winds, the characteristic form of sastrugi is a short, scalloped ridge lying across the wind direction and in form not unlike an ox-yoke—something intermediate between a barchan and a transverse ridge. Barchans of snow almost identical in form with sand barchans, are produced apparently under like conditions, the chief differences being that lighter winds suffice

[81] Vaughan Cornish, "On Snow-waves and Snow-drifts in Canada," *Geogr. Jour.,* Vol. 20, 1902, pp. 137–175.

[82] P. N. Tschirwinsky, "Schneedunen und Schneebarchane in ihrer Beziehungen zu äolischen Schneeablagerungen im Allgemeinen," *Zeitsch. f. Gletscherk.,* Vol. 2, 1907, pp. 103–112.

to accomplish the result with the less ponderous snow, and that the resulting forms set quicker in the snow (see Fig. 32, *a*).

Cornish has realized the full importance of snow-blast erosion in modifying the form of snow drifts. His *barchans of erosion,* in plan resemble the barchans of deposition from which they are derived, but unlike the depositional forms their broader surface is concave upward instead of convex, and their steeper face is toward the wind (see Fig. 32, *b*).

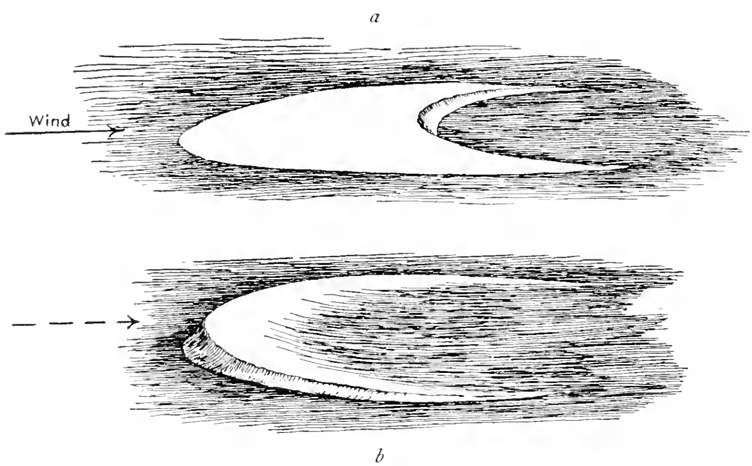


Fig. 32. Barchans in snow. *a*, of deposition; *b*, of erosion (after Cornish).

Some facts of importance which concern the density of the snow are emphasized by Cornish, and apply with especial force to the surface snow of inland-ice. It was found that crusts upon the surface of snow do not necessarily imply melting, but are produced in temperatures below the fusion point. When the air temperatures at Winnipeg ranged from 25° to —28° F. the snow surface over the river set so hard that the mocassined heel did not dent it. Pieces of this snow broken off and held up to the sunlight showed a "mosaic of small translucent icy blocks cemented firmly by opaque ice." The effect upon snow density of the radiation from the surface and of pressure from the wind, were strikingly brought out by a number of observations. Newly fallen snow in Canada has a density of about 0.1. Over the level surface about Winnipeg in the month of January and at a temperature of 10° F., the snow was found to have a density within the upper two feet of 0.38; while in the woods at the same time and at the same depth, here without a crust, its density was 0.19. Thus it is seen that the snow in the woods is about twice as heavy as newly fallen snow, but only about half as heavy as that which has been chased about by the wind. At Glacier House in the Selkirks, where the snow is shielded from the wind within a narrow valley, experiments showed a density of 0.106 at the surface, whereas at a depth of one foot below the surface the density was 0.195, and at a depth of four feet, 0.354. The middle value being that of the snow in the woods at Winnipeg, it is seen that the weight of an additional three feet of snow is necessary in order to pack snow as tightly as is done by the wind blowing over the prairie. After a time, as a result of this treatment by the wind, an eight-inch snowfall dwindles by packing in the woods to four inches, and over the open plain to a two-inch layer.

In eroding a drift, the wind first attacks the softer surface layer. This removed, the snow of the blast adheres less to the surface of the drift, and in consequence abrades it more vigorously. Thus, notches in the ridges, instead of being mended by the detritus, are increased by it, and transverse ridges are presently cut through, and we pass by stages from an arrangement of ridges transverse to the wind to that of longitudinal structures having the greatest extension parallel to the wind.[83] These longitudinal sastrugi appear to be the dominant ones, and from them the direction of prevailing winds may be determined as has been already proven in the Antarctic. On the Siberian tundras the sastrugi are often the only guides of direction which the natives have.[84]

Source of the Snow in Cirrus Clouds.—What has been learned of the circulation of air above the continental ice of Greenland makes it extremely unlikely that any such excessive alimentation upon the eastern margin through ordinary snow fall, as has been

[83] Cornish, *l. c.*, pp. 159–160.
[84] Tschirwinsky, *l. c.*, p. 107.

advocated by von Drygalski, can occur.[85] Such moisture-laden air as can, under normal conditions, reach the interior plateau must descend from higher levels in the anti-cyclone above the central boss, and be distributed by the outward flowing surface currents. From such altitudes the moisture would probably be congealed in the form of fine ice needles, such as are believed to exist in cirrus clouds. The snow which covers the surface at these levels appears, moreover, to have this character. Of greatest interest in this connection is the observation of Nansen that while the sky was, during the time of his crossing, in the main clear, those clouds which were present were generally either cirrus clouds or some combination of cirrus with cumulus and stratus clouds. No cumulus clouds whatever were observed. In tabular form his results are as follows:

Form of Clouds.	No. of Days.		Per Cent.
Cirrus	23	51	44
Cirro-stratus	17		33
Cirro-cumulus	11		21
Cumulo-stratus	22		42
Stratus	10		19

As already stated, such snow as reaches the central area must, it would seem, be derived from the cirrus clouds which at higher levels move in toward the anti-cyclone and descend as surface currents over the "Great Ice."

Depletion of the Greenland Ice from Surface Melting.

Eastern and Western Slopes Compared.—Though it is probably not true, as has been claimed by von Drygalski, that the eastern border of the continent is the locus of nourishment for the ice, it is almost certain that the losses are much greater along the western margins. For this there are several reasons. In the first place, the eastern base is apparently characterized by lower temperatures. The cold ocean current, which carries ice bergs and flows from the

[85] "The east is to be regarded as the region of origin of snow, the west as the terminal region of the Greenlandic glaciation." (E. von Drygalski, "Die Eisbewegung, ihre physikalischen Ursachen und ihre geographischen Wirkungen," *Pet. Mitt.*, Vol. 44, 1898, pp. 55–64.)

Arctic Ocean southward in Baffin's Bay, follows the western shore, while a warmer counter current flows northward along the eastern or Greenland coast at least in its southern stretches. Tarr thinks this current may reach as far as Melville Bay.[86] In addition, the gentler slopes of the western surface of the ice are perhaps more favorable to the warm and dry "thaw wind"—the well-known foehn of Greenland.

Again, ablation or surface melting is to a large extent dependent upon the quantity of rock débris which is blown onto the ice surface from its margins. In southern Greenland, at least, the wider ribbon of exposed shore land upon the western coast conspires with the prevailing western winds to make a more effective marginal attack upon the anti-cyclone of the continent. Nansen reports that he found on the east coast none of the rock dust first described by Nordenskiöld as "cryoconite," though it extended inward from the western coast as much as 30 kilometers.[87]

Still further it is to be remembered that the ice of the west margin is intersected by many deep fjords, which communicating with the open sea, remove an enormous quantity of ice in the form of bergs. Upon the eastern coast the pack-ice prevents the removal of bergs except from the southern latitudes.

Effect of the Warm Season Within the Marginal Zones of the Inland-ice.—In winter the entire surface of the ice and the border of the land as well, are covered with an unbroken layer of fine, dry snow. The suddenness of the change to summer within the land zone outside the ice front, has been emphasized by Trolle. The temperature of the snow upon the land in northeast Greenland rose gradually with the arrival of summer until the melting point was reached, and then in one day all the snow melted. "The rivers were rushing along, flowers were budding forth, and in the air the butterflies were fluttering."[88]

The snow upon the surface of the inland-ice where studied by

[86] R. S. Tarr, "Difference in the Climate of the Greenland and American Sides of Davis' and Baffin's Bay," *Am. Jour. Sci.*, Vol. 3, 1897, pp. 315–320.

[87] Mohn und Nansen, *l. c.*, p. 90.

[88] Trolle, *l. c.*, p. 66.

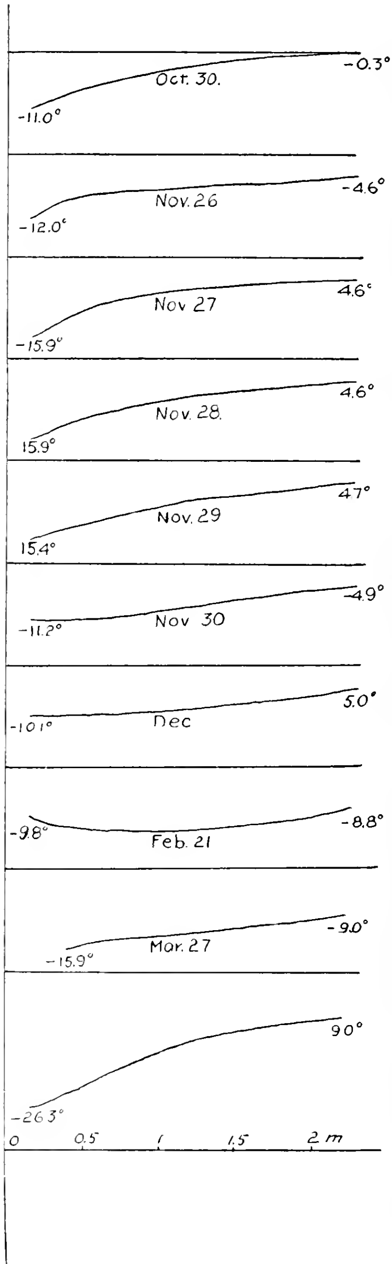


Fig. 33. Diagrams showing the distribution of temperatures within the surface zones of the inland-ice (after von Drygalski).[89]

von Drygalski within the western marginal zone, was found to have temperatures which in the winter season were normally lowest just below the surface, and which approximated to the zero of the centigrade scale at depths of generally a few meters only. In October with a sub-surface temperature of —11°C. the zone of zero temperature was reached at a depth of a little more than two meters. The sub-surface temperature steadily lowered from this time as the colder months came on, and the depth of zero temperature descended to below the limit of the experiments, which was only a little more than two meters. The form of the temperature curves in dependence upon depth showed clearly, however, that at very moderate depths equalization occurred. Late in March the lowest temperatures were reached with —26.3° C. for the immediate sub-surface temperature, and —9° C. for the temperature at depth of two meters. Warm weather at the surface resulted in a warm wave which descended through the snow, following the colder one, and so resulted in a maximum temperature not immediately below the surface but at increasing distances from it depending upon the duration of the warmer air temperatures at the surface. Thus, a ten day foehn in January raised the temperature at a depth of 2.2 meters, by a half a degree. It required over two days for this rise in temperature to proceed to a depth of 1 meter, and ten days for it to reach the depth of 2 meters. Similar effects are produced with the coming of the more prolonged warm weather of the summer season (see Fig. 33).

When the surface zone of the snow has reached the fusing point of snow, melting begins rapidly. Peary has drawn a graphic picture of the effect of the warm season upon the margins of the Greenland ice. Late in the spring the warmth of the sun at midday softens the surface first along the outermost borders of the ice, and this, freezing at night, forms a light crust. Gradually this crust extends up in the direction of the interior, and as the season advances the surface of the marginal rim becomes saturated with water. This zone of slush follows behind the crust towards the interior in a continually widening zone as the summer advances. Within the outermost zone the ice is so decomposed that pools come to occupy depressions upon the surface and streams cut deep gullies into the ice. At the same time the ice shows a more dirty appearance through the concentration of the rock débris due to the melting of its surface layers. By the end of the season, pebbles, boulders and moraines have in places made their appearance on the surface, and the streams have left a surface of almost impassable roughness.[90]

Differential Surface Melting of the Ice.—In his ascent of the western margin of the ice near the latitude of Disco Bay, Peary encountered lakes surrounded by morasses of water saturated with snow. The ice within this zone is crevassed, and down the fissures some of the surface streams disappear, at times in a large water-fall, and again in a "mill" of its own shaping. Baron Nordenskiöld

[89] E. von Drygalski, "Grönland-Expedition," *l. c.*, pp. 460–466.

[90] Peary, *Geogr. Jour.*, *l. c.*, p. 218. See also Nordenskiöld, "Grönland" (German ed.), pp. 125–138.

earlier observed almost identically the same phenomena along the line of his route. The intricate ramifications of the superglacial rivers and the occupation of almost the entire remaining surface of the ice by shallow ice wells and basins along his route are shown in Figs. 34 and 37.[91] These ice wells are in no wise restricted to inland-ice but are found in mountain glaciers as well, and represent but one of a series of allied phenomena dependent upon differential melting due to the presence of fine rock particles upon the ice. They were quite thoroughly described by Agassiz in his "Système

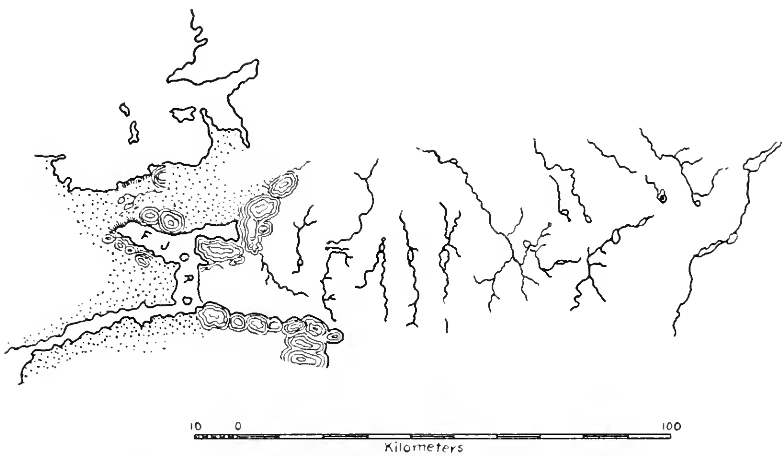


FIG. 34. Map showing the superglacial streams within the marginal zone of the inland-ice of Greenland (after Nordenskiöld).

Glacière." The particles of rock if not contiguous upon the ice surface absorb the sun's rays and cause excessive melting of the ice above and beneath them. They thus sink down into the ice and form dust wells (Fig. 35, *a*). The thin walls which separate those wells which are close together, being now attacked by the warm air on their sides instead of on the top only, they in their turn melt away to form a small basin, which soon either wholly or in part fills with water (Fig. 35, *b*). Where in contact with their neighbors and where of such thickness of accumulation as not to be heated through by the sun's rays, these rock particles behave in quite a

[91] A. E. Nordenskiöld, "Grönland," pp. 197–204, map 3.

different manner and protect the ice beneath them from the sun (note margins of wells and basins in Fig. 35, *a* and *b*). The same effect is brought about if the fragments are too large, for the thick-

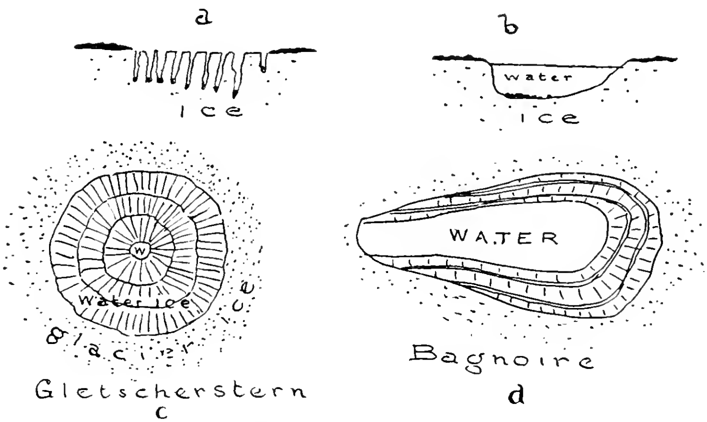


Fig. 35. Diagrams to show the effects of differential melting on the ice surface: *a*, dust wells; *b*, basins; *c*, glacier stars; *d*, bagnoires.

ness of surface layer of rock which can be sensibly warmed by the sun's rays is quite independent of the size of the fragment. Thus

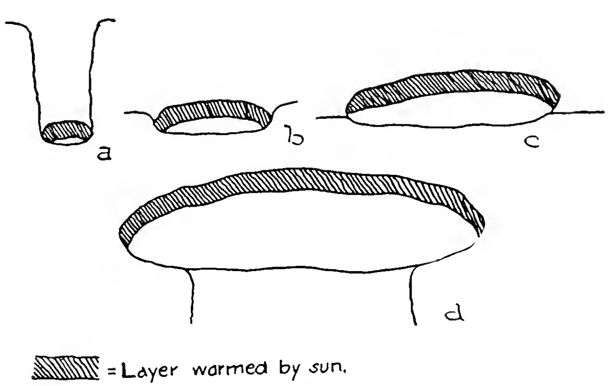


Fig. 36. Fragments of rock of different sizes to show their effect upon melting on the ice surface.

the familiar ice tables developed especially upon mountain glaciers are formed. Fig. 36 brings this out by showing the relation of the warmed surface layer to the whole fragment—(*a*) in a dust well, (*b*) in a pebble that sinks slightly into the ice until it reaches equilibrium, (*c*) in a slab of such size as to neither facilitate nor retard surface melting, and (*d*) in a large protective slab of rock.

The basins which result from the dust wells induce still other interesting structures. At night the water within these basins freezes in the form of needles which everywhere project inward from the steep walls of the basin. After repeated freezings the basins are often entirely closed by these needles and thus form "glacier stars" (see Fig. 35, *c*). Elongated basins have been given the name bagnoires (see Fig. 35, *d*).

From studies of such phenomena resulting from differential melting as developed upon the Great Aletsch glacier, we have found that the segregation of the rock débris upon the bottom of the basins later protects those areas after melting of the general surface has

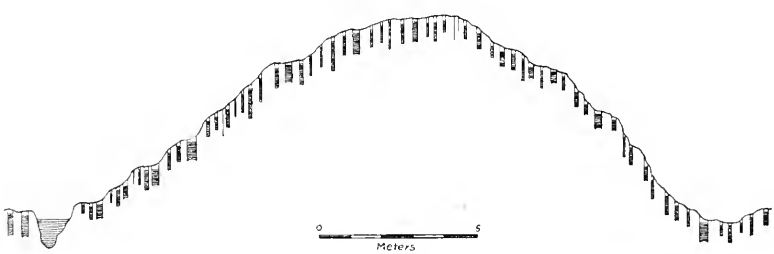


FIG. 37. Section of the so-called "cryoconite holes" upon the surface of an ice hummock (after Nordenskiöld).

drained them of their water. Thus the familiar débris-covered ice cones come into existence and further increase the irregularities of the ice surface. The dust wells and basins which were described by Nordenskiöld covered over large areas the sides of steep hummocks in the ice as well as its more level surfaces (see Fig. 37).

On his return from his attack upon the inland-ice near Disco Bay, Peary travelled for seven hours through half-frozen morasses alternating with hard blue ice honey-combed with water cavities. Then the character of the ice completely changed, the slush and the water cavities disappeared, and the entire surface was granular snow-ice, scored in every direction with furrows, one to four feet deep, and two to ten feet in width, with a little stream at the bottom of each.[92]

Moats Between Rock and Ice Masses.—Wherever the ice sends a tongue down a valley, the edges of this ice shrink away from the warmer rocks on either side, thus leaving lateral canyons walled with ice on the one hand and with rock upon the other. Down these canyons are the courses of glacial streams.[93] An excellent example of such a lateral stream is furnished by the Benedict glacier (Plate XXIX, *A*).[94]

In most cases where nunataks project through the ice surface, the absorption of the sun's rays by the rock melts back the ice so as to leave a deep trench surrounding the island and much resembling the moat about an ancient castle. Snow drifted by the wind often bridges or partially fills the moat. Upon nunataks forty miles within the border of the ice in northeast Greenland the Danes found water running in the ravines and disappearing under the ice at the margin of the nunatak where it "formed the most fantastic ice-grottoes, where the light was broken into all colors through the crystal icicles."[95]

Such moats have been mentioned by nearly all explorers upon the ice. It has been claimed by von Drygalski that this phenomenon is characteristic of the west coast margin only, more ample nourishment upon the eastern coast making the snow rise about the rock like a water meniscus. In support of this view he cites Garde, who has figured such a case from the extreme south of Greenland.[96] Peary, however, has shown that the moats upon the west coast are often largely filled with snow.[97] Stein mentions this as a common feature after snow storms,[98] and Chamberlin[99] asserts that wherever

[92] *Jour. Am. Geogr. Soc., l. c.*, p. 282.

[93] Peary, *Jour. Am. Geogr. Soc., l. c.*, p. 286.

[94] R. E. Peary, "North Polar Exploration, Field Work of the Peary Arctic Club," 1898–02, Ann. Rept. Board of Regents Smith. Inst. for 1903, 1904, p. 517.

[95] Trolle, *Scot. Geogr. Mag.*, Vol. 25, 1909, pp. 65–66.

[96] "Die Eisbewegung," etc., *Pet. Mitt.*, Vol. 44, 1898, pp. 55–64.

[97] Peary, *Geogr. Jour., l. c.*, p. 217.

[98] Stein, *l. c.*

[99] Chamberlin, *Jour. Geol.*, Vol. 3, 1895, pp. 567–568.

the motion of the ice is considerable the trench does not appear, but the ice impinges forcibly upon the base of the nunatak.

Englacial and Subglacial Drainage of the Inland-ice.—In addition to the superglacial streams which are so much in evidence, others which are englacial run beneath the surface of the ice, as has often been discerned by putting the ear close to the ice surface. Nordenskiöld reports one instance where water spouted up from the surface mixed with a good deal of air and spray.[100] Salisbury also has mentioned a huge spring upon the surface of the ice in north Greenland that shot up to a distance of not less than ten feet above the bottom of the basin from which it issued. Owing to the fact that near the margin of the ice its surface is much crevassed, comparatively little water can continue to the border in surface streams. Salisbury mentions an instance where an englacial stream with a diameter of about five feet issued from the vertical face which formed the ice front. Most of the water flowing upon the surface descended, however, to the bottom and issued largely below the surface of the fluvio-glacial materials. It is, he says, a rare exception to find a visible stream issuing from beneath the ice at its margin. In most cases, the water undoubtedly comes out in quantities, though beneath the surface of the outwash apron, as could be detected by the ear.[101] Peary has observed that a greater abundance of water issues from beneath the ice-cap in extreme northeastern than in northwestern Greenland.[102]

The Marginal Lakes.—Wherever the ice has withdrawn from the rock surface and where ice drainage permits of it, small lakes marginal to the inland-ice have come into existence. Special interest attaches, however, to those bodies of water which are impounded by the ice itself along its margin, because of the light which is thrown upon the origin of somewhat similar bodies of water about the great continental glaciers of Europe and North America during late Pleistocene times. Attention was called to such ice-dammed lakes situated upon the margin of the Frederikshaab tongue of the inland-ice by the Jensen, Kornerup and Groth expedition of 1878.

[100] A. E. Nordenskiöld, "Grönland," p. 137.

[101] Salisbury, *l. c.*, pp. 806–7.

[102] Peary, *Geogr. Jour.*, *l. c.*, p. 224.

A map of this region was published by Jensen (see Fig. 38).[103] Here the lakes filled with water from the melting of the glacier by which their outlets are blocked, stand at different levels. The Tasersuak on the south standing at a level of 940 feet above the sea, is blocked by ice at both ends and is covered by bergs which are calved from the ice cliffs. This lake drains through a canal upon the ice to a much smaller lake standing at a level of 640 feet, and thence through a small river to the head of the Tiningerfjord. To the

FIG. 38. Map showing the margin of the Frederikshaab ice apron extending from the inland-ice of Greenland and showing the position of ice-dammed marginal lakes (after Jensen).

northward of the apron of ice another long fjord is blocked by a T-shaped extension of ice into its central portion. Thus there result two fresh water lakes standing at different levels, the lower one, like the Tasersuak, with ice cliffs at both ends, and the other blocked at one end only by the ice. A slight retreat of the inland-ice of this district would retire the T-shaped extension of the glacier, and the two smaller lakes would thus become united into one at the level of the lower. A still further withdrawal of the Frederikshaab

[103] "Meddelelser om Grönland," Heft 1. This map has been many times copied, best by Nordenskiöld in his "Grönland" on p. 161.

glacier tongue would open an outlet for this lake to the sea at a still lower level. Souvenirs of these events would be left in a series of parallel shore lines ascending in step-like succession to the head of the fjord (see Fig. 39). Suess has used this illustration to solve the

Fig. 39. Diagram showing arrangement of shore lines from marginal lakes to the northward of the Frederikshaab ice tongue, if its front should retire past the outlet of the lower lake.

vexed problem of the *seter*, the abandoned shore lines of Norway which have this peculiarity of arrangement.[104]

The famous "parallel roads" of glens Roy, Glaster and Speen in the Scottish Highlands, which have in similar manner vexed geologists but which were finally given a satisfactory explanation by Jamieson,[105] find here a living model. Still later a nearly identical example from Pleistocene times has been supplied from the Green Mountains to the eastward of Lake Champlain.[106]

About the Cornell tongue of the inland-ice of Greenland are many marginal lakes situated where the border drainage has been blocked by the glacier itself. These lakes have been described by Tarr, who says:[107]

> In its passage down the valley, between the ice and the land, the marginal stream finally enters the sea. During its passage it now and then encounters tongues of ice, and for a distance flows along them, and finally beneath them, where the glacier edge rests against a moraine, or the rock of the land. Again it falls over a rock ledge as a cascade, or even a grand waterfall; and every here and there it is dammed to form a marginal lake.

[104] Besides the Jakobshavn ice tongue, there is another lake confined in like manner to the Tasersuak. (Ed. Suess, "The Face of the Earth," Vol. 2, pp. 346–363.)

[105] Thomas T. Jamieson, On the parallel roads of Glen Roy and their place in the history of the glacial period. *Quart. Jour. Geol. Soc.*, Vol. 19, 1863. pp. 235–259.

[106] Because here the ice similarly blocked the natural outlet.

[107] R. S. Tarr, "The Margin of the Cornell Glacier," *Am. Geol.*, Vol. 20, 1897, pp. 150–151.

Dozens of these, great and small, were seen along the margin; and they varied in size from tiny pools to ponds half a mile in length, and 200 to 300 yards in width.

Since the water of the marginal streams is everywhere milky with sediment, these lakes are receiving quantities of muddy deposits, and in them tiny deltas are being built. Where the lake waters bathed the ice front little icebergs are coming off, in exactly the same way as in the fjord at the glacier front, and these are bearing out into the lake large rock fragments which are being strewn over the bottom or on the shores. Also at the base of the cliffs, as well as on some of the deltas formed by rapidly flowing streams, pebbles and boulders are being mixed with the clay.

Nearly every lake shows signs of alteration in level resulting from the change in outflow either to some point beneath the ice, when the lake may be entirely drained, or to some lower outlet for the lake opened by a change in the ice front, or by the down cutting of the stream bed where it is eating its way through a morainic dam. The different elevations are plainly evident from the absence of lichens on the rocks, the clay clinging to the rocky shores, and the beach terraces along the old shore lines. In one case, at the western end of mount Schurman, a lake of this type, with a depth of at least 100 feet has recently been drained. Where these extinct lake beds exist one sees revealed an expanse of muddy bottom with scattered blocks of rock.

In Plate XXX, *A* and *B* are represented after Tarr, in the one case, one of the marginal lakes, and in the other, the formation of a delta under the conditions described. From the Karajak district on the northern side of the Upper Nugsuak Peninsula[108] von Drygalski has described in addition to the usual rock basin lakes left by the withdrawal of the ice front, a true ice-dammed lake which appears upon his map as the *Randsee*.[109]

No one of the marginal lakes thus far described furnishes a parallel to the interesting Pleistocene glacial lakes of the Laurentian basin of North America, since these developed for the most part upon a surface of relatively mild relief, and the shores not formed by the glacier itself were generally moraines registering an earlier position during the retirement of the ice front. Perhaps an existing example comes nearest to being realized in connection with those glaciers which descend the eastern slopes of the Andes and enter the great lakes impounded behind moraines of an earlier

[108] "The Cornell tongue is situated upon the southern side of the same Peninsula."

[109] E. von Drygalski, "Grönland-Expedition," Vol. 1, pp. 61–63.

extension of the same ice tongues.[110] In these cases the ice fronts of the glaciers are cut back into cliffs from which are derived the bergs that float upon the surface. The ice cliff and some of the bergs of Lake Argentino are shown in Plate XXX, *B*. According to Moreno, Lake Tyndall is bounded on the west by true inland-ice, the remnant of the larger sheet of Pleistocene times.

Ice Dams in Extraglacial Drainage.—In north Greenland outside the ice front, the brooks sometimes offer a striking example of ice obstructions forming by irrigation. This is often the case where their beds are wide and are covered with boulders. The water generally continues to run beneath the stones for a great part of the winter. Later, however, its outlets may freeze up, whereupon the water rises, inundating the stones and covering them with an ice crust. Through successive obstruction, overflowing and freezing of these streams, the ice dam which results may attain to such a thickness that it is still to be found at these places late in the summer when the ice and snow have elsewhere disappeared from the low land.[111] The significance of such dams as obstructions during a readvance of the ice front may well be considerable.

Submarine Wells in Fjord Heads.—Rink states that the sea flowing into the fjord in front of the glacier tongue which ends below the water level, is kept in almost continual motion by eddies not unlike those which are seen where springs issue from the bottom of a shallow lake. Such areas upon the surface of the fjord may generally be recognized by the flocks of sea birds which circle above them and now and then dive for food.[112] The existence of such fresh water streams as this implies may also be inferred from the strong seaward current that prevails in the fjords and which is so effective in clearing them of bergs. Such a whirlpool of fresh water or "submarine well" was observed by Rink in the Kvanersok-

[110] Francisco P. Moreno, "Explorations in Patagonia," *Geogr. Jour.*, Vol. 14, 1899, pp. 253–256. Also Hans Steffen, "The Patagonian Cordillera and its Main Rivers between 41° and 48° South latitude," *ibid.*, Vol. 16, 1900, pp. 203–206. Also Sir Martin Conway, "Aconcagua and Tierra del Fuego," London, 1902, pp. 134–135.

[111] Henry Rink, "Danish Greenland, Its People and its Products," London, 1877, p. 366.

[112] Rink, *l. c.*, pp. 50, 360–363.

fjord (lat. 62° N.) which was over 100 yards in diameter. The kittiwakes swarmed over the spot, and the water was muddy, although no brooks were observed along neighboring shores. This well Rink believed, from reports furnished by the natives, to be much smaller than the similar ones in some other fjords.

According to Rink[113] the lateral lake which borders the inland-ice of Greenland in one of the branches of the Godthaabfjord-Kangersunek, suffers changes of level just when the submarine wells before the ice cliff in the fjord showed marked changes in volume. Thus, whenever the water of the lake suddenly subsides, the submarine wells from the bottom of the fjord burst out with violence. On the other hand, when the water in the lake is rising, the wells are relatively quiet. These sudden discharges of the water from lateral lakes, save only that their outlet is submarine, seem to be in every way analogous to the spasmodic discharges of the Märjelen See upon the margin of the Great Aletsch Glacier in Switzerland. When, as occasionally happens, this lake empties through the opening of a passage beneath the glacier, the villages which are situated miles below in the valley are suddenly inundated with water.

Discharge of Bergs from the Ice Front.

The Ice Cliff at Fjord Heads.—Wherever the inland-ice reaches the sea in the fjord heads, and where it comes directly to the sea in broad fronts, as it does near Melville Bay, at Jokull Bay and on the north side of northeast Foreland, it is here attacked directly by the waves and is further undermined through melting in the water. The crevassing of its surface over the generally steep descents to the fjords, in a large measure facilitates the attack of the water upon the ice by offering planes of weakness similar to the joint planes in rock cliffs attacked by the sea on headlands. The fjords, though often quite narrow, are generally of great depth, so that although the ice cliff often rises to a height of several hundred feet, and in such cases must be assumed to descend to a depth below the surface of from five to seven times this distance, its base probably everywhere rests upon the bottom of the fjord. To this a possible exception

[113] Rink, *l. c.*

has been noted for the great Karajak glacier, of which a relatively flat front section may be assumed to be the surface of a floating portion[114] (see Fig. 40). To this interesting example of a floating glacier tongue in connection with the inland-ice of the northern hemisphere, we may recall the probably floating front of the Turner

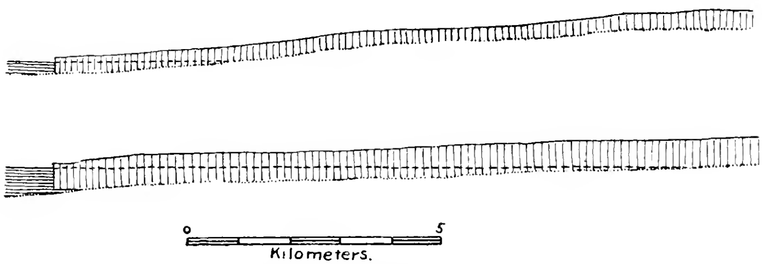


Fig. 40. Sections from the inland-ice through the Great and Little Karajak tongues to the Karajak fjord (after von Drygalski).

glacier, a dendritic glacier of the tide-water type in Alaska. For its type this example is apparently unique.[115]

Manner of Birth of Bergs from Studies in Alaska.—The birth of bergs from the parent glacier has been often described by travellers and the superlatives of the language have been drawn upon to express the grandeur and beauty of the observed phenomena. Simple as the process may appear to the casual tourist who makes the usual summer excursion to Alaska, it is not free from serious difficulties, and has given rise to conflicting views among specialists. The water in front of the ice cliff is generally so muddy, and the danger of approaching the ice front so great, that exact data are necessarily difficult to obtain. The smaller bergs composed of white ice, which are seen to fall into the water from the cliffs at almost all hours, offer no difficulties of explanation, but they are likewise without great significance as concerns the manner of formation of those great floating masses of ice which are carried far

[114] E. von Drygalski, "Grönland-Expedition," Vol. 1, pl. 43. See also R. D. Salisbury, The Greenland Expedition of 1895. *Jour. Geol.*, vol. 3, 1895, p. 885.

[115] R. S. Tarr, and B. S. Butler, "The Yakutat Bay Region, Alaska, Physiography and Glacial Geology," Prof. Pap. No. 64, U. S. Geol. Sur., 1909, pp. 39–40, pl. 10–a.

to sea and scattered over wide areas of the ocean before their final dissolution in the warmer southern waters.

The larger bergs instead of falling from the cliffs, suddenly rise out of the water as ice islands, often several hundred feet in front of the ice cliff. A wholly satisfactory solution of the problem of their birth involves a nice quantitative adjustment of several factors, all of which are undoubtedly more or less concerned. On the one hand, there is wave action which is effective especially near the water level and has a direct range of action extending from a distance below the surface equal to the length of a storm wave in the fjord, and to a height above the quiet level equal to the height of the wave's dash. If there were no melting in the water, and if the lower layers of the glacier moved forward as rapidly as the upper, the tendency would undoubtedly be to develop an erosion profile in every way like that of a rock-cut terrace upon the sea shore. With emphasis upon this element in the problem Russell has assumed that the ice cliff at the fjord is prolonged outward beneath the water as an ice foot which thins gradually toward the toe. Upon this hypothesis the bergs which rise from the water are born from the foot where the increasing buoyancy of the outer portion overcomes the cohesive strength of the material at the surface where rupture occurs. This view accounts particularly well for those bergs which rise from the water far in advance of the cliff (see Fig. 41).[116]

Laying stress rather upon melting in the water and upon the rapid forward movement of the upper layers of ice near the glacier margin, Reid has arrived at a wholly different conclusion concerning the origin of larger bergs:[117]

> The more rapid motion of the upper part would result in its projection beyond the lower part, and this would become greater and greater until its weight was sufficient in itself to break it off. The extent of the projection before a break would occur, depends evidently upon the strength of the ice. . . . That the ice for several hundred feet below the surface does not in general project farther than that above is evident from the fact that I have frequently seen large masses, extending to the very top of the ice front,

[116] I. C. Russell, "An Expedition to Mt. St. Elias, Alaska," *Nat. Geogr. Mag.*, Vol. 3, 1891, pp. 101–102, fig. 1.

[117] H. F. Reid, "Studies of Muir Glacier, Alaska," *ibid.*, Vol. 4, 1892, pp. 47–48.

shear off and sink vertically into the water, disappear for some seconds, and then rise again almost to their original height *before* turning over. If there were any projection within 300 feet of the surface this mass would have struck it and been overturned so that it could not have arisen vertically out of the water.

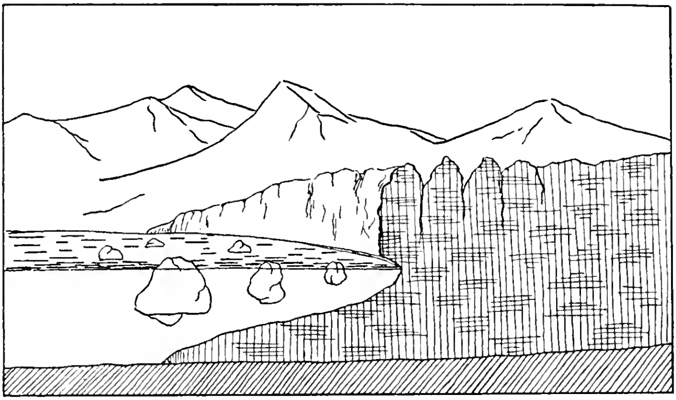


FIG. 41. Origin of bergs as a result especially of wave erosion (after Russell).

Reid thinks there are three ways in which bergs come into existence at the end of a glacier:

(*a*) A piece may break off and fall over—this is the usual way with small pinnacles; (*b*) a piece may shear off and sink into the water—this is the usual way with the larger masses; or, again, (*c*) ice may become detached under water and rise to the surface.

The supposed successive forms of the ice front, according to Reid, are shown in Fig. 42.

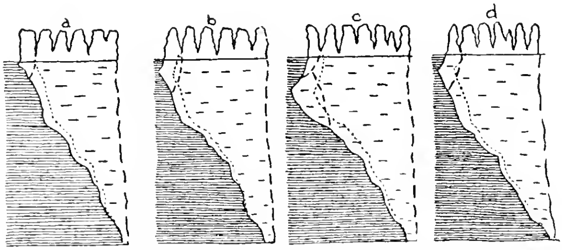


FIG. 42. Supposed successive forms of a tide-water glacier front (after Reid).

It is easy to see that Russell's and Reid's views might each apply in special cases dependent: (*a*) upon the narrowness or the sinuosities of the fjord, which would determine the reach of the waves; (*b*) upon the steepness of the slope back of the ice cliff, which would regulate the different velocities of surface and bottom layers of ice, and determine the measure of crevassing; (*c*) upon the irregularities in the floor of the ice tongue, which would largely fix the amount of shearing and overthrusting; (*d*) upon the presence or absence of warm ocean currents, which would regulate the rate of melting of the ice by the fjord water; and (*e*) upon the freezing of the water surface,[118] which must put a bar upon the action of the waves during the colder period.

Studies of Bergs Born of the Inland-ice of Greenland.—Though ice bergs are discharged from the inland-ice throughout practically the entire extent of the coast line of Greenland wherever inland-ice reaches the sea, yet the great bergs which push out into the broad Atlantic arise either on the west coast between Disco Bay and Smith Sound, or on the east coast south of the parallel of 68°. To the north of this latitude the bergs are firmly held in the heavy pack-ice, while the bergs of southwest Greenland form for the most part in such narrow fjords that they are too small to travel far before their final dissolution.

The size of the Greenland ice bergs has probably been much overestimated. Of 87 measurements made by von Drygalski on the large bergs calved in the Great Karajak fjord, the highest reached 137 meters above the water, or about 445 feet. This mass of ice was, however, against the glacier front, and probably rested on the bottom. None of the others measured were much above 100 meters high or about 325 feet.[119] The berg shown in Fig. 43, photographed by an earlier explorer in Melville Bay, measured 250 feet in height.

During fourteen months spent in the immediate vicinity of the steep front of the Great Karajak ice tongue, von Drygalski carried out extensive studies upon the calving of bergs, and has distinguished three classes. Those of the third class form almost con-

[118] R. S. Tarr, "The Arctic Ice as a Geological Agent," *Am. Jour. Sci.*, Vol. 3, 1897, p. 224.

[119] E. von Drygalski, "Grönland-Expedition," etc., *l. c.*, pp. 367–404.

stantly and consist of larger or smaller fragments which separate along the crevasses and fall into the sea. Only twice were calvings of the second class observed, namely, in late October and in early November. Of one of these von Drygalski says:

> I heard a thundering noise, but at first neither I nor the Greenlanders who were with me saw anything. Suddenly a great distance away from the margin of the glacier, an ice berg emerged from the sea, rose out of the water, though not to the height of the cliff, and then moved away accompanied by a continuous loud tumult and by a rise in the level of the water, through the agency of which it moved away from the cliff quite rapidly. It did not come from the cliff, but certainly emerged from below. The Greenlanders, whom I afterwards questioned about it, gave me the same impression. . . . The margin of the glacier was unchanged.

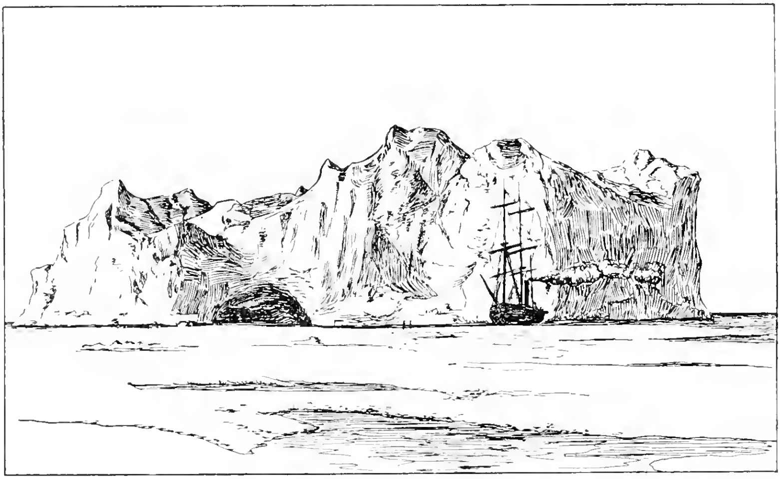


FIG. 43. A large berg floating in Melville Bay and surrounded by sea ice.

Here it was noticed that the berg was long though not as high as the ice cliff which terminated the glacier. It is the opinion of von Drygalski that bergs of this class come from the lowest layers of the glacier. Because of the pack-ice which in winter forms in front of the glacier, the ice cliff is at that time not cut away so fast, and it was, in fact, observed in the winter farther out than during the summer. This explanation in the main is in agreement with that of Russell.

Bergs of von Drygalski's first class, which are the most massive of all, separate from the entire thickness of the ice front. Two such bergs were observed in process of calving by von Drygalski and other members of his party. The same loud sound which had been heard at the birth of bergs of the second class accompanied the birth of those in the first class, *but the movement of separation from the glacier was visible at the same instant.* A portion of the cliff front was seen to separate from the cliff, being thereby thrown somewhat out of equilibrium and started in a pendular vibration which produced great waves in the fjord and increased slightly its distance from the newly formed ice cliff. It was here observed that the main pinnacle of the berg slightly exceeded in height the highest pinnacles of the new glacier rim. This, it will be remembered, is in contrast with the bergs of the second class which did not reach to the height of the cliff. Bergs of the first class usually regain their equilibrium after rhythmic oscillations and float away in an upright position. The bergs of the second class often turn over displaying the beautiful blue color of the lower layers. Salisbury's two types of Greenland icebergs seem to correspond with von Drygalski's bergs of the first and second classes.[119a]

The water waves which are sent out to the shores at the birth of a great ice berg extend 50 kilometers or more within the fjord, driving the smaller floating bergs together and thus assisting in their fragmentation and consequent dissolution. The calving of bergs of the first class von Drygalski believes occurs where the depth of the fjord has so far increased that the ice begins to leave the bottom and assume a swimming attitude. The buoyancy of the water is, he believes, thus the true cause of the separation of the bergs.

> Depths which are four to five times as great as the thickness of the inland-ice above the sea level, are not measured in Greenland in front of attached ice masses, because the latter become in that case broken up into ice bergs.[120]

This view gains strength from Salisbury's studies of the glaciers ending in Melville Bay and apparently floated for a very short distance back from their fronts and generally in the middle only.[120a]

University of Michigan,
Ann Arbor, Mich.

[119a] *Jour. Geol.*, Vol. 3, pp. 892–897.
[120] E. von Drygalski, *l. c.*, p. 404.
[120a] Salisbury, *Jour. Geol.*, Vol. 3, 1895, pp. 885–886.

SOLAR ACTIVITY AND TERRESTRIAL MAGNETIC DISTURBANCES.

By L. A. BAUER,

Director of Department of Research in Terrestrial Magnetism, Carnegie Institution of Washington.

(*Read April 23, 1910.*)

It has already been my privilege to lay before this society, on various occasions, some of the results obtained since 1905 of the magnetic survey work, chiefly of the oceans, conducted under the auspices of the Carnegie Institution of Washington. At the present rate of progress it seems reasonable to suppose that by the year 1915 it will be possible to construct a new set of magnetic charts as based upon homogeneous data largely obtained by one organization. Along with the magnetic survey of the oceans, begun on the "Galilee" in the Pacific Ocean in 1905 and continued until May, 1908, and now being conducted on the "Carnegie" in the Atlantic Ocean and later to be extended to the Indian Ocean, that of land areas is likewise in good progress, parties of the Carnegie Institution of Washington having already made notable expeditions in magnetically unexplored regions in all parts of the globe.

Provision has thus been made for a rapid and continuous accumulation of the data required for mapping the so-called "permanent" magnetic forces of the earth. As a matter of fact, however, the earth's magnetism is subject to continuous change, and owing to the so-called secular changes but a few years—five to ten—suffice to completely alter a chart showing the direction of the compass at various points over the globe. Hence, the work as being executed likewise embraces the determination of the secular changes in the earth's magnetism at a sufficient number of stations to permit applying the necessary corrections for keeping magnetic charts up to date.

However, there is another side to magnetic work, certainly not less interesting, and possibly equally important to that mentioned—the investigation of the causes of those mysterious influences which daily, hourly, year in and year out, impress themselves upon the earth's magnetism. It is confidently believed by many that we must look primarily to the fluctuations of the delicately poised and sensitive magnetic needle for the key which will unlock the door to knowledge of the mysterious forces, coming from the regions beyond and conditioning the phenomena on which our welfare and development may, in no small measure, depend.

But aside from any such possible practical results, the belief is not without foundation that just as the great doctrine of evolution resulted from the persistent and searching inquiry into the causes producing the variations and mutations in biological phenomena, *our knowledge of the earth's magnetism is primarily to be advanced through the study of what causes the earth's magnetism to vary.* It also appears that when we shall have made an exhaustive study of the periodic magnetic variations, such as the solar-diurnal, the lunar-diurnal, the annual, etc., we shall find that none of these is strictly cyclic, but that each runs its course, through periods incommensurable with one another, the resulting residual effects at any moment of time being of sufficient magnitude to be reckoned with. Thus it may be possible to account for the secular variation of the earth's magnetism without resorting to any other cause than those daily in operation.

I shall now give a sketch of the chief results obtained along a line of inquiry being conducted coöperatively between two departments of the Carnegie Institution of Washington—the Solar Observatory at Mt. Wilson, California, under the direction of Professor George E. Hale and the Department of Research in Terrestrial Magnetism—viz., as to *the connection between solar activity and the earth's so-called magnetic storms.*

At an instant, without any previous warning, magnetic needles are caused to swing out of their usual position or direction, earth electric currents are generated of sufficient strength to interfere with telegraph and cable lines, brilliant and wide-spread auroras are evoked, and even electric car line traffic is momentarily suspended—by what? That is the problem. Possibly at the same time there may be seen remarkable formations of sun-spots and the conclusion is drawn that there is some connection between the solar and terrestrial phenomena, especially so as similar coincidences have very often occurred in the past.

However, the connection is far from being such an immediately evident one. Many cases of magnetic storms might be cited when no sun-spots existed, at least on the face of the sun then visible. Then again, the area of a sun-spot is no true index either of the character or of the magnitude of a magnetic storm which may occur at about the same time. Thus one of the severest magnetic storms within the past fifty years—that of October 31 to November 1, 1903—was associated with a group of sun-spots considerably smaller than that in the earlier part of the month which was accompanied by a minor magnetic disturbance. Attempts at a direct connection between individual sun-spots and individual magnetic storms have not been wholly successful. While there have been a few cases—very few in fact—in which an apparently immediate connection was discovered between some striking solar phenomenon and a terrestrial magnetic fluctuation, there are vastly more instances for which no such individual relation existed. If, however, the subject is approached statistically, and the average state of the solar surface, as gauged by sun-spots or other eruptive phenomena, is compared with the average state of the earth's magnetism for a sufficient interval of time—a year or say several months—and these average values plotted, then a most striking resemblance is manifested between the two sets of phenomena. The parallelism, as intimated, is not so pronounced, however, for shorter intervals, for example that of a month.

The magnetic data generally used in such comparisons are the ranges or amplitudes of the daily fluctuations of the magnetic needle or say the frequencies of magnetic storms. During years of high sun-spot frequency, *i. e.*, increased solar activity, the diurnal swing of the needle is very appreciably greater than in years of low sun-spot frequency. And likewise without question the greatest number of, as well as generally the severest, magnetic storms occur in years of increased solar activity. While investigations exhibit a linear relationship between the average sun-spot frequencies and the average diurnal range for a fairly long interval of time, so that one set of phenomena could be almost directly deduced from the other, a similar relation has not been found to hold between the *magnitude* of a magnetic disturbance and any measure of solar activity thus far proposed.

Fresh interest was shown in magnetic storms a year ago by Hale's discovery of magnetic fields in sun-spots and some persons immediately jumped to the conclusion that the origin of our magnetic storms had been discovered. However, the rapid decrease in the strength of the sun-spot fields with elevation, observed by Hale, shows that they, at the distance of the earth from the sun, could not by direct action produce an effect to be detected by the most sensitive modern magnetic apparatus, whereas the effects actually observed during a magnetic storm exceed by 100 and even 1,000 times the limit of measurement. It ought to be stated that Hale himself never, as far as I know, expressed any opinion as to the possibility of a direct magnetic action of sun-spots.

In order to pave the way towards a solution of some of the difficulties mentioned, the following investigation was originally begun, partly as the result of a desire on the part of Professor Hale to ascertain as quickly as possible what solar phenomena should be given chief attention in the proposed study. He wished to know, for example, how closely the curves resulting from his new method of measuring the solar activity—by the total area of the bright calcium flocculi seen on the sun's disc—corresponded with the well-known fluctuations in the earth's magnetic activity. He divided the sun's disc into zones 10° wide and determined the area of all the flocculi present in each zone. The sum of all the zones is the total area for the period in question and the solar activity was taken to be directly proportional to this area. Since the rotation period of the sun varies with solar latitude, means are taken for each zone corresponding to the rotation period for that zone in order to eliminate the effect of the solar rotation.

In my paper presented to the society a year ago I was enabled to communicate some preliminary results obtained from our comparative study of the variations in the sun's activity, as shown by the calcium flocculi measurements at the Mount Wilson Solar Observatory for the period May, 1906, to January, 1909, and the variations in terrestrial magnetic activity during the same period, as based upon the observations at the five magnetic observatories of the U. S. Coast and Geodetic Survey. Probably the most important result then derived was that, in general, the earth's magnetization suffers a diminution during a period of intense solar activity. This pointed to the fact that the general, or average effect, of a magnetic storm was to superimpose on the earth's magnetization a system of magnetic forces equivalent to a demagnetizing system whose magnetic axis was reversed from that of the earth's magnetic field, so that the magnetic north pole of the disturbance system would lie in the southern hemisphere. While this relation between changes in solar activity and those in the earth's magnetism held, in general, for the period examined (May, 1906–January, 1909) there were some manifest contradictions also, so that for these an increase in solar activity corresponded to an increase in the earth's magnetization. These same contradictions were found to hold generally if we replaced Hale's measure of solar activity by the Wolfer curve of sun-spot frequency. There was thus again revealed the difficulty of establishing a relation which would link solar with magnetic phenomena in such a definite way that one set might be predicted from the other.

Additional discoveries regarding terrestrial magnetic disturbances may now be reported upon. A recent examination of the times of beginning of magnetic disturbances, as recorded at observatories over the entire globe, showed that, without doubt, magnetic storms do not begin at absolutely the same instant of time, as heretofore believed. Instead, they are found to progress over the earth in some definite manner and at a measurable speed. For the abrupt disturbances, which are usually comparatively minute in their effect on the compass needle, a complete passage around the earth would require from 3½ to 4 minutes. For the bigger effects, or for the larger magnetic storms, the differences of time between various stations are such that if these larger effects also traveled around the earth completely, it might take them a half hour or more. The following main conclusions were drawn:[2]

[2] *Journal Terrestrial Magnetism and Atmospheric Electricity*, Washington, D. C., Vol. 15, No. 1, March, 1910 (18 and 20).

It is thus seen that the disturbances of May 8, 1902, and of January 26, 1903, both traveled around the earth eastwardly, at an average velocity of about 6,700 miles per minute, taking from 3½ to 4 minutes to make the complete circuit. The disturbance of May 8, 1902, as determined from the times of beginning at the various stations, began in about the meridian of 75° west, whereas the initial meridian for the disturbance of January 26, 1903, computed in a similar manner, was found to be, roughly, 160° west. . . .

Magnetic storms do not begin at precisely the same instant all over the earth. The abruptly beginning ones, in which the effects are in general small, are propagated over the earth more often eastwardly though also at times westwardly, at a speed of about 7,000 miles per minute, so that a complete circuit of the earth would be made in 3½ to 4 minutes. For the bigger and more complex magnetic disturbances the velocity of propagation may be cut down considerably. The time of beginning of the disturbance may be appreciably different, for the various magnetic elements, according to the character of the operating systems.

The moment it is granted that magnetic storms do not occur over the earth simultaneously, then a new point of view is presented for the investigation of the relation between magnetic storms and solar activity, and a definite criterion is set for testing any theory advanced. A second decisive test is furnished by another important fact disclosed by our study of the direction of motion of the two magnetic disturbances of May 8, 1902, and January 26, 1903. The electric currents which we should have to suppose circulating in the regions above us to produce the disturbance effects as actually recorded for the cases cited, would have to go around the earth eastwardly, if they are to be ascribed to a motion of negatively electrified particles. This is in fact the very direction in which the times of beginning of the disturbances were found to progress.

Were we to suppose now that magnetic disturbances are due to the entrance in the earth's magnetic field of small negatively electrified particles brought to us from the sun by the pressure of light, or were we to adopt the hypothesis of cathode rays coming from the sun, then, in either case, it would be found that the effect of the earth's magnetic field is to deflect these particles in such a way that, in the equatorial regions, for example, they would be made to circulate around the earth from east to west. But this direction is contrary to that in which we found the negative electric currents would have to go for the disturbances of May 8, 1902, and January 26, 1903, to harmonize with the times of beginning noted at stations around the globe, and as shown to be necessary to account for the actual disturbance effects. Hence the direction test already excludes the possibility of ascribing certain of our magnetic storms, at least, to the effect of negatively electrified particles and cathode rays received from the sun. In a similar manner, it can be shown that positively electrified particles would likewise not accomplish the desired result.

But the time required for the quickest disturbances to get around the earth—about 3½ to 4 minutes—furnishes another crucial test. Knowing the electric charge carried by the particles, their velocity, and the deflecting effect of the earth's magnetism, it is possible to compute the radius of the circle around which they would have to move were they to come to the earth approximately, for example, in the plane of the equator and accomplish the circuit in 3¾ minutes. This radius turns out to be 580 times that of the earth's radius. Hence, these particles could never approach the earth closer than 2,300,000 miles! And if we compute the strength of the current necessary to produce at that distance even one of these comparatively minute magnetic disturbances, we find it would have to be on the order of 60,000,000 amperes, or sixty times that deemed sufficient to account for the big disturbances. We are accordingly forced to look elsewhere for the chief source of our magnetic storms. [The value of the radius, 2,300,000 miles above given applies to an electronic mass. Where we to increase the latter 1,000,000 times the radius would still be 23,000 miles. If we take the mass of the carrier of the electric charge the size of that of a particle brought to us by the pressure of light, the radius turns out less than that of the earth, hence an impossible result.]

In addition to negatively electrified particles coming from the sun, we also receive radiations such as the gamma rays of radium or the Röntgen rays, which are not deflected by the earth's magnetic field as they do not carry electric charges. Their chief effect would be to ionize the gases of which the atmosphere is composed, *i. e.*, make these gases better conductors of electricity. Ultra-violet light has the same effect. Now we know that a small part of the magnetic forces acting on a compass needle is due, not to magnetizations or electric currents below the earth's surface, but to electric currents in the atmosphere. If then the regions of these upper electric currents are at any time made by some cause more conducting, electricity is immediately set in motion, which in turn induces a subsidiary magnetization in the earth. The effect then which we actually observe on the compass needle is the joint result of the newly generated electric currents in the atmosphere and the induced magnetization of the earth.

The direction followed by the new current depends upon its origin, upon the direction of the electromotive force of the primary electric field already existing at that point, and upon the deflecting effect of the earth's magnetic field and of the earth's rotation on the flowing current. *In other words, while we must doubtless look chiefly to extra-terrestrial agencies for the ionizing of the air and thus splitting it up into carriers of positive and of negative charges, we are compelled to look to the atmospheric electric field and to the earth's rotation for furnishing the energy necessary to drive the ions over the earth, and by their motion produce the effects observed during a magnetic storm.*

We shall tentatively designate the theory which thus aims to account for terrestrial magnetic disturbance as "the ionic theory of magnetic disturbances." It is of interest to quote here from Schuster's extremely suggestive paper[3] on the "Diurnal Variation of Terrestrial Magnetism":

> Outbreaks of magnetic disturbances, affecting sometimes the whole of the earth simultaneously, may be explained by sudden local changes of conductivity which may extend through restricted or extensive portions of the atmosphere. I have shown in another place that the energy involved in a great magnetic storm is so considerable that we can only think of the earth's rotational energy as the source from which it ultimately is drawn. The earth can only act through its magnetization in combination with the circulation of the atmosphere, so that magnetic storms may be considered to be only highly magnified and sudden changes in the intensity of electric currents circulating under the action of electric forces which are always present.

How can we account, on the basis of the "ionic theory," for the velocity of motion of magnetic disturbances and, hence, for the time required to make a complete circuit of the earth? Starting from the well-established law as to motions of ions in gases, it is found that in

[3] *Phil. Trans. R. Soc., A,* Vol. 208, 1908, 184–185.

an electric field of one volt per centimeter (the average potential gradient found from atmospheric electricity observations on the surface), the velocity of the ions would at the height of about 75 kilometers or 47 miles be such that a complete circuit of the earth could be made in 3¾ minutes. Preliminary calculations show that at that height the existent electromotive force may be on the same order as actually assumed in the calculation. The height of 75 km. is about the average of that to which polar lights are seen. We thus place the electric currents, which may produce our magnetic disturbances, at a height where we know, from polar lights, electric currents actually exist.

Should the currents get lower down, then since their velocity varies inversely with atmospheric pressure, they will travel more slowly and the time required for a complete circuit of the earth is correspondingly increased. Their actual effect on a magnetic needle is, however, increased as they get nearer the surface. Hence we may say that the nearer a current gets to the earth's surface, the greater, in general, the disturbance effects and the slower the rate of propagation—this is in accordance, as we have seen, with actual observation. It seems probable that the reason for the remarkably large effects experienced during the magnetic disturbance of September 25, 1909, must be ascribed chiefly to the fact that the currents generated succeeded in getting closer to the earth than for the average magnetic storm.

It is thus seen that on the basis of the ionic theory of magnetic disturbances it is possible not only to explain, in a perfectly natural manner, why magnetic storms do not begin at absolutely the same instant of time over the earth, but also to account for the direction of propagation and the reason for the possible different rates of progression. Thus far, however, the examination has applied exclusively to the sudden beginnings of disturbances or to the simple disturbances characterized by Birkeland as "equatorial perturbations," which our analysis has shown to be chiefly due to a simple, uniform magnetic or electric system superposed upon the existing field. It seems probable that for this class of disturbances, the electric currents producing them are farthest away from us, *i. e.*, they are in the stratum of the atmosphere where their velocity is such that a complete circuit of the earth, *if made*, would require from 3 to 4 minutes. In this connection it is worth while to record that, judging from Dr. van Bemmelen's observations at Batavia, Java, this interval of 3 to 4 minutes is also the average duration in general of the "starting impulse" at any one station.

After an interval of 3 or 4 minutes, the current either dies out or gets so far away as not to produce any effect. But it may also develop into a steady current continuing for an hour or more and then break out anew into a second starting impulse and finally develop into a complex current system with principal and secondary current vortices, as shown by the more complicated cases of magnetic disturbances.

Professor E. E. Barnard, a member of this society, has recently published[4] for the period 1902–09 a most valuable series of auroral observations made at the Yerkes Observatory from which we quote as follows:

> The streamers which spring from the arch as a base, and which always have a decided lateral motion and last for a minute or so only, almost always move to the west. On several occasions, however, I have seen them divide the arch, with respect to their motion, so that the ones to the west moved west and those to the east moved east. This is very rare. The motion is about 2° in one minute (and not 2 minutes to the degree as I stated in *Astrophysical Journal*, vol. 16, 143). I have wished to determine this motion more accurately, but we have had so few ray-producing auroras in late years, and the rays are so transient, that I have not been able to do so. It would be interesting to know if this motion is constant in a streamer and for all streamers.
>
> The pulsating bright masses that usually appear in the northeast or northwest, but which are sometimes seen under the pole, are among the most interesting phenomena. They are sometimes present when there are no other evidences of an aurora.

If the motion of the streamers is at the rate of 2 degrees in one minute, it would take five to ten minutes or more for the motion to traverse a band from ten to twenty degrees wide. Now the possibility of such a slow motion is contrary to what the cathode ray theory with charged particles moving on the order of 60,000 miles a second would premise, but it is in strict accordance with the results announced regarding the non-simultaneity of magnetic disturbances and their rate of progression over the earth.

[4] *Astrophys. Jour.*, Chicago, Ill., Vol. 31, 1910, 208–233.

According to the theory of magnetic disturbances as set forth above, the various manifestations of solar activity with their resulting emanations and radiations are not the direct but the indirect cause of the earth's magnetic storms. Their effect appears to be more in the nature of a releasing or trigger action, setting in operation forces already in existence in the upper regions of the atmosphere; terrestrial sources, in reality, however, supply the energy required for a magnetic storm. To connect then the well-established, general relationship between magnetic disturbances and the sunspot period, we must suppose that the radiations which alter the conductivity of the atmosphere vary in their amount and intensity in accordance with the periodicity of the solar phenomena.

In the above theory of magnetic disturbances we had to depend on an already existing electric field. If we analyze the results of magnetic observations made at various points over the earth's surface, it is found that about 95 per cent. of the measured magnetic force can be accounted for by an internal magnetization of the earth or by an equivalent electric current system in its interior. The outstanding 5 per cent., however, can only be due to an electric field in the atmosphere.

A preliminary examination discloses the possibility that these atmospheric electric currents may be ascribed to Foucault, or eddy currents, induced in the more or less conducting layers of the atmosphere as, during its general circulation around the earth, the air currents are made to cut across the earth's lines of magnetic force.

The general atmospheric circulation consists in the main of two great atmospheric whirls, the one in the middle and higher latitudes of the northern hemisphere whirling around the north pole in a counter-clockwise direction supposing we are looking down on the north pole. A similar whirl exists in the southern hemisphere except that, if we could look down on the south pole we would see that the whirling is done in the clockwise direction. Could we, however, look at the two whirls in the same direction, for example, from the south pole to the north pole, then the motion would be in the same direction for both, viz., clockwise.

Between the two main whirls, which we shall designate as the "polar whirls," there is another—"the equatorial whirl"—the motion of which is anti-clockwise looking again from the south pole to the north pole the air currents (trade winds) blowing westwards. And, as known, on each border of the two sets of whirls, there exists a belt of high barometric pressure. The direction of motion of the air in the whirls is conditioned by the deflecting effect of the earth's rotation on the air-currents, which are primarily set in motion because of the temperature differences between the polar and the equatorial regions.

Considering first the polar whirls, since the air in them has a motion relative to that of the earth, having a greater velocity than that of the earth's rotation, the currents of air are made to cut across the lines of magnetic force, and that too in the regions of the earth where the most effective induction component—the vertical component of the magnetic force—is strongest. The electric currents thereby induced would, in general, follow a direction at right angles to the motion of the conducting air currents. Consulting a chart of the winds for various seasons, it is seen that the precise distribution of the induced electric currents will be rather complicated. In fact the determination of the exact course of Foucault currents is, in any case, not a simple matter. The displacement of the magnetic axis of the induced electric current system with reference to that of the earth, as dependent on the relative magnetic permeability of the two media involved, introduces another factor to be taken into account.

Since the equatorial atmospheric whirl is opposite to the polar ones, the Foucault negative currents, if brought about, would, generally speaking, be reversed and go around the earth in an opposite direction to the polar negative currents. But the equatorial currents cut the vertical component of the earth's magnetic force in regions of small vertical force, which in fact reduces to zero over the magnetic equator, hence they are considerably weaker than the polar ones. Their strength is further diminished by the fact that in the upper equatorial regions the whirl may be reversed (the anti-trades) and so there would be superposed on the surface equatorial electric currents a set of opposite electric currents.

Hence, as a first approximation, we may confine ourselves chiefly to the polar atmospheric electric currents. Owing to the direction in which they go—approximately southwest to northeast, if negative ones, and reverse for positive currents—it follows that their effect on a compass needle placed on the earth's surface is similar to that of the earth's own magnetism. Or, in other words, the magnetic system equivalent to the atmospheric electric system is precisely similar to that of the earth and the magnetic axis of the atmosphere is hence in the same general direction as the earth's.

There is this difference, however, between the two magnetic fields of the earth and of the atmosphere, viz., that while their north magnetic poles are both in the northern hemisphere, that of the earth is below the surface, whereas that of the atmosphere is in the regions above; hence, while the effects on the compass needle are the same for both, the effects on the dip needle are opposite. The earth's field makes the north-seeking end of the needle dip below the horizon in the northern magnetic hemisphere, whereas the magnetic field of the atmosphere, were it alone acting (the earth's field being eliminated) would make the north-seeking end of the needle point above the horizon. Hence the effects of the two fields on the vertical magnetic component are opposite in the same hemisphere.

In accordance with the well-known laws of induced magnetism or electricity, the atmospheric magnetic field would have to be related to the earth's own field in the following manner: First, the strength of the induced electric currents must be directly proportional to the earth's intensity of magnetization, the electrical conductivity of the atmospheric layers in which the currents flow, and the velocity of the air-currents; secondly, the magnetic axis of the atmospheric electric field must suffer a displacement with reference to the earth's magnetic axis in a direction opposite to that of the earth's rotation (since the earth is moving more slowly than the air-currents); hence, the atmosphere's north magnetic pole would have to lie west of that of the earth's. The angle of displacement depends upon the same quantities as did the strength of the currents, different functions, however, being involved; it cannot exceed 90°.

Examining the results to date from actual observations, a general agreement is found with the hypothesis—the electric currents in the atmosphere not only follow the general direction prescribed, but have a magnetic field whose axis is actually found displaced from that of the earth through an angle of 32° to the west and south. If we examine into the various effects of the primary electric field, many of the phenomena disclosed by the observations in atmospheric electricity, *e. g.*, relation to barometric changes, give additional support to the theory above set forth.

The mechanical effect of the currents induced in the atmosphere will be to increase the velocity of the earth's rotation or more likely to cause a displacement of the earth's magnetic axis eastward. We thus have introduced one of the several systems which together cause the secular variation of the earth's magnetism. This subject, as well as the conversion of electrical into thermal energy and the possible meteorological consequences thereof cannot be entered into here. So likewise mere reference can be made to the subject of the possible vertical earth-air electric currents, which the theory discloses and which I have already partially investigated. Thus far only the effects of the primary circulation of the atmosphere have been considered; manifestly there will be other effects from the secondary motions of the atmosphere.

If the primary atmospheric electric field is brought about electro-dynamically as explained, then it is reasonable to suppose that any periodic or spasmodic fluctuation in the general motions of the atmosphere will result in a corresponding change in the electric field, which in turn may give rise to a variation observed in terrestrial magnetism and atmospheric electricity. Now Schuster had previously shown that the diurnal variation of the earth's magnetism corresponded precisely to what would result from Foucault currents electro-dynamically induced by the daily oscillatory movements of the atmosphere with reference to the earth's lines of magnetic force. The motion of the air currents it was necessary to suppose for the production of the electric currents corresponded precisely to that as indicated by the diurnal oscillation of the barometer. To explain the difference of the effects on the magnetic needle between summer and winter Schuster had to make the plausible hypothesis that the conductivity of the atmospheric regions was altered in the course of the year by the variation in the supply of the ionizing agencies coming from the sun. Schuster's currents are accordingly the diurnal variation of those of our primary atmospheric field on which we must depend, coupled with the earth's rotation, to furnish the actual supply of energy required to produce and maintain a magnetic storm.

The hypothesis above advanced for the formation of the primary atmospheric electric currents must, at present, be regarded as a tentative one. The precise determination of the characteristic features of this electric field cannot be attempted until the completion of the general magnetic survey of the globe. It ought to be pointed out, however, that the theory advanced to account for our magnetic disturbances is in no wise dependent upon the correctness of the hypothesis as to the origin of the primary electric field. While we do not know, at present, all we should like, we are sure of the existence of this field and that is all the theory requires.

In conclusion let me give you briefly another result obtained from our analysis of magnetic disturbances, viz., that as to the earth's magnetic permeability of which we have had thus far no knowledge. Taking the magnetic permeability of air as unity, it is found that for magnetizing forces on the order of 1/10,000 part of a C.G.S. unit, the earth's average magnetic permeability is about 2. The probabilities are that this value may be somewhat increased as the analysis progresses.

MAGELLANIC PREMIUM

FOUNDED IN 1786 BY JOHN HYACINTH DE MAGELLAN, OF LONDON

1910

THE AMERICAN PHILOSOPHICAL SOCIETY

HELD AT PHILADELPHIA, FOR PROMOTING USEFUL KNOWLEDGE

ANNOUNCES THAT IN

DECEMBER, 1910

IT WILL AWARD ITS

MAGELLANIC GOLD MEDAL

TO THE AUTHOR OF THE BEST DISCOVERY, OR MOST USEFUL INVENTION, RELATING TO NAVIGATION, ASTRONOMY, OR NATURAL PHILOSOPHY (MERE NATURAL HISTORY ONLY EXCEPTED) UNDER THE FOLLOWING CONDITIONS:

1. The candidate shall, on or before November 1, 1910, deliver free of postage or other charges, his discovery, invention or improvement, addressed to the President of the American Philosophical Society, No. 104 South Fifth Street, Philadelphia, U. S. A., and shall distinguish his performance by some motto, device, or other signature. With his discovery, invention, or improvement, he shall also send a sealed letter containing the same motto, device, or other signature, and subscribed with the real name and place of residence of the author.

2. Persons of any nation, sect or denomination whatever, shall be admitted as candidates for this premium.

3. No discovery, invention or improvement shall be entitled to this premium which hath been already published, or for which the author hath been publicly rewarded elsewhere.

4. The candidate shall communicate his discovery, invention or improvement, either in the English, French, German, or Latin language.

5. A full account of the crowned subject shall be published by the Society, as soon as may be after the adjudication, either in a separate publication, or in the next succeeding volume of their Transactions, or in both.

6. The premium shall consist of an oval plate of solid standard gold of the value of ten guineas, suitably inscribed, with the seal of the Society annexed to the medal by a ribbon.

All correspondence in relation hereto should be addressed

TO THE SECRETARIES OF THE

AMERICAN PHILOSOPHICAL SOCIETY

No. 104 SOUTH FIFTH STREET

PHILADELPHIA, U. S. A.

The American Philosophical Society

Announces that an Award of the

HENRY M. PHILLIPS PRIZE

will be made during the year 1912; essays for the same to be in the possession of the Society before the first day of January, 1912.

The subject upon which essays are to be furnished by competitors is:

The Treaty-making power of the United States and the methods of its enforcement as affecting the Police Powers of the States.

The essay shall contain not more than one hundred thousand words, excluding notes. Such notes, if any, should be kept separate as an Appendix.

The Prize for the crowned essay will be $2,000 lawful gold coin of the United States, to be paid as soon as may be after the award.

Competitors for the prize shall affix to their essays some motto or name (not the proper name of the author, however), and when the essay is forwarded to the Society it shall be accompanied by a sealed envelope, containing within, the proper name of the author and, on the outside thereof, the motto or name adopted for the essay.

Essays may be written in English, French, German, Dutch, Italian, Spanish or Latin; but, if in any language except English, must be accompanied by an English translation of the same.

No treatise or essay shall be entitled to compete for the prize that has been already published or printed, or for which the author has received already any prize, profit, or honor, of any nature whatsoever.

All essays must be typewritten on one side of the paper only.

The literary property of such essays shall be in their authors, subject to the right of the Society to publish the crowned essay in its Transactions or Proceedings.

The essays must be sent, addressed to

The President of the American Philosophical Society,

No. 104 South Fifth Street, Philadelphia, Penna., U. S. A.

4248

PROCEEDINGS

OF THE

American Philosophical Society

HELD AT PHILADELPHIA

FOR PROMOTING USEFUL KNOWLEDGE

VOL. XLIX. JULY, 1910. No. 195.

CONTENTS.

PHILADELPHIA

THE AMERICAN PHILOSOPHICAL SOCIETY

104 SOUTH FIFTH STREET

1910

Members who have not as yet sent their photographs to the Society will confer a favor by so doing; cabinet size preferred.

It is requested that all correspondence be addressed

To the Secretaries of the

AMERICAN PHILOSOPHICAL SOCIETY

104 South Fifth Street

Philadelphia, U. S. A.

PROCEEDINGS

OF THE

AMERICAN PHILOSOPHICAL SOCIETY

HELD AT PHILADELPHIA

FOR PROMOTING USEFUL KNOWLEDGE

Vol. XLIX | July, 1910 | No. 195

THE INFLUENCE OF MENTAL AND MUSCULAR WORK ON NUTRITIVE PROCESSES.

By PROFESSOR FRANCIS G. BENEDICT.

Director of the Nutrition Laboratory of the Carnegie Institution of Washington, Boston, Massachusetts.

(*Read February 4, 1910.*)

We often wonder at the marvels of modern surgery and the wonderful advances made in surgery during the past fifty years. At the same time we are often surprised by the fact that medicine as such has not made corresponding advances and that doctors are inclined now to give less medicine than ever before. In one particular, however, medicine has made wonderful strides, namely, in the so-called "preventive" medicine. In fact, it is a common newspaper joke that ere long we will be employing our doctors and paying them only during such a period of time as they keep us well and thus stimulate their efforts towards using preventive medicine to its fullest. Pasteur, Lord Lister and Koch are household names today and stand for wonderful series of painstaking researches which have developed the fact that many diseases formerly thought inevitable in the course of a lifetime can, by reasonable care, in a large number of cases be avoided.

In order to bring preventive medicine to the present successful stage, innumerable experiments, involving microscopy, chemistry and physiology, were necessary to demonstrate in just what manner these baneful organisms (for in many instances they have proved to be organisms) enter into our bodies and produce diseases. Modern hygiene is based in large part upon the bacteriological researches of these pioneers.

It is my privilege to explain to you how it is hoped to develop another line which, while it has by no means the attractive outlook presented by bacteriology and preventive medicine in general, may yet prove to be of the greatest value to mankind. I refer to investigations in the nutrition of man. If by proper study we can find what foods are best adapted to different purposes, if in what quantities they should best be ingested, what preliminary treatment is most desirable, we will have solved a great many problems regarding the diseases of digestion and will have made a large contribution to a wider branch of preventive medicine.

While the compound microscope can be used for studying the tissues, it requires a very different type of apparatus for studying the changes that take place in the whole body and the apparatus which, in our investigations, compares in a way to the compound microscope of the bacteriologist has the formidable name of respiration calorimeter. The microscope can reveal to us clearly what happens after the tissue is dead; it cannot, except in a few instances, give us a true picture of the processes which take place in the living body. These processes are extremely complex but we do know that as a result of food ingested, we obtain from the body heat, muscular work and mental work, and that there are certain excretions. During youth there is also a noticeable growth, while after the growth has been established, there is also repair of waste tissue. It is in studying these particular functions that we find it necessary to resort to a special apparatus—the respiration calorimeter.

We eat a great variety of foodstuffs and it is necessary for the body to so break down and rearrange the materials in these foods that the body can make the best use of them. For example, ordinary sugar cannot be used directly by the body but must first be broken down into dextrose and this dextrose is probably in part converted into another compound which is distributed through the muscles and particularly in the liver—a substance very closely allied chemically to sugar and called glycogen.

Like the frontiersman building his log house, the standing tree is of no use to him, but after the tree has been felled and the log hewn into the proper shape, then and then only can he begin to construct his house. But it is only during youth that the body is particularly engaged in building new tissue and the chief object of these rearrangements of the materials of the food is to so deposit the material in the various parts of the body that it can be used as fuel and be properly burned or oxidized. While, then, the food is first acted upon by the various digestive juices to prepare it for use by the body and as a result of digestive processes, the original material of the food is transformed to substances more or less closely resembling the components of the body, it is true, however, that when the body has completely and finally utilized these products, in general they are broken down to relatively simple compounds.

When coal or wood are fed to the boiler in the power house, heat is liberated and from the heat can be obtained power. In burning, the coal or wood is converted in large part into two simple products, carbon dioxide and water vapor. In order to convert the fuel into carbon dioxide and water, a large amount of oxygen gas is consumed and this, as you know, is obtained from the atmosphere.

We have frequently been told that the body is a good deal like a machine and consequently we can subject the body to tests very much like those given to a machine. For example, we can consider the food like coal—a fuel to the boiler in the boiler room—and the respiratory gases like the fuel gases leaving the chimney, and the feces and the urine like the ashes. With the boiler it is relatively simple to get a sample of the coal and analyze it and to take a sample of the ash and see what unburned portion of the material is present. It is somewhat more complex to take a sample of the flue gases and see how much unburned material passes up the chimney, but it is infinitely more difficult to make an experiment on a man. While we can analyze food, urine and feces, when we come to the respiratory gases, we must have a special respiration apparatus for the purpose.

Briefly, it is an air-tight copper-walled box, through which a ventilating current of air is passed. The air leaving the chamber contains carbon dioxide and water vapor produced by the man and is deficient in oxygen which has been taken out of it by the subject; this air is passed through purifiers where the carbon dioxide and water are removed and then the air is returned to the chamber to be breathed over again, but before entering the chamber, the deficiency in oxygen is made up by admitting pure oxygen out of a steel cylinder, such as is used by a physician at the bedside.

It is possible with an apparatus of this type, then, to determine how much carbon dioxide and water are produced in twenty-four hours and how much oxygen gas is absorbed. If, together with

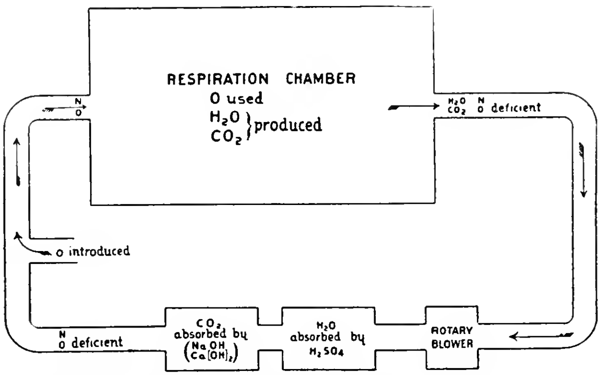


FIG. 1. Schematic outline of ventilation system in the respiration calorimeter at Wesleyan University, Middletown, Conn.

these data, we have the analyses of the urine and the feces, it gives us a very perfect picture of the transformations that have taken place inside the body during the period the subject has been inside the respiration chamber.

With the boiler, this is about as far as we can ordinarily go, as there is no satisfactory method for measuring the total amount of heat given off by the boiler in a large power house, but with a man it is possible to measure not only the products of combustion and the oxygen consumed but also to measure directly the amount of heat produced by the oxidation or combustion of the substances inside the body. This is the calorimetric feature of the apparatus. The air-tight copper box is surrounded by a number of cases which gives it a refrigerator type of construction and provision is made for the prevention of any loss of heat through the walls. There being no loss of heat, the man, acting as a small furnace, would soon produce enough heat to make him very uncomfortable. An ordinary man at rest gives off about as much heat as a 32 c.p.

electric lamp, 100 calories, so it is necessary for us to bring away the heat from the chamber as fast as it is liberated, or otherwise the man would become uncomfortably warm. As in the winter our houses are heated with hot water, this little house is cooled by cold water. Through a series of pipes inside the chamber is passed a current of cold water, then by noting the temperature at which the

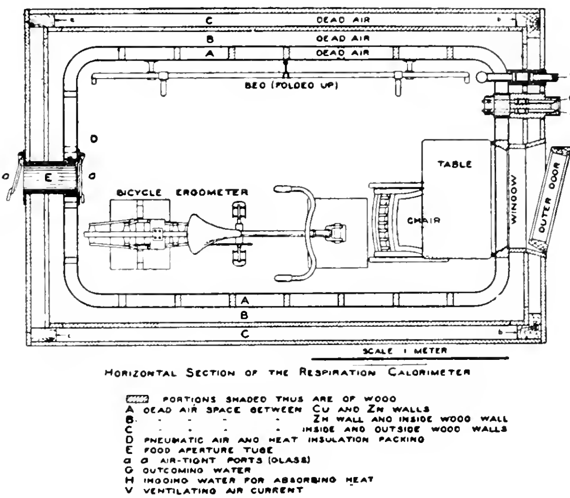


Fig. 2. Horizontal section of the respiration calorimeter showing heat insulation.

water enters and at which it leaves the chamber and the amount of water passing through, it is very easy to determine the exact amount of heat brought away in this manner. Thus this apparatus is well named a respiration calorimeter, "respiration" because it is an apparatus for measuring the respiratory processes and "calorimeter" because it measures the heat given off by the subject.

It is difficult for us to imagine that there may be heat or energy in a glass of ice cream soda or a piece of toast or a slice of ham and yet there have been one or two rather remarkable instances where some of our foodstuffs have been by force of dire necessity put to practical use for the production of heat. A few years ago the farmers of Kansas had an over-supply of corn and with the price of fuel as it was at that time, it became a common practice among the Kansas farmers to actually burn corn. Several years ago, a large wheat steamer became stalled in lake Huron and in working her way against the ice for several days, exhausted her fuel supply. As a matter of fact, she was brought into Detroit by burning the wheat in her bins under the boilers. One of the most striking instances that I ever heard of was one that occurred on a coastwise steamer bound from Boston to one of the Maine ports. This steamer in the latter part of the year found herself encountering a severe gale and shortly the coal supply was wholly exhausted. Recourse was had to the demolition of the interior woodwork of the vessel, stateroom partitions, mattresses, furniture, but as a matter of fact, the steamer was brought into Portland harbor by burning a large cargo of hams under her boilers.

As we commonly eat our food materials, for the most part they are too moist to burn, although when the different food materials are deprived of a greater part of their moisture by drying, they burn quite readily. In so burning, they are converted to carbon dioxide and water and a definite amount of heat is liberated during this combustion.

Whenever organic material is burned and completely oxidized to carbon dioxide and water, there is a definite amount of heat liberated for each gram burned. Fats liberate very much more heat than do equal weights of sugars or starches, and protein, the third important element in our food, liberates about the same amount of heat as do the carbohydrates. If, therefore, we determine as we can with a special apparatus, the heating value or fuel value of different ingredients in our diet and then measure the total amount of food ingested, measure the heat produced by man and make due allowance for the heat in the feces and the solid matter of the urine, it is possible for us to strike a complete balance of income and outgo of energy and see whether our measurements are in error or not. So, on the one hand, we have a heat balance obtained by determining the intake and fuel and the output of unburned material in the ash and the heat directly produced. On the other hand, we have from the chemical analyses of the respired air and the excreta a means of knowing just how much protein, how much fat, and how much carbohydrates have been burned in the body during a certain experiment. If our chemical analyses have been accurate, we should be able to compute from the fuel values of protein, fat and carbohydrate thus obtained the heat these substances would be expected to produce when burned in the body. Obviously, this result should compare with the results as actually determined. When our apparatus functionates so perfectly that we can secure results in this way, we can feel that we are on safe ground and our results give us a very perfect picture of what takes place in the body. Having found such an apparatus and method, it only remains for us to apply this apparatus in as many ways as time and expense will permit.

Among the numerous questions that can be solved by an apparatus of this type is the effect of work on changes in material in the body, which we call metabolism. We know that when a man is sitting quietly at rest in a chair, he gives off, say, 100 calories of heat per hour. This is about the amount of heat given off from a 32 c.p. electric lamp. If, however, a man stands up and walks about, we have found that his heat production rises considerably and we find, also, that not only does the heat elimination increase but the carbon dioxide in the breath increases correspondingly.

Muscular exercise with varying degrees of intensity produces varying amounts of carbon dioxide and heat as is shown by the table

TABLE I.

Average Normal Output of Carbon Dioxid and Heat from the Body.

Conditions of Muscular Activity.	Average Quantities per Hour. Carbon Dioxid. Gms.	Heat. Cals
Man at rest, sleeping	25	65
Man at rest, awake, sitting up	35	100
Man at light muscular exercise	55	170
Man at moderately active muscular exercise	100	290
Man at severe muscular exercise	150	450
Man at very severe muscular exercise	210	600

herewith. The classification of muscular exercise on this basis is very unsatisfactory, as different people may have different impressions of what is meant by "moderately active muscular exercise," for example. It is certain, however, that under these conditions, the characterization so far as the sleeping period is concerned is pretty well fixed and it is also true that man at very severe muscular exercise means practically the limit of human endurance.

Using the values in this table it is possible to roughly compute the average daily output of heat from a man at light muscular work or the heat from a man with different degrees of muscular work. Assuming, for example, that the man is at light muscular work, the computation is as follows as given in Table II herewith.

TABLE II.

AVERAGE DAILY OUTPUT OF HEAT OF A MAN AT LIGHT MUSCULAR WORK.

Daily Program.	Heat Output.
At rest, sleeping, 8 hours, 65 calories per hour	520
At rest, awake, sitting up, 6 hours, 100 cal. per hour	600
Light muscular exercise, 10 hours, 170 cal. per hour	1,700
Total output of heat, 24 hours	2,820

The computation of the average daily output of heat on this basis is at best somewhat unsatisfactory, as we do not know with sufficient accuracy the heat output during the various degrees of muscular activity. Nevertheless, it serves to give a general idea of the possibilities of using standard normal values for getting a rough approximation of the energy output of different men with different conditions of muscular activity.

We are all of us firmly of the opinion that when we are doing strenuous mental work, we are doing a great deal of work. We feel sometimes perfectly exhausted at the end of a severe mental task, and consequently it will be interesting to note to what degree mental work influences the interchange of material in the body. Does severe mental application call for a greater production of heat or a greater oxidation of the tissues? In order to test this question satisfactorily, it became necessary for us first to try to find out how to get men to work hard mentally for a period of several hours.

While at Wesleyan University, it occurred to me that it might be desirable for us to study the metabolism of students during the mid-year examination period, for if there is any time in which a college student performs mental work, it is during the examination period. Consequently it was arranged for students to take their examinations in the respiration calorimeter.[1] The chamber is large, fairly well lighted, quiet, with a good ventilation and every possible convenience for quiet, sustained mental effort without distractions. The men entered into the experiments heartily and twenty-two such experiments were made. In order to control the experiments and give us a basis for comparison, the same men were on subsequent days placed in the calorimeter for the same length of time, during which period no mental work was done and, indeed, we went so far as to attempt to eliminate the results of the muscular work of writing by giving them plain copying to do on paper in the control period. By this means we were able to compare the results of the experiments with mental work with the control experiment. During these mental work experiments, the men were encouraged to note down all their personal impressions. Some of the observations were very interesting. Several men said it began to grow cold and then to grow warm, while as a matter of fact, the temperature of the chamber did not vary by more than two or three hundredths of a degree. Other men noted that they perspired freely during the examination period and that they had been under a tremendous strain and effort.

TABLE III.

Metabolism During Mental Work.

(Quantities per hour.)

	Examination Period.	Control Period.
Carbon dioxide	33.4 grams	32.8 grams
Oxygen	27.3 grams	25.9 grams
Water vapor	39.2 grams	37.8 grams
Heat	98.8 calories	98.4 calories

Averages of 44 experiments.

But all of these personal impressions fall wholly out of consideration when we compare the results of the experiments with the students during the examination period and during the control period. There was practically no difference between the results with

[1] For a detailed discussion of these experiments see Benedict and Carpenter, "Muscular and Mental Work and Efficiency of the Body as a Machine," United States Department of Agriculture, Office of Experiment Stations, Bul. 208, 1909.

the men during the mental tests and during the control periods. This, to the layman, is certainly a most surprising outcome, for, as you know, it is the popular impression that a sustained mental effort results in a complete physical exhaustion. Here, then, is a problem that is pretty thoroughly solved by means of this large apparatus which had been constructed for experiments of exactly this kind.

I ought to add here, perhaps, that incidentally among the old legends that have been handed down to us and still find credence in public opinion is the belief that fish is an excellent brain food. This has long been wholly exploded. In fact, it has been easily traced to an old saying of Professor Moleschott,— "Ohne Phosphor kein Gedanke," and as fish is known to contain phosphorus, it was thought that fish was an excellent brain food for mental workers. As a matter of fact, there are no researches that show that there is any influencing of the chemical processes of the body as a result of mental exertion.

Of special interest, perhaps, from the standpoint of hygiene in these experiments is the fact that several of the men expressed a feeling of restraint and a lack of freedom to stretch themselves and possibly to move about somewhat as a result of staying in the chamber. Of course our experiments would have been seriously inconvenienced if the men had moved about freely, so that we asked them to diminish as much as possible all extraneous muscular exertion other than that required in using the pen. This longing for the use of the arms and body during mental exercise is simply another means of indicating that our bodies need as perfect circulation in all parts as possible, that when long-continued and cramped positions are maintained, there is a feeling of uneasiness and discomfort.

It is a most wise and growing custom for book users to vary the body position during sustained mental work. The increasing use of the standing desk while reading, and the pacing of the floor during mental work or dictation of addresses and literary productions testify to the fact that for best mental effort, there should be physical exercise, though this is far from affirming that the center rushes invariably become literary men. In our studying we should certainly pay more attention to physical exercise and fresh air, and the old picture of the bookworm with his feet on the top of the desk, the back hunched into an arm chair and the head enveloped in tobacco smoke can hardly be considered as portraying the ideal condition for creative work in the light of modern physiology and psychology.

Muscular Work.

While mental effort is without appreciable influence on metabolism, I now wish to call to your attention a most interesting series of experiments on the influence of severe muscular work, particularly such as bicycle riding, upon the transformations of the body. The narrow confines of our respiration chamber make it necessary for us to restrict our experiments to those forms of muscular activity that do not require a large amount of apparatus or large space. A form of stationary bicycle called a bicycle ergometer is ideal for such experiments. We placed inside the respiration chamber a stationary bicycle, the rear wheel of which was replaced by a large copper disk rotating in a magnetic field. With this apparatus it was possible to put an electric brake, so to speak, on the copper disk and make it more or less difficult to drive the pedals Without going into details, I will simply say that it is possible to calibrate this apparatus and to know that every revolution of the pedals with varying strengths of the electric brake results in the transformation of a definite amount of energy into heat, consequently when a man rides, we have only to count the number of revolutions of the pedals to know how much heat has been transferred from the muscles of his body to the pedals in the form of effective work. This apparatus, together with the respiration chamber, has given us a means of studying man as a machine in a way that I think has never been done before.

One of the important problems of interest to the engineer is the efficiency of his engine. How much energy of the coal can a power plant convert into effective mechanical work? Obviously, the best power plant is that which produces the best mechanical work out of one ton of coal. With man, we also have a machine. The fuel in the boiler is again the food and the muscles are the levers by which the energy is transferred from the muscles to some effective apparatus so that it can be used as external work. This apparatus, the bicycle ergometer, is admirably adapted for the use of the powerful leg muscles and hence we were able to study this to a considerable degree.

The experiments were made something in this way. The subject was first placed in the respiration chamber for part of a day and the heat production measured during rest, while he was sitting quietly reading a book. This is our "base line," as the surveyor says, or our basis for comparison. On subsequent days similar

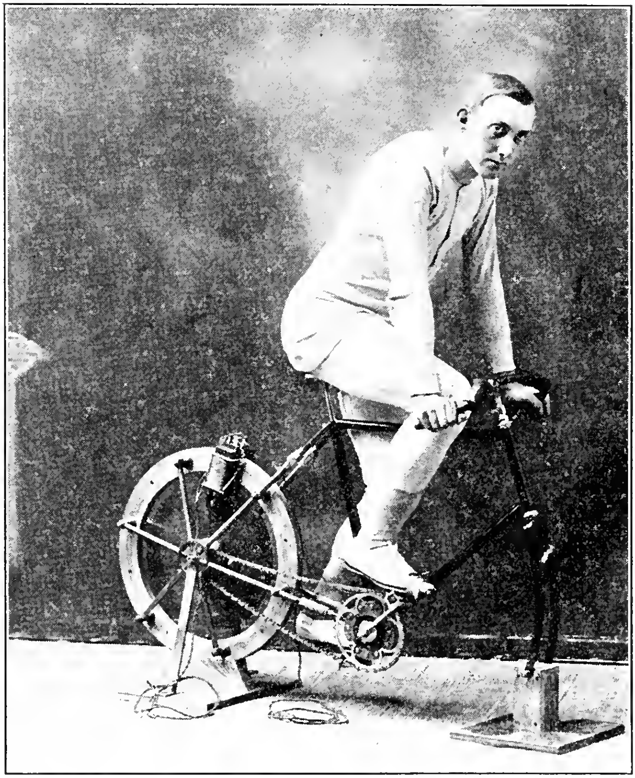


Fig. 3. Bicycle ergometer for studying problems relating to muscular work.

experiments are made in which the man rides the bicycle ergometer with varying resistance of the electric brake. During these experiments the heat production and the carbon dioxide eliminated are measured directly and also the amount of work in terms of calories put on to the bicycle accurately determined by noticing the number of revolutions of the pedals. We thus have a determination of the total heat production and the heat of mechanical work which we can compare with the total heat production and thus find the efficiency. The degree of resistance of the electric brake varied from mere coasting when no resistance is applied but the legs were made to rotate as in coasting down a hill without a coaster brake, to such a high resistance that the wheel would hardly be carried by the dead point. The subjects used for these experiments varied. Two of them, coming out of the wild woods of Maine, had never seen a bicycle. Others were college athletes and were on the college track team, and finally one of the most interesting experiments was made with one of America's professional bicyclists, Mr. Nat Butler of Cambridge.

In Table IV, I have summarized a number of experiments with these different subjects.

TABLE IV.

Experiments on Bicycle Riders.

(Calories per Hour.)

	Resting.	Working.[2]	Work Done.	Efficiency, Per Cent.
J. C. W.	112	339	49	21.6
B. F. D.	106	318	45	21.2
A. L. L.	105	326	46	20.8
E. F. S.	117	399	51	18.1
N. B.	92	619	112	21.3
		471	79	20.8
		401	65	21.0
		382	60	20.7

In the first column under the title "resting" is recorded the number of calories produced per hour by these different men when sitting quietly reading in the respiration calorimeter. These experiments are almost identical in their nature with the control experiments in the mental work experiments and are intended to show the heat requirement for maintenance during rest. These men gave off, as you see, not far from 100 calories per hour, which again is about the heat production of a 32 c.p. lamp. As they were

[2] Including heat produced by the ergometer.

all of athletic build, the heat production is somewhat larger than would be expected from men in sedentary occupations and you will remember in the mental tests, the heat production per hour averaged 99 calories. The average here is a little higher and is readily accounted for by the fact that the men were muscular. In the second column is recorded the heat production per hour during the period when the subjects were engaged in severe muscular work on the bicycle ergometer. Here the differences between the different subjects are very great. The lowest heat production was in the case of B.F.D. 318 calories, and the highest nearly twice as great, namely, 619 calories by N.B. While at first sight, it would appear that there was a very marked difference in the work or efficiency from these figures given in this column, it is necessary for us first to find out how much of this enormous heat production was actually converted into mechanical work, and we find that in the third column there were very large differences in the amounts of work accomplished by these men. These differences can be explained on two grounds, first, the rate of revolution of the pedals was different, and second, the degree of resistance in the electric brake was very different. It is interesting to note that the first three men during work gave off about 330 calories per hour and accomplished about 46 to 47 calories of effective work, thereby showing very little difference in either the total heat production or in the work done. The fourth man, E.F.S., gave off considerably more heat during the working period and also accomplished more work; and then we come to the astounding results of the experiment with Mr. Butler in which he gave off 619 calories and accomplished the enormous amount of 112 calories of muscular work. The remaining experiments with Mr. Butler were made with the idea of decreasing the muscular work and noting the effect on the heat production. You can see in a general way that when the total heat falls off, the work done falls off.

Perhaps of greatest interest is the comparison of these different men with regard to their efficiency. There are a number of methods whereby the efficiency can be computed from the figures given in the table. Theoretically we can say that in the first instance for every 49 calories of heat produced, there were required 339 calories of total energy. This would give us an efficiency of not far from 14 per cent. On the other hand, another method of computing the efficiency, and perhaps one that is fairer, is the method in which the total resting metabolism is deducted from the total heat production and we compare the work done with the increased amount of heat required to do this work. Thus, in the first experiment out of 339 calories produced during work, 112 calories can reasonably be assigned to the production necessary for maintenance while the subject is sitting quietly reading in the calorimeter. Deducting the 112 from 339, we find that 227 calories of heat result in the production of 49 calories of efficient work. On this basis, the efficiency is 21.6 per cent. A similar calculation for the other subjects shows the remarkable fact that there are but slight differences between these men as regards their efficiency. In other words, for every calorie of efficient muscular work done, there are about 5 calories of extra heat produced. In the case of the enormous heat production of Mr. Butler, we find that the differences in the two methods of computation are not as noticeable for it makes but a small change in the percentage whether we divide 112 multiplied by 100 by 619 or whether we divide 112 times 100 by 527. In the latter case, the efficiency is 21.3 per cent., while in the first it is about 18 per cent. Obviously, the greater the amount of work accomplished per hour, the less the influence of deducting the resting metabolism.

How does a man compare with an engine as regards efficiency? I have no doubt but that some fault could be found with my reasoning when attempting to compare a man with a machine but the best steam engines of the present day do not average an efficiency much greater than 14 per cent. Certain internal combustion motors realize somewhat more. At any rate, we can say that a man is a wonderfully efficient machine.

The figures in Table IV shed a most interesting light on the question of training. It has commonly been supposed that when a person is trained, the muscles become more effective and consequently there is a greater production of work for the same expenditure. In these figures here we find that in the first place the two men, A.L.L. and E.F.S., who were wholly untrained, and indeed wholly unfamiliar with the bicycle, accomplished as much work as did the college athletes, J.C.W. and B.F.D., and, indeed, with an efficiency very little less than that of the first two. When we come to the figures of Mr. Butler, we find that he was able by virtue of his skill and strength to accomplish a very great deal *more* work than any of the other men, but as a matter of fact, his *efficiency* was not materially greater than that of the college athletes, or indeed, the untrained men. So far as these two untrained men are concerned, their training consisted in riding the bicycle ergometer one half hour the first day and a half-hour increase for each succeeding day for six days prior to the test. Thus on the sixth day they rode three hours at one stretch and the total training occupied but 10.5 hours. This was done in an attempt to accustom the leg muscles to the exercise before the experiment began, so that they would not be sore when used in the subsequent experiment, so while they did have a small amount of training, it was far from the ordinary training a college athlete would pass through in preparing for an intercollegiate contest. It is obvious that Mr. Butler was able to accomplish an enormous amount of work by virtue of his long experience and well-developed musculature, but it is indeed astounding that his muscles had no greater efficiency than did those of much less trained college athletes.

It is very clear from these experiments that in order to produce muscular work, there must be a very large by-production of energy in the body. When we consider that a man at rest, sitting quietly, requires 92 calories for his maintenance, and when we know, as we do from other experiments, that the same man asleep would require not far from 60 calories, we see that in Mr. Butler's case during a severe muscular work period, there was a heat production in his body amounting actually to 10 times that when he was asleep.

In order to produce this heat in the body there must have been combustion, vigorous combustion, combustion either of body substance, in case the subject did not have food enough, or combustion of food material previously eaten. We have found as a result of a large number of experiments that a man at rest, doing no visible external muscular work, requires not far from 2,000 calories for maintenance during twenty-four hours. You can see that in three hours Mr. Butler produced nearly this amount when at severe muscular work; consequently he must have burned up in these three hours as much material as would ordinarily be burned by a subject at rest in twenty-four hours. On this same basis, he would need three meals every three hours or one square meal an hour.

I think figures like these go to show most conclusively that the proposition we hear frequently made to cut down our total food consumption is founded on entirely erroneous grounds. If we do muscular work, we must burn up material. We must draw on body substance or we must supply food. I have no doubt that many of us are too fat, not grossly over-weight, but somewhat over. People of sedentary habits have a tendency to take on weight and become 25 or 30 pounds over-weight without much difficulty, but I contend that the average man is not too fat and that his diet is not too large for him. People say continually, "We eat too much." Doubtless at times we do eat too much. I have no doubt that every one of us remembers the feeling of satiety with which we rose from our last Christmas dinner. Doubtless this feeling of satiety was carried farther than health, and in some cases, good breeding would justify, but Christmas dinners are not an everyday occurrence and it is not logical to say that because we over-eat on one day, that we continually over-eat.

It is the custom of athletes in general to consume large amounts of proteid food, particularly beef, eggs and milk. There is a tradition which has been handed down for many years that this diet is best suited for athletes, and that a large quantity of protein is advisable for a high efficiency and large and sudden drafts on strength. It is beginning to be questioned whether or no this after all is the logical diet for training. I have not time to go into this discussion, but there are at present some very strong advocates of a low protein diet, both for ordinary life and for athletic training and while in my mind their case is far from proven, some of the arguments are certainly most striking in their effectiveness, showing that our good friends, the vegetarians, have developed a remarkable degree of endurance when compared with their meat-eating competitors. All of these problems will gradually be worked out and on a sound scientific basis, but they have nothing to do with the general fact that when muscular work is accomplished, food must be supplied, and we cannot cut down our diet without losing flesh and simultaneously cutting down our muscular work. A great many men who consider themselves as living a sedentary life, actually, accomplish much more muscular work than they realize. For example, I have made an experiment a good many times by carrying a pedometer. During one winter when I was writing a large report and my exercise was confined to walking to and from the laboratory, two blocks from my house, and going about among the workers in the laboratory, I thought I was doing a very small amount of work, having almost no exercise, and yet I found to my astonishment that I recorded almost unerringly every day a distance walked of about seven miles. Obviously during that winter, since I held my body weight, my food must have been instinctively so adjusted as to exactly supply the demands required for the winter's work. When we consider that the intake of food is determined in a large measure by appetite and the feeling of satiety and the consumption of the food is dependent on the muscular activity, I do not know any factor regarding the body functions that is any more delicately adjusted than is the balance between the intake of food and the food requirement.

I think, then, we can take it as thoroughly settled that for the large majority, if the appetite is ordinarily followed, it will result in a most perfect adjustment of the food intake to the food requirement. Obviously it is important that we select foods that agree with us; excessive amounts of sweets and foods difficult of digestion are certainly to be avoided, but whether we follow the no-breakfast or the no-dinner or the no-supper plan, it is absolutely certain that in the course of twenty-four hours, or perhaps in the course of one week, what we lost in the meal voluntarily given up is compensated by increased consumption at the other meals. This I know is contrary to personal impression but we must, if we wish to make accurate observations on our diet, wholly eliminate our personal impressions. Nothing but the scales and an accurate table of analyses of food materials will give us results of any value. My personal belief is that instead of giving up one or two meals a day, it would be better for us to eat more often and less in amount. Particularly is this the case with those who are desiring to reduce the body weight. It is sometimes amusing to think of people who are going through all kinds of muscular exercise to work off their fat and at the same time these muscular exercises are stimulating a most voracious appetite which must be appeased. In my judgment, instead of decreasing the consumption of food to reduce the fat of the body, it is better to produce a feeling of satiety and thus cut down the ravenous appetite. This can, I believe, best be done by more frequent small meals than by leaving out any one meal and eating to complete satiety at the others.

I have outlined to you very hastily this evening a few of the important problems that we are able to study by means of the respiration calorimeter. These researches that I have thus far outlined were carried out in the chemical laboratory of Wesleyan University and it may be of interest to you to know that the researches were deemed so important that a special laboratory has been constructed for their prosecution in Boston—the Nutrition Laboratory of the Carnegie Institution of Washington—where it is hoped to study more accurately the questions relating to health and, indeed, to disease. The laboratory is located near the Harvard Medical School and in the vicinity of a large number of hospitals which are either built or are planned. In this way we hope to be able to get into most intimate touch with pathological material.

At Wesleyan University only one calorimeter was used for all the different kinds of experiments, but in the new laboratory three calorimeters are already completed and two others projected, thus providing an apparatus which will be particularly adapted to the kind of experiment planned. In the chair calorimeter a large number of experiments have already been made with diabetic subjects, and the bed calorimeter has been used for studying bed-ridden patients, not only for those having diabetes but a most interesting series of observations have also been made on women before and after confinement. The laboratory is also thoroughly equipped for studying all the excreta and many problems involving muscular work.

THE TUNNEL CONSTRUCTION OF THE HUDSON AND MANHATTAN RAILROAD COMPANY.

By JOHN VIPOND DAVIES.

(*Read February 18, 1910.*)

The tendency of population in the civilized world, as it increases with advancing years, is toward congregation into great cities instead of a uniform distribution, and this, as is readily understood, is due to the tremendous concentration of manufacturing industries and commerce and the interrelated business brought about and made possible by improvement in the world's transportation facilities.

While the tremendous growth of modern cities is due to transportation facilities, yet the very increase in population and area has created local transportation problems, within the cities, which were not dreamed of only a century ago, even in the great cities of the old world. This increase in the size of the old cities could not be foreseen; so that, instead of their growing in accordance with a preconceived plan, having ample provision for arteries of communication, we have, unfortunately, development without a plan, the newer portions spreading over the surrounding area, merging intervening villages and towns, and creating problems which can only be solved at great expense. Even in the cities of the newer countries, this growth could not have been foreseen or adequate provision made for it.

As an indication of how little this tendency of population to concentrate in the cities could be foreseen, we have only to consult the United States Census to find that at the first census, in 1790, three-tenths per cent. of the population were living in six cities of eight thousand population or over, while in 1900 thirty-three and one-tenth per cent., or about one third of the population, were living in five hundred and forty-five such cities, and that the population of greater New York is now equal to that of the whole of the United States in 1790. During this period the population of the United

States has increased over nineteen times, while that of the area now comprising New York City has increased seventy times.

New York is the heart and center of the second largest aggregation of people in the world, and differing from London, which ranks first, has reached its present position during a comparatively short life. New York was not laid out as a complete city, but has developed by the fusion of scores of villages. Manhattan Island, when it became populated north of Greenwich village, and was mapped on the present plan, was so laid out because at that time it was contemplated that the North and East Rivers, being the main arteries of traffic, needed the greatest number of thoroughfares with the closest intervals, to connect the water fronts. Consequently, the cross-town streets are spaced close together (about 260 foot centers) while the north and south avenues are widely separated. The growth of New York far to the North and East, beyond the East River, has entirely altered the traffic conditions and has produced one of the most difficult conditions in the solution of the present problem. The municipal limits of the Greater City of New York includes, by no means, all of the metropolitan district, which comprises all the suburbs tributary to New York as a place of business, all of which is essentially part of the aggregation of population to be dealt with in the traffic problem. The estimated population of New York City on December 31, 1909, was 4,516,000. The suburbs on Long Island and Westchester County represent 325,000, while the suburban district in New Jersey represents 1,691,000, making a total of 6,527,000 persons.

The last report of the Public Service Commission returns the total passenger transportation in Greater New York in 1908, on the various subway, elevated and surface lines, omitting transfers, as 1,358,000,000 rides. The service in the New Jersey district was 284,000,000 rides. In that year the steam railroads hauled within the same district over 100,000,000 persons, so that the total passenger rides per annum in the city and vicinity of New York amounts to the fabulous total of 1,742,000,000, an amount equal to one ride per annum for each person in the world.

In the Borough of Manhattan this traffic represents over 400 rides per head of population per annum, while in the entire city the movement has grown, by steady progression, from 164 rides per head of population per annum in 1884, to 300 rides in 1908. The advance in this growth has been marked as each improvement in methods of transportation has been introduced, particularly when electric traction succeeded steam, horses and cable, and again when the subways were opened.

Prior to the opening of the various bridges and tunnels, the ferries in New York Harbor carried 208,000,000 persons per annum, of which amount some 120,000,000 crossed the Hudson River.

No other excuse or explanation than these figures is needed for the construction of the various tunnels under the North and East Rivers.

In the cities the main arteries are unduly congested, and many of them are inadequate to provide for the various classes of transportation imposed upon the surface. In some cases the capacity has been increased by construction of elevated railroads, which, while convenient for the passengers and cheap to construct, are a serious impediment to the full use of the surface, are eyesores to behold, and are a nuisance to the health and nerves of the general public. The use of the sub-surface then, is essential to the development of the increased facilities for rapid transit, where the railroad can be out of sight and its operation out of hearing; where it is not affected by climatic conditions, and where the maintenance costs of structure and equipment is minimized.

In our cities the development of subways near the surface, while convenient for public use, introduces very serious questions in relation to sanitation. The first subway built can be carried out by extensive diversion and reconstruction of the sewerage system, but a point must soon be reached where the entire sewerage system must be considered as preëmpting a definite horizontal section upon which no subways may encroach. After that, subways can be laid out with hardly any limit, at greater depths and to almost any nummer, tier upon tier, as necessity demands, in the solid rock foundation underlying our streets.

We are rapidly getting to this point in New York, as is illustrated at Sixth Avenue and Thirty-third Street, New York, where, in addition to the elevated and surface lines, provision is made for a

Broadway Subway near the surface, below that the Hudson and Manhattan Tunnel, below that again the Pennsylvania Tunnels, and for a cross-town line at a level below these.

The Hudson and Manhattan Tunnels are the direct lineal descendants of the original Hudson River Tunnels commenced in 1873. They are developed from the traffic conditions outlined above.

The original undertaking, if it could have been carried to completion at that time, would, unquestionably, have proved unsuccessful in operation. It was then contemplated to build this tunnel from Fifteenth Street, Jersey City, about midway between the Erie and Lackawanna Railroads to a union station at Washington Square, New York, and to transport therein the steam engines and trains of all the railroads terminating on the westerly shore of this river. The method of construction adopted by Colonel Haskins, while feasible for working in the river silt, would never have been adequate to complete the work, and it is only in the past decade that the art of engineering has reached the point of providing the methods for construction and of operation suitable to the completion of this work and to meet the demands of the travelling public, within the scope of modern operation.

In the subject matter following, describing the methods of tunnel construction, the work of the Hudson and Manhattan Railroad tunnels is directly considered, but except in size of structure, and as providing for totally different types of equipment and operation, the matter is in every respect applicable to the work on the Pennsylvania Railroad tunnels, and it may be noted that the ratios of tunnel sizes to weight of equipment are almost the same in the two propositions. The Hudson and Manhattan system comprises four complete tube tunnels under the Hudson River, a belt line connecting the principal steam railroads terminating in Jersey City and Hoboken, and two terminals in New York City—one up-town and one downtown. It is essentially a distributing and collecting terminal in New York for the transportation lines in New Jersey; and a distributing and collecting agent in New Jersey for the people of New York. Its building has involved more varied character of construction than any underground project ever executed, and certainly represents every known type of tunnel construction. The most spectacular portion of the work, but by no means necessarily the most difficult, is the tunnelling under the rivers.

The Hudson Valley is a deep and narrow gorge, having on the west bank the red sandstone formation of the Newark series, and on the east side the micaceous gneiss of New York. For the most part, this valley—the floor of which is some 250 feet below sea level—is filled with silt. This material at the depth of the tunnel sections is a firm substantial clay. Its specific gravity, wet, is about 1.65, dry, about 2.50; weight per cubic yard, wet, about 103 pounds, dry about 156 pounds. Its chemical analysis is as follows:

	Per Cent.
Water	29.50
Silica	47.52
Iron oxide	2.69
Alumina	11.66
Lime	1.88
Magnesia	1.18
Soda	1.80
Potash	1.82
Chlorine	0.30
Sulphuric anhydride	0.11
Carbon dioxide	0.88
Phosphoric acid	0.13
Organic matter	0.53
	100.00

It will flow under pressure, but will stand very considerable pressure. It is impervious to water, though water will convert it rapidly into a demoralized condition. It is ideal material to tunnel in under modern shield methods.

In stating that the work involved all types of tunnel construction, it is to be understood that portions of the work were in solid rock—in certain locations Newark sandstone, and in others gneiss, and later—in part below the waters of the Hudson River, where at times, alongside the American Line Pier, there was a shell of only five feet of rock between the tunnel roof and the river bed—portions in sand and gravel saturated with water, portions in quicksand also saturated, portions in made or filled ground—the most troublesome material to work in—portions in river silt, where, theoretically, there should have existed twelve feet of silt cover and where at times there was actually none at all until clay was dumped to make an artificial cover, and that finally the work was carried on in every combination of these materials. Below the Hudson River, where the deep channel of 65 feet of water flowed, the tubes on both the up-town and down-town systems passed from silt to full rock and from full rock to full sand; consequently the sections at every point continually changed.

The essential factors in all tunnel construction may be summed up as follows: (1) The removal and disposition of the material; (2) the support of roof and sides during construction; (3) the elimination or disposition of water entering the work; (4) the provision of a safe place for workmen engaged on construction; (5) the construction of a permanent lining and waterproofing it.

Carlyle, after disserting on the weakness of man, writes: "Nevertheless he can use tools, can devise tools. . . . Nowhere do we find him without tools, without tools he is nothing, with tools he is all."

In no line of engineering work are the tools more essential than in the modern art of tunnelling. In every type of structure and in every material the proper and efficient tools are the prime factor in the advanced new art of this work.

In rock tunnels the combination of the modern compressed air operated rock drill with fumeless high explosives; in soft ground the use of compressed air, mechanical haulage, electric light and power, steel temporary false works and reinforced concrete or iron plate lining; and for subaqueous work the use of a shield and other mechanical appliances, has to-day converted into an essentially mechanical process what at one time was replenished by brute force. The power plant needed for carrying out such a piece of construction is of tremendous extent. For the Hudson and Manhattan tunnels, for example, and exclusive of the Sixth Avenue subway, the combined power plants aggregate some 13,500 horse power boiler capacity, operating high and low pressure air compressors, electric generators, and hydraulic pumps delivering water to the shields at a pressure of 5,000 pounds per square inch, as well as saw mills and machine shops. Air pressure was maintained continuously from these plants with no break or interruption for support of some part or other of the work under construction, from September, 1902, to June 14, 1909, and during the construction period over 250,000 tons of coal was consumed, while the value of the plant exceeded $750,000.

The plants installed for the Pennsylvania Railroad Tunnel construction were much larger and more valuable than those of the Hudson Company.

The modern method of shield tunnelling is an evolution, and except in the mechanical details and design, is in no respect new.

Referring to the before mentioned essential factors in tunnel construction, the conditions are met by provisions of the tools or means of construction as follows:

1. For the support of the soil, and the elimination of water, and to provide for the safety of the workmen, we carry out the work under air pressure.

2. For the support of the soil, to provide safety for men, for economical and rapid construction, and in certain cases also for the removal of the soil, we install a *hydraulic shield.*

3. For the permanent lining we require material strong enough to stand the external pressures as soon as erected, and therefore a metal *plate lining* is requisite.

4. For putting in place this permanent lining there is installed, either attached to the shield or operating independently, an *erector.*

5. For water-proofing and protecting from corrosion the iron lining, where the same is erected in sand, or gravel, or partially in rock, *cement grout* is pumped into the rear of the lining by a grouting machine.

The use of air pressure was a very early invention. About 1830 Admiral Cochrane, afterwards Lord Dundonald, took out letters patent for use of air pressure applied to caisson or tunnel construction.

The diving bell had been in use at that time quite extensively for laying foundations under water and for carrying on other engineering works. This represents the simplest form of application of compressed air due directly to the water depth, but obviously without the renewal and replenishment of the air. The greatest possible capacity of bell could only, by any possibility, enable workmen to continue work therein for a very short period, and to renew the air the bell had to be brought to the surface. The admiral conceived the idea that if air were pumped into a working chamber it could be applied at a pressure equal to the full head and thereby exclude water; and, going further still, that if such a chamber were connected with the surface or outer air by a tube or other connection and fitted with large valves, it would be feasible for the workmen to freely pass into and out of the working chamber by simple operation of the valves. This simple method he considered as equally applicable to a caisson or tunnel and for the purpose of excluding water. This is all the air lock is today—a large receptacle of sufficient size for cars, buckets or men, fitted with flap doors, one at each end, and opening the same way, that is inwards, against the higher pressure, and connecting the working chamber with the outer air at atmospheric pressure. It is immaterial whether this air lock is fitted vertical, as is usual in caisson work, or horizontal, as commonly applicable to tunnels, the essential condition being that the lock be built into a bulk-head or diaphragm enabling a pressure of air higher than normal atmosphere to be maintained inside the working chamber, which, in tunnel construction, is usually a length of completed tunnel, while in caisson work it is the lower portion, between the air floor or diaphragm and the cutting edge. The "modus operandi" is extremely simple. To enter air pressure, the person enters through the open outer door or valve and closes it behind him. He is then, so to speak, in a room with both doors shut and at atmospheric pressure. He then admits air under pressure until the pressure within the room is equal to that of the working chamber inside, when, obviously, the inner door can be opened and the passage effected. The use of double and triple locks allowing varying stages of pressure are usual in tunnel work, when high pressures of over twenty pounds are used, as increasing the personal safety of men and expediting the operation in the decompression. The honest observance of rigid rules of health makes the the risks to workmen by air pressure almost nil. These are: (1) Selection of men, limiting the age of men who have not been in the habit of working under pressure to, say, not over thirty years; and careful medical examination of every employee for physical and organic condition, and the exclusion of highly strung nervous subjects; (2) the honest coöperation of employer with the medical officer, so that no man is employed who has not been passed as sound; (3) the rule that no man should work when sick; (4) the rule that, while the time interval for compression should be reasonably slow, the all-important thing is to limit the time rate for decompression. This is the reason for the use of varying stages of pressure.

In the tunnel work by the Hudson companies, from 1902 to 1907, there were 40,000 men examined for work under pressure up to 42 pounds above normal. In that time, in the tunnel work (not including caisson construction) only three men lost their lives owing to caisson disease. Work in caisson is in this respect more hazardous than in tunnel, even at the same pressures, owing largely to the small capacity possible within the working chamber and the higher temperatures under which work has to be carried on.

It was before mentioned that the original idea of using air pressure was simply to exclude water. Later, when about 1869 Colonel Haskins conceived the idea of our Hudson tunnels, he also evolved the idea that air pressure balancing the external earth pressure would retain it, neglecting the fact that the elastic properties of air allow it to take any form, and that consequently rigid support to maintain the shape of the chamber is essential to the application of air pressure for balancing earth pressures. This oversight was the direct cause of a great disaster which occurred in the early days of the Hudson River Tunnel. The invention of the "shield" by Sir Marc Isambard Brunel, in 1818, marks the most interesting step in the art. He had been asked if it was possible to build a tunnel under the Neva at St. Petersburg, and working out a scheme, took pattern from nature. The *Torcdo navalis* destroys wooden piles by boring holes through them just at the mud line. The worm attaches itself to the wood and then bores the hole as it moves forward, lining its hole with a calcareous shell and discharging the borings through its body. Its body also closes the hole against water coming in; for if the worm penetrates the wood, or admits water, it dies, and obviously, therefore, it is carrying on its boring operations under air pressure equal to the water head.

Brunel's shield comprised a cutting edge and front shell to form the tunnel and hold the sides and roof; the front, or breast, being held by shutters adjustable to the work as excavated. His shield was to be advanced by screw jacks. The modern shield is a great piece of machinery, but in principle is but little different from that of Brunel. The shield is particularly adapted to the construction of a circular tunnel; though roofing shields, simply to support the roof while lining an arch, are not at all uncommon.

Any other form than the circle would be very difficult to use, owing to the trick a shield has of rotating as it advances. The shield structure consists of a drumshell having an internal diameter slightly larger than the external diameter of the finished lining. The front end is shod with very heavy steel castings to form a sharp V-shaped cutting edge protecting and stiffening the shell. The shell is built up of rolled steel plates in two- or three-ply thickness, with all rivets on outside and inside countersunk smooth and no projecting butt straps at joints. This construction gives a smooth external surface to slide through the soil and a smooth flush internal surface in the tail section. The central section of this drum shell is strengthened by a massive girder construction, which forms the main structure of the shield to withstand the pressures and strains. The rear of this girder construction is plated solid and mud-tight, except for doors in each pocket section which can be opened at the control of the workmen, either to admit materials or allow passage for men. This solid plate of the shield is known as the diaphragm. The depth of the girder construction is regulated largely by the length of the hydraulic jacks, which project through the diaphragm and are heeled as near the cutting edge as possible. To provide for the jacks there is constructed within the outer drum shell an annular space, stiffened between each jack by heavy girders connecting the outer shell to the internal jack space shell. The portion of the shield projecting in the rear of the diaphragm, consisting of only a portion of the drum shell (finished flush inside and outside) is known as the "tail." This only serves the function of providing a safe place in the shelter of which to erect the permanent lining, and slides along overlapping the tunnel tube as construction advances.

If the shield is of large size, there are fitted in front sliding tables, advanced in front of the cutting edge of the shield by hydraulic power, for the double purpose of forming platform for men to work upon while drilling, excavating or breasting the upper face, or to shelter the men working below while doing the similar bottom work, and also to push forward against the timbers used to breast up the face, and to hold them in position during the operation.

The machinery with which the shields are equipped consists of numbers of hydraulic jacks fitted at close intervals around the periphery, the jack cases heeled against and pushing close to the cutting edge by admission of water pressure to the jacks, forcing forward the entire structure of the shield by reaction of the rams against the last erected ring of permanent plate lining. The jacks are controlled by an arrangement of valves, so devised that any jack can be operated singly, or any group of jacks can be operated together by one man, from a convenient platform. By applying pressure to either quarter of the shield, the proper direction is given to advance the shield. The doors are very simple, and the design best adapted depends partly on the character of soil in which the shield is being used. In our work nearly every type of door has been fitted, but the simplest and best adapted to the usual condition of use and emergency is the loose slat. A shield is, needless to say, a very massive piece of construction, as the strains put upon it are enormous. One of the Hudson and Manhattan shields complete weighs about 67 net tons, while a Pennsylvania North River shield weighs about 195 net tons. Presuming that work is being carried out in silt under the Hudson River with pneumatic pressure of 30 pounds per square inch and with all the shield doors closed tightly, the shield is pushed through the clay as though one pushed a stick into a heap of dirt. In that case, with sixteen jacks worked under 5,000 pounds hydraulic pressure, there is a total force of 2,500 tons. This, on the Hudson and Manhattan shields, represents a pressure on the soil equivalent to eleven tons per square foot. It is not often that such a pressure as this is necessary to produce penetration with consequent flow and displacement in silt, nor could it be used in any stiffer material without probable damage to the shield. Sand or gravel, which will not flow, must be excavated or removed, and removed in advance of the forcing forward of the shield, or injury will be done to the structure. In Hudson River silt the process is very rapid, the complete cycle of operation involved in pushing this shield, cleaning up the silt which leaks through the door joints, placing and erecting a complete ring of plates for lining, bolting up and being ready for the next cycle, requiring only thirty-seven minutes, and in this way as much as seventy-two feet of finished tunnel has been erected in a single day.

In the design of a shield there have been numerous so-called refinements introduced, some patented, some tested and discarded; but in our own experience, the greater simplicity the greater service, and any addition to such a machine which is not necessary it is better without. For example, working in sand or gravel, or when the face consists in part of rock and in part of silt or sand, the entire face of soil has to be excavated, and as this is done the breast must be retained from caving or falling in. This is usually done by skilled men placing boards, well and properly braced by struts, through the shield doors back to the completed tunnel, so that they are independent of the movement of the shield when it is next shoved ahead. The addition to the shield of an elaborate mechanically operated system of shuttering, like a glorified edition of Brunel's idea, is no advantage in time nor labor, but enormously increased cost to construct and additional trouble to maintain.

The most difficult combination to deal with in shield tunnelling is a partial face of rock overlaid by silt or wet sand. This construction was first met with and successfully overcome in the East River Gas Tunnel in New York. In this case the soft ground overhead must be excavated and the exposed face securely supported by timber while the rock underlying is drilled and blasted. This work, carried out in air pressure, necessitates very small charges of dynamite and is very tedious and expensive.

One of our shields with which the east-bound up-town tunnel was constructed from under the Hudson River through Morton Street and Church Street and into 6th Avenue as far north as 12th

Street, travelled a total distance of 4,525 feet, of which amount 2,075 feet was constructed with rock over the partial cross-section of the tunnel, and overlaid with wet sand.

During the progress of this shield and for the purpose of blasting and removing the rock, there were exploded 26,000 sticks of dynamite in front of the cutting edge, causing very great damage to the structure of the shield, so that at the time it arrived at 12th Street and 5th Avenue, the shield was in such condition that it was with considerable difficulty that the tunnel lining could even be erected.

As the shield advances, the permanent lining must be erected in the rear. In Brunel's day brickwork was used, and it has recently been used to some extent, but even with the use of quick setting Portland cement neither brickwork nor concrete is successful for subaqueous work, as the materials cannot reach any strength, in the time during which it is feasible to leave the shield, before advancing it again after construction of a ring. Structural iron, imbedded in concrete at the points where jacks react, has been tried without satisfaction, and the use of wood as a temporary lining, to be backed up after advance of the shield by brick or concrete, while satisfactory for small tunnels, is never likely to be much used for any large sized tubes as needed for railroad tunnels. It is essential, for any tube tunnel construction with a shield, to have a permanent lining which can be erected as the shield advances and rapidly put in place, and which, as soon as erected, is strong enough to take all permanent stresses and to permit of the reaction of the jacks of the shields. A metal lining is the only solution of this problem. Cast iron or cast steel has almost exclusively been used, as the forming of the segments is so accurate, the joints are the least in number, and the material is such as to give the longest life with the least depreciation. If an internal concrete lining is afterwards used, then undoubtedly a satisfactory structural steel lining could be designed, but probably with no economy in cost of construction. We have used this to a small extent for experimental purposes. For any such metal lining, the complete ring has to be erected from within the tail of the shield, which is only a couple of inches larger inside than the external size of the finished tube.

Consequently all the plates cannot be made with radial joints, and a key has to be employed which is inverted from the ordinary keystone form, so that it can be placed in position from within the arch instead of from without. The usual length of the key is commonly equal to the distance center to center of the bolts in the circumferential joints, although often made very narrow like a wedge. The rings used in the Hudson tunnels were two feet long face to face, which means that every ring erected represents two feet of finished tunnel. The rings used in the Pennsylvania tunnels are 30 inches long. The rings are bolted together at close intervals at the circumferential joints by heavy steel bolts, and the segments making up a complete ring, by bolts in each longitudinal joint. The number of segments or plates going to make up the ring depends on convenience of handling, the usual limitation being for a length not exceeding six feet. For the Hudson and Manhattan tubes this worked out to nine segments, all having the same length, and a key. The segments weigh approximately 1,200 pounds apiece, and the placing of these at any point in the circle would be difficult and slow without a machine erector. This is one of the most human-like machines possible. It reproduces the human arm as exactly as possible. The erector (singular or plural, for in a big-sized tunnel more than one erector may be used simultaneously) is a machine either attached to the diaphragm of the shield or entirely independent. We have used both types and have invariably found the independent type most elastic and convenient in operation. This machine may be operated entirely by hydraulic power, or, as is very usual, by hydraulic power for the in-and-out movement of the arm and by pneumatic engine or electric motor for the revolving movement. The end of the arm is fitted with a simple attachment, representing a hand, which grips a plate segment in the middle so that it will balance. The plate is first dumped from a tunnel construction car on to the bottom portion of the tail of the shield already pushed forward and ready to allow the erection of a ring; then the arm of the erector hangs vertically and is attached to the plate; the arm recedes by operation of the hydraulic ram lifting the plate a few inches, then the arm, which is pivoted about its center and partially counterbalanced for the weight of the plate, revolves until the plate reaches the position in the circle which it is permanently to occupy, when the arm moves forward, shoving the plate into position, and the bolts are inserted and secured. Plate after plate is erected, until finally the insertion of the key completes a ring, when the shield is again advanced.

By subdivision of all bolt holes absolutely uniformly around the pitch circle in the circumferential joint, it is feasible to shift the position of the key at every ring and thereby stagger the longitudinal joints, increasing materially the rigidity and strength of the tunnels.

For subaqueous tunnelling in silt or sand, this method of the tube construction is essential to safety to employees as well as to safe and certain construction of the work itself; and with it and with competent labor employed, subject to certain limitations as to depth, it is difficult to imagine any condition which would prevent successful execution of a piece of work.

In the tunnel work of the Hudson & Manhattan Railroad there is a greater length constructed under the land than under the water, although every portion, except the Sixth Avenue Subway, north of 12th Street, is below tide level. A portion of this work has been in solid rock, though most of it is built through sand and gravel formation. Under these conditions the use of a shield has not at all times been necessary, and a very large portion of the tunnels could, for economic reasons, be advantageously built with concrete lining, under pressure but without the shield. The function of the engineer is to determine when this can properly be done, and when it can, the saving by substitution of concrete for metal lining is very great, though the metal lining can be more easily made absolutely water tight in wet ground than a concrete lining.

A considerable amount of tunnel construction in solid rock has been carried on with a shield, using iron lining, on account of the likelihood of troubles arising, owing to fissures or thin cover in the roof, which, in the absence of the shield for protection and support, might be troublesome to execute.

The whole art of tunnel construction in soft ground by any process depends on the proper support of every hole exacavated, and as excavated. A small hole in any kind of soil may be safely made without immediate support; but as soon as it is enlarged, the soil will cave in, unless given support to hold the material *in situ*. This is the purpose of the shield, and it is also the theory upon which tunnels are constructed by the ordinary mining methods. With the latter method air pressure has always been used to keep the water back. Narrow advance headings are driven corresponding to each side wall, and about at the springing line, in which timber mud sills are laid, acting as foundation for the temporary steel sets which support the roof. Then as the soil in the upper portion of the tunnel between side bearings is removed, length by length, these steel centers are put in place to carry the wooden planks known as "laggings," which are placed tight together to give continuous solid support to the external soil. Then the lower portion of the excavation is removed and posts with lagging put in below the sills, to hold the roof and sides until the permanent lining of reinforced concrete can be put in place.

In these subaqueous tunnel operations, as the influx of water is the "bugaboo" of the "sand hog," for the restraint of which air pressure is employed, so the greatest danger arising from the use of air pressure is the "blow out." Safety in such operations is dependent on the maintenance of a nice balance between the air pressure within and the water pressure without the tunnel. In the case of a diving bell or caisson, the exact amount of air pressure necessary to balance the column of water extending vertically from the rim of the diving bell or cutting edge of the caisson to the surface of the water, can be automatically determined. If too much pressure is used, the excess will escape under the cutting edge, and if not enough, the water will rise in the working chamber above the cutting edge. In the case of a tunnel, however, no such exact determination can be had, as the bottom of the tunnel, being deeper, requires a greater pressure to exclude the water than the top, and if this pressure is used, there will not be enough water pressure at the top to prevent the air from escaping, and if only enough is maintained to balance the water at the top, the bottom will be flooded. Fortunately the overlying material, to a greater or less extent, dependent on its character, permits of the maintenance of a greater pressure than that due to the hydrostatic head, so that the tunnel engineer exercises his judgment and fixes on a pressure that will not escape through this material and will exclude as much water from the bottom of the tunnel as possible. Some escape of air occurs in nearly all materials, excepting the most impervious, and this escaping air, if pressure is too great, blows the particles of materials away and enlarges the passages until they become so large that the air escapes faster than it can be supplied, when the air pressure drops and an inrush of water occurs. This is what is meant by a "blow-out."

For any railroad tunnel, in order to minimize the gradient of approaches, the grade is necessarily established at the least depth feasible below the bed of the river or below the surface; and where, as in the Hudson River, the water is sixty-five or seventy feet deep, and man cannot, with safety, work under air pressure exceeding forty-five pounds per square inch, which represents a hydrostatic depth of salt water of one hundred and two feet, it is obvious that the amount of cover over the roof of a tunnel and below the bed of the river must be very small. If the soil is in any degree porous, and pressure is allowed to drop, there is a grave liability of the ground becoming "demoralized" by infiltration of water, and if pressure is then raised to check the inflow, it sometimes happens that the cover of soil cannot withstand the increase, and the roof is blown off or a big hole blown out, and water then comes in in large quantities, often beyond control, so as to flood the tunnel works within the air locks.

The instinct of an unskilled foreman is usually to raise pressure in case of a leak commencing. If nothing happens it may be a good thing to have done, but an expert will usually lower the pressure in this case, putting up with the nuisance and difficulty of having a good deal of water in his tunnel and thereby allowing the greater external pressure to squeeze what soil exists into the pockets of the shield and thereby choke the leak. There is far greater skill and caution needed to raise air pressure than to lower it, in case of any leakage occurring. In the event of a "blow" it may be small, in which case it can be stopped from inside by stuffing up into the hole bags of sawdust, hay, balls of clay, and in fact anything handy and available to fill a hole. Usually this prompt treatment will relieve the condition so as to allow the shield to be pushed past the blow hole. Sometimes, however, the hole will increase in size, often quite suddenly, and get beyond control, in which case all doors in the shield are closed up and the men are removed before the tunnel fills with water. In one famous incident which occurred in one of the East River tunnels, a small blow had developed and a man was in front of the shield stuffing bags of hay in the hole, when the pressure sucked the bag into the hole suddenly, and the man, failing to let it go quickly enough, was carried, hanging to his bag, through the ground into the waters of the river, when he rose to the surface and was picked up alive by a passing boat. When the condition is reached that the tunnel is completely flooded, the steps to be taken to recover it must be from outside. Soundings are then taken very carefully and accurately to determine the locality and extent of the blow hole in the bed of the river. A large quantity, usually several hundred cubic yards of good stiff silt or clay, is loaded in a dumping scow, and at "slack water" the scow is towed into position over the hole, being located by instrumental observation from triangulation points on shore, and on signal from the two observers of simultaneous correct position the scow is instantly dumped. Soundings are then again taken to ascertain if the hole is completely filled, and if not, scow after scow is dumped at the same point. Following that, air is then pumped into the tunnel, forcing the water out through the blow pipes; as the pressure is gradually raised it soon becomes evident whether or not the hole is plugged. In the construction work on the up-town tunnels of Hudson and Manhattan, there occurred fully a dozen blows, necessitating dumping clay, although only in four or five cases was the tunnel flooded. In one of these most persistent cases, which had been occasioned by gross incompetence on the part of the night foreman, who advanced the shield with too many doors open, dumping was carried on until half the tunnel was full of silt, and then the leak was only stopped by spreading a sail sheet over the hole and dumping additional material on top. When the tunnel was recovered and cleaned out, the sail covered the door opening. The increased skill and experience attained by the workman before the down-town tunnels were built was shown in the fact that only two blow-outs occurred, in neither of which cases was the tunnel lost, and each of these was due to striking fissures in the rock formation close in shore. It is not unusual to provide against the chances of "blow outs" by artificially increasing the cover over a tunnel by dumping good stiff clay as a thick blanket over the tube location before the work is executed, and ultimately dredging and removing this clay. This method was carried out, to a very large extent, in the East River tunnels of the Pennsylvania Railroad.

One important item of work in connection with subaqueous tunnelling is that of "grouting." The purpose of this is two fold: (1) To fill the small annular space due to the shield's being slightly larger than the tunnel lining, as well as any other voids or cavities in the rear of the working face, and render the exterior soil solid, thereby giving more perfect support to the lining; (2) to waterproof and protect the material of the lining itself. Grouting consists in mixing hydraulic cement with an excessive quantity of water, so as to make it almost liquid, and then pumping it, either with a force pump or grout machine, through holes provided in the lining, into any spaces or interstices in the surrounding material. Cement mixed neat or in a very rich mortar with sand, and particularly if it has originally been mixed with an excess of water, when set up and thoroughly crystallized, makes an exceedingly impervious material. In the iron lining plates, there are usually provided one hole to each segment of plate, screw-tapped to permit the insertion of a piece of iron pipe having a hose attachment connected to the grout machine. This machine was invented by Greathead, and consists of a small tank in which blades or paddles are rapidly revolved on a shaft by an air engine or motor. To the tank is fitted an extension box with a flap door opening inwards, through which the change of cement (usually one barrel at a time) with the water needed to make slurry of it, can be admitted. The only other attachments are a large pipe for admission of air at high pressure, and a discharge pipe fitted with a cock. The operation is simple. Having put into the tank the cement and water, the paddles are run until they are thoroughly mixed, the flap door closed, and air admitted at high pressure into the tank. As soon as the discharge valve is opened, the mixed grout is forced through the hose to its position outside the lining. The penetrating power of grout under high pressure is remarkable, and it will follow the easiest channel. It is not desirable to use grout where the exterior soil is silt, as the pressure of water softens the silt into mud, preventing the setting of cement and demoralizing the silt in proximity to the lining. Further, it is not required in that character of soil to fill the voids, as clay will flow sufficiently to fill all voids itself. In sandy soil, either with metal or concrete lining, grout is always used, and usually in the case of tunnel built in rock. Along the streets of New York we have repeatedly traced dead connections with sewers by finding grout follow open pipes into houses and fill the cellars. Lifting the tracks of railroads or lifting asphalt pavements of streets sixty feet overhead of the tunnel, have not by any means been unknown occurrences, and the bodily raising of a standing building has been done on one occasion. By filling all voids promptly as the shield progresses, when tunnelling under land, the settlement of soil can be reduced to an extremely small amount, so much so that short lengths of tunnel have been driven under occupied dwellings without the tenants being aware of the fact.

The subject would hardly be complete without a brief reference to the construction of portions of the tunnel work by caisson methods. The general underlying principles of caisson construction are similar to those of shield tunnel work—progress being made vertically instead of horizontally—but for a great many years the use of caissons has been almost exclusively for the construction of piers for bridges, or where solid support is necessary for building foundations.

In locating the tunnels for the Hudson and Manhattan system, where the north and south line connecting the steam railway terminals on the New Jersey side intersect the up-town pair of river tubes, a complicated problem existed as to how to connect these lines, so as to enable the trains crossing the river to run southerly to the Pennsylvania and Erie Railroad terminals, northerly to the Lackawanna terminal, and from the latter point to the down-town river tubes. To enable these train movements to be executed necessitated what in railway parlance is known as a "Y" junction. In case the tracks for trains moving in opposite directions had been placed side by side at the same elevation, as is usual, dangerous grade crossings would have been established at three points of juncture, which would have impaired safe and rapid operation. The idea was therefore conceived of placing the tracks for trains moving in opposite directions close together, but one above the other, so that at points of intersection they would be on different levels. At the three junction points where each single tunnel diverged, it was necessary to build a switch chamber or section of tunnel, having the width of a single tunnel at one end and gradually widening until sufficient width was obtained for the construction of the two diverging tubes; and owing to the separation of grades in the tubes, these chambers needed to be double decked. As the shield is only adapted to the construction of a fixed size and form of tunnel, it could not be used, and some other method had to be devised.

To construct these chambers by underground mining methods involved very serious hazards, due to their great size and the unstable character of the material at the roof level. On the other hand, to dig an open pit from the surface was not feasible, owing to the great depth and danger of infiltration of water from the river nearby into the excavation. The method finally adopted was to build these sections of tunnels on the surface as monoliths of concrete reinforced with steel, and then to sink them as pneumatic caissons to their final position. They were built complete with the exception of the bottom of the lower chamber, which was left open so as to serve as a working chamber for the excavators. The material excavated was passed out through shafts extending through the upper chamber, and thence through an air lock to the surface, whence it was removed or dumped on the descending caisson in order to give additional weight to cause the caisson to sink by overcoming the friction of the surrounding material on the sides. When each caisson had been sunk to the proper level, they were completed by an inverted concrete arch closing the bottom and sealing the connecting tunnels joined to them. In some cases the shields were driven to the caissons, then rolled through and continued on the opposite end. Each of these caissons took about three months to sink, and the largest weighed twelve thousand tons. During the construction and sinking, by arrangement with the railroad companies, traffic was suspended on the railroad track immediately above the caissons.

The conditions were very much worse, however, in the case of the construction of the approaches to the Church Street Terminal Station under Cortlandt and Fulton Streets, where the arrangement of switches desired was such that the tunnels could not be built by shields and the caisson method was used. In this case the business had to be continued on the surface of these streets at all times, from beginning to end of the construction work, and further, all sewer pipes, steam pipes, electric conduits, etc., had to be in use and operation throughout the entire period of construction. The difficulty, due to adjacent buildings not having foundations which extended below the cellars, made necessary other methods than those adopted in Jersey City. The cofferdam enclosing the entire area of the buildings had already been sunk, by a series of caissons, to bed rock around the entire area, and these approaches had to be constructed contiguous with the exterior of the cofferdam taking in practically the entire width of Cortlandt Street and Fulton Street, building line to building line.

In this case an absolutely new departure in caisson construction was adopted, which involved sinking boxes, forming short sections of tunnel, with solid sides, the same being built up as the caissons descended. These boxes had to be sunk entirely from below the level of the street, and their sides were built up five feet at a time as they sank, special types of air locks being necessary to enable the work to be carried on in this manner. These caissons, too, contrary to the usual custom, were sunk without roofs, that is to say, the permanent floor of the tunnel was designed as the roof of the working chamber, and the sides of the box built up of reinforced concrete, but the ends with steel plates reinforced with pin-connected truss girders. These boxes were sunk by loading with pig iron in order to obtain the necessary weight, and when they had reached their permanent position timber struts were put in to take the external pressures on the side walls, pending the construction of the roof. When the various caissons had been sunk, one against the other, until the whole strip of tunnel had been completed, the removable steel ends of the boxes were taken out and the permanent roof put on to the side walls.

The difficulties of doing this work under the conditions may be appreciated when it is understood that, in Cortlandt Street alone, the 6th and 9th Avenue Elevated Roads had to be supported, and not only a public sewer in each street had to be maintained, but also a twenty-four inch steam pressure main (under 90 pounds of steam pressure all the time) as well as a bank of electric conduits which carried about seven thousand wires, comprising every telephone and telegraph wire in the down-town section going out of New York City.

The tunnels under these streets could undoubtedly have been built by the tunnel methods indicated in the earlier part of this paper, possibly at some saving in cost, but any such methods would have involved such risks of damage to buildings and adjacent properties that such a saving would have been far more than offset by damages in case a building was wrecked. By this method the construction of these difficult portions of tunnels, where junctions between the different lines were involved, could be carried on with almost absolute safety, and as the results proved, there was practically no injury to any properties adjacent or immediately above the construction itself.

The work which has recently been carried out in New York City, the construction not only of the tunnels of the Hudson and Manhattan Railroad, but also those of the Pennsylvania Railroad, have developed almost every possible combination of conditions which could by any possibility arise in tunnel construction, and the enormous magnitude of work carried on in the last six or seven years has been done with practically no disasters of any kind. These methods described have brought the modern construction of tunnels down to almost an exact science. The hazards and dangers are infinitely less than in the case of bridge construction, and tunnels are applicable in many cases where bridges are not.

In addition to this, the different tunnels permit very much greater elasticity in the development of transportation facilities than the bridges do, and traffic can be more economically and better distributed by developing underground routes than by construction of immense bridges which, on account of the lateral strength and rigidity, must be built on an enormous scale, whereas tunnels can be constructed on any lines, wherever they will give the greatest facilities for distribution of the travelling public.

New York City, and in fact all of our great cities, are absolutely in their infancy in respect to the tunnel for transportation. There is practically no limit to the development of this line of work as the necessities arise and as traffic demands, and we have reached in New York City a point where the possible construction of tunnels can hardly, by any possibility, keep pace with the growth and development of the population and with the necessities for their transportation.

In respect to the rivers, the future, unquestionably, has within sight construction of highway tunnels between New York and New Jersey on the lines of the Blackwall under the Thames in London or similar tunnels under the Clyde at Glasgow, forming underground extensions of existing street thoroughfares; and the object of this presentation of the subject is to give an accurate idea of the principles underlying the construction of such arteries of transportation.

A BRAIN OF ABOUT ONE-HALF THE AVERAGE WEIGHT FROM AN INTELLIGENT WHITE MAN.

By PROFESSOR B. G. WILDER.

(*Read April 22, 1910.*)

For the privilege of examining and reporting upon this unusual —perhaps unique—brain I am indebted to Dr. J. H. Larkin, professor of pathology in the College of Physicians and Surgeons, New York city.

History.—According to Dr. Larkin's records Daniel Lyon died on the tenth of October, 1907, from asphyxia due to edema of the glottis. He was Irish, 46 years old, five and one-half feet high, and weighed 145 pounds. No relatives have been discovered and it is not known that any survive. At the time of his death he lived at 409 E. 17th St., New York City, and was a watchman for the New York Contracting Company at the Pennsylvania Terminal, 34th St. The legal representative of that company says that "from all reports there was nothing defective or peculiar about him, either mentally or physically." No photograph or hat-measurement has been obtained. No information has been gained by inquiries addressed to his alleged fellow-workmen or former places of residence, but Dr. Larkin was informed that he could read and write; that he was regarded as competent and in full possession of his faculties; and that as a laborer he had worked in one position for twenty years. There seems to be no reason why he should not be regarded as of ordinary intelligence; yet, as will be seen, his brain might have belonged to a feeble-minded person, or even an idiot.

Shortly after death the brain was removed in the presence of Dr. Larkin and the coroner's physician, Dr. Philip O'Hanlon. No head-measurements were made, but it did not appear to be unusual in either size or shape. The brain filled the cranium; there was no excess of liquid, and no evidence of compression. Upon accurate scales the brain was found to weigh exactly 24 ounces, or 680 grams, about one-half the average for male Caucasians. It was placed immediately in ten per cent. formalin, and there remained until sent to me more than two years later.

Examination and Results.—At its reception by me on the thirteenth of January, 1910, the brain weighed 714 grams, having enlarged slightly in the preservative. Subsequent immersion in alcohol reduced the weight to 682 grams on February 7 and on April 6 to 512, a trifle over 18 ounces. Of this total, after transection at the midbrain, the cerebrum proper represented 404 grams and the cerebellum, with the pons and oblongata, 108; the ratio is less than 4 to 1 instead of the usual ratio of about 8 to 1. Indeed, the cerebellum seems nearly normal in size and form, while the cerebrum lacks about one-half the usual weight and is peculiar in several respects.

Form of the Cerebrum.—The present disproportionate width and the flatness of the middle of the dorsum are probably due to the weight of the brain itself as it hardened in a liquid of less specific gravity. To the same cause may be ascribed some of the divarication of the occipital lobes and the concomitant exposure of the cerebellum; respecting the original conditions in these respects there are neither records nor recollections. The occipital lobes are very slender, and their cavities are slight. The cunei, associated with vision, are extremely narrow, but the parts of the temporal lobes associated with smell are well developed, and the postrhinal fissures distinct. On both sides the insula is partly visible, but that sometimes occurs with ordinary brains, and in the superior brain of a philosopher, Chauncey Wright. On the left the insula has been fully exposed by removing the operculums; it is small, presents no true fissures, and resembles a rounded ridge curved sharply about a deep pit at the dorsal side. The precunei, regarded as "association areas," seem fairly well developed. Whether there is a marked deficiency of the "speech center," the subfrontal gyre (Broca's convolution) I am not yet prepared to state. Transections of the right hemicerebrum at four levels indicate unusual smallness of the cavities, and a reduction in the alba or white substance rather than in the cortex or gray matter; upon these points and upon the size of the callosum I hope Dr. Spitzka may be willing to comment even at such short notice.

Fissural Peculiarities.—Of these there are several, and some are indicated upon the charts. Their detailed discussion would be out of place in a comprehensive society like this until such time as the normal human fissural pattern is familiar to pupils in or below the high school.

This brain is not ape-like. Even were it still smaller it is distinctly human. The mesal cleft or "valley" of the cerebellum is deeper than in any ape. The calcarine and occipital fissures are deeply continuous while, with rare exceptions, they fail to unite in apes. Even the insula, deficient as it is, does not resemble that of apes, and the "speech-center" is of the human type.

Brain-weight and Intelligence.—Upon the present occasion attention is particularly directed to this exemplification of the possibility that ordinary human intelligence may apparently coexist with a brain of only one-half the ordinary size, exceeding that of certain apes by only 180 grams (about six ounces), and not quite double the size of the brain of a congenital idiot.

DERMAL BONES OF PARAMYLODON FROM THE ASPHALTUM DEPOSITS OF RANCHO LA BREA, NEAR LOS ANGELES, CALIFORNIA.

By WILLIAM J. SINCLAIR.

(*Read April 22, 1910.*)

In the excavations conducted by the Department of Palæontology of the University of California in the Pleistocene asphaltum deposits at Rancho la Brea, near Los Angeles, large numbers of small bones have been found, resembling closely the dermal ossicles described by Dr. A. Smith Woodward (2, 3) from a piece of the skin of a gravigrade edentate, *Grypotherium listai*, found in a cave at Last Hope Inlet, Patagonia, and also resembling the previously known dermal bones of *Mylodon* (1).

In the asphaltum, two edentates have been found so far, one apparently related to *Megalonyx* and the other referable to the genus *Paramylodon*. As dermal bones, among the Gravigrada, are known only in the Mylodontidæ, there is every reason to regard those presently to be described as pertaining to *Paramylodon*. Comparison of a skull and jaw from the asphalt with the figures of *P. nebrascensis* Brown (4) from the Pleistocene locality of Hay Springs, Nebraska, seems to indicate that they should be referred to this species.

In a preliminary paper on the Rancho la Brea deposits, Professor Merriam (5) says, regarding the discovery of ossicles:

> During the first examination of the beds several small, pebble-like bones were obtained which resembled the dermal ossicles of the ground-sloth, *Grypotherium*, recently described by Dr. A. Smith Woodward from skin fragments obtained in a cave at Last Hope Inlet, Patagonia. The ossicles were in association with remains of a large ground sloth somewhat similar to *Mylodon* in foot structure. Realizing that the peculiar conditions of accumulation offered an especially favorable opportunity for preservation of the dermal armor of a ground-sloth, during the second study of the deposits an attempt was made to find a specimen in which the armor might be recognized. Several hundred yards from the location of the first specimen, a large scapula

resembling that of a mylodont was found partly exposed, with a row of small ossicles immediately over the outer side. The section of the bed containing these bones has recently been worked out, and the row of small bones proves to be the edge of a distinct layer including between 250 and 300 individuals. They mantle over the outer surface of the scapula, being removed from it by about an inch of asphalt.

The layer of bones as we find it has probably been disturbed somewhat and does not occupy its original position exactly, but the fact that it remains as a distinct layer with a tendency toward similar orientation of the individual ossicles indicates that the disturbance has not been great. As the position of the layer in the asphalt was nearly vertical, the presence of the large number of ossicles together may not be attributed to the washing together of scattered elements on the floor of a small basin of deposition.

The ossicles are not closely pressed together and are not superimposed. The individuals range in size from a cross-section of 6.5×4.5 mm. to 21×16 mm. Excepting a few of the largest ones, which are nearly square, the greater number are rounded and rather irregular in form. The outer side is in some cases more regularly modeled than the inner. The surface of the bones is somewhat roughened or pitted in some instances, but no markings are present which would be considered as definite sculpturing. The microscopic structure has not yet been examined.

In general the form, size and arrangement of the ossicles are much as in the bones in the *Grypotherium* skin from Patagonia. The skin fragment first described by Woodward was thought to represent mainly the region of the neck and shoulder. The Californian specimen mantles over the outer side of the scapula, and is presumably not far removed from its original position with relation to this bone.

Subsequent investigation has added little to this, so far as the localization of particular types of ossicles is concerned. Some idea of the diversity of forms assumed by them may be gathered from the accompanying figure, in which several of the types mentioned in the citation may be recognized (especially in Fig. 1, *b, c, d, g* and *h*), nor do these differ essentially from the great mass of scattered and unlocated ossicles. Pitted and smooth forms occur in the same individual. In some of the bones, except for minor undulations and the pores for the entrance of blood vessels, the outer surfaces are smooth and polished (Fig. 1, *c, f*). Others show a highly irregular pattern of small anastomosing ridges (Fig. 1, *b*), but, as previously noted, there is no constancy or regularity in the pattern. Grooves cut across some of the ossicles, as shown in the figure. Some of these may be due to the fusion of two adjacent elements, as suggested by Woodward for the origin of a similar structure in *Grypotherium*.

Thin sections of the ossicles of *Paramylodon* were submitted to Professor E. G. Conklin who has kindly examined them and furnished the following note:

Histological Character of Dermal Bone, Horizontal and Vertical Sections.—Penetrated by many canals for blood vessels, which, in general, begin perpendicular to the surface but soon branch repeatedly and frequently anastomose. Occasionally Haversian systems of lamellæ and lacunæ may be seen around these canals, but generally these lamellæ and lacunæ are very irregularly disposed between the canals. One of the most striking features is the presence of bundles of fibres which run in all directions through the bone, making the latter look almost like a woven fabric. These fibres are most abundant in the spaces between Haversian systems.

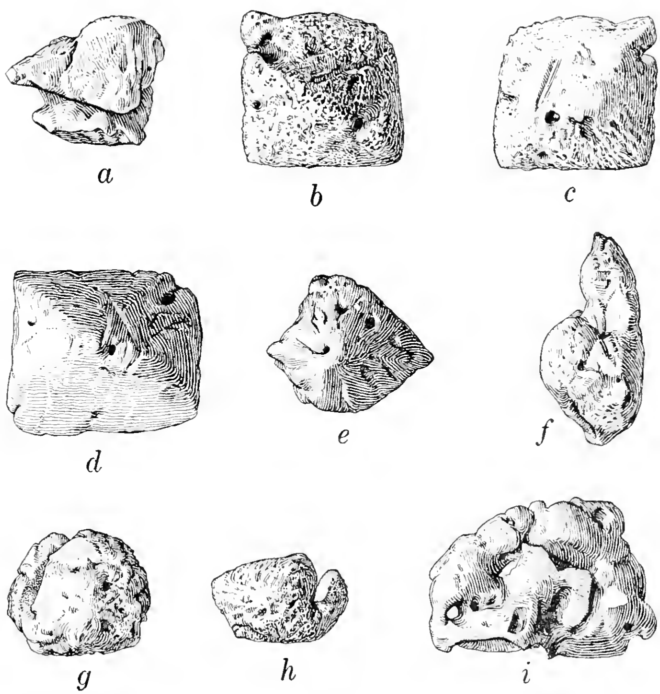


Fig. 1. Dermal ossicles of *Paramylodon nebrascensis* showing some of the various forms assumed. *a*, irregularly shaped ossicle with transverse groove; *b*, *c*, rectangular ossicle with groove toward one corner; *d*, another rectangular bone with pyramidal surface; *e*, *f*, ossicles of irregular shape, similar to some of those found overlying the scapula; *g*, *h*, ossicles of the same character as those overlying the scapula; *i*, pitted ossicle, outer (?) surface. All the figures are one and one half times the natural size.

Comparison with Woodward's figures of the histological structure of the ossicles in *Mylodon* and *Grypotherium* (2, Pl. xv., Figs. 7, 8) shows a close agreement between the latter and Paramylodon, the only differences appearing in the absence of all suggestion of a zonal arrangement of the lacunæ toward the lateral margin of the bone and the less frequent occurrence of Haversian systems, which are best observable in a transverse section of one of the *Paramylodon* ossicles. In all other respects there is the closest agreement, both in the presence of interlacing fibres traversing the mass, the absence of radiate structure at the periphery, and the development of Haversian systems about the vascular canals, features which are not developed in *Mylodon*.

In addition to the acknowledgments already made, the writer desires to express his indebtedness to Professor J. C. Merriam to whose kindness is due the opportunity to examine and describe this new material.

Princeton University,
April, 1910.

LITERATURE.

Figures in parentheses in the text refer to the titles in this list.

1. H. Burmeister.

1864–69. *Anales Museo Publico,* Buenos Aires, Vol. I., p. 173, Pl. V., Fig. 8, 1864–69. Describes and figures the dermal bones of *Mylodon.*

2. F. P. Moreno and A. Smith Woodward.

1899. On a Portion of Mammalian Skin, named *Neomylodon listai,* from a Cavern near Consuelo Cove, Last Hope Inlet, Patagonia. By Dr. F. P. Moreno, C.M.Z.S. With a Description of the Specimen by A. Smith Woodward, F.Z.S. *Proceedings of the Zoölogical Society of London,* 1899, pp. 144–156, Pls. XIII.–XV. Describes the skin and ossicles of *Grypotherium.* Figures histological structure.

3. A. Smith Woodward.

1900. On some Remains of *Grypotherium* (*Neomylodon*) *listai* and Associated Mammals from a Cavern near Consuelo Cove, Last Hope Inlet, Patagonia. *Proceedings of the Zoölogical Society of London,* 1900, pp. 64–78, Pls. V.–IX. Additional figures of the ossicles.

4. Barnum Brown.

1903. A New Genus of Ground Sloth from the Pleistocene of Nebraska. *Bulletin of the American Museum of Natural History,* Vol. XIX., Article XXII., pp. 569–583, Pls. L., LI., 1903. Defines the genus *Paramylodon.*

5. J. C. Merriam.

1906. Recent Discoveries of Quaternary Mammals in Southern California. *Science*, N.S., Vol. XXIV., No. 608, pp. 248–250, August, 1906. Describes the discovery of dermal ossicles in the asphaltum.

6. **J. C. Merriam.**

1908. Death Trap of the Ages. *Sunset Magazine*, Vol. XXI., No. 6, pp. 465–475, October, 1908. Figure of the ossicles as found in a sheet of asphaltum overlying the scapula of *Paramylodon*.

THE RESTORED SKELETON OF LEPTAUCHENIA DECORA.

By WILLIAM J. SINCLAIR.

(*Read April 22, 1910.*)

A recent examination of the *Leptauchenia* material in the Princeton University collection has made possible the presentation of the accompanying restoration of the skeleton (Fig. 1), together with some notes on the structure of this animal. Parts of two species are represented, referable apparently, to Leidy's *L. decora* and *nitida* respectively. All the specimens were collected some years ago by Mr. J. B. Hatcher at Corral Draw, in the White River badlands of South Dakota.

In a general way, there is a good deal of resemblance between the writer's restoration of *Leptauchenia* (Fig. 1) and Peterson's reconstruction of *Phenacocœlus*,[1] an animal about one fourth larger, presumably related both to the first mentioned genus and to *Cyclopidius*. The major portion of the skeleton is drawn from two individuals of *Leptauchenia decora* (Nos. 10753, 10773 Princeton University collection) supplemented occasionally by other specimens of the same species. The fore foot is from a somewhat smaller individual of *L. decora* (No. 10770) while the hind foot, with the exception of the tarsus, is enlarged to scale from *L. nitida* (No. 10765). On the whole, the restoration recalls to mind an animal of somewhat pig-like proportions due to the large head, short limbs and short tail which may have been longer than indicated. No certain conclusion can be drawn from the skeleton as a whole regarding the habits of the animal. That it was aquatic inferred from the prominent rim of the auditory meatus implying a "valvular closure of the organ of hearing" to prevent the inflow

[1] Peterson, O. A., "The Miocene Beds of Western Nebraska and Eastern Wyoming and their Vertebrate Faunæ," *Annals of the Carnegie Museum*, Vol. IV., No. 1, p. 32, Fig. 5, 1907.

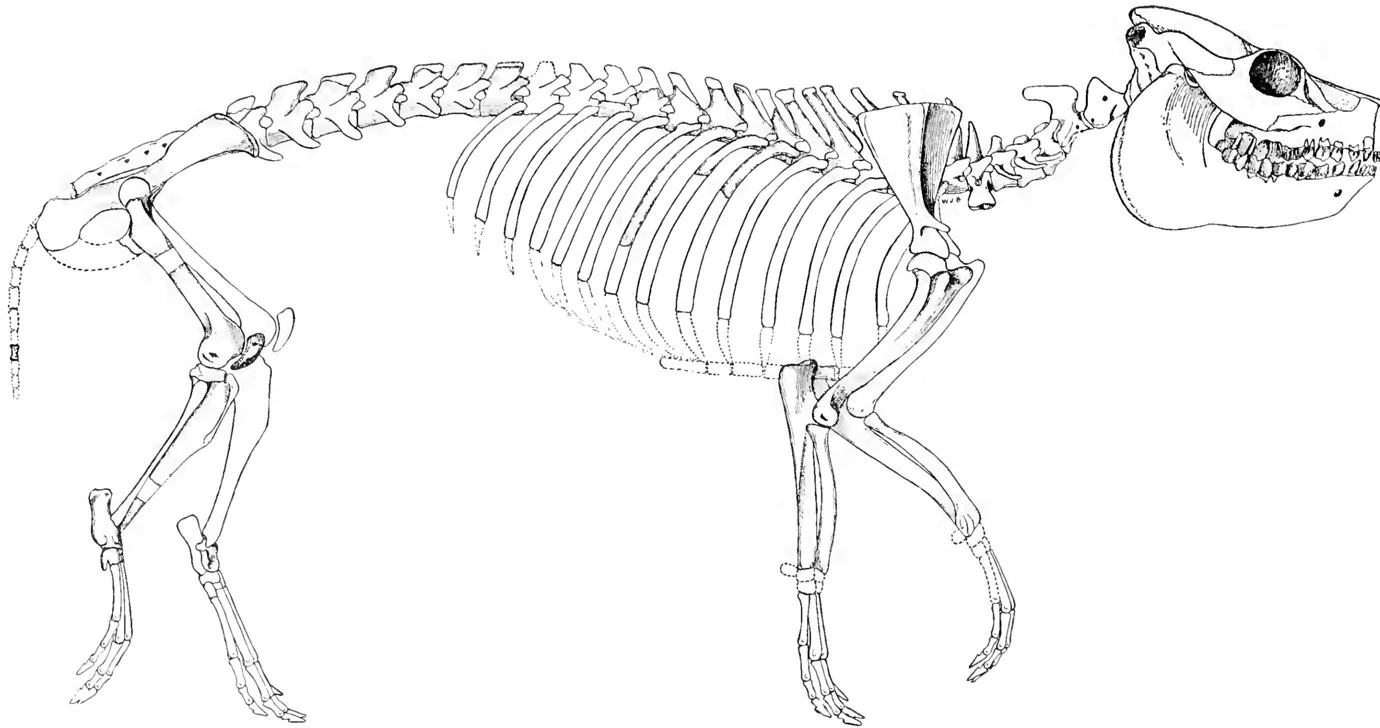


Fig. 1. *Leptauchenia decora.* Restoration of the skeleton based on two specimens (Nos. 10753, 10773) in the Princeton University Collection. Unshaded parts are supplied from other specimens or enlarged to scale from *L. nitida.* Broken lines indicate hypothetical reconstruction. One third the natural size.

of water as suggested by Cope for *Cyclopidius*[2] is not altogether substantiated by the feet, the slender toes of which terminate in small hoof-like elements well adapted apparently, so far as their structure is concerned, to running on firm ground. The slight development of lower incisors and canines indicates, perhaps, that *Leptauchenia* was not a grazing animal, for in modern grass feeders, while the upper incisors and canines may be absent, the lower teeth are broad and flat, well adapted to cropping grasses, while in *Leptauchenia* they are almost cylindrical. A study of the conditions of sedimentation involved in the accumulation of the so-called clays in which the remains of *Leptauchenia* occur will, it is believed, afford a safer clue to its habits than does the anatomy.

As it has not been possible to have detailed drawings prepared it has seemed advisable to omit full description and present merely some notes on the general structure of the skeleton.

Skull.—The prominent orbits, the large facial vacuities extending backward between the eyes, the high and almost straight sagittal crest, the elongated auditory meatus with thickened lip, the heavy arches and the deep mandible are at once apparent from the figure. Coupled with these as generic characters are the enormously expanded auditory bullæ and the reduced condition of the anterior dentition. Some differences in proportion appear between this and a previously published figure of the skull of *Leptauchenia*,[3] due probably to the fact that the latter is a composite.

Vertebral Column.—The dorso-lumbar vertebral formula is twenty, of which fourteen are dorsals. Six vertebræ are coössified in the sacrum, three of them being in contact with the ilium. As shown in the restoration, the anterior dorsals have high narrow spines, sloping backward. These decrease in elevation posteriorly and probably about the twelfth or thirteenth dorsal begin to assume the shape of the wide, transversely flattened lumbar spines. A few proximal caudals are preserved, too few to determine with certainty whether the tail was long or short, but suggesting the

[2] Cope, E. D., "Synopsis of the Species of Oreodontidæ," Proc. Am. Phil. Soc., Vol. XXI., p. 547.

[3] Scott, W. B., "Beiträge zur Kentniss der Oreodontidæ," *Morphologisches Jahrbuch*, Bd. XVI., Taf. XV., Fig. 15.

latter from their comparatively small size. In the cervical series, the atlas is characterized by a slender inferior arch and broad superior arch with strong backward slope to its dorsal profile. The canal for the vertebral artery perforates the base of the transverse process at the margin of the posterior cotylar surface, where it is inclosed by but a narrow bridge of bone, soon emerging on the lower surface of the process. Some distance anterior to the point of emergence, it again perforates the transverse process, joining the neuro-arterial canal near its point of emergence. The margin of the transverse process is broken in all the specimens examined, but seems to have been circular. The axis carries a large neural spine, of which the dorsal border slopes strongly backward and upward. All the transverse processes of the cervicals are perforated by the vertebral artery.

Girdles.—The scapula is a triangular element of which the outer surface is divided unequally into large post- and small pre-spinous fossæ by the prominent scapular spine, of which the acromion process is directed forward. Of the pelvic girdle, the ilium is broadly expanded in the transverse plane, rather more so than the figure would indicate. The gluteal fossa is shallow and the anterior superior spine quite prominent. The ilio-pectineal eminence varies in prominence in different specimens. Both ischial and pubic rami are stout. Their distal expansions are lacking in all the Princeton specimens.

Limbs.—The strong forward curvature of the radius and heavy olecranon process of the ulna are perhaps the most striking features of the fore limb. Apart from this, there are no peculiarities which call for special comment. It is possible that the lateral digits in both fore and hind foot have not been given sufficient length, the error, if such it be, arising from an attempt to scale the drawing from a few dissociated phalanges. But one terminal phalanx is represented in the Princeton collection, and of this the tip is broken off and the margins somewhat damaged. It seems to have been a small pointed hoof.

Princeton University,
April, 1910.

ANTARCTIC GEOLOGY AND POLAR CLIMATES.

By WILLIAM MORRIS DAVIS.

(*Read April 22, 1910.*)

It seems desirable at the present time of active interest in Antarctic exploration to call attention to a point that deserves the special scrutiny of geologists who may visit far southern regions.

Exploration already accomplished has shown that the Antarctic as well as the Arctic lands contain geological formations indicative of a much milder climate than that which prevails in high latitudes today. Thus far, the evidence of mild polar climates has been drawn almost exclusively from fossils or land plants contained in stratified continental deposits. The structure of non-fossiliferous continental formations at high latitudes has not been minutely studied in their bearing on climatic problems. Investigations of recent years in temperate latitudes have however shown that the detailed structures of land-laid stratified deposits may also be used with much success in determining the climate under which they were formed. The studies of Professor Joseph Barrell, of Yale University, published in the (Chicago) *Journal of Geology* for 1908, deserve especial mention in this connection; for they have clearly set forth the characteristics of continental formations in contrast to marine formations, and they have further suggested a variety of tests by which the climate under which continental deposits were formed may be inferred. Under an ordinary or normal climate, neither glacial nor arid, land-laid stratified deposits are chiefly the work of aggrading streams, and as such they will be characterized through the greater part of their mass by frequent and irregular changes in texture, with cross-bedding, lateral unconformities, red color, ripple marks, rain prints and mud cracks. Evidently then the detailed structure of continental formations in high latitudes may be nearly as significant of mild climate as is the occurrence of fossil land plants. Furthermore, the formation of continental deposits is greatly favored by large continental area; and conversely, large continental area may be inferred from an abundance of continental deposits; thus the problem of polar climates is linked with the equally interesting problem of the changes in land and sea areas through geological time.

There is an important theoretical matter to be mentioned in this connection. It will be remembered that Professor T. C. Chamberlin presented a communication at one of the April meetings of this society several years ago, in which he suggested that a mild polar climate might be caused by a reversal of the deep oceanic circulation, whereby the warm surface waters of the torrid belt would sink, creep along the bottom toward either pole, and rise in high latitudes, where their warmth would determine a climate very much milder than that of today. Evidently if this or any other process that is capable of producing a mild polar climate has been in operation at one time in the past, it may have been in operation at various other times; and thus a question rises to which Mr. Bailey Willis drew attention in his address before the geological section of the American Association at the Boston meeting of last winter; namely, what has been the prevailing climate of the polar regions through the geological ages? Naturally we open this inquiry with a predisposition to regard the climate now prevailing in high latitudes as the normal climate; but if it once be shown that a mild climate has sometimes prevailed there, it is entirely possible that a mild climate and not the rigorous climate of today really represents the prevalent conditions of the polar regions through geological time. Under Chamberlin's theory of mild polar climate, rain would be abundant but mud cracks would be rare; hence even so small a detail as the relative proportion of these minute structures in continental formations of high latitudes would have its significance. Marine formations will probably give less decisive evidence in this respect than continental formations; but marine formations would also have their importance, not only by reason of the fossils they might contain, but perhaps even more from the presence or absence of scattered boulders and gravels, such as might be dropped on a sea floor from floating icebergs. The prevailing absence of such intermixture of floated materials in the marine formations of former polar sea would be almost as significant as the occurrence of fossil land plants in the continental formations of high latitudes.

It is certainly very suggestive that the stratified formations of high latitudes have already repeatedly yielded evidence of mild climates, chiefly as above noted in the form of fossil land plants contained in continental formations, partly in the form of red sandstones; and it is certainly striking that little or no evidence of ancient glacial climates in the polar regions has been found either in continental formations in the form of tillite lying on striated rock-floors, or in marine formations in the form of coarse materials scattered through fine-textured sediments. The inference thus warranted in favor of not infrequent mild polar climates ought to be followed up by critical examination of all pertinent evidence, such as the detailed structure of non-fossiliferous continental formations in high latitudes may furnish.

Antarctic exploration is of particular importance in this respect; for the Antarctic regions are today at least of continental habit, in contrast to the Arctic regions which are, on the other hand, of oceanic habit. Speculations are already abundant as to the formerly much greater extension of Antarctic lands, so as to form connections with other continental masses; and on the present remnant of so greatly extended an area, the possibility of finding continental formations is increased. It therefore seems fitting to bring this matter before that Society which, more notably than any other in the United States, has recently taken active steps in promoting American participation in Antarctic work; with the suggestion that it should be presented to the attention not only of American geologists who undertake southern voyages, but also of the geologists in expeditions sent out by other countries. The problem thus offered for investigation may fairly be regarded as one that has far-reaching results; for if it should appear that the earth's polar climates have really been prevailingly mild, we should have to frame new conceptions of terrestrial physics.

Cambridge, Mass.,
April, 1910.

THE PROPAGATION OF EXPLOSIONS IN MIXTURES OF PETROLEUM VAPOR WITH AIR IN TUBES.

By CHARLES E. MUNROE,

Professor of Chemistry, George Washington University, Washington, D. C.

(*Read April 22, 1910.*)

On May 12, 1902, a series of accidents from fire and explosions occurred at or contiguous to the Sheraden Yard of the P. R. R. at Pittsburg, Pa., as the result of the collision of a tank car containing "stove naphtha" with another car, or cars, whereby the tank car was perforated and naphtha escaped. As the collision occurred in the late afternoon at a time when the yard switch lamps and the lamps of other safety devices had been lighted, the vapors of the naphtha which was spilled from the perforated tank car, became ignited and through the conflagration thus initiated other tank cars containing naphtha were heated to such a point as to force their contents out where they could contribute to the general conflagration, or, where, as in one instance certainly, the tank valve was too firmly set to thus yield to this effect, the tank itself was ruptured and its heated contents were discharged into the atmosphere, producing most disastrous results.

The Sheraden Yard was artificially constructed by filling a ravine between steep hills extending from the high ridge along the of level surface required for so extensive a railroad yard. As Ohio River, near the confluence of the Alleghany and Monongohela rivers, to the northwest of Pittsburg, so as to obtain the area might have been expected, the bottom of the ravine was occupied by a stream, known as Cork Run, and this was fed by several lateral streams which entered the ravine through fissures or ravines in the surrounding hills. Good engineering demanded the preservation of these water courses after the filling of the ravine, and this was done by means of sewers, the main one occupying, in general, the bed of Cork Run, and its laterals those of the tributaries to Cork Run. The total length of the sewerage system thus constructed to the point where the collision occurred was 2,785 feet and the different sections varied in diameter from 2 feet at the head to 10 feet at the mouth where the sewer opened into the unfilled portion of the ravine near the Ohio River.

Immediately beside and parallel with the Ohio River was the road bed of the P. & L. E. R. R., a culvert having been built over Cork Run. Immediately beside this railroad embankment, but between it and the bluff was a turnpike road including its wooden bridge across Cork Run, this bridge being 19 feet above the bed of the run. Cork Run from the mouth of the sewer to the point of its discharge into the Ohio River, a distance of about 116 feet, was practically an open drain. The building of the tunpike across the ravine made a large pocket or basin at this point.

Naturally use was made of Cork Run sewer in draining Sheraden Yard, and, as a part of this system, catch basins for surface water from the yard were built and connected to the sewer, one of these catch basins being near the point at which the collision occurred.

Among other explosions, one occurred in the basin between the mouth of the sewer and the turnpike which was sufficiently violent to wreck many buildings in the vicinity and to lift from its track a trolley car, which was just approaching the turnpike bridge, with sufficient force to injure some of the passengers.

Among other theories, it was alleged that this explosion was due to naphtha which ran into the sewer through the catch basin above referred to, where its vapors formed, with air, a combustible mixture; that the vapors in the sewer were ignited by the fire burning in the yard about the mouth of the catch basin; and that the flame was transmitted through the sewer and caused the explosion of the accumulated air-vapor mixture in the basin.

In considering the probability of this theory being the correct one, the dimensions of the sewer throughout its complete length was ascertained and, from the volumes calculated from this data, it was found that 3,451 gallons of naphtha would furnish sufficient vapor to completely fill the sewer, while 200 gallons of the naphtha would fill the sewer with a slightly combustible but non-explosive mixture. As the tank car, which was perforated beside the catch basin, contained 7,253 gallons of naphtha, there was a sufficient volume to more than satisfy the requirements for filling the sewer with naphtha vapors as set forth above.

That explosions occurred in or about portions of the sewer and its laterals seemed undoubted, yet recalling the safety lamp, the difficulty experienced in transmitting flames through tubes, and the varying capacities of columns of explosives of different diameters in propagating detonation, a doubt arose in my mind as to the flame traversing this sewer. Not finding any information on this matter in literature, experiments were made as follows:

Results of Experiments.

No.	Date, 1903.	Kind of Tube.	Diameter, Inches.	Length, Inches.	Ratio of D : L.	Result.
1	Apr. 23	Glass	1.5	38	1 : 25.33	Positive
2	23	"	1.5	32	1 : 25.33	"
3	24	"	1	49	1 : 49	"
4	24	Steel	4	240	1 : 60	"
5	22	Glass	0.4	25⅜	1 : 63.44	Negative
6	22	"	0.4	25⅜	1 : 63.44	"
7	22	"	0.4	25⅜	1 : 63.44	"
8	23	"	0.4	25⅜	1 : 63.44	"
9	23	"	0.4	25⅜	1 : 63.44	"
10	23	"	0.4	25⅜	1 : 63.44	"
11	23	"	0.4	25⅜	1 : 63.44	"
12	24	"	1	78	1 : 78	"
13	24	"	1	78	1 : 78	"
14	24	"	1	78	1 : 78	"
15	24	Steel	4	480	1 : 120	"
16	24	"	4	480	1 : 120	"
17	24	"	4	480	1 : 120	"

It having been observed that if vessels were filled with the combustible mixture of naphtha vapor and air and the mixture was ignited at the mouth of the vessel, the flame retreated to various depths depending on the relation of the diameter of the vessel to its length, experiments were made with tubes. In conducting the experiments in each case the tube or pipe, which was open at both ends, was inclined at an angle and a considerable quantity of liquid naphtha was poured slowly into it at the upper end. So soon as the last of the liquid had been poured into the tube a flame was applied to the upper end of the tube (called the mouth) to ignite the vapors and it was then noted if the flame traveled completely through the tube and issued from the bottom end or if it traveled but part way through and became extinguished. In every experiment there was some liquid naphtha as well as naphtha vapors throughout the tube at the moment when the flame was applied. Experiments were made in glass and steel tubes varying in diameter from .4 inch to 4 inches. The results are presented in the following table. When the flame traveled completely through the tube and issued from the bottom end the result is marked "positive." When the flame traveled but part way down the tube and became extinguished the result is marked "negative."

It appears then that where the length of the tube is 63.44 times its diameter or above this the flame does not travel through.

RESULTS OF RECENT RESEARCHES IN COSMICAL EVOLUTION.

(Plate XXXI.)

By T. J. J. SEE.

(Read April 23, 1910.)

Early in 1908 the writer was able to conclude the researches on earthquakes, mountain formation, and kindred phenomena connected with the physics of the earth, which this society did him the honor to publish in four memoirs,[1] and it then became possible to resume the study of the problems of cosmogony, which have been before him uninterruptedly since 1884. For a long time these problems have defied the powers of the mathematician and natural philosopher, yet for the last thirty years enough progress seemed always to be in sight to stimulate the hope and energy of investigators; and many papers have appeared on the subject, especially from Professor Sir G. H. Darwin, while lesser contributions have been made by Lord Kelvin, Newcomb and Poincaré, and others.

The problem of the heat of the sun, due to condensation under gravitation, had been successfully attacked by Helmholtz as early as 1854, and subsequently much improved by the researches of Lane, Lord Kelvin, Ritter and the writer;[2] but the discoveries in molecular physics made during the past decade have shown that the conclusions based on the theory of gravitation alone have to be modified to take account of the energy of molecular transformation made familiar in radio-activity. By this hitherto unsuspected reservoir of natural forces the maintainance of the activity of the sun and stars is greatly prolonged. Accordingly, instead of reckoning the life of the solar system in twenties of millions of years, as estimated by Helmholtz, it is now known that the actual duration

[1] Cf. Proceedings, 1906–1908.

[2] Cf. *A. N.*, 4053.

of our system is to be estimated only in billions of years. This prolongation of the period allowed for the activity of the sun is confirmed by other investigations made by the writer during the past two years, showing that the mode of formation of our system has been excessively slow and very different from what has been generally supposed; and that the principal agency which has operated in its development and in shaping the orbits has been the action of a resisting medium extending over immense periods of time.

I shall not attempt to treat fully of this large subject in the present communication, because it is to be discussed in considerable detail in the second volume of my "Researches on the Evolution of the Stellar Systems," which is now in press and expected to appear during the coming summer or autumn. But it may be useful to give a brief review of the subject to see where we stand, and to sum up what appears to be established by the investigations which have so fully absorbed my energies during the past two years. The present discussion must therefore consist mainly of a summary, and such explanations as will render it moderately intelligible to the general reader.

In the first place, it is desirable to remark that, for nearly a century, we followed Laplace's assumptions in regarding the planets as having been detached or thrown off from the sun, and the satellites as having been likewise thrown off from their several planets; and our problem was to find out how the postulated rings of vapor had condensed into the bodies now observed in the solar system. Thus with a fixed premise we were trying to find out how the planetary development had come about, and it scarcely occurred to any one that the premise itself might be false, and therefore all the efforts based on it in vain.

The turning point which enabled me to discover this error in the premises was a certain criterion based on the law of areas proposed by the distinguished French physicist Babinet, in 1861.[3] In this brief communication of three pages to the Paris Academy of Sciences, Babinet pointed out the contradiction of Laplace's cos-

[3] Cf. *Comptes Rendus*, March 18, 1861.

mogony resulting from the dynamical principle of the conservation of areas, and concluded that the classic nebular hypothesis was vitiated by a fallacy.

Like most negative critics, Babinet was beset by the weakness that he could tear down but could not build up. And as he substituted nothing for the theory of Laplace, the valuable criticism which he had made was scarcely noted by his contemporaries, and remained practically unknown to subsequent investigators. Thus we find in the writings of Lord Kelvin, Newcomb, Darwin, Tisserand and Poincaré no mention of the eminently useful criterion which Babinet had proposed in 1861; and it was allowed to slumber half a century in a forgotten number of the *Comptes Rendus*.

Looking back over this strange state of affairs, we naturally ask ourselves why Babinet did not proceed further with his investigations. To this question no certain answer can be returned, but it is probable that his inability to account for the remarkable roundness of the planetary orbits, which Laplace had explained by the process of gentle detachment, made him hesitate to go any further, and he gave up the effort in despair.

More than forty years ago the American astronomers Kirkwood and Pierce reached the conclusion that a mass so rare as the solar nebula must have been when it was expanded to fill the orbits of the planets, as imagined by Laplace, could not exert hydrostatic pressure so as to detach rings or zones of vapor; and they urged this objection as essentially fatal to the Laplacian theory. Here again the criticism was of the negative kind, like most of the criticism of Laplace's theory which appeared before and since their epoch; and it therefore shared the inherent weakness of all criticism which is not accompanied by work of constructive character. It does little good to break up our mental images if we cannot put better ones in their places; tearing down is easy, but building new structures much more difficult. And so long as the criticism was of purely negative character, it naturally failed to supersede Laplace's theory with a better one, and, by default, the ring theory has continued to hold its ground almost up to the present time.

It is not necessary to go into the details of more recent destructive criticism, except to say that no absolutely conclusive results were obtained until about two years ago, when I recognized from Babinet's criterion that we must unconditionally abandon the historical view that the planets have been detached from the sun and the satellites detached from their several planets. This led to the capture theory, which gives us the true conception of the origin of the solar system, and of other cosmical systems existing in space.

The earliest suggestion of the capture theory runs back a good many years, but it has not heretofore been accepted by professional astronomers familiar with celestial-mechanics, because there was no recognized way in which the very circular orbits could be accounted for, except by the detachment theory of Laplace. It is only since the writer's discovery of the paramount part played by the resisting medium, some two years ago, that the capture theory has taken a form consistent with the established principles of dynamics.

As throwing light upon the earliest notice of the capture theory, we may quote a passage from Professor Barnard's article on "Jupiter's Fifth Satellite" in *Popular Astronomy*, for October, 1893, which I came across in making up the engravings for Volume II. of my "Researches" now in press. Barnard says:

> As was the case when Professor Hall discovered the satellites of Mars, many theories have been offered to account for the presence of the new body (Jupiter's fifth satellite). The asteroid zone between Mars and Jupiter is an endless source of material for such theories. It was suggested in 1877 that the Martian satellites were asteroids captured by Mars from the asteroid zone lying outside his orbit. These same theorizers have not failed to come up again and suggest that the fifth moon of Jupiter is a captured asteroid from the zone of asteroids lying inside the orbit of Jupiter. They never try to account for satellites of Saturn, Uranus and Neptune this way, because the asteroid mine is too far off; yet they are similar bodies, and undoubtedly had a similar origin. There is no question that this satellite has been there all along, and for infinite ages has been performing its revolutions about the planet, undetected until the night of 1892, September 9.

This account by Professor Barnard is exceedingly clear, and of great historical interest. It confirms the statement often made that the idea of capture was first entertained by amateurs. The doctrine has grown, however, from the theory of the capture of comets, which has been a subject of investigation by the most eminent professional astronomers since the celebrated work of Laplace and Burkhardt, about 1805, on Lexell's comet of 1770. It is remarkable that in the course of the past century the capture theory of comets should have been highly developed by Leverrier, Adams, Schiaparelli, H. A. Newton, Callandreau, Tisserand and many others, while the capture of satellites should not have been seriously considered prior to the publication of the writer's "Dynamical Theory of the Capture of Satellites," in the *Astronomische Nachrichten,* Nos. 4341–42 (July, 1909).

The field of research opened up by the new theory is so large that it doubtless will be many years before it is exhausted; yet enough seems to have been done already to assure us that all the satellites are captured bodies. The result of this line of thought will be practically a new cosmogony. In order to render the results intelligible, we shall first outline the process of capture, and then recapitulate the conclusions at which we have arrived.[4]

(α) In the year 1836 the celebrated German mathematician Jacobi communicated to the Paris Academy of Sciences an integral of the differential equations of motion for the restricted problem of three bodies; the system being made up of a sun attended by a planet revolving about it in a circle, and a particle of insensible mass. Jacobi remarks that the integral may be applied to a body such as the terrestrial moon.

(β) In 1877 Dr. G. W. Hill developed and greatly perfected the theory of Jacobi's integral, and applied it to the lunar theory in a series of celebrated papers. Hill's work has since been the basis of the profound researches of Poincaré, Darwin and others on "Periodic Orbits" and related topics in celestial mechanics.

(γ) Dr. Hill showed that in the restricted problem of three bodies, implied in Jacobi's integral, there is a partition of the whole space into three parts—one about each of the large bodies, the sun and planet, and a larger domain enclosing both bodies—within which the power of control over the particle is vested in the two bodies individually and collectively, respectively. The closed surface about the earth includes the orbit of the moon, and the orbits of the other

[4] Cf. *A. N.*, 4341–42, 4343, and Publications A. S. P., No. 127, August. 1909.

satellites in like manner are within the closed surfaces about their several planets; and Dr. Hill remarks that this arrangement is necessary to secure stability. If a satellite is once within this region, with the surface of zero velocity closed about it, it cannot escape, but will always remain attached to the planet, and its radius vector will have a superior limit. How the moon and other satellites came within these closed regions Dr. Hill did not inquire; and subsequent investigators appear to have supposed that as these bodies cannot now escape from their planets, so also they cannot have come in from a remote distance, but must have originated where they now are. This is the view put forth by Moulton in his discussion of Professor W. H. Pickering's suggestion that Phœbe had been captured by Saturn; but such reasoning is easily shown to be erroneous by the following considerations:

(δ) Jacobi's integral, as originally given by him, is based on the differential equations for unrestricted motion in *empty space*, and no account is taken of the additional terms which must be added to the differential equations of the motion of the sun, planet and particle, when the motion is very slightly conditioned by the introduction of a nebular resisting medium, such as existed in the early history of our system, and is now observed to be widely diffused throughout nature. Jacobi's original integral, therefore, requires the addition of a secular term to represent the actual movement of a sun, planet and particle; and the complete expression for any particle whose coördinates are x_i, y_i, z_i becomes

$$x_i^2 + y_i^2 + \frac{2(1-\mu)}{\sqrt{(x_i - x_1)^2 + y_i^2 + z_i^2}} + \frac{2\mu}{\sqrt{(x_i - x_2)^2 + y_i^2 + z_i^2}} = C_i + \alpha_i t_i. \quad (1)$$

The secular term $\alpha_i t_i$ makes the constant C_i increase with the time.

(ϵ) Now the surfaces of zero relative velocity, which define the closed spaces about the planets, have larger values of C the nearer we approach to the sun or planet. This is easily seen in the accompanying plate from Darwin's celebrated memoir on "Periodic Orbits." When the particle or satellite revolves against resistance, therefore, the second member of (1) increases, and there is a secular shrinkage of the surface of zero relative velocity. *Accordingly the particle drops down nearer and nearer these centers, and the surface finally becomes closed, leaving it no longer free to move about both bodies in the hour-glass shaped space, as formerly, but*

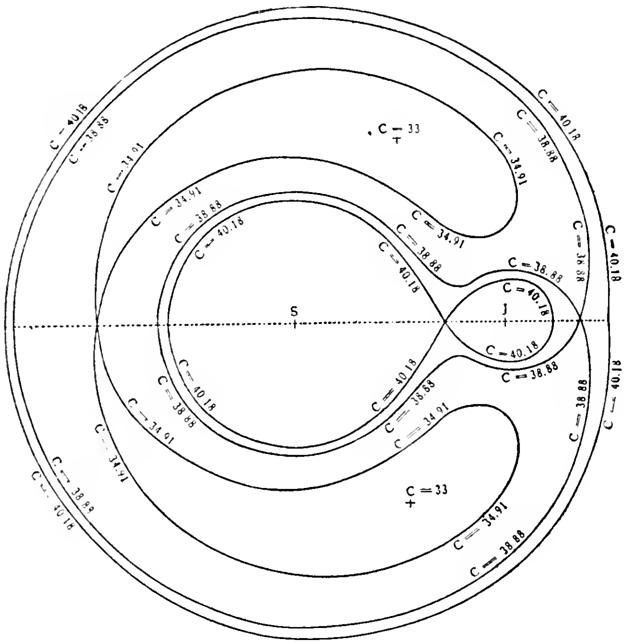


Fig. 1. Curves of Zero Velocity (Darwin). This diagram illustrates the hour-glass shaped space through which the particle may move and drop down nearer the sun or planet, till it becomes captured by one of the larger bodies.

restricted to the sphere of influence controlled by the sun or planet individually, as the case may be. The particle which once revolved about both the sun and planet can no longer do so, but becomes an inferior planet (satellite of the sun) or a satellite of the planet.

It is shown that the mean distance of the planet decreases as well as that of the satellite, but that the action of the resisting medium is relatively more effective on the satellite than on the planet in the inverse ratio of their radii, and therefore the planet approaches the sun very slowly and the satellite very rapidly. Accordingly we may generally ignore the effect on the planet, and consider only the effect on the satellite. It is thus clear how the satellite drops down near these centers of attraction, and is finally captured by one of them.

(ζ) This is how all the satellites of the solar system were captured. At first they moved principally under the attraction of the sun, and could pass from the sun's to the planet's domain, through the neck of the hour-glass shaped space connecting the two spheres of influence. When the neck is narrow, Darwin says that a particle which passes from the sun's to the planet's control may revolve about it hundreds of times before quitting the planet's sphere to return again to the sun's control. And if resistance is meanwhile encountered, so that the neck of the surface of zero velocity becomes closed, it is clear that the particle never will quit the sphere of the planet's control, but will abide there permanently as a satellite.

(η) Thus it incontestably follows that the satellites of Jupiter, Saturn and other planets formerly moved about the sun, and since they were captured have had their orbits reduced in size and rounded up under the secular action of the resisting medium formerly pervading our solar system. *Satellites may cross over the line SJ before coming completely under the planet's control, in which case they will move retrograde.* In such cases the neck connecting the two spaces is extremely narrow. But as the neck usually is not so narrow as to produce crossing satellites, most of them naturally move direct, in accordance with observation. *This is the reason also why the planets have direct rotations on their axes. The planets have in no case been inverted, as some have recently supposed, in order to account for the retrograde motions of the outer satellites of Jupiter and Saturn.*

(θ) In the case of the terrestrial moon it is shown that the earth simply captured one of the twenty-seven million such planets which went to form the sun's immense mass. The moon came to us from the depths of space, and never was a part of the earth, as has long been supposed. Professor Sir G. H. Darwin's celebrated work of 1879 is shown to be based on chance coincidences, and not actual physical history. All the details of the lunar terrestrial system are known to accord with the theory that the moon is a captured planet. (*a*) *It is shown by rigorous calculation from the theory of probability that the chances are infinity to one that the moon was captured like the other satellites.* (*b*) *It is likewise shown that the probability is infinity to one that the earth could not have rotated with sufficient rapidity to detach the moon.* As the theoretical possibility of the capture of the moon is beyond doubt, it is therefore certain that it actually occurred.

With this brief outline of the process of capture, we shall now sum up the conclusions at which we have arrived.

1. The planets never were detached from the sun by acceleration of rotation, as held by Laplace, but had their origin in the outskirts of the solar nebula, and have since neared the sun and had their orbits reduced in size and rounded up into almost perfect circles by moving for long ages against the resisting medium of nebulosity formerly pervading our system.

2. The view that the planets were formed at a great distance from the sun was advanced by the great Swiss mathematician Euler in 1749, before the theories of Kant (1755) and Laplace (1796) were promulgated; and he, too, based his conclusion on the secular effects of a resisting medium. Euler considered only the effect on the mean distance, but in 1805 Laplace showed by a more general investigation that the eccentricity also would be diminished; and it is this latter effect in producing the observed roundness of the orbits that gives us the principal clue to the true mode of formation of the solar system.

3. It therefore follows that the planets were developed in the solar nebula, but are in no sense of the word children of the sun; for they never were connected with the sun by any form of hydrostatic pressure and afterwards thrown off by acceleration of rotation, as has been generally believed.

(4) The satellites likewise never were mechanically connected with their several planets through any form of pressure and fluid equilibrium, but were originally independent planets, moving in regular elliptical orbits, and were afterwards captured and attached to

the planets about which they now revolve as satellites. Thus the satellites are in no sense of the word children of the planets and grandchildren of the sun, as we have been so long taught; but were formed in outer parts of the solar nebula, and simply survive out of a much vaster number of small bodies which have been swallowed up in laying the foundations of the sun and principal planets.

5. The best surviving illustration of the primordial condition of our system is afforded by the asteroids. The system was once quite filled with these satellites, and they revolved in orbits which were so eccentric that they crossed the paths of several of the planets. In time they have been largely absorbed in the sun and planets, and what remain have been thrown within the orbit of Jupiter, where they now revolve in comparative stability.

6. That our moon, likewise, was originally a planet which neared the earth and was finally captured and made a satellite. It was no part of the terrestrial globe detached by rapid rotation, as has been generally believed since the time of Anaxagoras, B. C. 500–428, and more recently taught by Laplace, Lord Kelvin, Sir George Darwin, Poincaré and other eminent writers.

7. The theories long current in geology and astronomy that the earth once rotated in two or three hours, so rapidly that it threw off a layer of peripheral matter now collected into the moon, are shown to be quite devoid of foundation. The effect of this advance will be to show that the globe never was highly oblate, and to correct the theories of geology, and greatly improve the science of astronomy.

8. The rotations of the planets on their axes have been produced by the capture and absorption of satellites. It is shown in the theory of the restricted problem of three bodies that most of the satellites should move direct, and it is by the capture and absorption of millions of these small masses that the planets have been given direct rotations on their axes.

9. The modification of the original obliquities of the planets is shown to depend on the capture and absorption of satellites. It is shown that in this way the obliquity of Jupiter was practically obliterated. If Saturn's mass were to be augmented till it became equal to that of Jupiter, his obliquity likewise would disappear.[5]

[5] Cf. *A. N.*, 4367.

The work of Stratton, implying planetary inversion, is shown to be inapplicable to the solar system, because based on a false premise.

10. As the satellites have all been captured, but were once independent planets, it will not seem strange that a few of them have retrograde motions; the capture theory explains this in a simple and easy manner, and it is in accord with the latest researches on the celebrated problem of three bodies, the treatment of which has been much improved by Jacobi, Hill, Poincaré, Darwin and other mathematicians.

11. As the planetary rotations are due to the capture and absorption of satellites, theory shows that the larger planets, as Jupiter and Saturn, ought to rotate most rapidly. This is in accordance with observation, and the nature of the cause at work shows that the earth never could have rotated much if any more rapidly than at present.

12. The cause of the secular acceleration of the moon's mean motion has been one of the leading problems of astronomy for over two centuries. In spite of all the researches of the greatest mathematicians, there remains an outstanding inequality of about $2''$ which cannot be accounted for by gravitational or other definite theory. This anomaly is now explained by the fact that the moon is a captured planet, and therefore slowly nearing the earth, owing to the action of a resisting medium, in the nature of cosmical dust pervading the regions where the planets move.

13. The roundness of the orbits of the planets and satellites has been remarked from the earliest ages of science, and this phenomenon, which led Plato, Aristotle and other Greek sages to declare that the heavenly motions are perfect because they are circular, is now shown to be due to the secular action of a resisting medium which has reduced the size of the planetary orbits and well-nigh obliterated their eccentricities; and not at all to these bodies having been detached by rotation and set revolving in orbits which were originally nearly circular, as incorrectly held by Laplace.

14. Around each planet there circulates a vortex of cosmical dust, of which the satellites alone are large enough and bright enough to be visible in our telescopes. The descent of this material

against the surfaces of the sun and planets gives rise to the observed but heretofore perplexing accelerations of the equatorial regions of these globes.

15. The descent of matter upon the sun increases its mass and gives rise to a small secular acceleration of the earth amounting to about 0″.75 per century, which, as Mr. Cowell has shown, is also indicated by the observations of the eclipses during the past 3,000 years.

16. And just as the earth never rotated very rapidly, and has not been appreciably retarded by the effects of tidal friction, so also Venus likewise has escaped a corresponding retardation of axial rotation, and still rotates in 23 hours 21 minutes, as has been held by observers since the days of Cassini, 1667. Accordingly it follows that the conditions of this planet are more like those of the earth than any other body of our system. Mars rotates 41 minutes slower than the earth, while Venus rotates 35 minutes faster; and as the former planet is about as much outside of the earth's orbit as the latter is inside, there is seen to be a profound physical cause which has operated to establish the period of 23 hours 21 minutes first inferred from observations taken over two centuries ago. The planet Venus, therefore, is habitable and probably inhabited by some kind of intelligent beings.

17. The moon was once an independent planet, and when revolving in the region of the asteroids, suffered numerous collisions with satellites; this is the cause of the immense, round, sunken craters which have puzzled astronomers since the time of Galileo. The traditional theory that they are volcanic is quite devoid of foundation, and now definitely and finally disproved.

18. The solar system extends much beyond Neptune, and several of the unseen planets revolving near the outskirts of the system may yet be discovered by observation. Neptune's orbit is too round to be the last of the planets, because this roundness indicates that the nebulous medium was quite dense at that distance, and consequently that the limits of the system are much further out.

19. It has long been known that the periodic comets are captured bodies which have suffered transformation of their orbits

under the action of the planets. The theory is now extended to the asteroids and satellites, and these two classes of bodies are shown to be closely related. The survival of satellites near the principal planets shows that our system was once filled with these small masses. Such moons naturally developed in the condensation of a nebula, and all nebulæ include multitudes of solid globes in addition to the gaseous matter shown by the spectroscope.

20. The collisions of satellites with the larger globes, as shown by the battered surface of our moon, give rise to part of the light of the nebulæ, and no doubt also to some of the cosmical dust with which they are filled.

21. The whirling of a spiral nebula is due to the unsymmetrical meeting of two streams of nebulosity, which thus coil up, and settle down under the effects of mutual gravitation; or to the mere gravitational settling of a nebula of unsymmetrical figure. The inevitable result is a whirling cosmical vortex, and eventually a star surrounded by a cosmical system.

22. Nearly all single stars have planetary system revolving about them, and in the immensity of the starry heavens an infinite number of the planets are habitable, and no doubt actually inhabited. Life is almost as general a phenomenon in the universe as matter itself, though our dominant materialistic philosophy is loth to admit it.

23. We can see only visual and spectroscopic binary stars in the sidereal universe, owing to the immense distances which render the smaller bodies wholly invisible in our telescopes; but we know, from the example of the solar system and from the causes which have operated in its formation, that planets and satellites exist everywhere about the fixed stars.

24. In star clusters the motion is shown to be of a spiral character, as in the nebulæ, where we can trace the streams of movement by the nebulosity, and the same theory is applied to the milky way. It is shown by calculation based on the best modern data, that the extent of the sidereal universe actually exceeds the vast dimensions found by Sir William Herschel, and of late years held to be extreme.

It will readily be understood that the lines of argument by which these results are established are of mathematical character, and in accordance with the principles of dynamics. It is shown, for example, that the spirals observed among the nebulæ are *chance spirals*, and not true geometrical figures. And the general theory is established that the nebulæ are formed by the falling together of cosmical dust expelled from the stars by the radiation-pressure of their light and by electric forces. When this fine dust is precipitated under the action of cathode rays it forms meteorites, and the collection of meteorites forms satellites and larger cosmical bodies.

A nebula is a cloud of cosmical dust with moons and planets intermixed. The falling together of such nebulosity necessarily produces cosmical vortices, and these are the whirlpool nebulæ so long observed in the heavens but not heretofore understood. The expulsion of fine dust from the more active stars gives the primordial material for the formation of nebulæ; the condensation of the nebulæ in turn forms stars, so that the total process is a cyclical one, involving both the aggregation of matter into stellar centers and its subsequent diffusion to form new nebulæ, stars and systems.

As the second volume of my "Researches on the Evolution of the Stellar Systems" is a work of nearly 750 pages, it will readily be understood that this summary is too brief to give more than a faint outline of the work. Yet as the results are of general interest to a large class of readers, I have thought they might be mentioned in this brief review.

Our age is a peculiar one, in that with the progress of astronomy vast masses of observational data are accumulated by the persevering industry of self-denying men of science; but so long as these data cannot be put together to yield us the long-sought laws of cosmical evolution, the outcome is almost as vain as the weaving of Penelope's web. Natural philosophers believe, however, that the time is now auspicious for a great advance, not merely in the details, but also in the laws and principles of exact astronomical science. One of the ultimate aims of the physical sciences in all ages has been the discovery of the laws of cosmical evolution.

If with the modern improvements in the mathematical treatment of the problem of three bodies, and the observational data derived from photographic study of the nebulæ and clusters, as well as from the visual and spectroscopic binary stars, this progress be not possible in our time, it is difficult to see how better results can be expected in the future.

I have therefore labored with no ordinary energy to weave together the scattered and discordant threads of argument regarding phenomena which heretofore could not be brought into harmonious relationship. And I have been fortunate enough to attain an unexpected degree of success; so that I cannot doubt that the result will go far towards a permanent solution of the problems of cosmical evolution, which is certainly an urgent *desideratum* of our time.

U. S. Naval Observatory,
Mare Island, California,
April 8, 1910.

THE EXISTENCE OF PLANETS ABOUT THE FIXED STARS.

By T. J. J. SEE.

(*Read April 23, 1910.*)

The question of the existence of planets about the fixed stars is an old one, and has been more or less discussed by astronomers ever since the popularization of Copernican doctrines by Giordano Bruno, who suffered martyrdom at Rome in the year 1600. Up to the present time, however, there has been no rigorous criterion for the construction of a conclusive argument; and the discussion has been comparatively unprofitable, except in the development and expression of free opinion. Disputations leading to the expression of individual opinion may be of some value, because new ideas may thus be suggested, and accordingly such habits have been encouraged since the days of the Greeks, as we learn from the collections of opinions handed down by such writers as Diogenes Laertius.

But to render such efforts effective from a scientific standpoint it is necessary to find criteria which make it possible to build up a conclusive argument. The discussion then ceases to be a mere record of individual opinion, and becomes an integral part of science supported by the necessary and sufficient conditions required to ensure the validity of accurate mathematical reasoning. This improvement in our knowledge of the existence of planets about the fixed stars has been made possible by the writer's recent discoveries in cosmical evolution, and we shall, therefore, give a brief summary of the argument as it stands today.

So long as we did not know the exact process involved in the formation of the solar system it was possible to argue that just as planets exist about our sun, so too, they may by analogy be inferred to exist also about other fixed stars. This natural inference rests on the implied uniformity of the creative process involved in the development of the planets. Obviously we could not observe planets at the great distance of the fixed stars, while the double and multiple stars constitute systems of very different character. There was, therefore, no direct observational evidence that planetary formation was a part of the usual order of nature. The process by which our solar system arose was involved in great doubt and obscurity, and could not be definitely made out, notwithstanding the labors of many eminent mathematicians during the past century. No longer ago than 1906 the late Professor Newcomb declared[1] that he still retained "a little incredulity as to our power in the present state of science to reach even a high degree of probability in cosmogony."

I have recently shown that the principal difficulty in all the efforts of mathematicians for solving the problems of cosmogony has arisen from false premises which had come down from the days of Laplace, and thus vitiated all our reasoning. It had been uniformly assumed that the planets were thrown off from the sun, and that this process of detachment by rotation of the central mass had set them revolving in orbits of small eccentricity. Laplace's postulates in some form or other had been assumed by all investigators since 1796. And it is curious to notice that Laplace in turn had merely extended the conceptions developed by Newton in his treatment of the problem of the figure of equilibrium of a rotating mass of fluid.

For, in establishing the theory of universal gravitation, in 1686, Newton had correctly explained the figures of the earth and other planets as due to the effect of gravitational attraction combined with the centrifugal force due to axial rotation, thus giving various degrees of oblateness depending on the intensity of the forces, and the heterogeneity of the planetary masses. These results followed from the theory of gravitation, and Newton had applied to them the same masterly reasoning which he usually exhibited in the treatment of mathematical problems.

Not long after the epoch of Newton the problem of the determination of the figures of equilibrium of rotating masses of fluid was considerably improved by the researches of Maclaurin, while subsequently Laplace himself extended and confirmed the results

[1] In *Popular Astronomy* for November, 1906, p. 572.

of his predecessors. Though they did not deal with definite cases of extreme oblateness, the great mathematicians of the eighteenth century made it clear that very rapid rotation would be adequate to produce disc-shaped figures of equilibrium.

This subject is quite fully discussed by Laplace in the "Mécanique Céleste,"[2] where the following theorems are announced.

Any homogeneous fluid mass of a density equal to the mean density of the earth, cannot be in equilibrium with an elliptical figure, if the time of its rotation be less than 0.10090 day. If this time be greater, there will always be two elliptical figures, and no more, which will satisfy the equilibrium. If the density of the fluid mass be different from that of the earth, we shall have the time of rotation, in which the equilibrium ceases to be possible, with an elliptical figure, by multiplying 0.10090 day by the square root of the ratio of the mean density of the earth to that of the fluid mass. Therefore with a fluid mass whose density is a quarter part of that of the earth, which is nearly the case with the sun, this time would be 0.20180 day, and if the earth were supposed to be fluid and homogeneous, with a density equal to a ninety-eighth part of its present value, the figure it must take to satisfy its present rotatory motion, would be the limit of all the elliptical figures, with which the equilibrium could subsist.

What Laplace here points out was in fact established by Maclaurin in his "Treatise on Fluxions," Edinburgh, 1742. For if k^2 be the gravitational constant, the density of the mass and ω the angular velocity of rotation, then it was proved that for

$$\frac{\omega^2}{2\pi k^2\sigma} = 0.22467$$

the two possible figures of equilibrium coalesce into one, but for

$$\frac{\omega^2}{2\pi k^2\sigma} > 0.22467,$$

there is no ellipsoid of revolution which is a figure of equilibrium. For very small values of $\omega^2/2\pi k^2\sigma$, there are two distinct ellipsoids which are figures of equilibrium, one of them being nearly spherical and the other very oblate, the limits, for $\omega = 0$, being respectively a sphere and an infinite plane.[3]

[2] Liv. III., Chap III., § 20.

[3] Tisserand's "Mécanique Céleste," Tome II., chap. VI.

If a very flat disc could exist as a figure of equilibrium, it was natural to imagine that such figures might have had a part in starting the planets in their round orbits. The great roundness of the orbits of the major planets and of the satellites then known, and their uniform direction of motion near a common plane suggested to Laplace that these orbits had once been nearly circular, and that the bodies had developed from rings like those of Saturn. It appeared to him that they had resulted from the condensation of rings of vapor gently detached from the equators of the bodies which now govern their motions. This reasoning of Laplace was logical, and necessarily resulted from the researches of Newton on the figures of equilibrium of rotating masses of fluid, and the subsequent extension of these researches by Maclaurin, D'Alembert and other mathematicians; and the procedure seemed so plausible that its correctness was assumed by all subsequent investigators.

Thus Lord Kelvin, Newcomb, Darwin, Tisserand, Poincaré and others accepted the principles of Laplace as laid down in his formulation of the nebular hypothesis, and proceeded to work out the details of planetary development. It is true that Kirkwood, Pierce and others had made objections to the Laplacian hypothesis, based on the inăbility of a medium so rare as the postulated nebula to transmit hydrostatic pressure from the center outwards, but such destructive criticism was of little avail so long as the roundness of the orbits could be explained only by Laplace's hypothesis of detachment. The persistence of Laplace's classic nebular hypothesis, in spite of negative criticism, was therefore inevitable. But as the greatest mathematicians were unable to make out the process of planetary formation, on the detachment theory, the whole subject remained one of contradiction and obscurity. In his address to the British Association at Capetown, in 1905, Professor Sir G. H. Darwin said:

> The telescope seems to confirm the general correctness of Laplace's hypothesis. . . . Nevertheless it is hardly too much to say that every stage in the supposed process presents to us some difficulty or impossibility. Thus we ask whether a mass of gas of almost inconceivable tenuity can really rotate all in one piece, and whether it is not more probable that there would be a central whirlpool surrounded by more slowly moving parts. Again, is there any sufficient reason to suppose that a series of intermittent efforts would

lead to the detachment of distinct rings, and is not a continuous outflow of gas from the equator more probable? (p. 19).

So the matter stood until early in 1908, when the writer took up study of the spiral nebulæ and discovered that the roundness of the orbits of the planets and satellites was due to the secular action of a resisting medium, and not to detachment in orbits which were originally nearly circular, as imagined by Laplace.[4] The novelty of this view of the formation of the solar system is pointed out in a review by a distinguished authority in *Nature* of July 29, 1909, where we read:

> Dr. See contends that the planets and satellites of the solar system were captured and their orbits made remarkably circular by a resisting medium. In his view, therefore, Laplace's nebular hypothesis is altogether wrong, whereas the current view is that it is in the main right, though in need of considerable modification and extension. . . . Capture is a possibility, but Dr. See has done nothing to raise his theory beyond a mere conjecture, even though he points out, in addition, that a resisting medium would diminish the mean distance and the eccentricity of an elliptic orbit, and that in the case of Jupiter's satellites the outer orbits are highly eccentric and the inner orbits nearly circular.

This review was written before the papers on the dynamical theory of the capture of satellites,[5] and the capture of the moon[6] had appeared; so that the claim that I have not explained how capture takes place is no longer valid. On the contrary, this work has now been published nearly a year, and the correctness of it does not seem to be questioned in any quarter.

We shall not in this paper dwell on the work done by the author during the past two years, and embodied in Volume II. of the "Researches on the Evolution of the Stellar Systems," which is soon to appear, but shall merely remark that the Laplacian theory now appears to be permanently overthrown and the capture theory established in its place. It is proved that the embryo planets were formed in the outer parts of the solar nebula, and have been captured and attached to the sun after nearing it from a great distance, while the satellites have been likewise captured by their several planets.

[4] Cf. *A. N.*, 4308.
[5] *A. N.*, 4341–2.
[6] *A. N.*, 4343.

This proof that the planets never were any part of the sun, but have come to it from great distance, is accompanied by an argument based on dynamical principles showing that the same thing will happen for any other star developing in a nebula; and it is, therefore, certain that the other stars have planetary systems revolving about them. The argument is based on the recognized laws of dynamics and verified by the known history of the solar system, and therefore seems to be entirely satisfactory.

The difficulty and uncertainty, as to the existence of planets about the fixed stars generally, appears, therefore, to be overcome; and we conclude that the discovery of the true process of formation of our system enables us to affirm with confidence that nearly all the fixed stars have systems of planets revolving about them. Here is the foundation of the new line of argument.

1. The observed motions of the double stars show that the law of gravitation is universal, and that the same law of attraction that holds true for the bodies revolving about the sun also regulates the motions of the remotest stars.

2. This indicates that the forces are central, and that the same laws of areas and of motion hold there as in the solar system. Similar effects imply similar causes, and hence the double stars have been set revolving by projective forces and other causes analogous to those which set the planets in motion about the sun.

3. These movements resulted from nebular vortices, formed by the settlement and winding up of streams of nebulosity which did not pass through the center, but circled about it.

4. Such is the phenomenon shown in the spiral nebulæ, which are proved to exhibit the usual process in the development of cosmical systems. Nebulæ are formed from cosmical dust expelled from the stars, and it cannot fall to a center to produce a central sun without giving rise to a whirling vortex about that center, since but few of the streams will converge in a point.

5. Planets form in the streams which make up a spiral nebula, and in some cases they unite to form a double star, the system thus developing into a double sun; but in the more typical case the planets are too small to be seen and the stars appear to be single,

whereas in reality they are surrounded by planetary system not unlike that which revolves about our sun.

6. The spiral nebulæ indicate very plainly that the motion of the nebulosity is towards the center; that it was originally at greater distance, but at length captured and brought in nearer and nearer the center by the action of universal gravitation.

7. It is, therefore, plain that just as our planets were formed in our nebula at a great distance from the sun and afterwards had their orbits reduced in size and rounded up by moving against a resisting medium; so also planets revolving and passing gradually into order and stability are developed in the nebulous streams which by condensation have formed the other stars, and this makes possible the formation of planetary systems among the stars generally.

8. We may, therefore, feel entirely certain that the stars which appear to be single—about four fifths of all the stars—have planets revolving about them. And the other fifth are spectroscopic and visual binaries, the planets in this case being so large as to be visible in our telescopes or producing a relative motion in the line of sight which may be recognized by means of the spectrograph.

9. The historical difficulty of determining whether there are planets about the fixed stars may, therefore, be definitely overcome by the recognition of the true mode of formation of the planetary system. It is not exceptional, as was formerly believed. So long as we held that the planets were thrown off, it was not at all certain that the mechanical conditions would permit such detachments elsewhere in the universe, and our solar system might be held to be nearly if not quite unique. Now, however, all such views become inadmissible, and we see that our system is typical of the general order of nature.

10. Accordingly, although the planets about the fixed stars probably will always be invisible, and too small to be detected by the spectrograph, yet it is possible to be quite sure of their existence from the operation of known mechanical laws; and from the demonstrated mode of formation of the solar system. This is a practical application of the capture theory to the larger problems of the universe, and the result is of general interest to every

student of nature. The "Astronomy of the Invisible" thus takes on vastly greater importance than in the time of Laplace and Bessel. And we see that the sagacious suggestions made by these great astronomers nearly a century ago were well founded. And just as there are invisible planets about the luminous fixed stars, so also there probably are countless bodies everywhere in space which are essentially non-luminous. The amount of dark matter in the universe therefore is much greater than has been generally supposed, or heretofore considered probable.

U. S. NAVAL OBSERVATORY,
MARE ISLAND, CALIFORNIA,
March 28, 1910.

ON THE DISTANCES OF RED STARS.

(ABSTRACT.)

BY HENRY NORRIS RUSSELL.

(Read April 23, 1910.)

Comparison of the parallaxes of stars, derived by the writer from photographs taken at the Cambridge Observatory (England) by Mr. A. R. Hinks and himself, and their spectra, determined at Harvard under the direction of Professor Pickering, shows a marked correlation between spectral type and parallax.

If the stars observed are divided into four groups: (1) Those chosen at random for comparison purposes in fields near the Milky Way, (2) similar stars remote from the Galaxy, (3) stars of considerable proper motion, (4) those shown by observation to be relatively very near us (parallaxes greater than 0″.10); then the percentages of stars of the different spectral types in these groups are as follows:

Group.	Color Spectrum.	White.		Yellow,	Orange,	Red,
		A	F	G	K	M
1		30%	25%	19%	26%	
2		6	19	42	29	4%
3				50	38	12
4				10	60	30

The percentage of orange and red stars increases steadily with increasing nearness of the group to our system.

Conversely, if the observed parallaxes of the stars of considerable proper motion are compared with the predictions of Kapteyn's formula, it is found that the means for groups including all spectral types are closely represented, but that the means by spectral types show marked systematic deviations, as follows:

Spectrum.	Number of Stars.	Mean Parallax.		Ratio.
		Observed.	Formula.	
F8	3	0''.042	0''.079	0.5
G	7	0''.029	0''.104	0.3
G5	2	0''.041	0''.098	0.4
K	4	0''.119	0''.103	1.2
K5	5	0''.253	0''.141	1.8
M	3	0''.220	0''.108	2.0

As all the stars under consideration lie within the same limits of apparent brightness, it follows that they are intrinsically fainter the redder they are. The reddest average only one fiftieth as bright as the sun.

On the other hand, it is well known that many bright red stars, such as Arcturus and Antares, are at great distances, and are probably at least one hundred times as bright as the sun.

All this can be explained on the hypothesis (now well established on other grounds) that the reddest stars are the lowest in effective temperature. With the latest data regarding temperature and surface brightness, it appears that the fainter red stars are somewhat smaller, and presumably denser, than the sun, while the brighter ones are very much larger than the sun, and presumably of very small density. The latter class probably represent an early stage in stellar evolution, and the former the latest stage that can be observed. In the intermediate stages the star would be hotter, passing through orange and yellow to white, and back to red as it approaches extinction. On this hypothesis there ought to be two distinct kinds of red and orange stars. The distribution of proper motions among these stars supports this theory, showing that those of given apparent brightness may be divided into two groups—one of intrinsically faint stars relatively near our system, the other of bright stars remote from it.

Princeton University,
April 20, 1910.

SPECIES IN AGAVE.

(PLATES XXXII AND XXXIII.)

BY WILLIAM TRELEASE.

(Read April 22, 1910.)

As Dr. Gray once said, species are nothing but human judgments (he even added very fallible judgments as some of us know to our sorrow), and as such they have changed and may be counted on to change with the minds that frame them, oscillating about the truth in a series of approximations to a definition of the also—but less rapidly—changing forms of living nature. A glance at the work of their makers shows that they have always been obscured by insufficient knowledge of real differentials, and even in the masterly synopses of Linnaeus and other epitomizers too few of them have usually been known to permit characters to be so framed as unquestionably to exclude those to be revealed by the exploration of new regions or by closer study at home. Botanists have rarely been able to build on the work of their predecessors without frequent reference to more than original descriptions, and in their effort to fix types they have been more helped by that uninspiring accumulation of dried plant remains, the herbarium, than by anything else unless indeed it be a well done illustration showing a pre-specific existence of a species—if such an expression may be used notwithstanding a sometimes handy convention that species are not to be sought earlier than the date of their formal binomial christening by the great Swedish naturalist.

Succulents have offered rather more than their share of trouble to those who have undertaken to describe and classify them, for one reason because they usually are, or appear to be, difficult of preservation in the herbarium. The fallacious notion, that, being easily brought in alive so that they may be grown in gardens, they are more surely preserved in this way for reference, may have had something to do also with damping the ardor of herbarium makers.

To this circumstance is attributable the fact that many species of this kind of plants have been described from garden specimens which have disappeared sometimes almost before the ink was dry on their descriptions, and that these have been drawn in many instances necessarily from easily transported rather than representative material. Very naturally, too, garden plants claiming specific recognition in a study of this kind have been accounted for though absolutely nothing was known of their source or origin; and descriptions are sometimes not free from at least the suspicion of being based on foliage of one species—possibly warped in character to ease its assimilation with an earlier description, flowers of a second, and possibly fruit of a third,—superadded in a laudable effort to complete the original account of a vegetating type, itself long lost.

Agave stands well to the front among genera exemplifying these difficulties, and it presents some that are almost its very own, because of having enjoyed a marked if transient garden popularity a little over a generation ago. Linnæus, in 1753, named only four species, two of which are now accredited to other genera. Twenty-five years ago, after excluding a large number of nominal species, Mr. Baker admitted 127 true agaves; and more than 200 species, of which many are nondescripts, must now be admitted to even a conservative list. Over one third of those recognized by Mr. Baker were based on vegetating plants—and on garden specimens at that; and most of the many others that he relegated to a synonymic place, as well as those that he had to lay aside as unidentifiable, had been described from garden plants, often of only a few years' growth.

In a study of the genus to which I have been devoting such time as could be spared for the last ten years, an effort has been made at once to understand the spontaneous representation of this genus—characteristic chiefly of the Mexican plateau, and to exhaust all possibilities of identification with nominal garden species before accepting as new to science even the most striking form met with in nature. During the fad for cultivating agaves, beginning about forty years ago, large prices were sometimes paid for good specimens. Although dealers and collectors never showed an undue zeal to reveal the location of the mines from which they were drawing wealth, the fact that the traffic was large and profitable is responsible for the preservation of some disjointed scraps of information that may now and then be pieced together into clues that show where some of the most extensive and varied collections were made, and thus indirectly ensure reference to wild plants for species based on long lost garden specimens. It is with no small satisfaction that by means of such a devious argument I have been able to follow in the footsteps of the collector Roezl, and to understand at least a part of the species of his collecting that had otherwise passed into troublesome uncertainty; and no opportunity has been missed to examine the precise locality indicated as having been visited by botanists whose writings or collections have entered into the history of the genus—as, for instance, the lava beds on which Schiede found one of its most persistent stumbling blocks, *Agave lophantha,* and one of its good but long-discarded other species, *A. obscura.*

In such a study, essentially an honest effort to see and account for the forms actually presented by nature, so that others may see and know them, one must always be moulded by the times that he lives in. If unable to apply a unit-character criterion in discriminating between species, I find myself equally unable to adopt so broad a gauge for their measure as to join under one name the manifold West Indian forms in which so good a botanist as Grisebach could seen only the century plant. I find, however, that in these plants, long-lived, slow-growing, and even in the field most commonly seen only in their vegetative dress, a successful study calls for attention to minutiæ that, being unnecessary for segregations in most groups, receive there less attention than they really merit, and are often looked on with suspicion when used. Ascertaining the stability and significance of these proves at once a fascinating and disturbing part of the study.

In fruit, flower, pedicel, bract and scape characters, the agaves do not offer variation or differentiation very unlike what is usual in other genera of equal range and size; but in many cases in which such characters are still unavailable, those drawn from the leaves, and utilized in the description of species from young garden specimens a generation ago, prove, within limits, entirely dependable. A few illustrations—from very many that might have been selected—will render this clear:

If the end-spine of *Agave americana*—as it occurs in our gardens everywhere, green, striped, yellow-margined or yellow-centered—had been attentively studied years ago, as it has been recently, in comparison with that of the now almost equally common yellow-margined *A. picta*, the former species would never have been made to include the latter (Pl. XXXII.). The spines of gray henequen (*A. fourcroydes*) and green sisal (*A. sisalana*) supplement other characters in segregating these constituents of what is still too often called *A. rigida;* and in this respect *A. angustifolia* differs so greatly from either as to make one who knows the differences wonder how, under whichever of its aliases it was encountered, it ever could have entered into this same modern complex called *rigida* (Pl. XXXII.). Three groups—superspecies, they might be called—of the now economically interesting zapupe agaves are distinguishable from one another, even to the touch, in this same character (Pl. XXXIII.),[1] and each group falls into species on its marginal arming. The likewise important group of mezcals grown for the production of Tequila spirits, known to science in one compositely described species (*A. tequilana*), shows a similar differentiation into an even greater number of forms (Pl. XXXIII.); and many of the great maguey forms grown all over the Mexican tableland for the production of pulque are unmistakably distinguishable on their spine and prickle characters.

These examples, I trust, may justify the devotion of a somewhat lengthy prologue to the argument that small things are not to be despised; or to a short epilogue drawing the conclusion that the arming of an *Agave* is no less significant in species discrimination than the disarticulation of the sepals of an apple, and that, in fact, neither stands alone.

Until within very recent years, few herbaria have possessed more than one or two leaf fragments of a given *Agave,* and where more than one occurred the chances were good that they were not co-

[1] *Trans. Acad. Sci. of St. Louis,* XVIII., no. 3, May, 1909.

specific, even if either of them might properly bear the name attached to it. Jacobi clearly saw the value of spine characters in this genus, in his study of its representatives in the garden collections of his day, and he applied them as consistently as he could in his descriptions; but unfortunately the material on which these were based was often immature and the descriptions are too frequently generalized. Of the many illustrations of *Agave*, sometimes exquisitely colored, almost none approximates the truth in these details any more closely than a studio-made volcano approaches the true declivities of its cinder-cone and foot-slope. Notwithstanding all of its defects—some of them very real and serious—photography now ensures the truthful picturing of minutiæ that the eye of the describer may mistake and the pencil of the delineator is quite likely to misrepresent. Even by its aid, however, part truths may appear as truths, and a real fact may enter as an unreal specific character. It is for this reason that my own conception of specific identities and differences in this genus oscillates as my study proceeds: the leaf characters of a first specimen being most commonly ignored until more and different material forces their recognition; but with increasing evidence that, chosen from mature leaves of adult plants, and used with judgment, they are dependable.

Obviously, no species in any group can be considered as fully defined until all of its characters are known; and no species is satisfactorily described until its characters have been tersely brought together in contrast with those of its allies, as is painfully evident whenever a new form is confronted with published descriptions, which may be equivalent to saying that no species can be satisfactorily described until all of its closely related congeners are known. Obviously, too, species based on incomplete material are more likely to prove capable of ultimate subdivision than those of which all characters are represented when the first description is drawn; and in placing reliance on minutiæ, more than the usual need exists for using these with the judgment derivable only from experience, and for selecting with the greatest care their adult state. With such care, and subject to these restrictions, the species of this difficult genus, even in their vegetating form, appear to be capable of clear delimitation; and in their essentials they show evidence of being much less mutable than they are commonly supposed to be when judged by the impression that they make on the untrained eye, or when differences in habit are given undue emphasis.

Explanation of Natural-size Illustrations of Agave Spines.

Plate XXXII. A, Three spines of *Agave americana.* B, Three spines of *A. picta.* C, Three spines of *A. sisalana.* D, Three spines of *A. fourcroydes.* E, One spine of *A. angustifolia.*

Plate XXXIII. F–J, Two spines of each of the "zapupe" agaves: F, *A. zapupe;* G, *A. Deweyana;* H, *A. aboriginum;* I, *A. Lespinassei;* J, *A. Endlichiana.* K–N, Spines of Tequila mezcals—two each except the lowest: K, "azul"; L, "ziguin"; M, "mano larga"; N, "Chato."

GROUPS GENERATED BY TWO OPERATORS EACH OF WHICH TRANSFORMS THE SQUARE OF THE OTHER INTO A POWER OF ITSELF.

By G. A. MILLER.

(Read April 23, 1910.)

Two special cases of the category of groups defined in the heading of this paper have been considered, viz., when the square of each of the two generating operators (s_1, s_2) is transformed either into itself or into its inverse by the other.[1] In each of these cases it was found that the orders of s_1, s_2 do not have an upper limit. It will be found that both of these orders must always have an upper limit unless at least one of these operators transforms the square of the other either into itself or into its inverse.

The given conditions give rise to the following equations:

$$s_1^{-1}s_2^2s_1 = s_2^a, \quad s_2^{-1}s_1^2s_2 = s_1^b.$$

If at least one of the two numbers a, b were odd the order of the corresponding operator would be odd, and hence the group (G) generated by s_1, s_2 could be generated by a cyclic group of odd order and an operator transforming this group into itself. As many of the properties of such a group are known we shall confine our attention in what follows to the cases when both a and b are even numbers, and hence we shall assume that the conditions under consideration are written in the form

$$s_1^{-1}s_2^2s_1 = s_2^{2\alpha}, \quad s_2^{-1}s_1^2s_2 = s_1^{2\beta}.$$

Some fundamental properties of s_1, s_2 may be deduced from these equations in the following manner:

$$s_1^{-2}s_2^2s_1^2 = s_2^{2\alpha^2}, \quad s_2^{-2}s_1^2s_2^2 = s_1^{2\beta^2}, \quad s_2^{-2}s_1^{-2}s_2^2 = s_2^{2(\alpha^2-1)}s_1^{-2},$$

$$s_1^{-2\beta^2} = s_2^{2(\alpha^2-1)}s_1^{-2}, \quad s_1^{2(\beta^2-1)}s_2^{2(\alpha^2-1)} = 1.$$

[1] Cf. *Paris Comptes Rendus*, Vol. 149 (1909), p. 843.

From the last equation it follows that each of the two operators $s_1^{2(\beta^2-1)}$, $s_2^{2(\alpha^2-1)}$ is invariant under G and therefore it results from the first set of equations that

$$s_1^{2(\beta^2-1)} = s_1^{2\beta(\beta^2-1)}, \quad \text{or} \quad s_1^{2(\beta-1)(\beta^2-1)} = 1 = s_2^{2(\alpha-1)(\alpha^2-1)}.$$

By combining the last equation with

$$s_1^{2(\beta^2-1)} = s_2^{2(1-\alpha^2)}$$

it results that

$$s_1^{2(\alpha-1)(\beta^2-1)} = 1 = s_2^{2(\beta-1)(\alpha^2-1)}.$$

By transforming s_2^2 by $s_1^{2(\beta^2-1)}$ and s_1^2 by $s_2^{2(\alpha^2-1)}$ it is clear that the orders of s_1, s_2 must divide respectively

$$2(\beta^{2(\alpha^2-1)} - 1) \text{ and } 2(\alpha^{2(\beta^2-1)} - 1).$$

The orders of s_1, s_2 must therefore divide the highest comon factor of

$$2(\beta - 1)(\beta^2 - 1), \quad 2(\alpha - 1)(\beta^2 - 1), \quad 2(\beta^{2(\alpha^2-1)} - 1)$$

and

$$2(\alpha - 1)(\alpha^2 - 1), \quad 2(\beta - 1)(\alpha^2 - 1), \quad 2(\alpha^{2(\beta^2-1)} - 1)$$

respectively.

The subgroup (H) generated by s_1^2, s_2^2 is evidently invariant under G and the corresponding quotient group is dihedral. As the commutator subgroup of H is composed of invariant operators under G the fourth derived of G is always the identity.

§ 2. *Groups generated by two operators which satisfy one of the following sets of conditions:*

$$s^{-1}s_2^2s_1 = s_2^4, \quad s_2^{-1}s_1^2s_2 = s_1^4; \qquad s_1^{-1}s_2^2s_1 = s_2^{-4}, \quad s_2^{-1}s_1^2s_2 = s_1^{-4}.$$

The first set of relations implies that $\alpha = \beta = 2$. Hence it results from the preceding section that the orders of s_1, s_2 must divide 6. If these orders are 6 H is of order 9 and s_1s_2 transforms the operators of H into their inverses. Hence s_1s_2 is of even order. If this order is 2 the group generated by H and s_1s_2 is of order 18, and it is completely determined by the facts that it contains the non-cyclic group of order 9 and an operator which transforms each operator of this non-cyclic group into its inverse. In this case G

is of order 36 and such a G can clearly be generated by the two substitutions

$$s_1 = abc \cdot de, \quad s_2 = ab \cdot def.$$

As we may annex to these substitutions any constituents of order 2 in new letters it results that the order of s_1s_2 is an arbitrary even number and hence the order of G is an arbitrary multiple of 36. To prove that there is only one such group of a given order the following considerations are helpful. The cyclic group generated by $(s_1s_2)^2$ has only the identity in comomn with H since s_1s_2 transforms each operator of H into its inverse. This cyclic group is invariant under G. In fact each of its generators is transformed into itself by half the operators of G and into its inverse by the rest of these operators since $(s_2s_1)^2 = (s_1s_2)^{-2} = s_2^{-1}s_1^{-1}s_2^{-1}s_1^{-1} = s_2^2s_2^{-2} \cdot s_2^{-1}s_1^{-1}s_2^{-1}s_1^{-1} = s_2s_1^{-1}s_2s_1^{-1}$. It is now clear from the general theory[2] of simply isomorphic groups that if s_1, s_2; s_1^1, s_2^1 are two pairs of operators satisfying the given conditions and if they generate groups of the same order these groups are necessarily simply isomorphic. Hence the theorem: *If each of two operators of order 6 transforms the square of the other into its fourth power these operators may be so selected that they generate a group whose order is an arbitrary multiple of 36 and there is only one group of each such order which can be generated by two of its operators of order 6 satisfying the given conditions. The second derived of each of these groups is unity.*

When the order of only one of the two operators s_1, s_2 is 6 that of the other must be 2, since it transforms the square of the former into its inverse. In this case H is the cyclic group of order 3 and the order of G is an arbitrary multiple of 12. The second derived of all of these groups is the identity since $(s_1s_2)^2$ and s_1^2 generate an invariant abelian group and the corresponding quotient group is the four-group. When the order of s_1 is 3 that of s_2 must be 2 and G is the symmetric group of order 6. If s_1, s_2 are both of order 2 they may be so selected as to generate an arbitrary dihedral group. Combining these results we have that when s_1, s_2 are both of order 2 they may generate one and only one group of

[2] *Bulletin of the American Mathematical Society*, Vol. 3 (1897), p. 218.

a given order and this order is an arbitrary even number greater than 2; when one of these operators is of order 6 while the other is of order 2 they may generate one and only one group whose order is a given multiple of 12; when both of them are of order 6 the order of the group generated by them is a multiple of 36 and they may be so selected as to generate a group whose order is an arbitrary multiple of 36, but each such group is completely determined by its order.

If we consider all the possible groups which can be generated by two operators satisfying the two conditions under consideration it results from the above that there are exactly three such for every order which is a multiple of 36, one of these is dihedral, another is generated by an operator of order 6 and an operator of order 2, while the third is generated by two operators of order 6. When the order is divisible by 12 but not by 36 there are two and only two such groups, while the dihedral group is the only one that can be generated by two such operators whenever the order is any other even number greater than 2.

When s_1, s_2 satisfy the relations $s_1^{-1}s_2^2s_1 = s_2^{-4}$, $s_2^{-1}s_1^2s_2 = s_1^{-4}$ their orders must divide 18 according to the general formulas of the preceding section. The following substitutions prove that these orders may be exactly 18:

$$s_1 = acegibdfh \cdot jk, \quad s_2 = abidceghf \cdot lm.$$

As $s_1s_2 = agd \cdot beh \cdot jk \cdot lm$ the order of the group generated by these two substitutions is 108. The smallest group that can be generated by two operators of order 18 which satisfy the given conditions is of order 54 and such a group is clearly generated by the two substitutions $acegibdfh \cdot jk$, $abidceghf \cdot jk$.

In general, s_1^3 is commutative with s_2^2 and hence with every operator of H. Similarly, it may be observed that s_2^3 is commutative with every operator of H. From this it results that every operator of the dihedral group generated by s_1^9, s_2^9 is commutative with every operator of H. It is also clear that these groups can have only the identity in common since the former contains no invariant operator of odd order. It therefore results that G must involve the direct product of H and this dihedral group. As these two groups clearly generate G it results that G is this direct product. That is, *if two operators of order* 18 *satisfy the conditions* $s_1^{-1}s_2^2s_1 = s_2^{-4}$, $s_2^{-1}s_1^2s = s_1^{-4}$ *they generate the direct product of a dihedral group and a certain non-abelian group of order 27. Moreover, every such direct product can be generated by two operators of order* 18 *satisfying the given conditions.*

If only one of the two operators s_1, s_2 is of order 18 the other must be of order 9 since s_1^2, s_2^2 cannot be commutative. That is, if an operator of order 18 and an operator whose order is not 18 satisfy the conditions $s_1^{-1}s_2^2s_1 = s_2^{-4}$, $s_2^{-1}s_1^2s_2 = s_1^{-4}$ these operators must generate the direct product of the group of order 2 and a certain group of order 27. When s_1, s_2 are both of order 9 they generate a group of order 27, and when both are of order 2 they generate a dihedral group. Combining these results we have that if two non-commutative operators satisfy the two conditions $s_1^{-1}s_2^2s_1 = s_2^{-4}$, $s_2^{-1}s_1^2s_2 = s_1^{-4}$ their orders have one of the following pairs of values 18, 18; 18, 9; 9, 9; 2, 2. In the first and last of these cases G may be any one of an infinite system of groups; viz., any dihedral group in the last case, and the direct product of such a group and a certain group of order 27 in the first case. In each of the other two cases there can be only one group, viz., the non-abelian group of order 27 which involves operators of order 9 in the third case, and the direct product of this group and the group of order 2 in the second case.

UNIVERSITY OF ILLINOIS,
December, 1909.

THE GREAT JAPANESE EMBASSY OF 1860.

A Forgotten Chapter in the History of International Amity and Commerce

By PATTERSON DUBOIS.

(Read April 21, 1910.)

The American trader with Japan, the traveller or sojourner in Japan, and the national representative or envoy to Japan, all find their transactions with the Japanese greatly facilitated by certain American ideas, patterns and methods now firmly rooted in Japanese practise.

First and foremost, the money system corresponds closely to that of the United States and the coinage itself is convenient, exact and trustworthy. In close accord with this, the American feels himself comparatively at home with the banking, postal and telegraph facilities, in public school aspects, and finally, he takes comfort in the assurance of an up-to-date dentistry and surgery.

But the international American little realizes how all these essentials of modern western life came to be imported from his own country and adopted by the keen-eyed Japanese as indispensable models for the Meiji or "enlightened rule."

The historic fact has dropped into almost total oblivion. This practical Americanizing of Japan harks back to the well-nigh forgotten visit of the great Japanese Embassy to the United States in 1860.

After the first assurance of friendly relations or amity, the rock-bottom of a stable, thriving and reciprocal commerce between two nations is to be found—as already indicated—in a scientifically exact and trustworthy coinage and system of exchange. Of this, more later. But first, as to the historical setting of one of the most picturesque and potent of all international events.

Americans delight to shout themselves hoarse in the praise of

Perry. We rightly glorify the tactful, gallant and picturesque manner in which he punctured the screen which shut out the western world from the eye of the Rising Sun. We have a sufficient feeling that he took the initative in securing friendly, to be followed by trade, relations in advance of all other nations—whatever slippery foothold the Portuguese, English and Dutch had at one time or another acquired only to lose again. We are sure enough that Japan's introduction to the circle of commercial nations was an American performance and that Perry took the initiative hand in it.

And there we stop. We know that American training goes back to Japan in the person of many a university graduate and we know of other influences, military, naval, industrial, commercial educational, professional, ethical, religious, which Japan has sought and obtained from both Europe and America. But we do not seem to realize that to certain impressions made upon the barbaric and wondering embassy of 1860 may be traced definite and continuous lines of development through the vitalizing of more or less latent Japanese powers and ideals. The work which Perry began in 1853–54 was only completed in the visit of the embassy of 1860 to this country. Indeed the embassage is the one signal factor in making the Perry incident permanently effective. Nor was it completed in the signing of the treaty in Washington, whatever might be officially held to the contrary. It is safe to say that the Japanese *learned more which has proved germinal in their re-making,* from their visit to the United States—but especially to Philadelphia, immediately following the diplomatic formalities in Washington, in 1860, than from any other one American source since Perry anchored in the Bay of Yeddo. Indeed I propose here to show such a historical continuity and persistence of certain formative elements of modern progress now accepted as the fixed order of things in Japan, and even incipiently in China, as can be traced to no other visible historical source than the embassage of 1860. And this, notwithstanding the complete revolution in the government and policy of Japan eight years later.

And yet, as I said at the outset, this embassy seems to have passed almost into oblivion. The president of the American Asiatic Association in an article (1907, *The Outlook*) speaks of it as "one of several missions which about that time were sent by the Shogunate to other countries." In a general sense this is true, but more strictly the truth is that in Townsend Harris's negotiation of the commercial treaty in 1858 the Americans insisted that the first embassy to go out from Japan to any country should visit the United States first of all.

An English authority, Mr. J. Morris, in his able book, "Makers of Japan,"—to which I make acknowledgment—admits that "Modern Japan dates from the advent . . . of the American squadron under Commodore Perry in 1853;" and that the Perry compact was "the thin edge of the wedge." And yet he gives no indication of any knowledge of the embassy of 1860 for in noting the mission of the Marquis Ito abroad in 1871, he says that his was not the first embassage, "for a mission of two feudal barons had been sent to Europe in the early 'sixties."

Even admitting that this embassy was one "of courtesy" merely, as Dr. Griffis and others say, we shall see that something more vitalizing and lasting than courtesy grew out of it.

That the embassy of 1860 should have dropped so completely out of sight as an international event will doubtless lead many to conclude that at best it was but a spectacular fizzle entirely wanting in constructive elements or continuing effects. Others will remind us that from the invasion of Perry up to and after the first foreign embassage all intercourse was through the Shogun or "Tycoon" whose rule was becoming rapidly challenged and his authority in a large part of Japan denied. But the embassy was neither a mere courtesy, a spectacular fizzle nor was it of temporary import. Nor does the subsequent overthrow of the Shogun *in any degree vitiate the contention that specific visible lines of Japanese progress emerge from the personal presence of the "princes" and their retinue in the United States in 1860.*

On the part of Japan all international adjustments at this period were made by authority of the Shogun—better known at that time in the West as the "Tycoon." And who was the Shogun, or what was the Shogunate? He was a military commander-in-chief, who had won his arbitrary power—or held it—as the strongest of the feudal barons. He was *de facto* ruler of Japan, his government being centered at Yeddo (now Tokio). The ruler *de jure* was the Mikado whose position was that of a sort of deified headship and an inert nominality.

The Shogunate began with the house of Tokugawa in 1603. It ended by the voluntary resignation of Prince Tokugawa Keiki in 1868 in the interest of national preservation and progress. For nearly two and a half centuries the Shogunate was the personification of a military supremacy in one of the most complete and exacting feudal systems known to history.

It was the Shogun Iyesada who became the treaty maker of 1854. It was the Ten-shi, or Mikado, the ruler *de jure*, Komei, who, under pressure from the Shogun's opposers, rallied his nerves to the extent of refusing to ratify these treaties even though he ultimately gave way. But he had a large official and popular backing. In fact the double-headed rulership had become a source of strife which threatened to disrupt the country. The opening of the door to the foreigner and the introduction of foreign methods were fast becoming the national dispute.

In spite of the expulsion of all foreigners, except a few severely restricted Hollanders, and the massacre of Christians in the seventeenth century, in spite of a universal espionage and lips sealed to foreigners, in spite of barbaric military standards and codes of honor—spears and swords outranking firearms—the scent of western enlightenment gradually penetrating the air quickened a new consciousness of unrealized power. Feudal rule had for some years given rise to murmurs and calls were heard for a full restoration of the Ten-shi, or Mikado, to real power. So Iyesada's treaties were made a plausible ground of opposition to the Shogunate even by the progressive clans who had introduced a number of foreign inventions. For the *purpose of getting rid of the Shogunate* the slogan of these very progressionists was *the expulsion of the alien.*

The Shogun adhered to his policy of admitting the alien (first forced by Perry) but as soon as the progressive clans succeeded in overthrowing the Shogun's supporters they at once advocated the policy of opening the country to strangers, also. It is important to note this as it goes to show that although the embassy of 1860 was sent out by the boy Shogun, Iyemochi, yet the American impress which it carried back was in accord with that progression which ultimately triumphed even though the Shogunate was abolished.

Perry landed his men at the little village of Uraga in the Bay of Yeddo in July, 1853. For two hundred and thirty years no stranger had entered the feudal empire of the Rising Sun. Perry delivered, through messenger, President Fillmore's letter and sent word to the Shogun Iyeyoshi that he would return the next year for an answer. When Perry returned in February, 1854, Iyeyoshi was dead and Iyesada reigned in his stead, and the treaty of amity was signed, March 31, 1854, opening two ports to us, Shimoda and Hakodadi, for trade and all ports for ships in distress. As a matter of fact Shimoda was not opened but Kanazawa was selected as the first port to be opened to trade.

In 1858 the Shogun Iyesada died and was succeeded by a fifteen-year-old boy Shogun, Iyemochi, and his regent prince; and it was under this rule that the embassy of 1860 was sent to the United States. This period suffered from internecine strife on questions of alien influence, feudalism and dynastic ambition. Moreover, the Mikado was being pressed by certain feudal lords to close the ports, abrogate the treaties and expel the strangers. But the foreign powers were also pressing the regent to stand by the agreements. Then, just after he had sent out the embassy, the regent was assassinated. This was in March, 1860. The country was in a ferment but the great embassy was on its way across the Pacific.

The thin edge of the wedge inserted by the Perry compact in 1854 was chiefly one of amity and of hospitality to our seamen. Nevertheless it was to pave the way for closer commercial relations, through another treaty. This latter treaty was negotiated by our Consul-general Townsend Harris, July 29, 1858.

Mr. Harris had raised the United States flag over an ancient Buddhist temple at Shimoda September 44, 1856, and established our legation there, being "the first of the diplomatic representatives of foreign powers," as Morris concedes, "to dwell in the newly awakened Land of Sunrise and the first to arrange a treaty of commerce." Harris had a hard time of it. President Pierce sent him out in 1855. He remained in seclusion for nearly two years at Shimoda—the tip end of the southeastern coast—before he could get an audience with the Shogun. Until he could do it in person he refused to deliver the President's letter. For about eighteen months he was without news from home—receiving neither letter nor newspaper. He entered Yeddo in November 1857, gained his long insisted-upon audience December 7, 1857, spent some months in parley and finally signed the treaty of commerce July 29, 1858. This treaty became the model for others between Japan and European nations. It is interesting, too, that when the British Earl of Elgin arrived for a similar purpose, Harris "lent" his secretary, Mr. Hewsken, to act as interpreter for the British envoys. Lord Elgin negotiated a treaty similar to Harris's and it was signed August 26, 1858. Thus the first British treaty was signed about six months after Perry's and the second one was signed one month after Harris's.

Now the agreement with Harris was that his treaty should be ratified in Washington. Under our treaties, however, no Japanese envoy was to go out until after America had been officially visited. After deferring the dreaded event as long as possible the Japanese finally applied to the United States for a man-of-war to transport the envoys.

It was on March 27, 1860, that the United States man-of-war *Powhatan* arrived at San Francisco, carrying the ambassadors and their immense retinue. After a few days of dining and sight-seeing the visitors reëmbarked for Washington by way of the isthmus—this being nine years before our east and west coasts were united by rail.

The United States man-of-war *Roanoke* awaited the orientals at Aspinwall (now Colon), a flourishing seaport and the Atlantic-side terminus of the forty-nine-mile railway connecting it with Panama, the entrepôt of the Pacific side. Aspinwall was on the *qui vive.* In anticipation the United States flag officer courteously invited the

British Rear-Admiral Sir Alexander Milne to come on board the *Roanoke* when the embassy should arrive. The invitation was declined, the rear-admiral subsequently remarking (so reported) that it was "a great farce, foolish and nonsensical." Nor would he raise his flag, though every other vessel did. His attitude gave the Japanese an unfavorable impression of the English nation—so reported later by one of the retinue.

Under command of Captain S. F. Dupont the ship *Philadelphia* left Washington, May 11, for Hampton Roads where the embassy was to be officially received. On May 12, at 9.30 p. m., the *Roanoke* arrived in the roadstead. She was boarded by Capt. Dupont; Capt. Taylor of the Marine Corps; Mr. Ledyard, son-in-law of the Secretary of State; Mr. Portman, the Dutch interpreter; Commander Lee (brother of Robert E. Lee and father of the late Fitzhugh Lee); Lieut. David D. Porter (later, commanded the expedition against Fort Fisher, later, Admiral); Mr. McDonald, invited guests, and reporters. After the formalities of presentation in the cabin of the *Roanoke* the treaty itself was exposed to view. Up to the present time it had been kept wrapped in a case of red cloth and sacredly secured in a superb lacquered chest about three feet long, eighteen inches wide, and twenty-six inches deep, never out of sight of a guard, and transported by poles resting on the shoulders of four men.

Two days later, May 14, the transfer of the entire company, box, bag and baggage, from the *Roanoke* to the *Philadelphia* was completed. The envoys and retinue of all grades numbered no less than seventy-six persons—the upper ranks gorgeously arrayed and plentifully begirt with swords. The luggage filled four cars on the Panama railroad. There were fifteen boxes of presents for President Buchanan and others. There was a beautiful "Sharpe's rifle" made by the Japanese as an improvement on the real American Sharpe presented to the Japanese by Commodore Perry six years earlier. The Japanese improvement consisted in an arrangement for cocking, priming and cutting off the cartridge, all at once. This has gone by now, but it was a forecast of Japanese aptitudes which we have seen illustrated in the late war. The money which the orientals brought bulked immensely, too, for it consisted of Mexican silver and United States half-dollars.

Under command of Dupont the *Philadelphia* proceeded at once to Old Point Comfort, where for the first time Fort Monroe submitted to an almond-eyed inspection. There were no snap-shot cameras in those days, but the foresighted orientals had brought with them alert and skilled artists who immediately busied themselves making sketches of our boasted stronghold. And indeed these deft wielders of the brush were thus busy throughout the entire sojourn of the embassy among us. Who shall say that the many cartoons of things American which were thus officially carried back to Japan are not to be counted among the germs of a later expansion? Who knows but that these pictures helped to leaven the motif, eight years later, of the Emperor's edict at his crowning—"The bad customs of past ages shall be abolished and our government shall tread in the paths of civilization and enlightenment. We shall endeavor to raise the prestige and honor of our country *by seeking knowledge throughout the world.*"

On the same day, May 14, the embassy was received at the Washington Navy Yard by the commandant, Capt. Franklin Buchanan—the man who, less than two years later, commanded the *Merrimac* in her destructive work, in which she was finally vanquished by the little *Monitor*.

The landing of the embassy at the Washington Navy Yard was a brilliant and imposing spectacle. From the navy yard the orientals were driven under military and civic escort to Willard's Hotel—then the center of Washington's social gravitation. It is not necessary to the argument to enlarge upon diplomatic details. Suffice it to note that the ambassadors and attaches, eight in all, on May 16, were driven to the Department of State, where letters were presented to Secretary Cass in Japanese, Dutch and English. All communication was done through two interpreters—Namoura-Gohajsiro, the Japanese who spoke Dutch, and Mr. Portman, the Hollander who spoke English.

The next day, May 17, the ambassadors presented the Tycoon's greeting to the President, of which the following was the published translation:

To His Majesty, the President of the United States of America. I express with respect: Lately the Governor of Shimoda, Insooye-Sinano-no-Kami and the Metske-Iwasi-Hego-no-Kami (Prince of Idzu), both Imperial Commissioners, had negotiated and decided with Townsend Harris, the Minister Plenipotentiary of your country, an affair of amity and commerce, and concluded previously the treaty in the city of Yeddo. And now the ratification of the treaty is sent with the Commissioners of Foreign Affairs, Simme-Buzen-no-Kami, and Minagake-Awzi-no-Kami to exchange the mutual treaty. It proceeds from a particular importance of affairs and a perfectly amicable feeling. Henceforth the intercourse of friendship shall be held between both countries and benevolent feeling shall be cultivated more and more and never altered. Because the now deputed three subjects are those whom I have chosen and confided in for the present post, I desire you to grant them your consideration, charity and respect. Herewith I desire you to spread my sincere wish for friendly relations and I also have the honor to congratulate you on the security and welfare of your country."

(It would be easy to improve the English of this translation, but I give it as it was rendered at the time.)

The third envoy, unnamed in this letter, was Oguri-Bungo-no-Kami, and was known as the censor (or spy); following in order were the treasury officer who had full authority over the finances of the embassy, the governor (or executive manager), aids, interpreters, doctors, guards of the treaty box, and servants including "spies"—in all, seventy-six. The three "princes" who head the list as ambassadors were of equal rank with those who negotiated the treaties with Perry and Harris. They were not hereditary princes of the blood but Samurai members of the Tycoon's foreign affairs council.

It will be remembered that when Harris signed the treaty of commerce in 1858 it was stipulated that the ratifications should be interchanged in Washington. The formalities of the ratification of Harris's treaty were now in order. That this was a matter of moment to both nations and regarded as more than a picturesque affair of good feeling will become apparent. This is what President Buchanan said about it in his annual message to Congress seven months later (1860):

The ratifications of the treaty with Japan concluded at Yeddo on the 29th July, 1858, were exchanged at Washington on the 22nd May last and the treaty itself was proclaimed on the suceeding day. There is good reason to expect that under its protection and influence our trade and intercourse with that interesting people will rapidly increase.

The ratifications of the treaty were exchanged with unusual solemnity. For this purpose the Tycoon had accredited three of his most distinguished subjects as envoys extraordinary and ministers plenipotentiary who were received and treated with marked distinction and kindness both by the government and people of the United States. There is every reason to believe that they have returned to their native land entirely satisfied with their visit and inspired by the most friendly feelings for our country. Let us ardently hope, in the language of the treaty itself, that "there shall henceforward be perpetual peace and friendship between the United States of America and His Majesty the Tycoon of Japan and his successors."

Three weeks of state formalities, sight-seeing, and social functions—including the President's dinner on May 25, brought this first stage of the embassage to a close by a formal leave-taking in the blue room of the White House, June 5, at which a large gold medal was presented to each of the princes. But is this all? Is there enough in these three weeks of state and social function to generate definite lines of development and to vitalize latent powers and ideals such as we wonderingly view in these latter days? No. The embassy had one other official commission which could be fulfilled only in Philadelphia. *It was in the matter of money and exchange.*

After a brief stop in Baltimore the orientals arrived Saturday, June 9, in Philadelphia. For weeks the city had been stirred to its depths planning and making arrangements for the unprecedented event. The industrial metropolis had much to show to a nation so skilled in artizanship as the Japanese. Best of all, here was the mint, which, after treaty formalities, was *the* chief focal point of the visit to the United States.

The train drawn by an engine wrapped in the Japanese and United States flags, arrived in the afternoon at the old "Baltimore Depot" at Broad and Prime Streets (now Washington Avenue). Here the envoys with Captain Dupont and Lieutenant Porter, were met by Mayor Henry, members of council, judges and other public men numbering about two hundred—and dense crowds of the populace. The long procession of nearly a hundred carriages filed up Broad Street under an imposing escort of about two thousand cavalry and infantry led by General Robert E. Patterson and staff, and greeted by the din of popular huzzahs. Thousands of eyes were strained to glimpse the novel type of physiognomy peering curiously out from the carriages. The Japanese artists were busily sketching as they rode our streets. Extraordinary curiosity arose from the closely drawn shades of one of the carriages, which eventually gave rise to a rumor that the occupant was in disgrace because he had "stopped watching the treaty box"—its sacred import having been already heralded by the newspapers. The procession brought up at the Continental Hotel where the embassy was regally entertained. The police carried the treaty chest into the hotel and guarded it under lock and key. During the week's sojourn in Philadelphia, crowds surrounded the hotel straining for a vision of the orientals as they appeared at their windows, but, said the reporters, the people "couldn't see a mite of difference between them and negroes!"

The Japanese were not unaware of the unique reputation of the Philadelphia Mint. All its early officers had been scientific men or high mechanists and not politicians. Politics had but recently invaded the management but the assayer and his assistant in a peculiar sense had given the mint a world-wide reputation for specialized accuracy. Their published works and the unvarying maintenance of the standard fineness of the coinage, for over a quarter-century of their incumbency, together with their comprehensive knowledge of the whole intricate operation of minting, had been a highly important although a popularly unapprehended factor in the national credit. For a nation, then, that had in itself the seeds of progress, a rising curiosity as to western methods, a tendency to catholicity, a receptivity to impressions, and an adaptive originality—as the Japanese had, notwithstanding a long isolation followed by internal strife—for a nation of this sort, America, and more particularly Philadelphia and especially the Mint, was the place to come.

On the morning of June 13 the envoys were received at the mint. In his address of greeting the director said that an international coinage was not likely to be realized, but he advised the foreigners to adopt the American standard. A mutual knowledge of the currency of both nations would promote commerce as well as friendly relations.

Preliminary formalities being over the occasion was so extraordinary that a messenger was quietly despatched with orders to go to a nearby grammar school and procure a release for the day of three pupils, sons of the assayer and his assistants, bring them to the laboratory where their education might take a rare turn in a leisurely and close contact with the ambassadors and their attendants. I was the youngest of those three boys, and I am now drawing on memory which is both stimulated and held in leash by certain mementos, personal notes of the assistant assayer, various public prints and official documentary memoranda.

Entering the little "weigh room" attached to the laboratory the astonished youths saw the first and third ambassadors (the latter known as the censor), a stout functionary known as the governor, two interpreters—one Dutch and one Japanese—and perhaps one or two attachés, and a "prince" travelling unofficially for pleasure but in company with the embassy.

The high dignitaries were superbly dressed in silk brocaded, or inwoven, with gold, the pajamas or trousers being figured in exquisite patterns and as wide as skirts; the body-covering a loosely crossed waistcoat over which was a kimono or long loose coat. The paper handkerchiefs were carried in the crosswrap of the waistcoat. The fore part of the head was shaved while down the center of the crown a tightly twisted lock or short braid lay glued stiff and tied with white cord. The sandals were held in place with white silk cords. The ambassadors wore three swords, the lesser officers two—a sign of the Samurai or ruling military caste and a badge of honor now abolished.

As the boys entered, one of the expert under-assayers was seated at the delicate balance explaining the process. This was addressed to the Hollander, Mr. Portman, who in turn addressed the Japanese interpreter, Namoura, in Dutch. Namoura, in turn, translated the Dutch into Japanese and addressed the third ambassador who, in turn, communicated the information to the first ambassador —who received it with the utmost imperturbable and silent gravity. Whether he took it all in or not we boys could not tell.

The censor or third ambassador, Oguri, and the governor, Narousa, fat and spectacled with heavily rimmed glasses, were the active investigators. Oguri produced a sort of steelyard of ivory with which he proceeded to test our weights against his own. The censor also brought forth an *abacus* or counting machine consisting of fifteen rows of wooden buttons sliding by five on parallel wires. Quite in accord with these crude and antiquated methods was the demand that the gold *cobang* (about as big as the palm of the hand) should be assayed not by a sample cut from it, but by consuming the entire piece. The chief assayer, Jacob R. Eckfeldt, and his assistant, William E. DuBois, were for the moment nonplussed by so extraordinary a request. High accuracy demanded very small samples and their department was nothing if not scientifically precise. But the heathenish proposition was uncompromising and the whole *cobang* it must be. When, after two or three hours, the gold had been separated and weighed and its fineness thus ascertained the envoys were not satisfied. They must next know exactly of what the alloy was made up. Sure enough, we see in this very demand an indication of what we have seen since—thoroughness.

Three *cobangs* were thus tested and to satisfy the strangers, also a United States gold dollar. Notwithstanding the cumbrous method of using an entire piece the results were exceedingly satisfactory—the *cobangs* running about 572 parts fine in 1,000. (It should be stated that while the embassy was yet in Washington a number of their coins were forwarded to Philadelphia to be tested in advance of the embassy's inspection. There were about twenty-five pieces in all, gold and silver *itzebus* and gold *cobangs*. They ran with a fair approach to uniformity.) At intervals, during the long operations the orientals took a short squat on their heels to smoke their tiny pipes. A luncheon was served to them in the mint when the display of chop-stick skill—picking up one pea at a time at high speed with two sticks, for instance—was recreating to observant America.

After this the envoys requested the mint officers to meet them at the Continental Hotel and hold a conference on matters relative to a comparison of Japanese and American coinage and means of exchange. Accordingly, Director Snowden and his clerk Linderman; the assayer, Eckfeldt, and his assistant, DuBois; and the

Melter and Refiner, Booth, repaired thither in the afternoon. Exactly what took place there we do not know. But the indications are that it was a "campaign of education" and that it was a spoke in the wheel of closer and fairer monetary relations, including the possibility of international coinage—in which movement Assayers Eckfeldt and DuBois were conspicuous promoters. The next day the embassy was at the mint again and the work continued.

A carefully written account at the time said:

The very intricate business connected with the currency question between the United States and the Empire of Japan (the adjustment of which is one of the principal objects of the embassy) and which had been theoretically explained by the officers of the Treasury Department at Washington has been *solved to the satisfaction of the envoys by the assays performed in their presence at the mint.* The importance and value of this very desirable result cannot be overestimated and the thanks of the country are due to the officers of that institution for the very skilful manner in which they have discharged the duties imposed upon them by the government at Washington in connection with this business.

The tests having been concluded on the second day the envoys were again formally addressed by the director, who presented to them a certified copy of the results and also a full set of the current United States coins handsomely cased. The censor replied, thanking the officers of the mint for their courteous attention, expressing satisfaction with the results, and promising to lay the whole matter before his government *so that a system of exchange could be arranged between the two countries.*

A slight digression may be permitted just here to illustrate the need for such adjustment of exchange if commerce was to be encouraged by the treaties. I quote from the private note book of my father, then the assistant assayer of the mint. It was written the next year—1861.

Before the Japanese Embassy started for this country a silver *itzebu* was coined, which was intrinsically one third of the Mexican dollar. (The itzebu was the monetary unit of the empire.) About the same time, for Hakodadi only, a northern port where American trade chiefly centered, they coined a *half itzebu* (so stamped on its face) whose weight was equal to half a Mexican dollar (a little over in fact) and the alleged fineness the same. The object of this was to meet a treaty provision which made Mexican dollars interchangeable with silver *itzebus,* weight for weight. By this rule they would take from Americans a thousand dollars, and give in exchange two thousand of these new coins, being the equivalent in weight. But when the Americans came to buy goods with the coins they were informed that their current value was only a *half-itzebu*, according to the stamp. That is to say, a Japanese could *pay* them to an American, at the rate of two to the dollar; but could only *receive* them at the rate of six to the dollar. And if offered their *itzebu* (one-third dollar) they did not want it—had enough of them! Thus, there was a *half-itzebu* worth fifty per cent. over the whole *itzebu* for the sake of a shallow artifice. The embassy did not bring the former piece (the *half-itzebu*) with them but only the latter (the whole *itzebu*).

The *half-itzebus* were not equivalent in fineness although they were over-full in weight. They assayed 846 thousandths fine as against 900, and weighed 210.0 grains as against, say two tenths of a grain, over a half-Mexican dollar.

Thus, there was need for a basis of exchange. Moreover, the coinage had become debased, the feudal lords had secretly issued money, and the country was flooded with counterfeits. Paper money had depreciated and finance was unsound.

It has been shown in the earlier portion of this paper that at the time of the embassy Japan was in a state of internecine strife, that the Shogunate had been undergoing changes, and that a few years later (1867–8) the crisis came in which the real government was restored to the Mikado and the feudal Shogunate abolished. Just prior to this, however, in 1866, the Mikado, or Emperor, had ratified all foreign treaties of the Shogunate, and they continued in force. In 1870 the distinguished statesman, the Marquis Ito, was sent to the United States to study monetary methods and coinage standards. While here he wrote a memorandum on "Reasons for Basing the Japanese New Coinage on the Metric system." During Ito's absence from Japan the Hongkong mint was purchased and with British aid carried over to Osaka. Ito wrote home recommending the ultimate adoption of the gold standard although not yet quite feasible. However, the metric or decimal system was adopted and the currency closely conformed to that of the United States, the *yen* being the equivalent of our dollar and the *sen* the equivalent of our cent. It cannot be doubted that the first experience of the Japanese with the working of the metric system was obtained in our mint, as the assay process is based on it and the money scheme is decimal, while the British scheme is not. Neither can it be doubted that the impressions carried back by the envoys in 1860 were factors in Ito's mission ten and eleven years later under a new government.

Ito was one of the progressives from Perry days, who (as previously explained) assumed an anti-occidental complexion merely for the sake of opposing the Shogunate. Prince Iwakura who headed the embassy of 1872, was another of those connecting links. He had not been favorable to foreigners but yet he had incurred the enmity of the Shogun's opposers. Under the empire he became one of the ablest of the emperor's advisers and was especially interested in the revision of the old treaties, which it took years to accomplish. Another was Matsukata, the father of the gold standard in Japan and the introducer of the metric system; and still another was Shibusawa, the father of the national banking system.

In 1872 Ito was here again, this time on a mission of which Prince Iwakura was the head. In an eloquent address in San Francisco, Ito said: "A year ago I examined minutely the financial system of the United States and every detail was reported to my government. The suggestions then made have been adopted and some of them are already in practical operation." The result was as already noted. Banking and even a postal system on American models also followed in this year.

That the lessons of 1860 were active forces in this later day is further evidenced in a private letter to the heads of the Assay Department from Dr. H. R. Linderman, previously the chief clerk of the mint in the days of the embassy and later a treasury agent in Washington versed in mint matters. This letter requested that the two under-assayers do him the favor to prepare as early as convenient a brief description of the processes in use at the mint. "I desire this information," said the letter, "to incorporate in a paper which I am preparing for the Japanese at the request of the department. They shall have due credit for their work and will place me under obligations." This was in March, 1871—the very year of the enactment of Japan's new coinage law under the pressure of Ito and Matsukata.

The two young under-assayers were the schoolboys of 1860, Jacob B. Eckfeldt (the present chief assayer) and the present writer, whose recollections of the envoys of the Shogunate were still vivid in their memories.

In a very real sense, then, Ito had become the connecting link and the effectuating successor of the envoys of 1860, anti-Shogunate as he had been, and now under the new Imperial regime, as he was. Notwithstanding the great break with the past on the incoming of the Meiji or "Enlightened Rule," the restoration to power of the Mikado-Emperor, there was an efficient continuity of the westernism inaugurated officially by the Shogun. The treaties of Perry and Harris and the Washington ratification still held in spite of various attempts to discredit and revise them, even in the seventies. Not until 1894 did such revision take place. To the credit of the Shogunate in its later days of the sixties be it said, the degenerated condition of its antiquated and heterogeneous monetary system and coinage was realized. It was seen with alarm that western commerce under the treaties was suffering and would suffer unless there was reform in the coinage. Hence the capital importance of the visit of the embassy of 1860 to the mint at Philadelphia.

The story revealed in these last few paragraphs, however, shows that the spirit of the Shogun and the impressions carried home by his now almost forgotten embassy remained as a wholesome and permanent leaven. Under Ito, Matsukata, Iwakura and others, the impressions carried home by the first embassy came to fruitage not only in an Americanized monetary and coinage system but in what lies deeper than these—the moral standard of a trustworthy precision in the manufacture of coin in which the Philadelphia mint has led the world. That this principle and achievement had become an ambitious scientific and commercial *motif* in new Japan is further evidenced in other ways. For instance, in 1875 sample coins were sent from the imperial mint at Osaka to our Department of State with the request from the Japanese government that these coins be carefully tested at the Philadelphia mint. The result of the test (in which I myself had an active part) was very satisfactory.

Similarly, we received for assay through the Japanese legation gold and silver coins in 1876, 1877, 1878, 1879, 1880. These were what are known as pyx coins, which are selected at random through the year for an annual test. They were invariably close to the standard, tending somewhat to run over rather than under it. Thus the pace set before the embassy of 1860 was the pace which the mint at Osaka under our stimulus was setting for itself. The Enlightened Rule recognized that a nation's position among commercial nations rests in very large degree on the confidence to be placed in the scientific precision of its coinage.

Leaving now this great essential result of the first embassy, let us look for other indications of its influence in the making of a new Japan.

While in Philadelphia the two physicians attached to the embassy, Measaki and Moryama, together with the governor Narousa-Genosiro, and the interpreter, attended an operation for lithotomy performed by the distinguished Dr. Samuel B. Gross, at the patient's residence. The anaesthetic was administered by the famous Morton himself, the discoverer of sulphuric ether for this use. The whole performance was a revelation to the orientals. They smelt and poured the ether on their hands, astonished at the coldness resulting from its evaporation. After the operation they carefully examined the instruments and showed so much interest in the whole subject that they were invited to attend the Jefferson Medical College, in which Dr. Gross was a professor.

In an address delivered before the students of the Jefferson in February, 1906, Baron Takaki, Surgeon General of the Japanese Navy, said:

> Japanese surgery is founded on the teachings of Dr. Samuel D. Gross for so many years surgeon in the splendid medical college in which we are gathered. Dr. Gross' "System of Surgery" translated into German was taken by my countrymen and retranslated into Japanese and upon that has been built up Japanese surgery as practiced to-day.

The Baron said that the thanks of the Japanese nation are due to the medical profession in this country, and added,

> The United States has been our teacher. We have tried our best to prove our faith in your teachings and doctrines by effective applications of your principles in safeguarding the health of our people.

In view of the impression made upon the doctors of the first embassy by Dr. Gross himself it is hardly possible that his name was unknown in Japan until his book was carried there from Germany. Undoubtedly this is another one of those cases of the long slow-but-sure working of American leaven through many political vicissitudes.

It is inadvisable to prolong this paper for many further details of the doings of the embassy during the week in Philadelphia. Suffice it to note, in brief, that among the places visited were Johnson's type foundry—where the orientals were presented with a book of specimen types and cuts and a silver mounted case of type; M'Allister's optical and philosophical instrument establishment where they witnessed experiments with air pumps, electrical machines, etc., and a lantern exhibition in which the Drummond light excited great curiosity; to the great foundries of the Merricks and of Morris, Tasker & Co.; to the gas works, where "grand stands" had been erected and were filled with hundreds of invited Philadelphians of both sexes—chiefly to witness the ascension of two great balloons (or rather to see Japanese for the first time behold aërial travel); to Bailey's jewelry establishment where, after examining with magnifiers the works of watches, they ordered a lot of them to be sent to their rooms, and where they showed judgment in the purchase of opera glasses, appreciating the chromatic lens, and caring little for such merely ornamental work as their own artificers could equal or excell. They chose the plain and the useful, displaying a keen selective sense. Here, too, the envoys were presented with a medal especially designed and struck by the Bailey house to commemorate the occasion. In addition to these places visits were paid to Baldwin's locomotive works, Sellers's machine works, the water works, and Girard College.

The Japanese were loaded with all manner of specimens of tools, instruments and products of our skilled workers—including pictures of the Baldwin engines, stereoscopes and views, a superb Sloat sewing machine encased in wood from Mount Vernon and (so said) the Treaty Elm; a Disston saw, level, gauge and square; a set of teeth on a gold plate, and many other samples of American originality, skill and enterprise.

The mention of the set of artificial teeth on a gold plate suggests a pendant to Gross' surgery. The modern development of Japanese dentistry is wholly American. According to Dr. Chiwaki, the president of the Tokio Dental College,[1] dentistry, as an art, is about two centuries old in Japan. In some respects the old dentistry was barbarous and crude but artificial dentures were made of carved wood—also of alabaster or ivory riveted to a base of hard wood. But on the whole the art was clumsy and its pursuit became disreputable.

"Perry's feat, however," says Dr. Chiwaki, "brought about a many-sided change in the political, social and educational institutions of Japan; and, in consequence, the old system of dentistry could not remain unaffected." Two American dentists, Dr. Eastlake and Dr. W. St. George Elliott, opened offices at the beginning of the Meiji era. This was "the first direct cause of the development of our dentistry in the true sense of the word." Others followed, and the Japanese came to the United States for dental education. Japan now manufactures dentist's appliances and supports at least two dental colleges.

The introduction of American dentistry to Japan is not directly traceable to the embassy of 1860 but its acceptance is one of those forms of Japanese confidence in American models first gained as a national leaven in the days of the Shogun.

For the sake of completeness and also to note two or three incidents or facts of contributory import in estimating results this study must follow the embassy out of Philadelphia to New York, Saturday, June 18, where there was repetition of street procession and general ovation as in Philadelphia. (On this very day the news of the assassination of the regent arrived by letter to the New York *Tribune*.)

According to an account in the *Tribune* the street scenes on the route of entry from the Battery to the Metropolitan Hotel on Broadway, were free from those "riotous excesses" which characterized the multitude at Philadelphia. Never, said the *Tribune*, had more human beings been congregated at and below the Battery

[1] *Dental Cosmos*, October, 1905.

than the envoys found awaiting them as the boat from South Amboy arrived. But the Metropolitan Hotel was at no time so riotously besieged as was the Continental in Philadelphia.

Barring the visits to two or three manufacturing establishments, the time was chiefly occupied with social functions, shopping, boat excursions, theaters and in packing the mountainous wares which they had bought and which had also been lavishly bestowed upon them largely for advertising purposes. In time, the envoys and lesser officers acutely discerned that they were being exploited. Many invitations were declined. Finally, so indecorous a pressure was put upon them to visit the opera in spite of their resolute declination that a serious affray was narrowly averted.

The embassy, having grown weary of their spectacular exploitation in New York, resolved to cut Boston and Niagara out of their program and set sail for Japan as soon as possible. They accordingly departed by the largest of our naval fleet, the *Niagara*, on Saturday, June 30, first steaming around the world's wonder, the *Great Eastern*, which had arrived only two days before and which now succeeded the Japanese as a popular ferment.

In the retrospect: Those were stirring times. The greatest ship in the world had crossed the Atlantic, Garibaldi had just taken Palermo, Lincoln had been nominated, and the Democratic Party had split into the Douglass and Breckenridge factions. The ocean cable itself was only two years old; the John Brown insurrection had occurred only nine months before; Mr. Lowe, the aëronaut, was planning a balloon voyage across the Atlantic; and the Prince of Wales was soon to be entertained.

The New York *Tribune* gave up two pages of small type to a description of the voyage of the *Great Eastern*, and Mr. Greeley editorially declared her to be a wonder without much maritime significance for the simple reason that only three or four harbors in the world could receive so huge a ship. The same big-brained, generally level-headed editor was unable to attach any practical importance to the visit of the Japanese. He saw through New York eyes and thus rhetorically delivered himself:

If they [the Japanese] have the acuteness to see, as possibly they have, the uses to which they have been put, to gratify the inordinate vanity, the inordinate greed, and the inordinate folly of those with whom they have come chiefly in contact, and if they believe that in these they see reflected the character of the whole people, then heaven help our reputation in Japan when these sons of hers go home. But let us hope they did not understand. In the simplicity of their natures and manners let us trust that they have gone back to their own country impressed not only with our material superiority but believing also that in all Christian graces, in the amenities of social life, in the refinements of personal good breeding we are unmeasurably their masters. . . . Of almost all that an intelligent traveller would like to be informed they have gone away as ignorant as they came. . . . Against the acquirement of all useful knowledge except in a few rare instances which make the rule more apparent, they have been sedulously guarded and the opportunity lost which will never recur again of impressing a people eager in the attainment of the arts of peace, with the true source of the wealth and power of Christian civilization.

Another New York paper thus commented:

They are small of stature, tawny of complexion, sleepy and feeble in their physical appearance and habits, and with only those characteristics calculated to excite a momentary curiosity.

The Philadelphia view was different. *The Inquirer* said:

They saw the triumphs of science and art made subservient to the comfort and happiness not to special classes merely, but to all. They cannot separate these things from the effects of our political institutions and it will be extraordinary indeed if they disconnect them from the benign influence of Christianity.

This is the true note—the note which this paper has essayed to demonstrate as proved by time. Mr. Greeley in the case of the Japanese, as in that of monster ships like the *Great Eastern,* was a bad prophet. He argued that the embassy avowed before arrival that it had no ministerial powers except those of signing the treaty and collecting information concerning our currency with a view to better ultimate international adjustment.

But Mr. Greeley saw nothing in this. He referred to the conferences at the mint but was unable to figure out anything feasible. The relations which gold bore to silver in Japan and their artificial value in coinage forbade any basis of equitable exchange. Indeed, he believed, if through their labors at the mint, the Japanese were to adopt the new standards for estimating the values of the precious metals, "it is easy to see that the monetary affairs of the empire might be thrown into great confusion."

But we in this day know that the Japanese were not so simple in their natures, not so sleepy and feeble, as the New York editors supposed. Neither did they go away as ignorant as they came. Nor were they so ignorant when they came. Long before Perry's day Japan had had her martyrs to progress and reform. News from the outside leaked in and shadows of western mechanism and methods fell on the isolated empire. Men like Fujita Toko and Sakuma Shuri had telescopic vision and sensitive hearing. So the envoys of the Tycoon knew that there were advantages to be improved in going to the United States over and above that of signing the Harris treaty. They had the penetration to see that a sound currency and facilities of finance were the pivot of international commercial relations. They were impressed with the fact that international confidence rested chiefly on that scientific accuracy which they saw in the operations of coinage and especially those that guarded the integrity of the standards of fineness. The problem which Mr. Greeley saw as insoluble, was gradually worked out by Ito and Matsukata until Japan possessed a system of coinage modeled on and comparable with that of the United States, and resting on a gold basis.

A letter written to President David Starr Jordan by the distinguished Japanese scientist, Dr. Mitsukuri, in 1900, confirms the trend of this paper as a contention for an unbroken continuity of influence on the development of Japan—in spite of the dismal deliverances of these American prophets of 1860.

Dr. Mitsukuri says:

> The history of the international relations between the United States and Japan is full of episodes which evince an unusually strong and almost romantic friendship existing between the two nations. In the first place, Japan has never forgotten that it was America who first roused her from the lethargy of centuries of secluded life. It was through the earnest representations of America that she concluded the first treaty with a foreign nation in modern times, and opened her country to the outside world. Then, all through the early struggles of Japan to obtain a standing among the civilized nations of the world, America always stood by Japan as an elder brother by a younger sister. It was always America who first recognized the rights of Japan in any of her attempts to retain autonomy within her own territory. A large percentage of foreign teachers working earnestly in schools was Americans, and many a Japanese recalls with gratitude the great efforts his American teachers made on his behalf.

Then, kindness and hospitality shown thousands of youths who went over to America to obtain their education have gone deep into the heart of the nation, and, what is more, many of these students themselves are now holding important positions in the country, and they always look back with affectionate feelings to their stay in America.

In conclusion, it is immensely interesting to see that what Japan came to America for on her first embassy is precisely *that which she has retained as the essential element of her international development.* She afterward went to Germany for army organization and got it; she went to Great Britain for naval ideas and got them; she came here for coinage, exchange, and got them. Moreover, her friendship with the United States has been practically continuous while from 1861 to 1863 she was in hot water with England and France. Incidentally, she carried away our surgery, and no one knows how many minor constructive principles; later she borrowed our banking and postal systems, transplanted our dentistry, and made obeisance to American invention by overspreading the empire with our telegraph.

The embassy of 1860, as was said at the outset, was but the completing touch of the treaties of Perry and Harris. All these constitute a single event but an event that is a gigantic factor in the world's progress. Why the most practical part of it—the embassy—has dropped into such profound oblivion is beyond comprehension. Perhaps it was one of those events which are too broad and too potent to be discerned in less than a half century as the mark of a world-moving era.

MAGELLANIC PREMIUM

Founded in 1786 by John Hyacinth de Magellan, of London

1910

THE AMERICAN PHILOSOPHICAL SOCIETY

Held at Philadelphia, for Promoting Useful Knowledge

announces that in

DECEMBER, 1910

it will award its

MAGELLANIC GOLD MEDAL

TO THE AUTHOR OF THE BEST DISCOVERY, OR MOST USEFUL INVENTION, RELATING TO NAVIGATION, ASTRONOMY, OR NATURAL PHILOSOPHY (MERE NATURAL HISTORY ONLY EXCEPTED) UNDER THE FOLLOWING CONDITIONS :

1. The candidate shall, on or before November 1, 1910, deliver free of postage or other charges, his discovery, invention or improvement, addressed to the President of the American Philosophical Society, No. 104 South Fifth Street, Philadelphia, U. S. A., and shall distinguish his performance by some motto, device, or other signature. With his discovery, invention, or improvement, he shall also send a sealed letter containing the same motto, device, or other signature, and subscribed with the real name and place of residence of the author.

2. Persons of any nation, sect or denomination whatever, shall be admitted as candidates for this premium.

3. No discovery, invention or improvement shall be entitled to this premium which hath been already published, or for which the author hath been publicly rewarded elsewhere.

4. The candidate shall communicate his discovery, invention or improvement, either in the English, French, German, or Latin language.

5. A full account of the crowned subject shall be published by the Society, as soon as may be after the adjudication, either in a separate publication, or in the next succeeding volume of their Transactions, or in both.

6. The premium shall consist of an oval plate of solid standard gold of the value of ten guineas, suitably inscribed, with the seal of the Society annexed to the medal by a ribbon.

All correspondence in relation hereto should be addressed

To the Secretaries of the

AMERICAN PHILOSOPHICAL SOCIETY

No. 104 South Fifth Street

PHILADELPHIA, U. S. A.

The American Philosophical Society

Announces that an Award of the

HENRY M. PHILLIPS PRIZE

will be made during the year 1912; essays for the same to be in the possession of the Society before the first day of January, 1912.

The subject upon which essays are to be furnished by competitors is:

The Treaty-making power of the United States and the methods of its enforcement as affecting the Police Powers of the States.

The essay shall contain not more than one hundred thousand words, excluding notes. Such notes, if any, should be kept separate as an Appendix.

The Prize for the crowned essay will be $2,000 lawful gold coin of the United States, to be paid as soon as may be after the award.

Competitors for the prize shall affix to their essays some motto or name (not the proper name of the author, however), and when the essay is forwarded to the Society it shall be accompanied by a sealed envelope, containing within, the proper name of the author and, on the outside thereof, the motto or name adopted for the essay.

Essays may be written in English, French, German, Dutch, Italian, Spanish or Latin; but, if in any language except English, must be accompanied by an English translation of the same.

No treatise or essay shall be entitled to compete for the prize tht ha s been already published or printed, or for which the author has received already any prize, profit, or honor, of any nature whatsoever.

All essays must be typewritten on one side of the paper only.

The literary property of such essays shall be in their authors, subject to the right of the Society to publish the crowned essay in its Transactions or Proceedings.

The essays must be sent, addressed to

The President of the American Philosophical Society,

No. 104 South Fifth Street, Philadelphia, Penna., U. S. A.

PROCEEDINGS

OF THE

American Philosophical Society

HELD AT PHILADELPHIA

FOR PROMOTING USEFUL KNOWLEDGE

VOL. XLIX. AUGUST–SEPTEMBER, 1910. NO. 196.

CONTENTS.

PHILADELPHIA

THE AMERICAN PHILOSOPHICAL SOCIETY

104 SOUTH FIFTH STREET

1910

Members who have not as yet sent their photographs to the Society will confer a favor by so doing; cabinet size preferred.

It is requested that all correspondence be addressed

To the Secretaries of the

AMERICAN PHILOSOPHICAL SOCIETY

104 South Fifth Street

Philadelphia, U. S. A.

PROCEEDINGS

OF THE

AMERICAN PHILOSOPHICAL SOCIETY

HELD AT PHILADELPHIA

FOR PROMOTING USEFUL KNOWLEDGE

VOL. XLIX | AUGUST–SEPTEMBER, 1910 | NO. 196

THE EFFECTS OF TEMPERATURE ON PHOSPHORESCENCE AND FLUORESCENCE.[1]

BY EDWARD L. NICHOLS.

(Read April 22, 1910.)

Fluorescence and phosphorescence are closely connected phenomena, the precise relation of which is not completely settled. In a general way we may say that the emission of light by a body under any one of the numerous stimuli which produce luminescence when observed during excitation is termed fluorescence; the after glow is phosphorescence. According to the quite generally accepted view, first expressed by Wiedemann and Schmidt[2] and since developed by Merritt[3] and in somewhat different form by Lenard[4] and others, luminescence is a phenomenon of dissociation in which negative ions or in the language of Lenard "electrons" are separated from the molecules by the action of light, cathode rays, X-rays, the radiation from radioactive materials, etc. These are supposed to return later to the aggregation from which they have been torn loose or to some other molecule and to produce by their collision the vibrations which are the source of the emitted light.

[1] The apparatus used in the experiments described in this paper was purchased in part under a grant from the Carnegie Institution.

[2] Wiedemann and Schmidt, *Annalen der Physik*, LVI., 1895, p. 177.

[3] Nichols and Merritt, *Physical Review*, XXVII., 1908, p. 368.

[4] Lenard, *Annalen der Physik*, XXXI., 1910, p. 1.

The formation of these free ions is a gradual process; measured in terms of the time of vibration of light, indeed, it is almost infinitely slow. If the fluorescence of a body subjected to moderate illumination be measured from second to second it will be found to increase in brightness, first rapidly, then, more and more slowly; approaching a maximum in some cases only after several minutes.

From such observations a sort of saturation curve may be plotted. In Fig. 1, which is from measurements by Professor Merritt and the present writer,[5] curve *A* is such a saturation curve

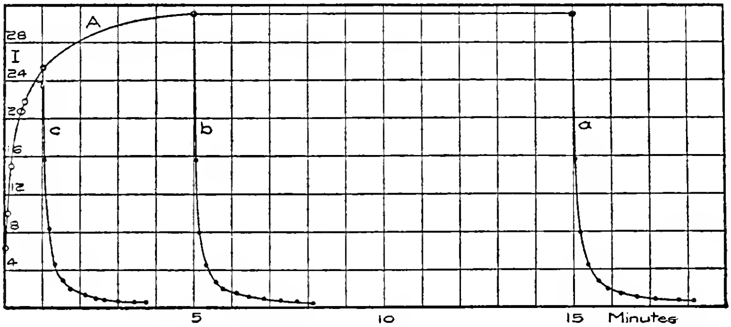


FIG. 1. Curves of saturation and of decay.

obtained by observing the increase in the brightness of fluorescence of a specimen of Sidot blende during an interval of fifteen minutes. Immediately upon the cessation of excitation the light from the fluorescent body begins to decrease; first rapidly, then more and more slowly and this phase of the phenomenon which may likewise be expressed by a curve, called the "curve of decay" is called phosphorescence. Curves *a*, *b* and *c* in Fig. 1 are the curves of decay of the phosphorescence of this specimen of Sidot blende when excitations is ended after 15 min., 5 min., and 1 min. respectively. The law of decay is always the same although the length of time required for the afterglow to become so feeble as to be invisible may vary from many hours[6] to an immeasurably small fraction of a second.

[5] Nichols and Merritt, *Physical Review*, Vol. XXIII., p. 46.

[6] Surfaces coated with the ordinary phosphorescent zinc sulphide or Balmain's Paint if exposed to sunlight and taken into a dark room have been know to continue to glow for months with sufficient intensity to fog photographic plates.

In the case of fluorescent liquids it was found by one of my former pupils, Dr. Waggoner,[7] that even with a special form of phosphoroscope, by means of which observation less than a thousandth of a second after the cessation of intense illumination were possible, no phosphorescence could be detected. It should be remembered, however, that 1/10,000 or even 1/1,000,000 of a second is a very long time measured in terms of the frequency of light, since the particles of a phosphorescent body emitting green light would oscillate some 500,000,000 times in a millionth of a second. We are not in position therefore to say that fluorescent liquids, in none of which phosphorescence has been observed, differ from phosphorescent bodies save in the rapidity with which the light decays.

By the use of this instrument Dr. Waggoner was likewise able to trace the phosphorescence of various compounds back to its very source at the cessation of excitation and to show how in the cases which he studied fluorescence merged into phosphorescence without discontinuity and the quality, or distribution of wave-lengths in any single band in the spectrum remained unchanged during the first few thousandths of a second. Professor Merritt and the present writer[8] had previously shown that in the case of a substance of slow decay (Sidot blende) the phosphorescence spectrum is identical, so far as the single band under observation was concerned, with the fluorescence spectrum and that "if any change occurs in the form of the phosphorescence spectrum during decadence, this change is extremely small."

According to this view the relation of phosphorescence to fluorescence would seem a very simple one but more detailed study develops complications such that the complete theory of the subject is as yet far from being perfected. Some of these complications are brought out particularly when we subject fluorescent or phosphorescent substances to change of temperature and it is with some of the phenomena accompanying such changes that I propose to deal in the present paper.

[7] Waggoner, *Physical Review*, Vol. XXVII., p. 209, 1908.

[8] Nichols and Merritt, *Physical Review*, Vol. XXI., p. 257, 1905.

When a body capable of phosphorescence is either heated or cooled and then exposed to light the intensity of its fluorescence is found to vary as are also the intensity and duration of its phosphorescence. Mere observations with the unaided eye suffice to show the following:

1. Very great changes occur in the rate of decay as the result of either cooling or heating.

2. The color of phosphorescence frequently differs at different temperatures.

3. The color of phosphorescence may be seen to vary markedly during decay, one tint gradually merging into another.

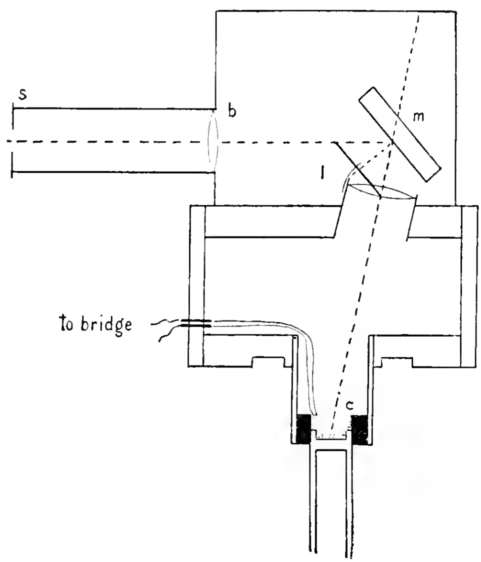


FIG. 2. Apparatus for cooling.

4. Fluorescence of one color is often followed by phosphorescence of another. W. S. Andrews,[9] for example, has noted the following in the case of artificial phosphorescent substances.

Compounds containing.	Fluorescence.	Phosphorescence.
(*a*) Zinc and manganese.	light pink	deep red
(*b*) Cadmium, manganese and sodium.	light pink	orange-yellow
(*c*) Cadmium and manganese.	yellow	light green

[9] Andrews, W. S., *Science*, Vol. XIX., p. 435, 1904.

To determine more exactly the effects of temperature on the duration of phosphorescence the following experiments, in which Mr. J. F. Putnam assisted me, were made. Several sulphides of known composition of the sort prepared by the method of Lenard and Klatt, a number of which are now on the market, were cooled by means of liquid air. The form of apparatus used is shown in Fig. 2.

The source of light was a flaming arc between carbons which were filled with salts yielding an ultra-violet spectrum of great intensity and unusual range. These carbons are known commercially as the "brilliant white." They gave an arc which under the conditions of our experiments excited the substances under observation to complete saturation in about six seconds.

The lamp was of the vertical carbon type with a large upper carbon, cored but not impregnated, and a smaller impregnated carbon below. The direction of the current was such as to make the latter the positive terminal. Such a lamp burns several minutes without feeding with an arc from one to two centimeters in length and of sufficient steadiness for the purpose in question.

A condensing lens of quartz 20 cm. focal length and 5 cm. diameter threw an image of the arc upon the wide horizontal slit *s*, the edges of which excluded light from both carbon tips. The light after passing the slit was rendered parallel by the quartz lens l' and fell upon a plane mirror *m* of speculum metal by which the beam was reflected obliquely downwards at an angle of about 70° through the similar lens l'' which caused an image of the slit upon the substance to be observed. The substance in a thin layer of powder was contained in a shallow capsule at the top of a bronze tube about 20 cm. long. When in position the top of this tube was surrounded by a massive collar of copper which in turn was supported by a tube of hard fiber which afforded excellent thermal insulation. The whole was boxed in to prevent the gathering of frost. Observations of the phosphorescent substance were made through a horizontal tube inserted in the side of the box above the capsule and having a rectangular prism *p* at the inner end, as shown in Fig. 3.

To cool the phosphorescent compound the lower end of the bronze tube was submerged in a cylindrical Dewar flask containing liquid air and by the vertical adjustment of this tube any temperature from that of the room to about — 185° could be reached and

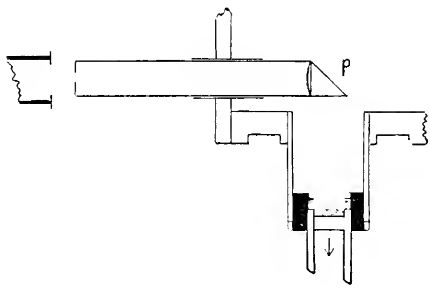


FIG. 3 Device for observing the cooled substance.

maintained. The temperatures were measured by noting the electrical resistance of a previously calibrated coil of fine copper wire *C* (Fig. 2) imbedded in the copper cylinder and immediately surrounding and in contact with the capsule. A shutter, the opening

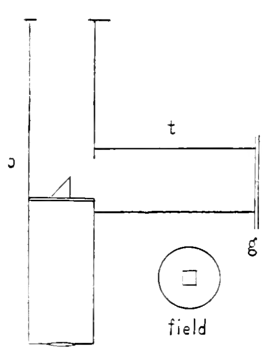


FIG. 4. The photometer.

and closing of which was automatically recorded by a chronograph, was used in making exposures to excitation.

For the determination of the curves of decay a specially designed photometer was used. This consisted of a slight tube *O*, Fig. 4, mounted horizontally in front of and coaxially with the observing tube in the side of the box already described. Within this sight tube and in the focus of the eyepiece *e* a disk of thin plane glass was mounted in the middle of which was fastened, with Canada balsam, a very small rectangular prism. Opposite this a a side tube *t* was inserted, the outer end of which was covered with a screen of ground glass *g*. When the screen was illuminated from without an observer at the eyepiece saw a rectangular patch of light—the face of the reflecting prism—surrounded by the field of light due to the phosphorescent surface. The brightness of this

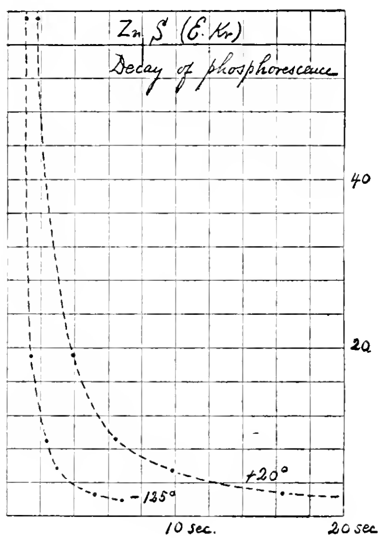


Fig. 5.

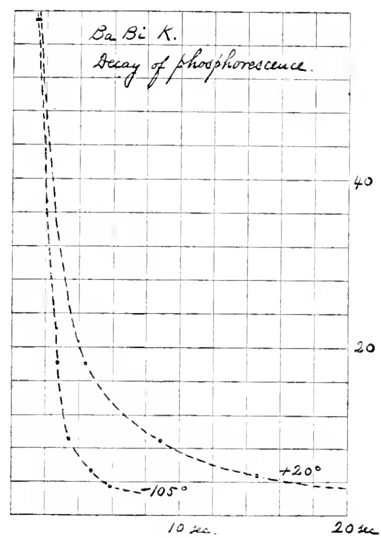


Fig. 6.

patch was varied, in making measurements, by the movement of a small frosted tungsten lamp (boxed in and viewed through a small aperture) which travelled along a photometer bar in the direction of the axis of the side tube.

To determine the curve of decay with this apparatus the comparison lamp was set at a selected point on the photometer bar, the shutter was opened for ten seconds to secure saturated exposure of the specimen under observation, and at the instant when the decaying phosphorescence had fallen to the degree of intensity which balanced the light of the comparison lamp as determined from the appearance of the contrast field of the photometer, a record was made on the same chronograph sheet on which the closing of the shutter had been automatically registered.

This procedure was repeated with the comparison lamp at various positions on the bar. Typical results obtained in this way are

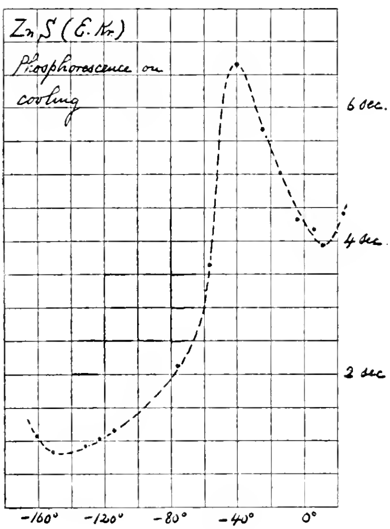


FIG. 7.

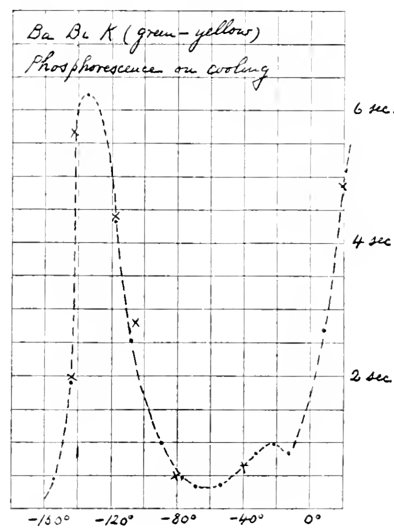


FIG. 8.

given in Figs. 5 and 6. Fig. 5 shows the curves of decay of phosphorescence in a sample of ZnS at + 20° and at — 125°. In Fig. 6 similar in curves for a phosphorescent barium sulphide prepared by the method of Lenard and Klatt and having as its active metals bismuth and potassium are given for + 20° and — 105°.

The more rapid decay exhibited by both of these substances at the low temperatures does not necessarily indicate lower initial intensity of phosphorescence. Indeed in the case of the Ba Bi K compound the initial intensity at — 105° which may be estimated from the decay curves by plotting these in the manner already described by Professor Merritt and myself[10] in which the reciprocal of the square root of the intensity is taken as ordinates is several times higher than the intensity at + 20°.

[10] Nichols and Merritt, *Physical Review*, Vol. XXII., p. 280, 1906.

So marked are the fluctuations in the duration of phosphorescence in these substances on cooling that at many temperatures the effect dies down more rapidly than it can be followed with the apparatus just described. It is, however, possible to secure an almost complete record of the fluctuations of phosphorescence with temperature by allowing the substance to cool slowly throughout the entire range of temperatures. The photometer carriage is set at a convenient distance and records made of the times required for the phosphorescence to attain this intensity at the various temperatures

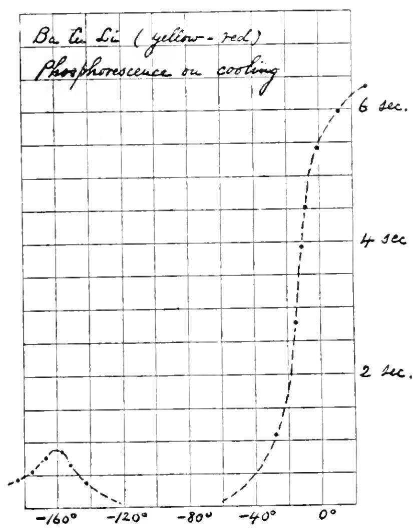


FIG. 9.

through which the substance passes in cooling. Measurements of this sort were made by Mr. Putnam and myself in the case of a number of phosphorescent sulphides. Three characteristic cases are shown in the curves in Figs. 7, 8 and 9. Two of these substances are those for which the decay curves have been given in the previous figures, namely, the phosphorescent zinc sulphide and the Ba Bi K compound. In all three of these cases it will be noted that the time required for the phosphorescence to fall to a given intensity varies greatly with the temperature and that the curves showing these changes have marked maxima and minima. In the case of the zinc sulphide, for example, the longest duration observed was at —40° but there is evidently another maximum at some temperature above that of the room and still another at or below the temperature of liquid air. The curve for Ba Bi K shows a very pronounced and remarkable maximum at —132°, above and below which temperature the duration of phosphorescence falls off with great rapidity. Between —40° and —80° the duration is so short that measurements can scarcely be made with this apparatus. In the case of the third compound in question (barium sulphide with

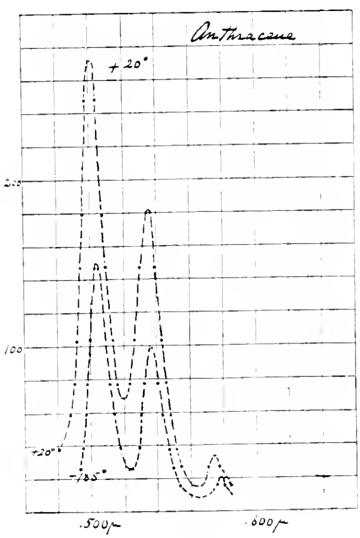


FIG. 10.

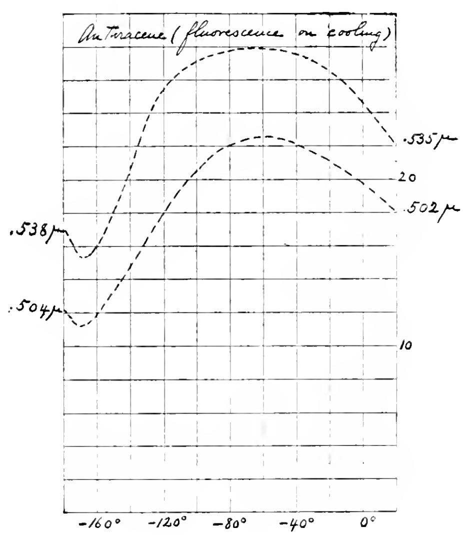


FIG. 11.

Fluorescence of solid anthracene.

copper and lithium as the active metals), see Fig. 9, the duration of phosphorescence falls off as the temperature diminishes until at about —60° it is difficult to observe the existence of any phosphorescence whatever. The fleeting glow at these temperatures appears of a different color from that observed either at higher or lower temperatures. As the temperature of liquid air is approached the duration increases again until it becomes measurable. It reaches a maximum at about —160° after which it begins once more to decrease.

The study of the fluorescence of these and other substances shows that the relation of fluorescence to phosphorescence is not so simple as at first appears. Observations with the spectrophotometer bring out various complexities in the fluorescence spectrum. Bands that seen with the spectroscope appear to be single are found to consist of two or more components more or less closely overlapping. Changes of temperature affect all wave-lengths of a single band in the same manner and, in some cases, neighboring bands are similarly affected. In the fluorescence spectrum of commercial anthracene for example, there are besides the blue and violet bands of the pure substance, two bright bands having their crests at .502 μ and .538 μ and a much fainter band with a maximum at .575 μ. At $-185°$ all three of these bands are reduced in intensity in nearly the same proportion (see Fig. 10). In the case of all three moreover the diminution is greater on the side towards the violet so that there is a slight shift of all three bands towards the red. The positions of the crests are now .504 μ, 538 μ and 577 μ respectively. At the temperature of liquid air moreover the bands are narrower and the yoke between them is much lower.

Observations upon the crests of the two brighter bands, as the substance is gradually cooled, show that the two crests rise and fall in intensity together preserving very nearly the same relative heights as indicated by the curves in Fig. 11.

This displacement of fluorescence bands towards the red on cooling is of frequent occurrence. In the case of the single band in the red-yellow of the fluorescence spectrum of solutions of resorufin in alcohol for instance (see Fig. 12) we find upon reducing the temperature from $+20°$ C. to $-95°$ C. that the intensity at the crest is reduced to about one half and the band is narrower than at room temperature. There is however no appreciable shift. At $-165°$ C. the whole band is shifted to the red and there is further narrowing which shows itself in the steeper slope of the curve on the violet side. The intensity however is the same as at $-95°$ C. Further cooling to $-185°$ C. greatly increases the intensity without further shift. Fig. 13 shows the variations in the brightness of the crest of this band throughout the range from $+20°$ to $-185°$. It might be thought that since the solvent is alcohol which freezes at — 112° the shift of the band is due to the change from a liquid to a solid solution, but this would not account for the changes occurring in the fluorescence of anthracene nor for the very marked shift towards the red observed in the case of willemite (Fig. 14).

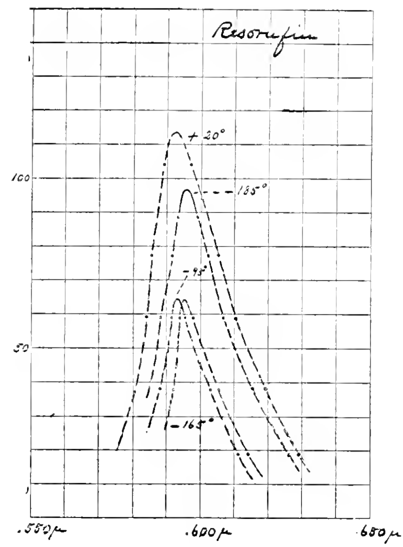


FIG. 12.

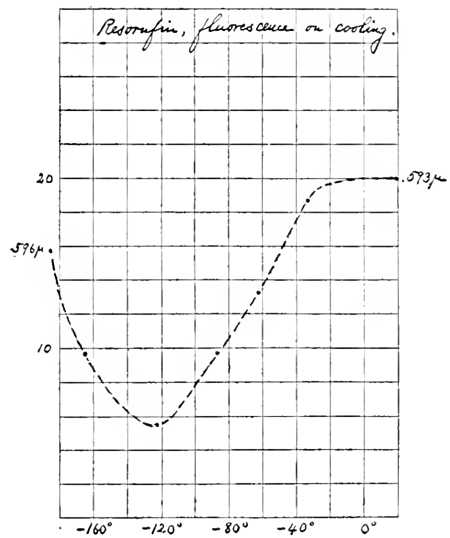


FIG. 13.

Fluorescence of resorufin.

In this fluorescent silicate, willemite, the band at + 20° is evidently complex, a closely overlapping band towards the violet being indicated by the slight notch or shoulder at .51μ (Fig. 14). Upon cooling to — 70° this component is suppressed and as a result the violet edge of the band is greatly shifted. Whether the shifting of the crest, which occurs on further cooling is altogether due to this cause cannot be definitely determined from these observations.

A very interesting example of a complex band exists in the case of a phosphorescent sulphide of strontium prepared by the method of Lenard and Klatt with bismuth as the active metal and a sodium salt as flux. Measurements of the fluorescence spectrum made with the spectrophotometer when this substance at + 20° is excited by the light of the mercury arc, give a curve of the form shown in Fig. 15. There is a narrow band with a sharp peak at wave-length. .494μ and a group of two or more much weaker, over-lapping bands towards the violet. At the temperature of liquid air the band towards the red is reduced in brightness and is shifted towards the violet, the group of bands of shorter wave-length, however, are greatly increased in intensity and the curve shows the presence of at least four crests, at .480μ, .474μ, .468μ and .463μ.

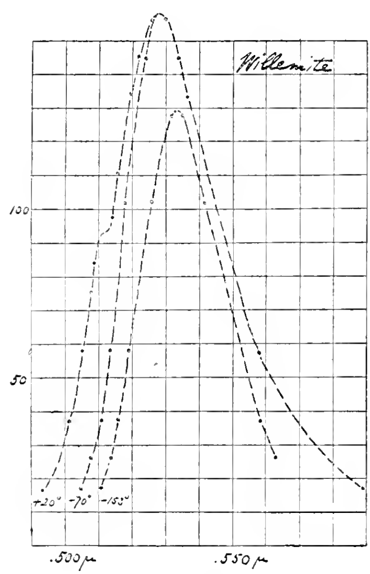


FIG. 14.

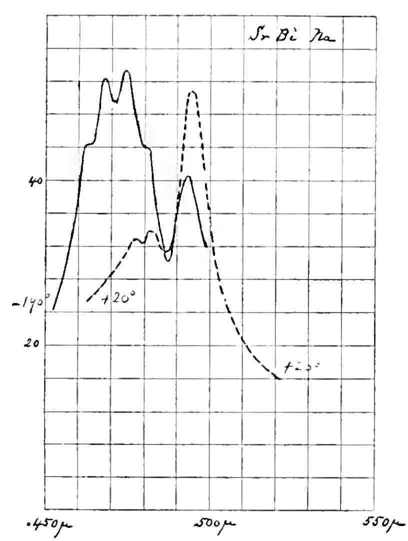


FIG. 15.

When viewed in the ordinary way with the spectroscope this entire group appears as a single broad band and indeed it has been so designated by Lenard[11] in a recent paper. When however we consider that each of its overlapping components is independently affected, by temperature as to wave-length, intensity and rate of decay it will be seen that any complete and quantitative study of the fluorescence and phosphorescence is a very complicated and difficult matter.

Numerous spectrophotometric measurements, of the spectra of

[11] Lenard, *Ann. der Physik,* IV., Vol. 31, p. 641, 1910.

fluorescent and phosphorescent substances made by Professor Merritt and the present writer, of which a few typical examples have been given in this paper, lead to the following general conclusions.

1. The emission spectra of fluorescent and phosphorescent bodies even when they appear to consist of a single broad band are frequently complex consisting of a group of overlapping bands.

2. The various components of a broad band vary in intensity (both relative and absolute) with the temperature.

3. Sometimes neighboring components of a band are similarly affected by a given change of temperature; sometimes they are oppositely affected, one component increasing in brightness while another diminishes.

4. Change of temperature is frequently accompanied by a shift of the bands of a fluorescence spectrum and this shift, which is sometimes toward the red and sometimes towards the violet, appears in many cases to be due to simultaneous and opposite changes in the intensity of overlapping components.

5. The rate of decay of phosphorescence depends upon the temperature and the complexity of the changes observed when the phosphorescent light is studied as a whole may probably be ascribed the independent variations, as to brightness, duration and position of the various overlapping bands of which the spectrum is composed.

6. Many of the changes in the color of fluorescence and phosphorescence may be ascribed to these independent variations in the intensity and differences in the duration of the overlapping components.

Physics Laboratory of Cornell University,
April, 1910.

GERMINAL ANALYSIS THROUGH HYBRIDIZATION.

By GEORGE HARRISON SHULL.

(*Read April 23, 1910.*)

The study of the various characteristics of plants and animals as independent units has made hybridization a valuable instrument in experimental morphology, and has given to the name of Mendel an enduring place as a true prophet in the history of biological progress.

The importance of the Mendelian contribution can scarcely be over-estimated. Before the "re-discovery" a decade ago, no one but Mendel had given an approximately correct interpretation of the composition and behavior of hybrid progenies, and the process of hybridization was therefore of no particular consequence for general biology. The *hybrid individual* was taken as the unit and comparisons between the hybrids and their parents were made in generalized terms involving the general aspect or *tout ensemble*. As only rarely were all the characteristics of either parent recombined in one of the offspring, the phenomena of segregation and recombination were considered of relatively rare occurrence, and described in terms of atavism or "throwing back" to the ancestral condition.

An important cause for the long delay in the discovery of the Mendelian phenomena was the distinction made between the offspring of species-crosses, which alone were distinguished as "hybrids," and the cross-bred offspring of more closely related forms, which were stigmatized as "mongrels." The difficulty of making species-crosses, the consequent rarity of such hybrids, and the usually uniform type of the offspring produced, all gave the impression of their greater scientific importance at a time when rarity and uniformity of phenomena instead of their general occurrence and variability seem to have made the stronger appeal.

Koelreuter, the first hybridologist, started the current in this direction by devoting his attention so strongly as he did to the phenomenon of sterility in hybrids, which he considered an important test of the specific distinctness of the parents. The very fact of fertility in the progeny of a cross seemed in later years to terminate its interest for him and only in rare instances in his writings do we find any data as to the characteristics of individuals belonging to second and later generations.

Gaertner dealt with the subject of hybridization in a much broader way and arrived at many interesting generalizations. However, he also worked almost wholly with species-crosses, purposely choosing his material with as wide differences as possible in order to facilitate definiteness of descriptions, but in this very effort to gain definiteness, the opportunity for studying the second and later generations was usually lost through the sterility of the first generation hybrids. He did, however, make some studies on such well-known Mendelian material as peas, sweet peas and Indian corn, but only in the last did he study a second generation, and in this the complexities introduced by "xenia" were doubtless the chief cause of his failure to find the simple law of segregation.

Practically all other hybridizers from Gaertner's time on to the beginning of the present century, considered the mere securing of hybrid individuals and their systematic description as the matters of prime value. Thus it was that the *combination* phenomena of hybridization alone occupied the stage, and the separation of the parental characters and their recombination in different individuals was only imperfectly recognized as variability and returns to one or other of the two parental types.

Two French investigators, Godron and Naudin, who were working synchronously with Mendel, seem to have come very near sharing Mendel's great discovery, but each of these two investigators by a strange chance observed a different phase of the Mendelian phenomena, Godron reaching the conclusion that in mongrels ("métis") all progenies return in several generations to the parental types and then breed true, while Naudin thought he had demonstrated that the progenies continue to vary after the F_2 and never become fixed. However, in the work of Godron and Naudin, their near discovery of Mendelian segregation was not due to a deliberate consideration of the various characteristics as units, but rather to the fact that several of the forms which they used in their cultures, differed from each other by single unit-characters, as exemplified for instance by the purple color of stems in *Datura Tatula,* contrasted with the green stem of *D. Stramonium,* or the usual prickly fruit of the Daturas contrasted with the smooth fruit of a var. "*capsulis inermibus.*"

The Mendelian method of following single characteristics possessed by the parents, not only into their F_1 progeny, but also through the second, third, and later generations, brought to light a regularity of behavior which has served to shift the stress from the simple combination phenomena involved in hybridization, to the phenomena of separation and recombination of such elementary differences as existed between the two parents.

The result of this important innovation of method has been to demonstrate beyond a peradventure, that many of the distinguishing characteristics of adult plants and animals are predetermined by corresponding differences in the constitution of the germ-cells; that these differences may be of an elementary character, capable of separation into different germ-cells; that when two parents used in any cross, differ by such elementary characters, half the resultant germ-cells have the capacity to produce any given elementary character of the one parent, the other half possess the capacity for the production of the corresponding or alternative characteristic of the other parent; that as a rule such unit-characters are wholly independent from one another and capable of rearrangement in every possible combination with one another; and, finally, that it is purely a matter of chance, which available type of sperm shall fertilize any given egg.

The separation of the unit-characteristics into different germ-cells in every possible combination with other characteristics gives the power in many cases to recognize all the unit-differences which served to distinguish the two parents. By the study of the hybrid progenies we are thus given an insight into certain phases of the protoplasmic constitution and the "mechanism" of heredity, which has been totally unattainable by other means.

In making analyses of such hybrid progenies and in working out the nature and delimitations of the unit-differences involved in Mendelian crosses, no assumption need be made as to the ultimate nature of the "genes"[1] or determiners. The attitude of nearly all experimenters in the field of genetics is one of more or less consciously suspended judgment on this point, and I believe that no other attitude is justified at the present time. So far as I am aware no investigator of the Mendelian phenomena "sees only particles" as Dr. Riddle[2] has erroneously assumed, although it must be confessed that his speech does sometimes seem to symbolize them. The Mendelian interpretations do not "stand as a formidable block in the path of progress," nor as any block at all, since all terminology is more or less symbolic, and comes to mean new things as rapidly as new truths are brought to light. All investigators in this field will be appreciative of the service Dr. Riddle has performed in bringing to their notice the recognized facts in the process of melanin formation, though they can scarcely fail to regret his unfamiliarity with the present state of genetic science, and with the attitude of those engaged in the investigations. If he had been thus familiar with work in genetics, he might very easily have shown that the facts of melanin chemistry are in harmony with the mass of other data for which the "Mendelian interpretation" has proved so illuminating.

Although the question of epigenesis *versus* preformation is emphasized as a fundamental difference between Riddle's views and those of the Mendelians, this supposed difference is mainly imaginary. Riddle's assumption of different "strengths" in the germ-cells as a possible method of accounting for the production of different colors or other characters in adult animals, involves a preformation of the same order as that assumed by the investigators of

[1] The genes are the differences, of whatever nature, whose existence in the germ-cells determines the capacity of the unit-characters to be present or absent in the individuals developed from those germ-cells.

[2] Riddle, O., "Our Knowledge of Melanin Color Formation and its Bearing on the Mendelian Description of Heredity," *Biol. Bull.*, **16**: 316–351, May, 1909.

Mendelian phenomena. Every thremmatologist is too familiar with the facts of ontogeny to give the slightest credence to anything approaching the old emboîtement hypothesis, but he must accept as a philosophical necessity the fact that there can be no action without an agent. There can be no "strength" without something to be strong. Preformation and epigenesis are simply inseparable phases of a single philosophical unity and any attempt to separate them is fallacious.

The statement that the "nature of present Mendelian interpretation and description inextricably commits to the 'doctrine of particles,'" presents Mendelism and its investigators in a false light, as no such commitment is involved. Despite the enormous activity and splendid progress that has been made in these ten years in tracing the Mendelian behavior until it has become evident that it is a well-nigh universal phenomenon,—no doubt practically co-extensive with sexual reproduction,—the changes in descriptive terminology to which Dr. Riddle deprecatingly refers, have been remarkably slight, and one reads Mendel's original account with wonder that it should still be so modern. Mendel's genius grasped the essentials of this type of inheritance so completely and presented it with such fulness and clarity, that it may doubtless always serve as a good elementary presentation of the subject. But while Mendel's paper is in such essential accord with "present interpretation" as to seem strictly modern, there occurs throughout his whole admirable discussion, not one word of suggestion that he attributes the occurrence of any external character to the presence of an internal particle.

Modern Mendelians as a rule have specifically declined to postulate the presence of material "particles" as the physical bases of unit-characters. Bateson, who has done more than any other to demonstrate the wide applicability of the Mendelian method, clearly placed himself from the first in opposition to any purely morphological interpretation of Mendelian phenomena by giving to his reports to the evolution committee the title: "Experimental Studies in the *Physiology* of Heredity," and he has from first to last carefully guarded all statements with reference to the nature of the genes, in such manner as to be entirely non-committal. Other investigators have either wholly ignored the question, or have usually couched their suggestions in such terms as to show that they were open to any new light upon the subject.

Although I have never looked upon the Weismannian conception of character-determiners as at all plausible, I do not agree with Dr. Spillman[3] that the facts presented by Riddle "*disprove*" the "particle hypothesis." The only manner in which Riddle despatched (?) the "particle hypothesis" was by ruling the observed facts of Mendelian heredity out of court. If the Mendelian phenomena are real, and no one can do careful investigation in this field without becoming convinced that they are, the postulation of "particles" or "bullets" having certain chemical and physiological properties, and behaving during the reproductive process in some such manner as the cytologists are fairly agreed that the chromosomes behave, would offer a complete explanation, and the correctness of such an explanation can not be *disproved* except by proving that some other method of determination is the true one. However, while the particle hypothesis is not disproved, I have no doubt that the Weismannian, and perhaps also the De Vriesian conception of the genes will seem less and less plausible as new facts accumulate.

In Dr. Spillman's[4] brilliant development of what he calls the "teleone hypothesis," a suggestion is offered which virtually makes the chromosomes the "bullets" whose differential properties determine the unit-characters. This interpretation of the Mendelian phenomena has much to commend it, especially as it calls for no structure and no type of behavior which are not already generally recognized as being universally present in the formation of the germ-cells, and it has the added advantage that it seems to be capable of experimental tests.

I can not see, however, that Dr. Spillman has presented "an explanation of Mendelian phenomena without resorting to the idea of unit-characters." If he appears to do so, it is only because he gives to them a new name. The unit-characters are the empirical phenomena for whose explanation the "bullets," "teleones" or genes of any other sort, are devised. It is no new idea that these

[3] In conversation.

[4] Spillman, W. J., "Mendelian Phenomena Without De Vriesian Theory," *Amer. Nat.*, 44: 214–228, April, 1910.

unit-characters are "differentials," as this was recognized by Mendel himself and has been common knowledge to all investigators of Mendelian heredity since.

The length of hair in guinea-pigs and rabbits, the stature of peas, sweet peas, beans, etc., the length of styles in *Primula* and *Œnothera,* density of the heads in wheat and barley, and in fact practically all other characters with which Mendelian investigators have worked, have been so obviously differentials that it is impossible to assume that any Mendelian has ever meant anything else by the expression, "unit-character."

This being true, the contention of Riddle that even in the absence of a given unit-character there is not a complete absence of the particular manifestation in which the essence of that unit-character consisted, or in other words, that the unit-character is simply a phase or "strength" of some "rather general protoplasmic power," is not likely to seriously disturb the Mendelians, since that is a fact with which they have long been familiar.

It appears to me that the unnecessary shifting of the terminology of clearly distinguishable empirical phenomena is undesirable. The unit-characters are *real things* capable of repeated demonstration. They are still differential *characters,* and possess the capacity to behave as *units*, entering into various combinations with other unit-characters and capable of reëxtraction from them, or of being absent altogether, regardless of the manner in which their behavior is explained. The genes, on the other hand,—the ultimate organs of the protoplasm or conditions of the protoplasmic substance upon whose existence depends the capacity to give certain series of reactions, or to pass through certain cycles of ontogenetic development,—are purely inferential. Their nature is not yet capable of demonstration. They are "unknown gods" to whom each new prophet may appropriately apply a new name whenever he ascribes to them new attributes.

While the ultimate nature of the genes lies wholly beyond the powers of present-day analysis, and there is nothing therefore to warrant a departure from the prevailing attitude of suspended judgment, the more intimately the unit-characters themselves are studied, the better will be the basis provided for an understanding of their common properties, and thus finally for an approximation to the nature of the genes which determine them.

The most hopeful directions of approach in the effort to learn more of the true inwardness of the unit-characters, are those of chemical analysis and experimental cytology. As applied to unit-characters, these are almost untouched fields at present, though several investigators have made a beginning. Miss Wheldale, especially, has made a hopeful beginning in the investigation of the chemistry of anthocyanin colors which have continually exhibited typical Mendelian behavior. Several unit-characters which have been recognized and described heretofore only in terms of color-factors, now seem to be capable of description in terms of a chromogen (present in all sweet peas and stocks investigated), and of activators, peroxidases, peroxides, and reducers, thus making the various colors " the result of definite oxidation stages of the chromogen." Riddle has come to much the same conclusion in regard to the nature of the melanin colors, from a consideration of the work of Bertrand, Gessard, Spiegler and others.

In experimental cytology there seems to have been nothing done as yet, which can throw light on the nature of those unit-characters involving the structure and size of parts. How are the number and direction of cell-divisions that shall take place in any cell-lineage determined? Are these also referrable to the presence of definite chemical substances or to definite configurations of protoplasmic molecules? To these questions I believe no satisfactory answer is now possible, but that these processes are controlled in many instances by characteristics possessed by the germ-cells, rests upon aboundant experimental evidence.

While waiting for further information from the chemist and the cytologist, there is still abundant room for the work of the experimental breeder. Owing to the characteristic distribution of the genes at the time of germ-cell formation already described, Mendelian hybridization provides a partial analysis of the germ-plasm, and thus gives some insight into the constitution of living protoplasm. It is of great importance that such analysis be continued until all the unit-differences of plants and animals have been studied, for only when this is done can the full scope and significance of the Mendelian phenomena be understood.

It need scarcely be pointed out that the complete tracing of the germinal analysis which takes place in Mendelian hybrids, is attended with many difficulties. The unit-characters represent capacities for reaction in a certain, very specific way to given conditions of environment. Individuals having the same unit-composition may react in a totally different way to a different environmental complex. Some unit-characters are so sensitive to slight differences of environment that they offer a wide range of fluctuation, or they may represent such a slight differential as to be readily distinguishable only in their plus-fluctuations. Two or more unit-characters may even be indistinguishable from one another as Nilsson-Ehle[5] has shown to be the case in certain unit-characters of wheat and oats, and East[6] in endosperm colors of corn. Many unit-characters are quite invisible except when occurring in combination with some one or more other characters, and this fact has led to what is called the "factor hypothesis." That the factors are real unit-characters, differing in no essential way from ordinarily visible unit-characters, is now in a fair way to be demonstrated by such work as that of Miss Wheldale, and others who are working along similar lines. The implication by some writers that the factor hypothesis is a late development of Mendelism is not correct, as Mendel himself suggested it tentatively. The difficulty of tracing invisible characters necessarily made the development of knowledge regarding them slower than that regarding the easily visible characters, but the essential correctness of Mendel's suggestion has been abundantly substantiated.

All of the foregoing difficulties can be overcome, and are continually being overcome by careful analysis and patient, long-continued breeding tests.

Finally, since we are examining the Mendelian process as one of germinal analysis it is appropriate to discuss for a moment the

[5] Nilsson-Ehle, H., "Kreuzungsuntersuchungen an Hafer und Weizen," 4to, pp. 122, 1909, Lund: Hakan Ohlsson.

[6] East, E. M., "A Mendelian Interpretation of Variation that is Apparently Continuous," *Amer. Nat.*, 44, 65–82, Feb., 1910.

"insoluble residue." Although Mendelian behavior has proved to be nearly universal in those sexually produced plants and animals which are capable of breeding together normally, there are certain clear limitations to the process of analysis. Several instances are known in which differential characters are not segregated, and no analysis takes place with respect to these characters, even when most of the differential characters of the same plants or animals Mendelize in a perfectly typical manner. The relative frequency of this type of behavior may be greater than is now supposed but so far as clear evidence is available permanently blended inheritance of this type is relatively rare except in species-crosses, and in these latter the data is usually too scanty for safe generalization.

Aside from these cases which show a distinctly non-Mendelian mode of inheritance, it must be remembered that Mendelian analysis can be made only in the presence of differential unit-characters possessed by individuals *capable of life and of sexual reproduction,* and that therefore, there can be no test, except under rare circumstances, of the Mendelian nature of the more fundamental vital characters. This leaves it an open question whether the whole of the germ-plasm is a complex of such genes as those which give rise to the phenomena of unit-characters, or whether, with its wonderful general powers of assimilation, growth and reproduction, it consists of a great nucleus of which the genes are relatively superficial structural characteristics.

However, although nothing inconsistent with life and reproduction are ordinarily amenable to Mendelian analysis, this need not detract from the fundamental importance of unit-characters in the study of heredity and evolution, for the phenomena appearing in these fields are subject to exactly the same limitations. All that we know about heredity and evolution *must start* with a plasma capable of life and reproduction.

While thus leaving the absolutely fundamental characteristics of living matter untouched, the Mendelian method and its results have brought into harmonious relations many of the most diverse phenomena of phylogenetic differentiation and it is only fair to assume that they hold still greater promise for the future.

THE NEW VIEWS ABOUT REVERSION.

By C. B. DAVENPORT.

(Read April 23, 1910.)

To the general principle that "like begets like" striking exceptions are not infrequently found. These exceptional cases fall into two classes. In the one class the child possesses some character not visible in either parent but found in a grandparent *e. g.*, blue iris in the chlidren of brown-eyed parents. This is known as *atavism*. In the other class, the child possesses some character not visible in immediate relatives but found in some remote ancestor or even ancestral species; *e. g.*, the reappearance of the Jungle fowl plumage among domesticated poultry. This is known as *reversion*.

Of these two phenomena reversion is the more striking and the explanation that has been current until recently, even among biologists, and is still common among breeders is essentially that of Darwin[1]—"An inherent tendency to reversion is evolved through some disturbance in the organization caused by the act of crossing."

The new explanation is based on the principle that the unit of inheritance is not the individual but some *characteristic* of an organism. Paternal or maternal characteristics are not inherited en masse for the most part but each character independently of every other. A second principle, no less important, is that inheritance is not truly from the parents but from the germ plasm; it is not the adult characters that are inherited but something in the germ cells that will eventually determine those characters. One may dispute the hypothesis of pre-formation in the germ but one can not deny that the egg of an ox is predetermined, certain conditions being fulfilled, to develop into an ox while the egg of man is similarly predetermined to develop into a man. There are probably numerous points of difference in the minute chemical structures of these

[1] Variation of Animals and Plants under Domestication," Chapter XIII.

two eggs and these differences determine the different end results. They may be called *determiners*. Ordinarily, when parents are similar, each unit character of the offspring develops from two similar determiners—one paternal and one maternal. Thus in its origin any unit character is duplex. When, however, the determiner is found in only one of the parents the character is simplex. Now such a character frequently develops imperfectly because of the partial stimulus to development.

If in an individual any character is simplex then the germ cells of that individual are typically of two kinds; half have the determiner for the character and half lack such determiner. Now if two such individuals be parents the chances of the uniting of (*a*) two germ cells with the determiner, (*b*) two germ cells without the determiner, and (*c*) one with and one without are as 1 : 1 : 2, or 25 per cent. of the offspring will have the character duplex; 50 per cent. will have it simplex, while 25 per cent. will lack the character —and will thus resemble one of the grandparents!

To illustrate. If the two grandfathers have blue eyes and both grandmothers brown eyes then the parents may both have simplex brown eyes; they will both form germ cells of which 50 per cent. have and 50 per cent. lack the determiner to form brown iris pigment. From such brown-eyed parents one child in four will have blue eyes like the grandfathers. This is atavism. Cases of atavism can, in general, be explained on the same ground as atavism to blue-eyed grandparents. Complications are indeed induced by sex-limited heredity, illustrated by the horns of sheep which appear, when simplex, only in males of certain strains. A further complication is seen in cases of apparent or partial blending as in human skin color. But in the great majority of cases atavism is a simple reappearance in one fourth of the offspring of the absence of a character due to the simplex nature of the character in both parents.

Reversion in the strict sense has a more complicated explanation. It depends in general on the circumstance that many apparently simple organs or color patterns or colors are really complex and require the coöperation of two or more elementary characteristics called *factors*. For generations a particular character may not appear but when two parents together produce the required factors the combination may be an apparently new, compound character; which we find elsewhere only in remote ancestry.

The facts of reversion are most notorious among domesticated animals and plants. The reason is that man has preserved just those strains of germ plasm that are peculiar in the absence of some typical characteristic or the presence of some new characteristic. These new conditions either cause the ancestral condition to fail of development or mask it over. In hybridizing we restore the factor that is missing from one strain by introducing it from another strain; or we remove the added factor that veils the ancestral condition. Thus the ancestral condition is restored—a reversion occurs.

The foregoing general statement may now be illustrated by some examples.[2] The goldfinch, which has plain chestnut sides, when crossed with a plain yellow canary produced hybrids that have stripes on back and flanks. Darwin mentions this case[3] and adds: "this streaking must be derived from the original wild canary." The results of breeding indicate that the yellow canary has one factor for these stripes—as it were, the potential pattern—but no pigment to bring it out. Adding the factor of pigment from the goldfinch the pattern appears in the offspring.

Reversion in poultry was studied by Darwin. He crossed a White Silkie hen with a Spanish cock which is perfectly black except for the iridescent glossy black in hackle, saddle, wing bar and some of the tail feathers. "As the cocks grew old one . . . became a gorgeous bird. When stalking about it closely resembled the wild *Gallus bankiva,* but with the red feathers rather darker." The hens were black. I have made the same cross with the same result. Moreover, when the hybrids were mated together some of the females as well as males assumed a perfectly typical Jungle fowl coloration. The "reversion" was now complete.

The interpretation of this case on the factor hypothesis is some-

[2] The colored lantern slide illustrations are not here reproduced; they appear in part in the author's works entitled, "Inheritance in Poultry," "Inheritance in Canaries," and "Inheritance of Characteristics in Domestic Fowl," all published by the Carnegie Institution of Washington.

[3] Var. Dom. A. and P., Chap. XIII.

what complex but perfectly clear. All fowl except the booted races (which are of Asiatic origin and allied to the Aseel fowl) contain the determiner—complex for the Jungle fowl color pattern. We may call it J. This factor alone is impotent without a color-producing enzyme (C). The Silkie fowl apparently lacks this in the plumage and so remains colorless. The Spanish has it and so produces the Jungle pattern of which the red portion is nearly coincident with the glossy portion of the Spanish plumage. But the Spanish has an additional factor, an additional black coat (N), which turns the red part of the pattern black. Thus the Spanish color factors are CJN whereas those of the White Silkie are cJn, the small letters indicating the absence of the factors concerned. When the germ cells of the two races fuse the fertilized egg contains factors CcJ_2Nn; which means, the color enzyme is simplex, the Jungle pattern is duplex and the supermelanic factor is simplex. Since N is simplex it is insufficient to cover all of the red in the male, where it is strongest and so the males show some red, only, as Darwin says, it is darker than in the Jungle fowl. Now the germ cells of these hybrids have their characters all simplex: and they are consequently of four kinds: viz: CJN, CJn, cJN, cJn. If two such hybrids be mated their germ cells unite at random. If two germ cells of the first type unite a black bird results; if two of the second type Games result; if two of the other two types unite whites result. I reared 362 offspring from the hybrids; the relation of expected to observed birds of each type of color is shown on Table I.

Table I.

	Black.	White.	Game.
Expected	205	90	67
Observed	210	95	57

One sees that it is not hybridizing *per se* that produces the Game or Jungle coloration, else all would be so colored. It is the union of the requisite ancestral factors one or more of which are missing from the domesticated, fancy strains.

Again, if a White Silkie be crossed with a White Leghorn, the offspring are all white except that the males show some red in those parts of the plumage that are red in the Jungle fowl. In the second generation some individuals, both males and females, show a typical Jungle fowl coloration. In this case the factors in the White Leghorn are the same as in the Black Spanish with the addition of another—the whitening factor (W), probably an antienzyme that stops the work of the pigmentation factors. Thus the formula of the White Leghorn is CJNW and its hybrids with the Silkie have the formula CcJ_2NnWw. The germ cells of the hybrids are of eight kinds, namely, CJNW, CJNw, CJnW, CJnw, cJNW, cJNw, cJnW, cJnw. These give 64 combinations in the fertilized eggs of the second hybrid generation. I raised only 85 chicks. The relation of expected and observed numbers is given in Table II. It will be seen that realization runs close to expectation.

Table II.

	White. (Including F_1 Type of Color.)	Game.	Black.
Expected	69	12	4
Observed	68	16	1

Finally, I may refer to a cross between the Black Cochin and the Buff Cochin. The Black Cochin has the color factor C and it has the supermelanic coat, but it seems to lack the red of the Jungle type of coloration. This modified Jungle type may be called I. The Buff Cochin is like the Black Cochin with the addition of a superxanthic coat (X) which produces the uniform red color. In the second hybrid generation these factors should appear in the combinations shown in Table III.

Table III.

1	$C_2I_2N_2X_2$	Black	2	$C_2IiN_2X_2$	Black	1	$C_2i_2N_2X_2$	Black
2	$C_2I_2N_2Xx$	Black	4	C_2IiN_2Xx	Black	2	$C_2i_2N_2Xx$	Black
1	$C_2I_2N_2x_2$	Black	2	$C_2IiN_2x_2$	Black	1	$C_2i_2N_2x_2$	Black
2	$C_2I_2NnX_2$	Black and red	4	C_2IiNnX_2	Black and red	2	$C_2i_2NnX_2$	Black and red
4	C_2I_2NnXx	Black	8	$C_2IiNnXx$	Black	4	C_2i_2NnXx	Black
2	$C_2I_2Nnx_2$	Black	4	C_2IiNnx_2	Black	2	$C_2i_2Nnx_2$	Black
1	$C_2I_2n_2X_2$	Buff	2	$C_2Iin_2X_2$	Buff	1	$C_2i_2n_2X_2$	Buff
2	$C_2I_2n_2Xx$	Buff	4	C_2Iin_2Xx	Buff	2	$C_2i_2n_2Xx$	Buff
1	$C_2I_2n_2x_2$	White	2	$C_2Iin_2x_2$	White	1	$C_2i_2n_2x_2$	White

each combination having the prefixed frequency and yielding the color indicated. Uniting black and black-and-reds, since red appears in one sex only and not until late in life, we get the relations between the calculated and observed proportions in 86 offspring shown in Table IV. Here again the observed distribution agrees with the hypothesis.

Table IV.

	Black and Black and Red.	Buff.	White.
Expected	65	16	5
Observed	61	17	8

We may conclude, then, that reversion is not due to the act of crossing in and of itself. It is rather due to the restoration of the ancestral factors, neither more nor fewer. The ancestral combination occurs in by no means all of the hybrids but only in a predictable proportion.

In conclusion, a suggestion may be offered as to the frequently observed fact of reversion to the ancestral coloration of domestic fowl which have become feral, as, *e. g.*, in the Hawaiian Islands. Darwin suggests that at least part of the reversion of the domesticated animals that have run wild must be due to the new conditions of life. One observation that I have made sheds some light on the question. When domestic races run wild much intermingling of races occurs and the primitive type of coloration, among others, recurs. When, as is usually the case, this is better fitted to survive because of inconspicuousness or other advantage, it will tend to escape the general slaughter. That selective elimination is real in poultry is shown by an experience of mine. In an open field about 300 chicks ran at large. About 40 per cent. of them were white, 40 per cent. black and 20 per cent. penciled or striped. Twenty-four were killed by crows—all either solid white or solid black except one that was coarsely mottled gray and buff. It is obvious that the self-colored poultry are at a disadvantage because of their greater conspicuousness, and they must be, in time, eliminated, leaving only the striped or penciled birds—those of the ancestral type of color. Reversion is here not a germinal but an environmental result, due, however, not to climate but to organic enemies.

FURTHER NOTES ON BURIAL CUSTOMS, AUSTRALIA.

By R. H. MATHEWS, L. S.

(Read May 7, 1910.)

In previous numbers of this journal[1] I have reported some peculiar burial customs and stones used in magical ceremonies by the Australian aborigines. As the subject has been well received, I beg to submit some additional information obtained by me during a number of years when visiting the Darling River between Brewarrina and Menindee.

I have before described the so-called "widows' caps" and oval balls of kopai, placed upon native graves. Another emblem of mourning sometimes found at aboriginal burying places in the Darling Valley was made as follows: A quantity of burnt gypsum, ground fine and mixed with sand or ashes, to which water was added, and the whole worked into a somewhat coniform cylindrical mass about a foot or fifteen inches long. The large end, or base, of the cylinder, was sometimes elliptical and sometimes approximately circular, having the major diameter from about 5 up to 8 inches, which gradually diminished till near the other end, or apex, which was rounded off like the end of an immense egg. See Fig. 1. In the basal end a deep recess was made, reaching back 6 or 8 inches, and in the largest examples more than a foot, into the middle of the cylinder, resembling in shape the interior of an immense wine glass or goblet. The great depth of this cavity in proportion to its diameter, and its conical shape shows that these articles were not suitable for head ornaments.

The largest example of this sort of article which has yet come under my notice is one in my own private collection, illustrated in the accompanying drawing. Unfortunately, the smaller end was broken off before it was found, leaving the specimen open at both extremi-

[1] PROC. AMER. PHILOS. SOC., Vol. 48, pp. 1–7, 313–318, and 460–462, with illustrations.

ties, with a cylindrical cavity reaching right through it, like a piece of drain-pipe. The thickness of the shell is irregular, being greatest near the base and middle of the shaft, where it is in places about two inches, thinning out to intermediate thicknesses down to less than half an inch at other portions. The outside of the wall or shell

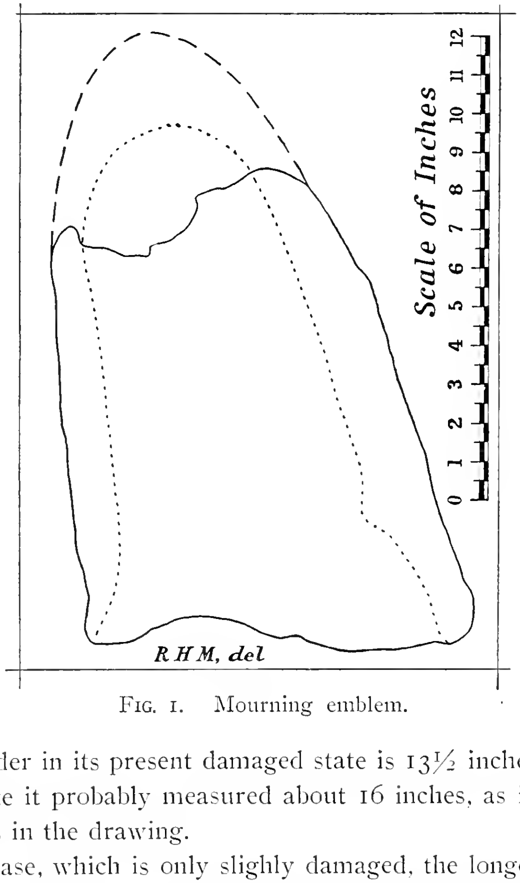


FIG. 1. Mourning emblem.

of the cylinder in its present damaged state is 13½ inches, but in its original state it probably measured about 16 inches, as indicated by broken lines in the drawing.

At the base, which is only slighly damaged, the longest diameter of the cavity or funnel is 8¾ inches, and the shortest 6 inches. The corresponding internal diameters of the orifice at the smaller end are 5½ and 4¾ inches respectively. The outside measurement of the circumference at the base is 28¾ inches and a similar outside measurement at the smaller end is 23 inches. The original circumference has evidently been a good deal reduced by the wasting away of the outer surface by the weather and wind-blown sand, during many years of exposure.

For the purpose of more fully illustrating this exceptionally large specimen, I have introduced a photograph, Fig. 2, which shows the base or larger end of it, with a view right through the hollow interior to the other extremity. Near the distal end of the funnel

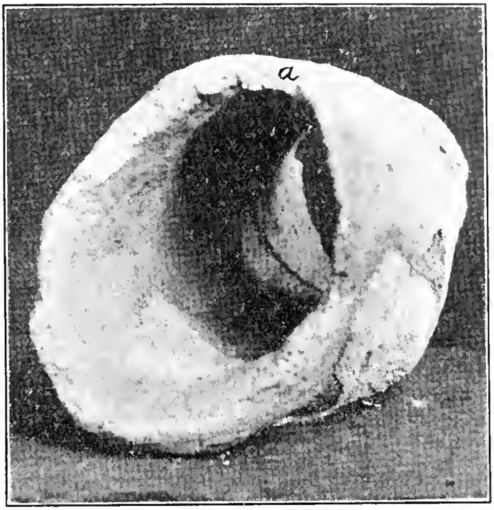


FIG. 2. Mourning emblem. View through the interior.

shaped cavity there is a discolored streak in the wall or shell, which can be seen in the photograph. Such a mark could have been caused by a stoppage of the work or additional material, which was not quite of the same shade along the joining line. There is a thin, irregularly shaped patch on the outside of the cylinder, which was evidently put on after the main trunk had been completed, either to secure a uniformly rounded contour or to remedy some defect in the original structure.

When the specimen was discovered, it was lying in the position shown in Fig. 2, with all the lower part embedded in the sandy bank of Lake Tongo, and had apparently lain in that position for a long time. The upper half, from *a* right back to the other end, was fully exposed to the weather for many years, and is consequently much diminished in thickness by disintegration. It is much the thinnest part of the shell, being in places less than half an inch thick. The lower half of the photograph represents the thickest part of the shell from the front to the rear.

My specimen was found on Tongo Station, near Lake Tongo, which flows into the Paroo River. Tongo is in the county of Fitzgerald, in the northwestern portion of New South Wales, about 80 miles north-northeast from Wilcannia, and is approximately in latitude 30° 30′ and longitude 143° 40′.

A somewhat similar specimen to that illustrated, but smaller, was found some years ago a few miles northwest from Tilpa, Darling River, and is now in a private collection to which I was allowed access. I had not time to make a complete drawing, but I took the following measurements. Complete outside length from base to apex, 12½ inches. The cavity in the base occupies the whole of the interior of the article, extending back in a conical form for 9 inches. The longer diameter of the orifice at the base is 7¾ inches, and the shorter 7 inches. There are no marks of a net on the inner wall and it is improbable that such ever existed. The circumference at the base, outside measurement, is 30 inches. The shell is somewhat thin throughout, ranging from a quarter to three quarters of an inch thick, until near the top of the cone, where it is about 3 inches in thickness. Like my own specimen, it has been reduced in size by exposure to the weather.

Another specimen which I have seen is a solid conical mass of kopai about ten inches long and six inches in diameter at the base or larger end. In the base is a shallow concavity, the depth of which is only about an inch and three quarters, without any indication of the marks of a net. Such an article could not be worn on one's head, because unless it were kept continually balanced, it would fall off. Its great weight would also be prohibitive. In my opinion, both this and the last described specimen, were not intended for wear, but were made for the purpose of being deposited upon graves by the relatives of the parties interred. This was done immediately after the burial, whereas the widow's cap was not deposited till the termination of her period of mourning.

As regards the purpose of articles such as that now illustrated, it is hard to obtain full particulars, because they are not used by the remnants of the tribes now living on the Darling River. It is not likely that they have been worn on the head, like the "widows' caps" described by me last year,[2] because the opening in some of them is too small to fit any adult skull, whilst others are too large and heavy. The great depth of the hollow—13 inches in my specimen—would be needless as a receptable for the head; and there are no impressions of a net on the inner wall similar to those found on "widows' caps." An old black fellow whom the white people called "Jimmy," a head man of the Ngunnhalgu tribe, who resided most of his later years at Marra Station, on the Darling, and who died about ten years ago, said the articles with the deep cavities were not worn on the head, but were laid upon the graves of old men and women of tribal importance, in the same way that the kopai balls were deposited.[3]

Mr. J. E. Suttor writes me as follows:

> In 1880, while mustering cattle on the back part of Curranyalpa run, I came across two old black men and a woman camped at a waterhole. They had shifted out from the Darling river to hunt opossums for the skins. The old woman was lying in the camp very sick. A few days later the two black fellows passed my camp, which was about five miles from their own, and told me the old woman had died. Going that way a copule of days afterwards I found the grave on a pine ridge close by the camp I had previously seen, and upon it were lying two hollowed kopai articles somewhat resembling widows' caps. At the place where the camp fire had been was a piece of bark, with the remains of kopai plaster upon it, together with some lumps of kopai, burnt and ready to break up, if more had been required.

The woman's husband and the other man, who was probably a relative, had each left a token of their sorrow upon the grave before they went away.

It is well known that human skulls were used as water vessels by the aborigines in several parts of Australia. Mr. E. J. Eyre saw some drinking cups of this sort, and gives an illustration of one.[4] The tribes referred to by Mr. Eyre adjoined the Darling River

[2] Proc. Amer. Philos. Soc., Vol 48, pp. 316–318, Figs. 5 and 6.

[3] *Op. cit.*, pp. 313–315, Figs. 1 to 4.

[4] *Journs. Expeds. Discov. Cent. Aust.* (London, 18—), Vol. 2, pp. 310, 316 and 511, plate IV., fig. 20.

people on the west. When surveying on the Darling and Paroo Rivers in 1884–5, I met an old black fellow who had a skull among his paraphernalia, which he used for drinking purposes on ceremonial occasions. Old "Jimmy," already quoted, told me that in addition to their use as mourning emblems, the kopai articles were imitation skulls which the spirits of the dead were supposed to use for their water supply in that arid district.

In Fig. 1, which is drawn to scale, the firm or continuous line represents the exterior of the article in its present state, viewed from the side. The broken line at the top of the drawing indicates the supposed form when first manufactured. The dotted line shows the limit of the internal cavity, if the shell were transparent and one could see through it. The total length of the article, outside, by the assumed restoration, is 16 inches; and the depth of the internal hollow space, as restored, is about 13 inches. No impressions or marks of a net are visible upon any part of the inner wall.

The present weight of the article is just a little over 15 pounds. At a moderate estimate, the portion broken off would weigh about 3 pounds, making the weight of the complete article about 18 pounds. Then we must take into consideration that the whole outside surface has suffered by disintegration during many years' exposure to the weather. The same would apply to the inside surface of the cavity, though in a lesser degree. The original weight has also been diminished by the drying of the material in the sun for such a long period. The reduction in the weight due to the two combined causes mentioned is difficult to estimate; but judging by the worn and contracted appearance of many parts of the surface, I think 2 pounds could be allowed for it. This would bring the total weight of the article, when it left the maker, up to about 20 pounds.

Conclusion.

Before concluding this paper, it has been thought desirable to supply a few additional remarks respecting my three previous articles on ceremonial stones and burial customs, published in the Proceedings of this Society, Vol. 48, pp. 1–7, 313–318 and 360–362. When the present article is perused in conjunction with the three papers quoted, the reader will have a fairly complete account of the customs dealt with.

Magic Stones.—In regard to the ceremonial or magic stones, old "Harry Perry," the black fellow already quoted, told me that among other uses, these stones were employed for bringing rain, and this statement was repeated by old "Jimmy," of Marra Station, and by other old blacks. An old man took one of these talismans and placed it in the fork of a tree, several feet from the ground, in such a way that the saucer-shaped depression in the base was uppermost. He then sat down and sang the usual rain-producing song, after which he went away to his camp. It was necessary that great secrecy should be observed, because if any person saw the operation it would be a failure.

Some of these magic stones were used for bringing "dust-storms" which are very common in that arid part of New South Wales, in which case the procedure was the same as for rain, excepting that the song was differently worded. Moreover, a stone to produce "dust-storms" was reddish in color, like those numbered 1 to 4 in Fig. 2, Vol. 48, p. 462.

It must be explained that the country along the valley of the Lower Darling and northward from that river to the Queensland boundary, comprises many large stretches of red-colored, sandy soil, upon which very little grass or herbage grows. When a strong gale sweeps over this district in a dry season, it disturbs the loose soil, and separates from it vast quantities of fine, reddish dust, which rise and darken the air like a fog, which is sometimes so dense that one cannot see more than a few yards in any direction. Such visitations are known as "dust-storms," and are also facetiously spoken of as "Darling showers."

On the approach of a "dust-storm," kangaroos, emus, etc., hurry away to the scrubs and timbered places, where they are in the habit of taking shelter in wet weather and in times of danger. The blacks are aware of this practice of the animals, and when they think a "dust-storm" is coming, a couple of men go away and hide themselves in one of these scrubs. Two or more scrubs may be manned in the same way. The hunters place themselves in such a position that when the animals come moping about in the fogginess caused by the dense clouds of dust, they can be speared or clubbed without difficulty. When a "dust-storm" was wanted for hunting purposes, a man who had a reddish or dust-colored ceremonial stone, took charge of the function.

Another use of a "dust-storm" was to obliterate the foot-marks of men or animals. A party who had been out on a marauding or murdering expedition, would bring up a "dust-storm" to prevent their enemies from following their tracks. Or a party of hunters could be frustrated in their operations if an adverse conjurer brought up a "dust-storm."

Mr. Tobin, who has lived a long time in the Darling district, tells the following. During a very dry time at Enngonia, on the Warrego River, a few old black fellows were camped on the bank. They were believed to possess the paraphernalia requisite for performing all the supernatural feats professed by a medicine man. One day a stockowner and one or two of his friends visited the camp, and asked the black fellows how it was that they did not bring a downpour of rain. He jocularly said that he would give so much flour and tobacco to any of the old natives who could break up the drought. One of them, called "Gurara Charlie," who was very anxious to take advantage of the offer, talked the matter over with his fellow conjurers, but it transpired that the stone which he possessed was only a *dust*-producing implement. One of the other blacks, "Jimmy Kerrigan," said that he had the right sort of stone for making *rain*, and he undertook the job for the promised reward. He was, unfortunately, not successful on that occasion, but attributed his failure to the malignity of an adverse conjurer who lived somewhere down the river.

When the stones were used for producing an abundant supply of nardoo, or other grass seeds, or for the increase of game, as stated at page 7, Vol. 48, the words of the incantation sung by the old performer were varied to suit the case. Moreover, when it was thought that enough rain had fallen, the magic stones were employed for bringing about fine weather. And when it was desired to prevent a "dust-storm" from rising, or to shorten its duration, these charms were likewise in requisition, with a suitable accompaniment.

Widows' Caps.—Although a widow's head-dress invariably consisted of a cap similar in shape to those illustrated at pages 316–317, Vol. 48, they were also worn by a woman for an adult son or daughter, or for a favorite brother or sister. Their use was not restricted to the women only. Old "Marra Jimmy," already quoted, said that men sometimes wore such a cap in mourning for their mothers, mother's sisters, their own elder sisters, their wives and other blood relatives of mature years. Generally speaking, however, the men used the kind of articles described in this paper, which were never worn on the head, but were deposited at the grave.

The facts just narrated would account for the comparatively large numbers of so-called "caps" which have been found on individual graves. Mr. T. Worsnop mentions four found on a grave, one of which weighed fourteen pounds.[5] A friend writes me that he has seen five caps similarly used. A station owner on the Darling River informs me that many years ago there was an aboriginal grave not far from his homestead, which had nearly a dozen articles which appeared to him to be caps, lying upon it. They were not all of one size, but comprised some very large ones, others of medium size, whilst others were smaller. In the cases just mentioned it is likely that some were widow's caps; some had possibly been worn by men; whilst others were manufactured as tributes of mourning. The latter kind could be placed upon the grave as soon as the body was buried, whereas a "cap" could not be deposited till the wearer's term of mourning had expired.

The "widow's cap" illustrated in a former article (p. 316, Vol. 48), is made from kopai, with only a small mixture of sand or ashes, because kopai is abundant over a large portion of the Darling valley. But on some of the tributaries of the Darling, such as the Macquarie, Mara, Bogan, etc., where gypsum is found only in small quantities, the mourning caps are made out of a brown-colored tenacious mud, obtained from the bottoms of waterholes and streams, without any admixture of gypsum. Yellow or reddish

[5] "Aborigines of Australia" (Adelaide, 1897), p. 62.

clay, sometimes found cropping out on the slopes of ridges, or banks of watercourses, was also utilized for the same purpose. The shape of the cap was the same, no matter what the material consisted of. Clay caps, when removed from the head, and exposed to the weather on a grave, soon became disintegrated and fell to pieces; hence none of the caps of this material have been preserved by the white people.

White is the favorite color for mourning among the Australian aborigines, but when it cannot be obtained other colors must be substituted for it. It is perhaps needless to add that a cap of any sort, whether made of kopai or mud, and whether worn by a man or a woman, was removed during sleeping hours. It was also left in camp when the wearer, of either sex, was away in search of food, or while doing any kind of work.

PARRAMATTA, N. S. WALES, March 5, 1910.

SOME RELATIONS BETWEEN SUBSTITUTION GROUP PROPERTIES AND ABSTRACT GROUPS.

By G. A. MILLER.

(Received June 4, 1910.)

Dyck observed that the necessary and sufficient condition that a transitive substitution group G of degree n is primitive is that its subgroup G_1 which is composed of all the substitutions of G which omit one letter is a maximal subgroup of G[1]. This establishes an important relation between primitive substitution groups and the properties of G considered as an abstract group. It has also been observed that abstract group properties correspond to the different degrees of transitivity of substitution groups[2], and that the number of ways in which an abstract group can be represented as a transitive substitution group can be directly deduced from the properties of the group. These and other known relations have done much towards uniting the theories of abstract groups and those of substitution groups, and towards making these theories mutually helpful. It is the object of the present paper to establish other important relations between these two theories, especially as regards abstract groups and simply transitive substitution groups.

§ 1. *Some properties of co-sets.*

If H represents any subgroup of index ρ under a group G all the operators of G may be arranged so as to give ρ distinct sets both as regards right-hand and as regards left-hand multiplication, in the following forms:[3]

$$G = H + HS_2 + HS_3 + \ldots + HS_\rho,$$
$$= H + S_2H + S_3H + \ldots + S_\rho H.$$

[1] *Mathematische Annalen*, vol. 22 (1883), p. 94.
[2] *Messenger of Mathematics*, vol. 28 (1899), p. 107.
[3] *Bulletin of the American Mathematical Society*, vol. 16 (1910), p. 454.

The sets HS_α, $S_\alpha H$ ($\alpha = 2, 3, \ldots, \rho$) are known as *co-sets* of G as regards H. Galois called attention to the importance of the case when each $HS_\alpha = S_\alpha H$ for every operator of H. That is, for every operator of H in the first member there is some operator of H in the second member so that this equation may be satisfied. The necessary and sufficient condition that H is invariant under G is that such equations may be established for every value of α from 2 to ρ.

The co-sets HS_α. $S_\alpha H$ will be called right and left co-sets respectively. The totality of these co-sets is independent of the choice of the operators $S_2, S_3, \ldots, S$; that is, there is only one category of right co-set, and only one of left co-sets as regards any given subgroup. Each left co-set is composed of the inverses of all the operators in a right co-set and vice versa. Hence we may say that the necessary and sufficient condition that a subgroup H is invariant under a group G is that every right co-set of G as regards H is identical with some left co-set as regards H, or that every left co-set is identical with some such right co-set. In other words, the necessary and sufficient condition that H is invariant under G is that the number of distinct co-sets to which H gives rise is equal to its index under G diminished by unity, or that the totality of its right co-sets is identical with the totality of its left co-sets. This theorem is included in the theorem that the necessary and sufficient condition that H is invariant under a right co-set is that this co-set is identical with some left co-set, or vice versa.

With the extreme case in which each right co-set is identical with some left co-set we may contrast the other extreme case when each right co-set has some operator in common with every left co-set. It has been seen that H is invariant in the first case and we shall see that it gives rise to a multiply transitive substitution group (K)[4] in the second case, excluding the trivial case when the order of K is 2. In the proof of this theorem it will at first be assumed that H is neither invariant under G nor does it involve any invariant subgroup of G besides the identity. That is, we shall at first assume that K is a transitive substitution group which is simply isomorphic with G.

[4] Dyck, *Mathematische Annalen*, vol. 22 (1883), p. 91.

The subgroup (K_1) which is composed of all the substitutions of K which omit a given letter a is simply isomorphic with H. Hence the co-sets of G as regards H have the same properties as the co-sets of K as regards K_1. In the latter case, each of the right co-sets is composed of all the substitutions of K which replace a by the same letter, while each of the left co-sets replaces a by all the letters of a system of intransitivity of K_1. Hence it results that in this case the necessary and sufficient condition that K is multiply transitive is that each right co-set as regards K_1 has at least one operator in common with every left co-set.

When H is invariant under G it results that K is a regular group and the theorem given above requires no proof since a regular group cannot be multiply transitive. When H involves an invariant subgroup of G but is not itself invariant under G, K will be the quotient group of G with respect to the maximal invariant subgroup of H which is also invariant under G. It is evident that the reasoning employed above applies directly to this quotient group, and hence we have proved the following theorem: *The necessary and sufficient condition that a given subgroup of a group gives rise to a multiply transitive representation of the group, or of one of its quotient group whose order exceeds two, is that every right co-set with respect to this subgroup has at least one operator in common with every left co-set with respect to the same subgroup.* This theorem may also be stated as follows: the necessary and sufficient condition that H gives rise to a multiply transitive group is that all the operators of G are included in the two sets H and HSH, S being any operator of G which is not also in H.

When the multipliers $S_2, S_3, \ldots, S_\rho$ in the right co-sets are the same as those in the left co-sets (this is possible for every subgroup of G) two co-sets are said to correspond when they have the same multipliers; that is HS_α and $S_\alpha H$ are two corresponding co-sets. It results from what precedes that this correspondence can be established in only one way when H is invariant under G and it can be established in $(\rho-1)!$ ways when H gives rise to a multiply transitive group. Conversely, when this correspondence can be established in only one way H must be invariant, and K must be multiply transitive whenever it can be established in $(\rho-1)!$ ways.

It should also be observed that the operators of each right co-set are evenly distributed among a certain number of left co-sets whenever they do not all occur in the same left co-set. Similarly the operators of each left co-set must be evenly distributed among the right co-sets. As the transitive constituents of K_1 are not necessarily of the same degrees it results that the operators of two right co-sets are not necessarily distributed among the same number of left co-sets, or vice versa. It may be observed that the method of dividing all the operators of a group into those of a subgroup and the corresponding co-sets was used by Abbati in 1802 and hence it is one of the oldest processes in group theory. This may add interest to the theorem given above in regard to the relation between a multiply transitive representation and the properties of the corresponding co-sets.

§ 2. *Transitive constituents of the subgroup composed of all the substitutions which do not involve a certain letter.*

Suppose that the operators of the right co-set HS_2 are found in the λ left co-sets S_2H, S_3H, . . ., $S_{\lambda+1}H$. The totality of operators HS_2H must therefore give all the operators of these λ left co-set, each operator occurring as many times as there are common operators in H and $S_2^{-1}HS_2$. Hence λ is the index under H of the subgroup composed of these common operators. On the other hand, λ is the degree of the transitive constituent of K_1 which involves the letter replacing a in the substitution corresponding to S_2 in K. Hence *the necessary and sufficient condition that K_1 involves a transitive constituent of degree n_1 is that G involves a substitution which transforms H into a group which has a subgroup of index n_1 in common with H.* In particular, the necessary and sufficient condition that K_1 omits β of the letters contained in K is that H is invariant under a subgroup of G whose order is β times that of H; when $\beta = \rho$, K is regular and vice versa.

From the theorems proved in the preceding paragraph it is easy to deduce abstract group theory proofs for a number of theorems relating to simply transitive primitive groups. If K is such a group, K_1 is maximal, and if one of the transitive constituents in K_1 is of order p, p being a prime number, K_1 involves an invariant subgroup of index p, as well as a transitive constituent of degree p.

Hence its order is a power of p. As H and $S_2^{-1}HS_2$ have a subgroup of index p in common and as such a subgroup is always invariant in a group whose order is a power of p, it results that these common operators form an invariant subgroup of K. This is impossible unless the order of this common subgroup is unity and hence K, cannot involve a transitive constituent of order p unless the order of K_1 is p.[5]

When K_1 involves a transitive constituent of degree n_1, H and $S_2^{-1}HS_2$ have a subgroup of index n_1 in common and vice versa as was proved above. It is also known that G involves a multiple of n_1h operators which transform H into a group having exactly a subgroup of index n_1 in common with H. It is evident from the above that there are exactly n_1h such operators for every transitive constituent of degree n_1 in K_1. Hence we have the following theorem: *If G involves kn_1h operators which transform H into a group having exactly a subgroup of index n_1 in common with H then must K_1 have exactly k transitive constituents of degree n_1 and vice versa.* In particular, K is multiply transitive when K_1 is transitive and of degree $\rho-1$. Hence the necessary and sufficient condition that K is multiply transitive is that G contains an operator which transforms H into a group which has exactly a subgroup of index $g/h - 1$ in common with H, g and h being the orders of G and H respectively. This theorem gives meaning in certain cases to the theorem, if a subgroup H_1 of a given group G has exactly ρ operators in common with a conjugate of a subgroup H_2 of G, then the number of the operators of G which transform H_2 into subgroups having exactly ρ operators in common with H_1 is $kh_1h_2 \div \rho$, h_1 and h_2 being the orders of H_1 and H_2 respectively. The given theorem gives a meaning to k whenever H_1 and H_2 belong to the same system of conjugates.

From the main theorem of the preceding paragraph it is easy to obtain the degrees of all the systems of intransitivity of K_1. To obtain the orders of the transitive constituents of K_1 it is only necessary to observe that if $S_\alpha HS_\alpha^{-1}$ has a subgroup of index n_1 in common with H and if the largest invariant subgroup of H contained in this common subgroup is of index m_1 under H then the

[5] *Proceedings of the London Mathematical Society*, vol. 28 (1897), p. 536.

corresponding transitive constituent in K_1 is of order m_1 and of degree n_1. The fact that its order is m_1 results directly from the facts that the n_1 right co-sets which involve all the operators of HSH have the property that each of them is unchanged when multiplied on the right by any one of the operators of this invariant subgroup. Moreover, it is evident that the identity of each transitive constituent of K_1 must correspond to an invariant subgroup of K_1. Hence it results that *If $H_1, H_2, \ldots, H_\lambda$ be any complete set of conjugate maximal subgroups of G then each index under any one of these subgroups as regards the largest invariant subgroup which it has in common with any other is divisible by the same prime numbers for every possible pair in the set of conjugate subgroups.*

As a special case of the theorems proved in the preceding paragraph we have the following: The necessary and sufficient condition that K_1 is composed of simply isomorphic transitive constituents is that every invariant subgroup of H which occurs in one of its conjugates under G occurs also in all of these conjugates. This condition must clearly be fulfilled when K_1 is transitive; that is, in this case H cannot have an invariant subgroup in common with any one of its conjugates unless this subgroup is also invariant under G. This theorem exhibits an interesting abstract group property which corresponds to the property that K_1 is composed of simply isomorphic transitive constituents and with those mentioned above establishes more completely the principle of duality as regards substitution groups and abstract groups.

§ 3. *Transitive representation as regards right and left co-sets.*

Since both the right and the left co-sets of G are determined by the subgroup H each of these two categories of co-sets together with H forms a totality whose elements are permuted among themselves when the former are multiplied on the right and the latter on the left by operators of G. The ρ sets obtained by adding H to the right co-sets will be called the *augmented* right co-sets. Similarly we shall use the term augmented left co-sets for the ρ sets obtained by adding H to the left co-sets. It is known, and also evident, that the permutations among themselves obtained by multiplying the augmented right co-sets on the right successively by all the operators of G constitute a substitution group K which is simply isomorphic to the quotient group of G with respect to the largest subgroup of H which is invariant under G.

To see very clearly the relation between G and K it is perhaps best to assume at first that they are simply isomorphic. We shall represent by K_1 the subgroup of G which corresponds to H in the simple isomorphism between G and K. Hence K_1 is also composed of all the substitutions of K which omit a given letter a, as was observed in the preceding section. As each of the co-sets is composed of all the substitutions of K which replace a by a particular letter we may name each of these co-sets by this particular letter. In particular, K_1 will be represented by a. If this is done it is evident that the permutations of the augmented co-sets due to multiplying all these co-sets on the right by the same substitution will be identical with this substitution. That is, the substitutions of K will only be repeated by the permutations of the augmented right co-sets when all of them are multiplied on the right by all the substitutions of K. That this arrangement was possible is a direct consequence of the manner in which K was constructed, the details which we gave are intended to exhibit more clearly how this may be done.

We proceed to consider the substitution group K' which corresponds to the permutations of the augmented left co-sets when these are multiplied successively on the left by all the substitutions of K, in order. We again suppose that K_1 corresponds to H and that it is composed of all the substitutions of K which omit a. The left co-sets are composed separately of all the substitutions of K which replace a particular letter by a. We shall name each of these co-sets by this particular letter and hence H will again be denoted by a. Multiplying each one of these augmented left co-sets on the left by the separate substitutions of K gives a permutation of these co-sets represented by the inverse of the multiplying substitution. Hence we again obtain a repetition of all the substitutions of K if we consider the permutations of the augmented left co-sets when these are multiplied on the left by all the substitutions of K in order.

From the preceding paragraphs it results that the right and left augmented co-sets may be so named respectively that there results the same substitution group when the left augmented co-sets are multiplied on the left as when the right augmented co-sets are multiplied on the right. When H is invariant under G K is regular, and K and K'' are each composed of all the substitutions on these letters which are separately commutative with every substitution of the other, K'' being obtained by left-hand multiplication when the co-sets (which reduce to single operators in this case) are named the same as in the right co-set. As K'' reduces to K when these co-sets are named in the manner noted above it results from what has been proved that K and K'' are conjugate. When K is composed of substitutions of order two in addition to the identity, the two given methods of naming the co-sets will coincide and hence the given process also gives, as a special case, a proof of the theorem that every group in which all the substitutions besides the identity are of order 2 must be abelian. It should be added that the main results of this section are not new but the subject is so important that these details should be of some interest as they throw new light on the entire process.

University of Illinois,
May, 1910.

SERPENTINES OF THE CENTRAL COAST RANGES OF CALIFORNIA.[1a]

(PLATES XXXIV AND XXXV.)

BY H. E. KRAMM.

COMMUNICATED BY PROF. J. C. BRANNER.

(Received June 6, 1910.)

CONTENTS.

INTRODUCTION.

It is the object of this paper to present a petrographical description of serpentines in the central coast ranges of California.

The areas under discussion are found in the region south from Clear Lake in Lake County to the New Almaden Mine in Santa Clara County, a distance of about 140 miles, and aggregates perhaps 300 square miles.

[1a] A thesis for the degree of A.M. presented to the Department of Geology of Stanford University, May, 1910.

These serpentines were designated by G. F. Becker as being derived from sedimentary rocks, which he believed had obtained the magnesia necessary for their transformation into serpentine by a process of substitution.[1] Previous to his report which appeared in 1888 some Colusa County serpentines had been described by M. E. Wadsworth as lherzolite serpentines.[2] Others, the Mount Diablo,[3] Potrero,[4] Angel Island,[5] San Francisco Peninsula[6] and the Oak Hill[7] serpentines have since been shown to be derivatives of eruptives.

These latter areas aggregate perhaps ten to fifteen square miles which is, according to Becker, about one per cent. of the total serpentines in the coast ranges. Their investigation, aside from having demonstrated that the rock is derived from eruptives, has also shown interesting variations in the serpentine itself.

Taking this into consideration, a study of the remaining areas seemed desirable, even though such might not produce new and startling facts.

The accompanying map shows the distribution of the rock in the central coast ranges. Existing maps proved of valuable assistance in the field as well as in the preparation of the map. While some of the data for it were thus obtained, other data were obtained by the writer, who is aware that there may be serpentines especially in Napa, Marin and Solano counties which may have escaped his attention.

[1] G. F. Becker, "Quicksilver Deposits of the Pacific Coast," Monograph XIII, U. S. G. S., 120–128, Washington, 1888.

[2] M. E. Wadsworth., "Lithological Studies," *Mem. Mus. Comp. Zool. Harvard College*, 1884, 129–132.

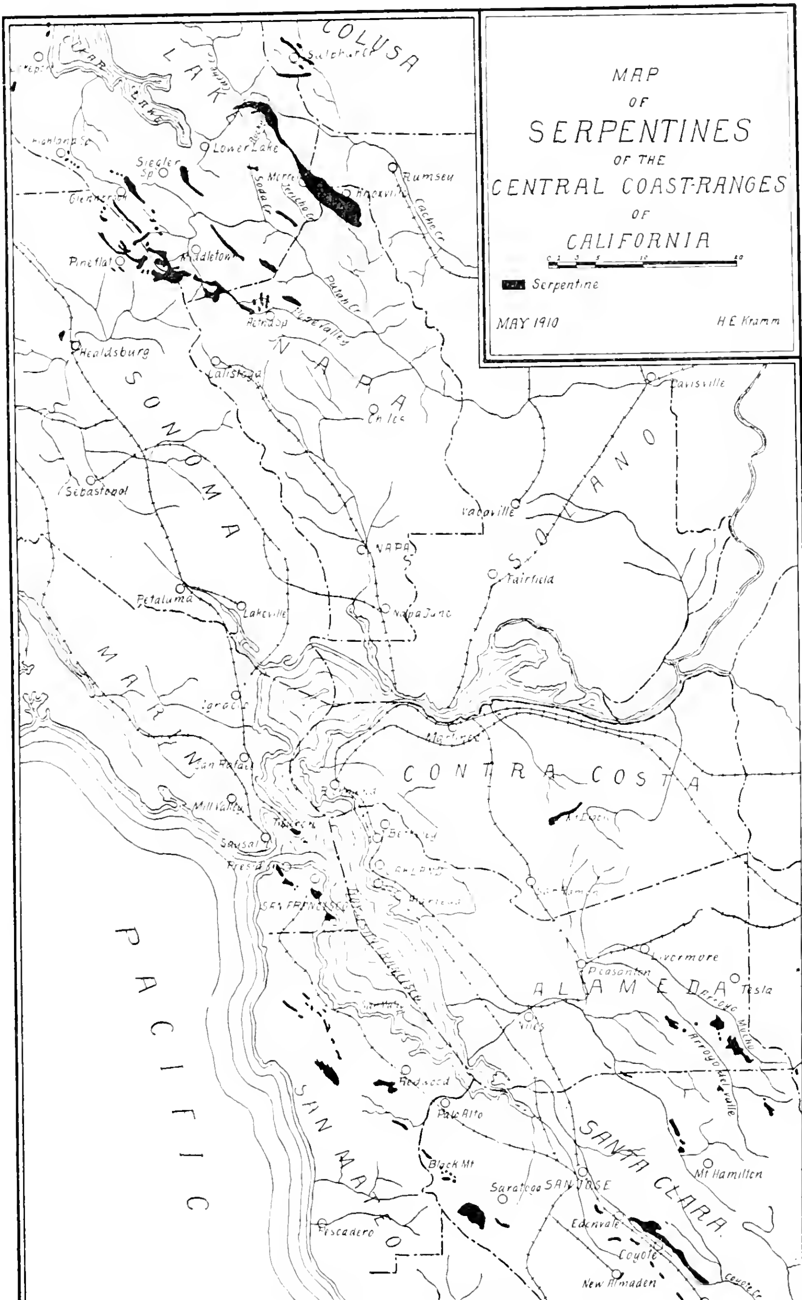
[3] H. W. Turner, "The Geology of Mount Diablo, California," *Bull. Geol Soc. Am.*, II., 388–391, 1891.

[4] C. Palache, "The Lherzolite Serpentines and Associated Rocks of the Potrero, San Francisco," *Bull. Geol. Dept., Univ. Cal.*, I., 164–169, 1894.

[5] F. L. Ransome, "The Geology of Angel Island," *Bull. Geol. Dept. Univ.* Cal., I., 219, 1894.

[6] A. C. Lawson, "Geology of the San Francisco Peninsula," 15th Annual Report U. S. G. S., 445, Washington, 1893–94.

[7] E. P. Carey and W. J. Miller, "The Crystalline Rocks of the Oak Hill Area near San José, Cal.," *Journal of Geol.*, XV., 160, 1907.


MAP
OF
SERPENTINES
OF THE
CENTRAL COAST-RANGES
OF
CALIFORNIA
Serpentine
MAY 1910
H E Kramm
COLUSA
LAKE
NAPA
SONOMA
SOLANO
MARIN
CONTRA COSTA
ALAMEDA
SAN MATEO
SANTA CLARA
PACIFIC
Lower Lake
Rumsey
Knoxville
Middletown
Pine flat
Glenbrook
Siegler Sp.
Healdsburg
Calistoga
Sebastopol
Vacaville
Davisville
NAPA
Fairfield
Napa Junc
Petaluma
Lakeville
Ignacio
San Rafael
Mill Valley
Sausalito
Tiburon
Martinez
Livermore
Pleasanton
Tesla
Niles
Redwood
Palo Alto
Black Mt
Saratoga
SAN JOSE
Mt Hamilton
Edenvale
Coyote
New Almaden
Pescadero

General Location of the Serpentines.

For the sake of description and convenience the areas have been divided into districts. These are named and situated as follows:

I. The Sulphur Creek district is about twenty miles east of Clear Lake. It is situated about the headwaters of the Sulphur Creek, a tributary to Cache Creek, and embraces the Lake and Colusa County dividing line.

II. The Knoxville and Clear Lake district comprises all those serpentine areas which are found about the point at which Lake, Yolo and Napa counties meet. It is limited in the north by Cache Creek and in the south by Putah Creek. It also includes the areas found in the immediate neighborhood of Clear Lake.

III. The Mayacmas district is bounded in the north by Putah Creek. It embraces the serpentines found along the Mayacamas range, which from Mount St. Helena extends to the northwestwards.

IV. The San Francisco district takes in all areas of serpentine found on the San Francisco peninsula, and areas in the vicinity of San Francisco.

V. The Coyote Creek and Black Mountain district comprises all serpentines which are found in the country drained by the Coyote Creek, south of San Jose, besides these the serpentines found in the Black Mountain region.

VI. The Mount Diablo and Mount Hamilton district includes all serpentine areas which are found in the country between these mountains.

I. The Sulphur Creek Serpentines.

The serpentines of this district form two belts, each one several miles in length, and in width varying from 100 feet to one half mile. Besides these, there are two smaller areas.

The first and most important belt is encountered about 100 yards west of the Wilbur Springs Hotel. From here it extends five miles in a northwest direction, forming the backbone of a ridge which stands out prominently above the surrounding country. To the south it extends a distance of one mile, and closely approaches the second belt of serpentine.

The second belt has an east-and-west trend, and follows for about three miles the ridge that forms the Colusa and Lake County dividing line, then extends one mile to the west.

Of the two smaller areas one is found 100 yards northwest of the Wilbur Spring Hotel, the other is best located by the Wideawake quicksilver mine. Both aggregate only a small fraction of a square mile, but the former has attained a certain local fame, due to the abruptness with which it terminates, exposing an almost perpendicular height of about 100 feet, which is known as the lovers' leap.

With the exception of the last the serpentine outcrops are very low. They seldom protrude more than a few feet above the surrounding soil, the removal of which keeps pace with the erosion of the rock. All stages of decomposition can be observed. Very fresh rock is found where Sulphur Creek cuts the first named belt of serpentine. It is exceedingly brittle, of a dark green color, massive, and shiny individuals of pyroxene give it a mottled appearance. As it weathers it assumes a brownish color, and finally gives rise to a brown soil.

Thin sections from fresh serpentine show a colorless mass with a slight green tinge with now and then patches of a more intense green, the whole somewhat pleochroic. Crossed nicols reveal serpentine to be the predominating mineral, with enstatite, diallage and olivine less abundant. Minor quantities of picotite, chromite and magnetite are present.

The microscopical investigation was supplemented by a chemical analysis made by the writer in the mineralogical laboratory of Stanford University, of fresh serpentine from the bed of Sulphur Creek, with the following results:

The analysis shows a very basic rock, and points towards a peridotite. It corroborates the microscopical investigation. Using the theoretical formula for serpentine the approximate ratio of serpentine to other minerals present can be calculated by assuming that all the water goes with the serpentine. The ratio thus obtained is four to one.

The above presented evidence shows that the parent rock was an eruptive, very basic, and contained the minerals which define a lherzolite. The serpentine is therefore a lherzolite serpentine.

Analysis of Serpentine, Sulphur Creek, Colusa Co.
H. E. Kramm, analyst.

	Per Cent.
SiO_2	37.62
Al_2O_3	1.20
Fe_2O_3	8.60
FeO	2.15
MgO	37.59
CaO	2.49
Na_2O	.27
K_2O	trace
H_2O	10.46
Cr_2O_3	.36
TiO_2	trace
	100.74

While the foregoing only treats the massive facies of the serpentine it is of interest to study the other facies present. The rock as a whole shows an advanced state of decomposition. It is much shattered and slickensided, due no doubt to the increase of volume of the parent rock in the course of hydration into serpentine, and the resulting pressure. Angular fragments, bluish-green to brown in color, crumbling away under slight pressure often enclose rounded boulders of an apple green tough serpentine. These are traversed in all directions by numerous veins of chrysotile, reaching sometimes a thickness of two centimeters. The chrysotile being more resistant towards weathering stands out from the main bulk. This facies is especially prominent where the creek cuts the first belt of serpentine. A similar rock is found at the Potrero in San Francisco and has been described by C. Palache.[8]

The decomposition of the serpentine changes its structural features. The usually massive fresh rock often becomes granular. Numerous veinlets of black magnetite give it a banded appearance and a high specific gravity. Cavities form, which are colored white by magnesite or hydromagnesite. (See Plate XXXIV, Fig. 1.) At this stage the serpentine has more or less transformed into a silicious mass, which is tough, and not breaking exhibits the characteristic greasy luster of opal, while remnants of the serpentine are

[8] C. Palache, "The Lherzolite Serpentines and Associated Rocks of the Potrero, San Francisco," *Bull. Geol. Dept. Cal.*, I., 166, 1894.

still recognizable, bastite being the most resistant. The silicious substance assumes a red or yellow color upon weathering, due to the oxidation of iron compounds, and finally crumbles away. This feature of the rock is fully described by A. Knopf.[9]

The serpentine is associated with a coarse-grained somewhat metamorphosed sandstone, in which biotite is prominent and which in places assumes the character of a fine-grained conglomerate. To the south of the first belt where the Manzanita Mine is located shales are found. The sandstone, according to G. F. Becker, is of Knoxville age.[10] This he substantiates by the finding of impressions of *Aucella Piochii* in the metamorphosed rock close to the Manzanita Mine on Sulphur Creek. Although no direct contact was observed by the author, it being covered by soil and brush, the approximate contact is marked by a more intense degree of metamorphism of the sandstone. It is here exceedingly hard and responds to the blow of the hammer with a metallic ring. In the bed of Sulphur Creek was also seen serpentine which carried angular fragments of the sandstone.

The contact metamorphism is not accompanied by crystalline schists, but is such as would be caused by a strong thermal action.

II. The Knoxville and Clear Lake District Serpentines.

The Knoxville Serpentine.

The district as has been defined is the strip of country bounded in the north by Cache Creek and in the south by Putah Creek. Its principal body of serpentine which covers an area of approximately forty square miles is perhaps the largest found in the cntral coast ranges. A narrow strip a few hundred feet wide runs parallel to Perkins Creek, two miles south of it. Crossing Cache Creek it broadens out, capping the ridge that has its highest point a mile southwest of the Shamrock Mine. At this point the serpentine has a width of about one mile. Maintaining this width, it follows

[9] A. Knopf, "An Alteration of Coast Range Serpentine," *Bull. Dept. Geol. Univ. Cal.*, IV., 425, 1906.

[10] G. F. Becker, "Quicksilver Deposits of the Pacific Coast," Monograph XIII., U. S. G. S., 183, Washington, 1888.

Rocky Creek south to its headwaters, forming a series of low peaks to the east of the creek. Occasional outcrops from here have a northwest-southeast trend, establishing a connecting link with the main body of serpentine, which following the same general direction gradually expands to a width of four miles, passes to the south of Knoxville, and finally terminates about three miles southeast of Knoxville in a series of rugged outcrops.

The area as a whole presents a monotonous aspect. One sees a series of rocky ridges densely covered with brush and here and there a tree.

In the northwest, Rocky Creek cuts its way through the area and at places exposes steep walls of serpentine a hundred and more feet high, rising almost perpendicularly. The bed of the creek is strewn with big boulders which sometimes reach a diameter of ten feet. This is especially noticeable near the Shamrock Mine.

In the southwest Hunting Creek has cut a deep narrow canyon which gradually widens out towards the south, and into which enter a number of smaller canyons formed by tributaries. As at Rocky Creek the serpentine here is exposed presenting steep walls, and its débris covers the creek bed.

The headwaters of Davis Creek which drains to the north, and is a tributary to Cache Creek, have left their imprints upon the topography. Excellent opportunities for collecting fresh specimens are afforded by these drainage channels.

In many respects the serpentine resembles the one found at Sulphur Creek. It however presents a greater variety of colors. They vary with the degree of weathering the rock has undergone and the amount of iron it possesses. Upon fresh surfaces it is usually a dark green or almost black. The first indication of weathering is a yellow or a red color. Exposure to the atmosphere and the sun bleaches the serpentine and causes a decrease in hardness. A greenish-white slippery mass which resembles talc is the end product.

While here, as in the Sulphur Creek region, the shearing action is intense, and manifests itself in many slickensided fragments, the nodular variety of serpentine is not represented to any extent. This indicates a more homogeneous original mass.

Of the many hand specimens collected from this region one is of especial interest (Plate XXXIV, Fig. 2). It was taken from an outcrop on the western bank of Rocky Creek, and about one half mile south of the Shamrock Mine. It is a bastite pseudomorph after a pyroxenite and has beautifully retained its characteristic structure. The individual crystals are of a lighter green tint. The whole mass appears somewhat dull with now and then a glittering surface. The rock is massive, tough, due to the interlocking crystals and is easily scratched by a knife.

The microscope reveals that alteration has not been complete. Prismatic crystals of enstatite with prominent cleavage at right angles to the elongation of the fibers are still present. The slide shows bastite, some antigorite and a few minute veins of chrysotile.

Of interest are numerous transparent grains irregularly distributed with a tendency to concentrate along the bastite bands. They present a medium high relief and are isotropic. Sometimes these grains approach octagonal outline, but mostly they are of oval shape. Optically they behave like spinel, the index of refraction of which, being lower than that of methylene iodide, differentiates it from garnet. Plate XXXV, Fig. 3 gives an idea of the spinel.

The same mineral was found in a pseudomorph after a websterite from Mount Diablo, where it is of a secondary nature, since it was not observed in fresh rock. This similarity of condition makes it safe to assume that it is a secondary mineral here also.

The total quantity of this variety of serpentine is a small one. The outcrop is surrounded by the kind of serpentine common in this area, which is altered to a considerable extent.

The nature of the primary rock is indicated by a thin section made of fresh serpentine in which remnants of olivine, enstatite and diallage are found. Other minerals present are picotite, chromite and magnetite.

The predominating rock with which the serpentine is associated is a medium-grained grayish-yellow sandstone interbedded with shales. As one follows the road from Lower Lake to Knoxville and approaches the serpentine, one sees enormous thicknesses of it exposed. The dip is high, usually near 90 degrees. The angles are not constant. They vary considerably, sometimes within a few hundred feet. It is possible to travel a short distance and measure any angle. The rock is compact, specked with grains of biotite and well consolidated.

Outcrops of what is evidently the same sandstone are found a few hundred yards northwest of Morrell within the serpentine mass. They resemble serpentine to such an extent that seen even from a short distance they are taken for it. They have the rugged appearance of serpentine outcrops and the same lighter tint green which it often assumes upon weathering. A close inspection reveals the grain of the sandstone, decided bedding planes, and a hardness which by far exceeds that of the serpentine. It also shows the intense degree of metamorphism. At places a slight schistosity has been developed. A thin section made of this rock contains the usual acid ground mass of a sandstone, a few flakes of biotite and considerable epidote.

Crystalline schists and boulders of metamorphic rock are found included in the serpentine on the crest of a ridge north of Knoxville. The serpentine is crushed to fine scaly masses resembling a shale.

About one half mile south of the Shamrock Mine a tributary to Rocky Creek cuts the sandstone which butts against the serpentine on the east of Rocky Creek. Narrow seams of interbedded shale which carry *Aucella Piochii* in abundance are exposed. Towards the serpentine the sandstone is strongly metamorphosed.

An actual contact of a shale with the serpentine is exposed by the Johnson shaft of the Knoxville Mine. The shale however does not seem to be much altered and it has not ben ascertained whether or not it contains fossils.

The mineralogical character of the serpentine combined with the above evidence demonstrates that it is derived from an eruptive rock which is intrusive into the Knoxville formation.

There are two smaller areas of serpentine in the vicinity of the Knoxville area. One is found about one mile south of Jericho Creek. It approaches Hunting Creek in the west. It extends in a northwestern direction approximately parallel to Putah Creek for four or five miles. Its width varies from a few hundred feet to one half mile. Bordering it in the north is found a dike of olivine basalt, in the south the metamorphosed sandstone.

The other area is exposed by the road from Lower Lake to Knoxville about six miles from the former. It is represented only by a few outcrops with an east-and-west trend.

Both areas have very low outcrops and mineralogically correspond with the principal body of serpentine described in the preceding pages.

The Clear Lake Serpentines.

A small area of serpentine is found on the ridge north of Borax Lake, west of Clear Lake and south of the Sulphur Bank Mine. No specimens were obtained from this area. Becker[11] describes the serpentine, which seems to resemble closely the one met with at Knoxville and Sulphur Creek. Analyses are given of two varieties of serpentine, the first is of a black impure looking mass with phenocrysts, which are probably bastite, the second is of a purer variety. For comparison they are here appended.

Analyses of Serpentine from Near Borax Lake, Lake County.
Analyst not stated.

	I. Per Cent.	II. Per Cent.
SiO_2	39.64	41.86
Al_2O_3	1.30	.69
FeO	7.76	4.15
MgO	37.13	38.63
Cr_2O_3	.29	.24
NiO	.33	trace
MnO	.12	.20
H_2O	13.81	14.16
	100.38	99.93

It is seen that in chemical character the serpentine closely approaches that of Sulphur Creek. The total iron although given here as ferrous probably includes also iron in a ferric state.

Another serpentine area is located by Siegler Springs and Howard Springs about seven miles southwest of Lower Lake. It has a northwest-southeast trend. At Siegler Springs its width is ap-

[11] Monograph XIII., U. S. G. S., p. 111, 1888.

proximately one half mile. It widens out towards Howard Springs, where it has a width of about one mile. From here a series of outcrops point to the southeast and can be followed for about three miles.

The serpentine, being more resistant towards erosion than the associated sandstone, occupies an elevated position in places rising several hundred feet. It is fairly well preserved, of a dark green color, with coarse phenocrysts of bastite, and is exceedingly brittle. Thin sections show that the rock is almost wholly made up of serpentine. Of original constituents enstatite and chromite only are found, while the mesh structure due to olivine is very prominent. Its similarity to the Knoxville serpentine leaves no doubt as to its origin from a lherzolite.

The road from Kelseyville to Cloverdale past Elliott Springs cuts several areas of serpentine. The first one to the east of the road is one and a half miles east of Highland Springs, and its extent is one quarter square mile. The second and most important one is met 200 yards south of Fowler. The road cuts it and exposes a vertical wall of 50 feet of serpentine of a bluish green color. Slickensided fragments and an advanced state of decomposition are pronounced features. Fresher specimens show the porphyritic appearance caused by protruding foliated crystals of bastite. The area covers one half square mile.

Single outcrops are found a mile north of Elliott Springs, near Elliott Springs, and also three quarters of a mile south of it on the slope of the mountain ridge which divides Sonoma from Lake County. All of the serpentines are intrusive in sandstones. A feature of the contact is the occurrence of glaucophane and muscovite schists thus pointing towards rocks of Franciscan age. Microscopically the serpentines resemble those of Howard Spring.

Serpentine is also found west of Clear Lake and in close proximity to it. There are three small areas. The most northern one borders Clear Lake and is best reached by following the road from Lakeport to Upper Lake. It is five miles from the first named place. Another area is a few hundred yards south of Lakeport to the west of the road leading to Kelseyville. The third area is found to the east of the road from Lakeport to Glenalpine about one mile from the latter place.

The serpentine of these areas occupies low dome-shaped hills, over which numerous boulders of the rock ranging in diameter from one to three feet are strewn. From dark green to greenish blue they show the typical lherzolite serpentine in all its stages of decomposition.

III. The Mayacmas District Serpentines.

A narrow strip of serpentine extends in a northwest direction for about twenty-four miles, following more or less closely the Mayacmas range. It is very irregular in shape.

Near Ætna Springs are found a number of small areas with low outcrops which seem to form a connecting link with the Pope Valley serpentine and the Mayacmas area. Beginning at the Ætna Mine a narrow belt can be traced to the northwest by a number of quicksilver mines, the Twin Peak, Corona, Mirabel and Great Western Mines which are situated on its contact. The outcrops are very low along this belt. The contact, which is with a sandstone in the north, is partly with a basalt, partly with a sandstone in the south. Brush and soil cover it and only occasional outcrops indicate the general direction of the serpentine. The width of the belt varies. At places it is a hundred feet wide, but never more than one half a mile. Several narrow bands branch off to the south approaching Mount St. Helena.

At the Great Western Mine the belt assumes greater dimensions. It becomes more than a mile wide and forms the crest of the Mayacmas range, which here has an altitude of 2,900 feet and gradually rises as Pine Mountain is approached to 3,500 feet. At Pine Mountain the Mayacmas range divides into three branches, all extending in a northwest direction. The most southern one consists of a series of ridges and peaks of which Geyser peak is best known. The Big Sulphur or Pluton Creek cuts a steep canyon on its northern flank, forming a dividing line with that range of which Mount Cobb is the highest point looming to a height of 4,500 feet into the skies. The range furthest north is not continuous with the Mayacmas range. Putah Creek cuts a deep canyon which isolates it. It reaches an altitude of about 3,000 feet and Mount Harbin and Mount Hanna are its prominent peaks.

Half of a mile west of Pine Mountain the serpentine ceases to be a continuous area. Isolated outcrops are found in three directions which have the general trend of the branches of the Mayacmas range. They are along the southern flank of the southern range and a number of quicksilver mines indicate the general direction. From the road connecting these mines they are seen as rugged barren masses, prominent on account of the bluish green color and the dimensions of the outcrop. Float of actinolite schist is quite abundant, and glaucophane schist is seen in place.

Another series of outcrops follows the range north of the Big Sulphur or Pluton Creek south of Mount Cobb. They are found as far west as the headwaters of Squaw Creek.

Occasional areas are also found north of Cobb Mountain, having a general trend towards Pine Mountain. The largest of these is a few hundred yards west of Glenbrook Springs and covers perhaps one half of a square mile.

The serpentine as a whole along this belt differs from the previously described ones in that it is in a more advanced state of decomposition. Specimens even of moderate freshness could not be obtained. The nodular variety is prominent in the neighborhood of Ætna Springs but the nodules also, are well on the way towards decomposition.

Where the serpentine is not slickensided, a feature which is predominant, the structure is granular. Serpentine with this structure seems to be more resistant. The outcrops are higher and are more bold. Often they are protected by a layer of moss. If such an outcrop then has jointing in approximately parallel lines it resembles sandstone to such an extent that to differentiate it, it is necessary to break the rock.

All slides made from specimens along this belt show similar features. Antigorite more or less stained with oxide of iron still shows the structure due to its origin from olivine in some slides, while in others decomposition has erased all genetic indications and nothing but a homogeneous greenish mass remains. Enstatite is still observed in fresher specimens and is besides chromite the only constituent of the original rock remaining behind. Diallage is not present, but since it gives rise to antigorite and seems to be more susceptible to decomposition than enstatite, it was probably also a constituent of the original rock. Traces of picotite with a broad surrounding mass of chromite are seen (Plate XXXV, Fig. 4), and veins of magnetite are in abundance. With an advance of decomposition the carbonates dolomite, calcite and magnesite appear. They have no crystalline form, but are either granules enclosed in serpentine or vein filling.

The serpentine found near Glenbrook Springs differs somewhat from the above in structure and appearance. It is massive, well preserved and slickensided fragments are rare. It appears to be porphyritic, due to a yellowish brown ground mass which is very uniform and dotted by numerous green phenocrysts of bastite standing out prominently. Thin sections show that this coloration is caused by an abundance of oxide of iron.

Analysis of Serpentine from Mayacmas Range.

H. E. Kramm, Analyst.

	Per Cent.
SiO_2	39.98
Al_2O_3	1.12
Fe_2O_3	13.19
FeO	1.05
MgO	30.49
CaO	.46
Na_2O	.28
K_2O	.25
H_2O	13.26
Cr_2O_3	trace
TiO_2	trace
	100.08

Serpentine is also found on the road from Middletown to Lower Lake, about two miles south of Guenoc. The area has a northwest-southeast trend, an average width of a quarter mile and a length of about five miles, forming a low ridge which rises not more than a few hundred feet above the surrounding country. The serpentine is well preserved, and its similarity to the Knoxville serpentine makes further comment unnecessary. Herewith is given an analysis of a Mayacmas range serpentine made by the writer. It was taken near the Missouri Mine about six miles northwest of Pine Mountain and is fairly well preserved.

Serpentine is also found bordering Pope Valley. The extent of these areas however has not been determined. Specimens from these areas show a typical lherzolite serpentine.

IV. The San Francisco District Serpentines.

The San Francisco Peninsula Serpentines.

Serpentine is found in the vicinity of San Francisco and at San Francisco proper. According to Lawson[12] they are grouped into three linear tracts which traverse the peninsula of San Francisco in a northwest-southeast direction. The most northern one extends from Fort Point on the Golden Gate to Hunters Point on the Bay of San Francisco, a length of ten miles and with a maximum width of 1½ miles. It is probably a continuous belt although sand and alluvium cover portions of it and make it appear as four distinct areas. These areas have the character of laccolites or dikes which are intrusive into the Franciscan series of rocks.

The character of the serpentine has been in detail investigated by Palache.[13] It is represented in two facies which are named the slickensided and the massive facies. The slickensided is similar to the one at Sulphur Creek described by the writer. The massive facies is dark green in color, translucent on thin edges and has a splintery fracture. The parent rock is a lherzolite.

Analysis of Serpentine from Presidio, San Francisco.

J. D. Easter, Analyst.

	Per Cent.
SiO_2	39.60
Al_2O_3	1.94
FeO } MnO	8.45
MgO	36.90
Cr_2O_3	.20
H_2O	12.91
	100.00

FeO in this case probably also includes Fe_2O_3.

[12] A. C. Lawson, "Geology of the San Francisco Penninsula," 15th Annual Report U. S. G. S., 445, Washington, 1893.

[13] C. Palache, "The Lherzolite Serpentines and Associated Rocks of the Potrero, San Francisco," *Bull. Geol. Dept. Univ. Cal.*, I., 164–169, 1894.

An analysis of a Presidio serpentine made by Easter is given on account of the similarity with the one made by the writer on Sulphur Creek serpentine.

The second belt of serpentine occupies the southeastern portion of Buri-Buri ridge from San Andreas Lake to San Mateo Creek, and nearly the whole of Las Pulgas ridge, with a total length of 11½ miles and a maximum width of over one mile.

The third belt consists of a linear group of dike-like masses distributed along San Mateo Canyon between Sayer and Cahill ridges, on the pass between Cahill and Fifield ridges, and thence obliquely along Fifield ridge and across San Pedro Valley nearly to the ocean. The linear extent is six miles. There are also some few minor outcrops not referable to these belts.

All these serpentines exhibit similar features to those which Palache designated as lherzolite serpentine.

The Angel Island Serpentine.

Angel Island is situated in the bay of San Francisco 3½ miles north of the city of San Francisco. The geology of the island has been described by Ransome. Serpentine is found along the western portion of the island as a large dike having a maximum width of 500 feet. Intrusive into the San Francisco sandstone, it has metamorphosed it to some extent. The two facies, the slickensided and the massive are present. The slickensided differs from the one found at the Potrero in that the nodules are not as large and the matrix is less broken.

Analysis of Serpentine from Angel Island.

F. L. Ransome, Analyst.

	Per Cent.
SiO_2	42.06
Al_2O_3, Fe_2O_3	2.72
FeO	2.88
MgO	39.53
H_2O	12.04
	99.23

The massive facies is of unusual interest. It is made up of interlocking crystals of diallage with small amounts of magnetite and possibly some chromite. It would therefore correspond to a diallagite.

An analysis made by Ransome upon serpentine taken from a nodule of the slickensided facies is given above:

The Tiburon Peninsula Serpentine.

Although a narrow channel of the San Francisco Bay separates it the Angel Island serpentine is probably continuous with the one which is found north of it at the extreme tip of the Tiburon peninsula. It covers an area of perhaps one square mile. The road following the western shore line exposes the slickensided facies about 100 yards east of the ferry station.

The serpentine is much decomposed but the nodules, which often are several feet in diameter show fairly fresh serpentine which is very brittle. Vein serpentine is common.

The outcrops of this part of the area are small. The soil is of a reddish color and vegetation is scant. The crest of the ridge presents different features.

Outcrops reach a height of about 15 feet and consist of the siliceous mass, to which serpentine gives rise, and which is known in the coast ranges as the quicksilver rock. From a distance they resemble a conglomerate, close inspection however reveals the honeycombed tough silicious mass with magnesite stains here and there. Remnants of the serpentine can still be observed. The whole presents a rugged appearance. Magnesite float is abundant.

Slides of the fresh serpentine show the following minerals bronzite, diallage chromite and magnetite. The mesh structure due to olivine is prominent.

V. The Coyote Creek and Black Mountain Serpentine.

Serpentine is also found on what is known as the Los Lagrinas ridge a part of the Mount Hamilton range south of San Jose east of the Coyote Creek in Santa Clara County. The ridge consists of a series of kidney-shaped hills bordering the valley in the east, and reaching a maximum elevation of 300–400 feet above the valley level. About four miles south of Coyote station is the extreme southern end of the serpentine area, which covering the western slope of the ridge extends north to within one half mile of Edenvale.

What is probably an extension of this area, separated by alluvium is found on the opposite side of Santa Clara Valley. It follows a group of well-rounded crests which in a wide circular sweep reach to within four miles of San Jose. This area is known as the "Oakhill," and its crystalline rocks are described by Carey and Miller.[14]

The serpentine here is of a dike-like nature intrusive into radiolarian charts and sandstones of Franciscan age. It is variable in character. A massive phase shows glistening phenocrysts of diallage in a dark-green ground mass of serpentine which grades into a diallagite. The structural and mineralogical composition of the serpentine indicates that it is derived from a peridotite containing olivine, diallage and magnetite, with enstatite but sparingly present.

Associated with the massive green serpentine, which is predominant, is found the slickensided facies and the "mottled" serpentine. The latter consists of the green serpentine to which white colored spots varying in size to a diameter of two inches give a mottled appearance. The white substance may possibly be magnesium silicate. A partial analysis of massive conchoidal serpentine is here appended. Plate XXXV, Fig. 5 represents a phase of the mottled serpentine.

Of much interest is the associated olivine-gabbro which on one hand passes into peridotite and pyroxenite, and on the other into hypersthene gabbro and norite. An analysis of what may be considered a pyroxenite-peridotite consisting principally of diallage with small quantities of olivine partly transformed into serpentine, and some magnetite was made by the writer and is given below.

The serpentine of the main area which follows the Los Lagranas ridge does not differ from the one above described. It is intrusive into Franciscan cherts and sandstone. The area is almost destitute of soil and the barren slopes are strewn with boulders of the dark green rock. The outcrops are low and seldom protrude more than three feet above the surrounding soil. The massive facies is predominant. A feature is the abundance of magnesite which is found as float, and also in veins of considerable size.

Mineralogically the serpentine is a lherzolite serpentine. The mass as a whole appears to be uniform, but variations occur. A

[14] E. P. Carey and W. J. Miller, "The Crystalline Rocks of the Oak-Hill Area near San José, Cal.," *Jour. of Geol.*, XV., 160, 1907.

small lenticular body of pyroxenite is found about one quarter mile southeast of Coyote station. It is made up entirely of enstatite which is unaltered, and an analysis of which by the author is here given.

ANALYSES.

	Serpentine.	*Pyroxenite-peridotite.*	*Enstatite-pyroxenite.*
	I. Per Cent.	II. Per Cent.	III. Per Cent.
SiO_2	37.71	42.76	56.98
Al_2O_3	1.81	5.71	1.73
Fe_2O_3	10.47 (with FeO)	3.16	4.04
FeO		3.30	4.18
MgO	35.60	27.11	27.40
CaO	——	10.03	3.26
Na_2O	——	2.24	.59
K_2O	——	.49	.35
H_2O	——	4.85	2.04
Cr_2O_3	——	.22	.09
TiO_2	.09	.17	none
	85.68	100.04	100.66

I. Serpentine of the Oakhill area by U. S. G. S. Analyst not stated.
II. Pyroxenite-peridotite of Oakhill area. H. E. Kramm, analyst.
III. Enstatite-pyroxenite from near Coyote. H. E. Kramm, analyst.

South of the Oak-Hill area on the west side of Santa Clara valley, opposite the Coyote area, and having an approximately parallel trend with it, are several isolated areas of serpentine. The most northern one terminates on the road leading from San Jose to the New Almaden Quicksilver Mine. The most southern one is found about two miles northwest of Morganhill. A small area is also found near New Almaden.

The serpentine in general resembles that of the Coyote area, but shows a somewhat greater degree of decomposition.

About three miles southwest of Redwood in San Mateo County is a considerable area of serpentine. Its maximum width is somewhat over a mile, its linear extent four miles with a trend northwest-southeast.

Smaller areas are also found at the following places: one east of Searsville Lake about six miles west of Palo Alto, several south of

Black Mountain and one about four miles south of Saratoga. All of these are mapped in the Santa Cruz folio of the U. S. G. S.

Small pyroxenite bodies which consist of diallage are frequent in the Black Mountain areas. A feature of the Redwood area is tremolite secondary after serpentine, and talc after tremolite.

The similarity of the serpentine itself to that of the Coyote area makes further comment unnecessary.

VI. The Mount Diablo and Mount Hamilton Serpentines.

A lherzolite serpentine and a websterite are found at Mount Diablo in Contra Costa County. According to H. W. Turner[15] the area as a whole is dike-like, its length about five miles and its average width less than one half mile. The peridotite has largely been converted into serpentine, but specimens are still found which contain olivine, enstatite and diallage.

The pyroxenite is made up of bronzite and diallage, and is most prominent on the southwestern part of the area, occupying approximately one quarter of a square mile, but is also found at various other parts of the area.

The dike nature of the serpentine is best shown where the Arroyo del Cerro and its branches cut across the area east of the western fork of Pine Creek. The serpentine here varies in width from a few feet to 150 feet, and is enclosed in dark calcareous shales containing at several points near the serpentine "*Aucella Piochi*," with a strike and a dip about that of the shales which enclose it.

North of the serpentine area is found a diabase and the Knoxville sandstone, to the south it is bordered by metamorphosed sandstones.

A gabbro crops out north of the point where the serpentine dike crosses Bagley Creek. Between this gabbro and the serpentine lies like a body of *Aucella*-bearing shale, and up to present time no genetic connection between the two has been demonstrated.

The following analyses are given:

[15] H. W. Turner, "The Geology of Mount Diablo," *Bull. Geol. Soc. Am.*, II., 383, 1891.

Analyses of Serpentines, Mount Diablo.

	I.	II.	III.	IV.
SiO_2	53.25	36.57	40.50	41.52
P_2O_5	——	——	trace	——
Cr_2O_3	.54	.33	.41	——
Al_2O_3	2.80	.95	.78	1.57
Fe_2O_3	.69	7.29	4.01	3.50
FeO	5.93	.37	2.04	1.07
NiO	.07	.31	.11	——
MnO	.09	.10	.13	.29
CaO	16.22	.14	.39	.44
MgO	19.91	40.27	37.43	36.84
Na_2O	.19	.31	.28	——
K_2O	trace	trace	.16	——
H_2O at 105° C.	.05	.94	2.81	3.32
H_2O above 105° C.	.24	12.43	10.94	12.51
	99.98	100.01	99.99	101.06

I. Fresh pyroxenite with some olivine. W. H. Melville, analyst.
II. Bastite with fine seams of chrysotile. W. H. Melville, analyst.
III. Serpentine. W. H. Melville, analyst.
IV. Serpentine weathered interwoven with quartz and calcite. W. H. Melville, analyst.

Areas of considerable extent are found about fifteen miles southeast of Livermore to the west of the Arroyo Mocho in Contra Costa County, a few miles north of the Santa Clara and Contra Costa dividing line.

Other areas are to the west of the Arroyo del Valle to the south of Livermore in Contra Costa County, and in Santa Clara County. Still others are about six miles to the northwest of Mount Hamilton in Santa Clara County.

The location of these areas is best shown by the accompanying map.

These areas were not visited by the writer, but specimens of the serpentine are in the Stanford University collection.

Some of these are altered to the greenish-yellow slippery mass in which all genetic indications are obliterated; others exhibit the characteristics of a lherzolite serpentine. The presence of lenticular bodies of pyroxenites is indicated by specimens of diallagite and enstatite-pyroxenite which come from the serpentine areas northwest of Mount Hamilton.

Age of the Serpentines.

The similarity mineralogically and chemically of the serpentines makes it reasonable to suppose that the intrusion of its parent rock took place simultaneously throughout the coast ranges of California.

In the preceding pages, under areal description, it has been shown that in the Sulphur Creek and Knoxville districts, beds carrying *Aucella Piochii* are strongly metamorphosed by serpentine intrusion. The evidence points to a time of intrusion following the deposition of the Knoxville beds. This agrees with evidence obtained by H. W. Turner at Mount Diablo and with that of Fairbanks.[16]

Fairbanks reports Knoxville beds in which he found *Aucella Piochii* to be upturned and broken by serpentine masses. He puts on record other instances showing a shattering and metamorphosing of the Knoxville sandstone by the serpentine, and speaks of the finding of fragments of Knoxville shale in the serpentine and of serpentine in the Knoxville shale and sandstone. It is his opinion that the intrusion of the peridotite masses was at least partly responsible for the unconformity between the Chico and Knoxville beds.

The serpentines of the southern areas under discussion are usually associated with the Franciscan series of rocks, cherts and sandstones. Evidence which points to the time of intrusion of these dikes has to the writer's knowledge not been found up to the present time. This is to some extent caused by the lack of the Knoxville formation where the serpentine occurs.

Quicksilver in Connection with the Serpentine.

It is hardly possible to discuss the serpentines of the Coast Ranges without also bringing in quicksilver, and the relation which exists between its deposits and the serpentine.

The well known name "quicksilver rock" implies that decomposition product of the serpentine, which is a mixture of carbonates, compounds of iron and the three forms of silica quartz, chalcedony and opal. This rock occurs as dike-like masses in the body of the

[16] *American Geologist,* IX., 161–166, 1892.

serpentine itself, but is most common on contacts with the surrounding country rock, shale or sandstone.

The quicksilver is found filling existing pores, cracks and fissures of this rock. A few cases are known in which the ore is found in sandstone, which however is always in close proximity of serpentine bodies.

The principal ore is cinnabar. Mercury in a free state, metacinnabarite, and calomel are of less importance. The most common associates are marcasite, pyrite and chalcopyrite.

Under high temperature and pressure, in the presence of sulphohydrates and carbonates of the alkalies, mercury is in solution as the soluble salt $HgS + 4Na_2S$. Release of pressure, cooling especially in the presence of ammonia, dilution of the mineral-bearing solution, excess of hydrogen sulphide and the presence of bituminous substances are all factors which enter into the precipitation of the ore, bituminous matter leading towards a total reduction.

The intrusion of the peridotite masses in shattering the country rock provided channels through which the mineral-bearing waters circulated. These waters not only deposited the ore, but it is reasonable to suppose, that they were also to some extent instrumental in the decomposition of the serpentine. That conditions in this cherty decomposition product were exceptionally favorable for the deposition of the ore is substantiated by the fact that to the writer's knowledge cinnabar has never been found in the serpentine itself, nor is it found to any extent in the country rock. The few cases known in which the ore is found in sandstone show the sandstone to be of porous nature and as has been said located in the neighborhood of serpentine dikes.

As to the source of the quicksilver, Dr. Becker[17] suggests the base granite which underlies all sedimentary rocks in the Coast ranges.

Mineralogy of the Serpentines.

Primary Minerals.

The primary rock, as has been shown, with the exception of lenticular bodies of pyroxenites, is badly altered. Primary minerals

[17] G. F. Becker, "Quicksilver Deposits of the Pacific Coast," Monograph XIII., U. S. G. S., 449, Washington, 1888.

named in the foregoing are olivine, enstatite, bronzite, diallage, picotite and chromite.

Olivine is present only in the freshest variety of the rock and is even there not discernible in the hand specimen. Under the microscope it shows high relief, and is colorless. Cleavage cracks traverse it, which are often at rectangular position to each other and are usually filled with black opaque masses of magnetite. Crossed nicols show that in these cleavage cracks serpentinization has taken place and given rise to the mesh structure with small rounded fragments of the olivine occupying the central part of a mesh. A number of these rounded fragments usually extinguish together and thus indicate the size of the original crystal. Its lack of cleavage, its high relief and its bright interference colors distinguish it from the pyroxenes.

Enstatite is seen in the hand specimen as coarse prismatic crystals of a dark green color and with shiny cleavage surfaces which are parallel to the longer axis. Bastite is a pseudomorph after it, but can be recognized from it in that the crystals are easily scratched by a knife and are of a lighter green color. Under the microscope enstatite is seen in coarse platy crystals with a medium relief and a slight pleochroism from colorless to a light green. The cleavage is prominent. When basal sections are present, they sometimes present a dim prismatic cleavage.

Interference colors are of the first order, usually a bright yellow, the extinction is parallel and the slower ray is parallel to the elongation of the crystal. Plates parallel to the principal cleavage do not give an interference figure, which distinguishes it from bastite. Intergrowths with diallage are frequently observed.

Diallage is not quite as abundantly represented as enstatite. Under the microscope crystals of irregular outline are seen which resemble enstatite in cleavage lines and pleochroism. Crossed nicols however reveal bright interference colors, red and blue of the second order, and an oblique extinction. The maximum angle made with the principal cleavage was found to be 42 degrees. The slow ray is parallel to the elongation of the crystals. Besides the perfect (100) pinacoidal cleavage a well developed prismatic cleavage is seen in basal sections. Of enstatite and diallage the former appears to be more resistant towards serpentinization. Partially altered fragments of it were even found in nearly decomposed serpentine.

Bronzite does not differ much from enstatite. Due to its higher iron content it has higher interference colors, red of the first order to blue of the second order, and is as a rule pleochroic from colorless to a bright yellowish-green.

Of the minor constituents *picotite* is probably the most interesting. Its grains are of considerable size and irregular in outline. They were always found to be surrounded by black opaque masses resembling either chromite or magnetite which also fill numerous cracks traversing the crystal in all directions. (See Plate XXXV, Fig. 4.) It is of a coffee-brown color, has a high relief and is isotropic.

It was possible to isolate some of these crystals by digesting with hydrochloric acid and passing the residue through Thoulet solution, then separating the picotite by hand. The crystals were .5 mm. in diameter, glassy and hard enough to scratch quartz.

The black coating was stripped off and subjected to the action of a magnet. It was not magnetic and gave chromium reactions before the blowpipe. It is therefore chromite.

A series of slides made of specimens of serpentine varying in degree of decomposition revealed interesting facts. As the decomposition advances the outer opaque covering increases in size while the picotite decreases. In a fairly decomposed serpentine the picotite was still visible as a dot. In a much decomposed specimen, picotite disappears and only the irregular masses of chromite remain. It seems therefore that chromite is secondary after picotite.

Chromite itself was found as a primary constituent. It differs from that considered as secondary in that the masses assume a more geometrical outline. They are usually opaque, sometimes slightly translucent with a reddish brown tint.

Considerable quantities of chromite are known to occur in connection with the serpentine, but are not utilized commercially at the present time. Dr. Becker mentions chromic iron as occurring not far from the Royal claim near Knoxville. It is of nodular form in a seam which has been exposed by the weathering of the serpentine. The writer has not been able to locate this.

Another noteworthy place where chromite is found in quantities is at Cedar Mountain in Alameda County. An analysis of chromite from this locality corresponds closely with that of a mineral intermediate between picotite and chromite from a dunite from Dun Mountains in New Zealand, which for comparison is appended.

	I.	II.
SiO_2	none	——
Al_2O_3	18.79	12.13
FeO	16.99	18.01
CaO	trace	——
MgO	8.41	14.08
Cr_2O_3	55.74	56.54
NiO } CaO	trace	trace
MnO	trace	0.46
H_2O	.09	——
	99.82	101.22

I. Chromite from Cedar Mountain. H. E. Kramm, analyst.

The mineral was purified by powdering it and passing through Thoulet solution. All iron was determined in the ferric state and calculated to ferrous iron. There was no doubt ferric iron present, but the amount could not be determined on account of the difficulty of getting chromite into solution.

II. Chromite-Picotite from New Zealand. Mineralogy, 6th ed. Dana, p. 228. Analyst T. Petersen.

Secondary Minerals.

The course of hydration and subsequent decomposition of the serpentine necessitates a change of molecular arrangement, and gives rise to a number of secondary minerals. According to Tschermak the conversion of olivine gives rise to serpentine, magnesite, limonite and silica. It seems very probable that the excess of magnesia will combine with free silica to form secondary serpentine. This is substantiated by the fact that fissures in the rock are invariaby filled with chrysotile.

Decomposition and the action of ground waters assist in the development of another series of minerals which are probably oxide of iron, magnetite, hydromagnesite and vein serpentine. It is reasonable that this vein serpentine is not necessarily confined to the serpentine body itself, but may find its way into the surrounding country rock, where under favorable conditions it is deposited.

This would explain the occurrence of serpentine veins in sandstones, a feature observed in the Coast Ranges and construed by Dr. Becker[18] as showing the conversion of the sandstone into serpentine.

While nearly all of the secondary minerals can be found in every serpentine locality in minor quantities, local conditions favor the accumulation of some few. Of minerals which are of a secondary nature serpentine itself is the most important. It is represented by its two varieties, antigorite and chrysotile.

The antigorite shows under crossed nicols as an aggregate of irregularly distributed minute bands and scales which have low interference colors, usually gray with a bluish tint. Due to the irregularity of distribution, extinction is compensatory.

Bastite is a variety of antigorite and is microscopically prominent in coarse prismatic phenocrysts, pseudomorphous after enstatite or bronzite. It has a prominent pinacoidal (100) cleavage and is distinguished from the pyroxenes in that it is soft and is readily scratched by a knife.

Under the microscope these pseudomorphs are irregular in outline, and consist of coarse bands of serpentine in parallel arrangement. A cross fracture is frequently observed, often at right angles to the fibers, but it may have any angle. The whole is somewhat pleochroic from colorless to light green. Extinction is parallel to the lines of prominent cleavage and the slow ray is parallel to the elongation. When alteration has been complete, the characteristic low bluish-tint interference colors of antigorite are exhibited. Using thin uniform sections these colors are raised as the degree of alteration becomes less and approach those of enstatite or bronzite. Thin cleavage plates give an interference figure.

The chrysotile consists of an aggregate of parallel fibers filling the numerous seams which traverse the rock in all directions. It has parallel extinction, the parallel position of the fibers and the bright interference colors, usually red or blue of the second order, make it easily distinguishable. Microscopically it is prominent as silky veins which often reach considerable thickness, and are the well-known serpentine asbestos.

[18] Ibid., p. 277.

Of the different types of structure, the mesh and the bastitic structure are invariably found. The former points towards an origin from olivine and fresh specimens often show remnants of this mineral in the center of the mesh. It consists of bands of serpentine which intersect irregularly, quite often in rectangular position to each other, surrounding aggregates of filled serpentine.

The bastitic structure is described under bastite, and needs no further comment.

No characteristic grate structure was observed. It is true, now and then a suggestion of it is seen.

Magnetite was never observed in fresh specimens of the rock but it is pronounced when the rock is altered. It then occupies cracks and seams in opaque irregular masses. Chemical and magnetic properties distinguish it from chromite. Large masses in connection with the serpentine are to the writer's knowledge not known in the Coast Ranges of California.

Magnesite is abundantly found as float and in veins of various sizes. It is usually massive, fine-grained, of a beautiful white color and with a conchoidal fracture. Besides being formed in the process of hydration of the primary minerals which give rise to serpentine, it is probably also produced when the serpentine breaks down.

The following analyses given by F. L. Hess[19] show the chemical character of the magnesite.

Analyses of Magnesite.

	I.	II.	III.
SiO_2	2.15	.30	49.85
Al_2O_3	1.22	.16	3.45
Fe_2O_3	1.16	.38	.18
CaO	5.28	1.34	.48
MgO	41.01	45.86	21.53
CO_2	48.72	51.80	23.96
	99.54	99.74	99.45

I. Magnesite from Chiles Valley, Napa County. P. H. Bates, analyst.

II. Magnesite from W. W. Burnett's ranch, Coyote, Santa Clara County. A. J. Peters, analyst.

III. Magnesite from near Morgan Hill, Santa Clara County. A. J. Peters, analyst.

[19] F. L. Hess, "The Magnesite Deposits of California," Bull. 355, U. S. G. S., Washington, 1908.

Tremolite was found at two places in connection with the serpentines in the neighborhood of the Culverbear group of quicksilver mines in Sonoma County and at the Redwood area of serpentine in San Mateo County. Small lenticular bodies are imbedded in the serpentine. More often they are found as float. They reach a diameter of four to five inches, are usually coated with iron stain, and are exceedingly tough. The mass has a somewhat schistose structure and when broken shows needle-like white crystals with a silky luster.

The microscope shows an interwoven mass of slender crystals. Some few are of stocky habit but with no definite shape. A cleavage parallel to the elongation and a cross fracture is present but not pronounced. The extinction is parallel in some sections, in others it varies. The maximum angle measures about 22 degrees. The slower ray is parallel to the elongation of the crystal and sections with parallel extinction show the emergence of an optical axis. The trace of the optical plane lies parallel to the cleavage. Sections cutting at right angles the plane of schistosity show a characteristic amphibole cross section with a pronounced prismatic cleavage of about 124 degrees.

Inclusions of a dark green mass of antigorite are frequent. The alteration of it into tremolite is plainly visible. It begins first on edges. Bunches of needle-like crystals are tangent to the more or less oval-shaped body. Their higher interference colors contrast sharply with the low birefringence of the antigorite.

The serpentine is dotted with specks of a brighter color. Under a high-power objective they resolve themselves into radiating bundls of fibers of tremolite. The process of alteration is therefore not confined to the boundaries of the mass but is also an internal one.

Tremolite was also observed in a section from a pseudomorph after a websterite from Mount Diablo.

Talc is very rarely found in connection with the serpentines. At the Redwood area it was found in place secondary after tremolite.

Hydromagnesite.—This mineral is a product of decomposition of the serpentine. Local conditions seem to influence its formation, as it is more abundant in some localities than in others. In the Sulphur Creek areas it is abundant.

It occurs in white chalk-like masses to which green inclusions of serpentine give a mottled appearance and which readily crumble away under slightest pressure. The ratio of serpentine to hydromagnesite is approximately as one is to two. An analysis of the purest sample gave results as follows:

Analysis of Hydromagnesite.

H. E. Kramm, analyst.

SiO_2	9.37
Fe_2O_3, Al_2O_3	trace
CaO	2.46
MgO	39.25
CO_2	29.45
H_2O	18.74
	99.27

Crystals of hydromagnesite are found near Cedar Mountain in Alameda County.

Calcite occurs as veins in the serpentine and is a prominent constituent of the silicious mass to which serpentine gives rise. It is also found closely associated with the serpentine in what is known as ophicalcite. Specimens of ophicalcite which are a mixture of about one half calcite and one half serpentine were found at the Mirabel Quicksilver Mine in Lake County and at New Almaden in Santa Clara County.

Dolomite has an occurrence similar to that of calcite.

Aragonite is found in the neighborhood of Pine Mountain. At the Helen quicksilver mine in Sonoma County it occurs as needle-like crystals and fibrous crusts.

Epsomite. This mineral is found lining the shaft and drifts in the Knoxville Mine. Hair like crystals, snow white in color, somewhat brittle, with a silky luster often reach a length of a foot or more.

Melanterite is usually found as greenish-white hair-like crystals reaching a length of several inches, lining shafts and drifts in quicksilver mines. In the Knoxville mine it also occurs in stalactitic masses of a pale green color, which seem to melt in their own water of crystallization. On exposure to light it becomes dry, assumes a yellowish-green color and changes into copiapite.

Copiapite is less abundant than melanterite, the oxidation of which gives rise to it.

Redingtonite was first found in the Redington Mine at Knoxville and described by Dr. Becker.

Knoxvillite, according to Dana, occurs with redingtonite at Knoxville.

Limonite and hematite are products of decomposition of the serpentines and impart the red color to the soil derived from it. Commercially they are not important on account of impurity.

Conclusions.

In the preceding pages the following facts are demonstrated:

(A) The serpentines are derived from basic eruptive rocks.

For this speaks the irregularity of the serpentine bodies, which is a typical character of eruptive rocks. A glance upon the map also shows that, with the exception of the Mount Diablo serpentine, the areas have an approximately northwest-southeast trend, which corresponds to lines of structural weakness in the Coast Ranges.

Furthermore, the serpentine contains olivine and chromite. The first, with the exception of altered magnesian limestones, is found only in eruptive rocks.

The second has, to the writer's knowledge, never been found in serpentine derived from sedimentaries.

The chemical composition of the serpentine shows it to be related to peridotites.

Pseudomorphs after pyroxenites are not very well possible in serpentine derived from sedimentaries.

(B) This eruptive rock was fairly uniform and its time of intrusion falls in a period which followed the deposition of the Knoxville beds.

The uniformity is demonstrated by the analyses of the rock which show it to be of a basic nature.

The mineralogical investigation shows it to be a lherzolite, and the serpentine derived from it a lherzolite serpentine.

However, variations in this rock occur which are represented by lenticular bodies of pyroxenites which are usually of small dimensions, but sometimes reach considerable size as in the case of the Mount Diablo pyroxenite.

The age of the serpentine is established by field evidence which agrees with that of Turner and Fairbanks and makes the serpentine post-Knoxville.

(C) Other facts which are of petrological interest are:

The lherzolite is in an advanced state of decomposition.

The freshest specimen found contained only about twenty per cent. of the original constituents of the rock.

Olivine readily undergoes decomposition. Enstatite and diallage are more resistant, and the latter seems to be more susceptible towards serpentinization than the former.

Picotite is quite abundant in fresh specimens of the northern areas and appears to give rise to chromite.

Secondary minerals besides the usual products of decomposition are spinel, tremolite and talc.

In closing the writer wishes to express his obligation to Professor A. F. Rogers, of Stanford University, under whose guidance and advice this paper was prepared.

TABLE OF ANALYSES.

Serpentines.

	I.	II.	III.	IV.
SiO_2	36.57	37.62	37.71	39.60
Al_2O_3	.95	1.20	1.81	1.94
Fe_2O_3	7.29	8.60 }	10.47	——
FeO	.37	2.15 }		
MgO	40.27	37.59	35.60	36.90
CaO	.14	2.49	——	——
Na_2O	.31	.27	——	——
K_2O	trace	trace	——	——
H_2O	13.37	10.46	——	12.01
MnO	.10	——	——	——
Cr_2O_3	.33	.36	——	.20
TiO_2	——	trace	.09	——
NiO	.31	——	——	——
	100.01	100.74	85.68	100.00

I. Bastite with fine seams of chrysotile from Mount Diablo. W. H. Melville, analyst.

II. Serpentine from Sulphur Creek, Colusa County. H. E. Kramm, analyst.

III. Serpentine from Oak Hill. Partial analysis by U. S. G. S.
IV. Serpentine from Presidio, San Francisco. J. D. Easter, analyst.

	V.	VI.	VII.	VIII.
SiO_2	39.64	39.98	40.50	41.52
Al_2O_3	1.30	1.12	.78	1.57
F_2O_3	——	13.19	4.01	3.50
FeO	7.76	1.05	2.04	1.07
MgO	37.13	30.49	37.43	36.84
CaO	——	.46	.39	.44
Na_2O	——	.28	.28	——
K_2O	——	.25	.16	——
H_2O	13.81	13.26	13.75	15.83
MnO	.12	——	.13	.29
Cr_2O_3	.29	trace	.41	——
TiO_2	——	taree	——	——
NiO	.33	——	.11	——
P_2O_5	——	——	trace	——
Total	100.38	100.08	99.99	101.06

V. Black impure serpentine from near Borax Lake, Lake Co. Monograph XIII., U. S. G. S., 1888, p. 111. Analyst not stated.

VI. Serpentine from Mayacmas Range. H. E. Kramm, analyst.

VII. Serpentine from Mount Diablo. W. H. Melville, analyst.

VIII. Serpentine weathered interwoven with quartz and calcite from Mount Diablo. W. H. Melville, analyst.

Table of Analyses.

Serpentines.

	IX	X.
SiO_2	41.86	42.06
Al_2O	.69	2.72 (with Fe_2O_3)
Fe_2O_3	——	
FeO	4.15	2.88
MgO	38.63	39.53
H_2O	14.16	12.04
MnO	.20	——
Cr_2O_3	.24	——
NiO	trace	——
	99.93	99.23

IX. Pure serpentine from near Borax Lake, Lake County. Monograph XIII., U. S. G. S., 1888. Analyst not stated.

X. Serpentine from Angel Island. F. L. Ransome, analyst.

TABLE OF ANALYSES.

Pyroxenites.

	XI.	XII.	XIII
SiO_2	42.76	53.25	56.98
Al_2O_3	5.71	2.80	1.73
Fe_2O_3	3.16	.69	4.04
FeO	3.30	5.93	4.18
MgO	27.11	19.91	27.40
CaO	10.03	16.22	3.26
Na_2O	2.24	.19	.59
K_2O	.49	trace	.35
H_2O	4.85	.29	2.04
MnO	——	.09	——
Cr_2O_3	.22	.54	.09
TiO_2	.17	——	none
NiO	——	.07	——
Total	100.04	99.98	100.66

XI. Pyroxenite-peridotite from Oak Hill near San Jose. H. E. Kramm, analyst.

XII. Fresh pyroxenite with some olivine from Mount Diablo. W. H. Melville, analyst.

XIII. Enstatite pyroxenite from near Coyote, Santa Clara Co. H. E. Kramm, analyst.

MAGELLANIC PREMIUM

FOUNDED IN 1786 BY JOHN HYACINTH DE MAGELLAN, OF LONDON

1910

THE AMERICAN PHILOSOPHICAL SOCIETY

HELD AT PHILADELPHIA, FOR PROMOTING USEFUL KNOWLEDGE

ANNOUNCES THAT IN

DECEMBER, 1910

IT WILL AWARD ITS

MAGELLANIC GOLD MEDAL

TO THE AUTHOR OF THE BEST DISCOVERY, OR MOST USEFUL INVENTION, RELATING TO NAVIGATION, ASTRONOMY, OR NATURAL PHILOSOPHY (MERE NATURAL HISTORY ONLY EXCEPTED) UNDER THE FOLLOWING CONDITIONS:

1. The candidate shall, on or before November 1, 1910, deliver free of postage or other charges, his discovery, invention or improvement, addressed to the President of the American Philosophical Society, No. 104 South Fifth Street, Philadelphia, U. S. A., and shall distinguish his performance by some motto, device, or other signature. With his discovery, invention, or improvement, he shall also send a sealed letter containing the same motto, device, or other signature, and subscribed with the real name and place of residence of the author.

2. Persons of any nation, sect or denomination whatever, shall be admitted as candidates for this premium.

3. No discovery, invention or improvement shall be entitled to this premium which hath been already published, or for which the author hath been publicly rewarded elsewhere.

4. The candidate shall communicate his discovery, invention or improvement, either in the English, French, German, or Latin language.

5. A full account of the crowned subject shall be published by the Society, as soon as may be after the adjudication, either in a separate publication, or in the next succeeding volume of their Transactions, or in both.

6. The premium shall consist of an oval plate of solid standard gold of the value of ten guineas, suitably inscribed, with the seal of the Society annexed to the medal by a ribbon.

All correspondence in relation hereto should be addressed

TO THE SECRETARIES OF THE

AMERICAN PHILOSOPHICAL SOCIETY

No. 104 SOUTH FIFTH STREET

PHILADELPHIA, U. S. A.

The American Philosophical Society

Announces that an Award of the

HENRY M. PHILLIPS PRIZE

will be made during the year 1912; essays for the same to be in the possession of the Society before the first day of January, 1912.

The subject upon which essays are to be furnished by competitors is:

The Treaty-making power of the United States and the methods of its enforcement as affecting the Police Powers of the States.

The essay shall contain not more than one hundred thousand words, excluding notes. Such notes, if any, should be kept separate as an Appendix.

The Prize for the crowned essay will be $2,000 lawful gold coin of the United States, to be paid as soon as may be after the award.

Competitors for the prize shall affix to their essays some motto or name (not the proper name of the author, however), and when the essay is forwarded to the Society it shall be accompanied by a sealed envelope, containing within, the proper name of the author and, on the outside thereof, the motto or name adopted for the essay.

Essays may be written in English, French, German, Dutch, Italian, Spanish or Latin; but, if in any language except English, must be accompanied by an English translation of the same.

No treatise or essay shall be entitled to compete for the prize tht has been already published or printed, or for which the author has received already any prize, profit, or honor, of any nature whatsoever.

All essays must be typewritten on one side of the paper only.

The literary property of such essays shall be in their authors, subject to the right of the Society to publish the crowned essay in its Transactions or Proceedings.

The essays must be sent, addressed to

The President of the American Philosophical Society,

No. 104 South Fifth Street, Philadelphia, Penna., U. S. A.

4248.

PROCEEDINGS

OF THE

American Philosophical Society

HELD AT PHILADELPHIA

FOR PROMOTING USEFUL KNOWLEDGE

VOL. XLIX. OCTOBER–DECEMBER, 1910. No. 197.

CONTENTS.

PHILADELPHIA

THE AMERICAN PHILOSOPHICAL SOCIETY

104 SOUTH FIFTH STREET

1910

American Philosophical Society

General Meeting—April 20-22, 1911

The General Meeting of 1911 will be held on April 20th to 22nd, beginning at 2 p. m. on Thursday, April 20th.

Members desiring to present papers, either for themselves or others, are requested to send to the Secretaries, at as early a date as practicable, and not later than March 18, 1911, the titles of these papers, so that they may be announced on the programme which will be issued immediately thereafter, and which will give in detail the arrangements for the meeting.

Papers in any department of science come within the scope of the Society, which, as its name indicates, embraces the whole field of useful knowledge.

The Publication Committee, under the rules of the Society, will arrange for the immediate publication of the papers presented.

I. MINIS HAYS
ARTHUR W. GOODSPEED
JAMES W. HOLLAND
AMOS P. BROWN

Secretaries.

Members who have not as yet sent their photographs to the Society will confer a favor by so doing; cabinet size preferred.

It is requested that all correspondence by addressed

To the Secretaries of the
AMERICAN PHILOSOPHICAL SOCIETY
104 South Fifth Street
Philadelphia, U. S. A.

PROCEEDINGS

OF THE

AMERICAN PHILOSOPHICAL SOCIETY

HELD AT PHILADELPHIA

FOR PROMOTING USEFUL KNOWLEDGE

VOL. XLIX | OCTOBER–DECEMBER, 1910 | NO. 197

FURTHER CONSIDERATIONS ON THE ORIGIN OF THE ZONE OF ASTEROIDS AND ON THE CAPTURE OF SATELLITES.

BY T. J. J. SEE.

(Read November 4, 1910.)

In Volume II. of my "Researches on the Evolution of the Stellar Systems," 1910, which has just been published, I have treated at some length of the most important problems connected with the origin of the solar system, and have shown that as regards mode of origin the asteroids are connected with the periodic comets and have been gathered within Jupiter's orbit by the action of that great planet. This conclusion had been anticipated to some extent by the late Professor Stephen Alexander, of Princeton University, as far back as 1851, and more recently by the late Professor H. A. Newton, of Yale, and by the late M. Callandreau, of the Paris Observatory. The inferences of Newton and Callandreau resulted from their mathematical investigations of the perturbations of Jupiter upon small bodies crossing his orbit. Fortunately the weight of these eminent authorities is such that we need not dwell on the mathematical methods of reasoning employed. Our present aim is rather to examine briefly the consequences which follow from this theory, as developed in the second volume of my "Researches," and to make somewhat clearer the significance of certain observed phenomena of the solar system, by an argument so brief and so much to the point that even a layman may grasp it without difficulty.

1. It was pointed out by Oppolzer in 1880[1] that the resistance to two homogeneous spheres revolving in a discontinuous medium of cosmical dust is inversely as their radii, and therefore relatively very large for a small body and very small for a large one.[2] The secular effect of such a cause, therefore, is to make the small body approach the sun very rapidly, while it scarcely modifies the mean distance of the large body, the latter change being so small that it often may be neglected entirely.

2. Now if we contemplate the arrangement of the orbits of the asteroids in the solar system, we find them grouped almost entirely within Jupiter's orbit, in accordance with the mathematical investigations of Newton and Callandreau, and moreover spread over the entire zone from Jupiter to Mars, and even extending beyond these limits. Thus Eros has a mean distance slightly less than that of Mars, while the Achilles group of asteroids projects beyond the orbit of Jupiter. How much wider the zone may hereafter be found to be, it is difficult to predict.

3. Since the asteroids have been thrown just within Jupiter's orbit by the successive actions of that great planet, and subsequently had their mean distances so decreased with the lapse of ages as to carry them down to the orbit of Mars, it follows that this spreading of the asteroids over such a wide zone affords a clear and unmistakable illustration of the effects of resistance and collisions—these small bodies having approached the sun much more rapidly than the giant planet Jupiter which gathered them in. No other interpretation can be given to the great width of the asteroid zone. For the perturbative action of Jupiter could throw the asteroids but slightly within his own orbit, and the great decrease in the mean distances of many of them must be accounted for in some other way. We conclude, therefore, that the more rapid dropping of the asteroids towards the sun illustrates the secular effects of resistance in the form of cosmical matter, such as meteoric swarms and comets,

[1] Cf. *A. N.*, 2314 and 2319.

[2] Cf. my "Researches," Vol. II., p. 293.

which must occasionally be encountered by the asteroids as well as by Jupiter. In the long run the secular effects of collisions with comets and similar *isolated bodies* is exactly the same as the effects of a medium of cosmical dust of nearly *continuous character;* so that we need not dwell on the character of the medium.

4. It is important to notice that as the small masses approach the center most rapidly, they would tend in time to overtake the larger planets nearer the sun. Thus the moon may have had originally a greater mean distance than the earth, but by degrees it was brought so near our planet that it passed under the earth's control and became a satellite. A similar conclusion holds for all the other satellites of the solar system. Besides crossing over the orbits of the larger planets, owing to larger eccentricity, these smaller bodies were originally at greater distances than their several planets, and in approaching them by degrees were at length brought within the range of the planetary attraction and captured, as explained in Volume II. of my "Researches on the Evolution of Stellar Systems."

5. In the PROCEEDINGS of this society, 1910, p. 213, we have explained the process of capture by a direct and simple method of reasoning, and in view of the considerations just adduced, one cannot doubt that this represents essentially the process of nature. The lesson taught by the great width of the zone of the asteroids is so very significant that it may serve as a practical demonstration of certain tendencies in the physical universe. The same conclusion may be otherwise verified, from a new and independent point of view, as follows.

It is shown by the exact data calculated from Babinet's criterion that the planets never could have been detached or thrown off from the central mass of our system, but were formed at great distances and have gradually neared the sun, as its mass increased and they revolved in the nebular resisting medium and gathered up more and more cosmical dust.

6. Since, therefore, the solar nebula as a whole did not rotate fast enough to detach the planets, or even exert a sensible centrifugal force, but they were originally independent nuclei formed in

the remoter parts of the nebula, at a great distance from the center, it will be doubly obvious that these insignificant secondary nuclei in the outer parts of the nebula could not have rotated rapidly enough to detach their satellites. This is emphasized also by the retrograde motions of the outer satellites of Jupiter and Saturn, which are entirely inconsistent with any theory of detachment. Such a view would be nothing less than absurd. The relatively greater energy of axial rotation of some of the planets is to be explained by the capture of nebulous matter circulating as a vortex in the powerful field of the sun's attraction, owing to its immense mass, which gives the particles impinging against the planet great relative moment of momentum.

7. The column of the accompanying table designated "centrifugal force" gives the fractional part of the centrifugal force due to rotation, when the central bodies are expanded to fill the orbits of their attendant bodies as imagined by Laplace. In this table the unit in each case is the amount of centrifugal force required to set the body revolving in its present orbit. The excessively small value of the centrifugal force due to the rotation of the expanded central mass is very remarkable; and emphasizes strongly the untenability of the view that the attendant bodies were thrown off. Nothing more misleading than this traditional view ever became current in the literature of science; and yet it still circulates in all old works on astronomy, and probably it will take many years to get it eliminated from the current thought of our times.

8. There is another impressive way of illustrating the untenability of the now abandoned detachment theory, as follows: It is shown in works on celestial mechanics that

$$v^2 = k^2(1 + m)(2/r - 1/a), \qquad (1)$$

where k^2 is the constant of attraction, m the mass of the attendant body, that of the central mass being unity, r is the radius vector, a the semi-axis major, and v the velocity. This gives for an attendant body of insensible mass

$$a = \frac{1}{\frac{2}{r} - \frac{v^2}{k^2}}. \qquad (2)$$

As the orbits of the planets and satellites are essentially circular, we may put $r = a = 1$; and then since

$$a = \frac{1}{\frac{2}{r} - \frac{v^2}{k^2}} = 1,$$

it follows that in these units $v^2/k^2 = 1$. If instead of the velocity appropriate to carry the attendant body around in its nearly circular orbit, we substitute in (2) the fractional values of v^2/k^2 given in the above table, as due to the rotation of the central mass postulated by Laplace, we shall find that in all cases the semi-axis major of the new orbit is about one-half that of the existing orbit. The largest value results from the inner ring of Saturn, where the equation would give $a = 1/1.86 = 0.54$, the value of v^2/k^2 in this case being about 0.14.

Table of Data Relating to the Solar System.

Planet.	Cenrifugal Force, calculated from data of Babinet's criterion, present orbital centrifugal force being unity.	Density of Central Body, when expanded to fill orbit, that of atmospheric air at sea level being unity.
Mercury	0.000000253	0.001776
Venus.	0.0000001 34	0.0002723
The Earth...........	0.000000098	0.0001029
Mars....	0.0000000064	0.00002913
Ceres..................	0.0000000035	....
Jupiter................	0.000000019	0.000000732
Saturn..................	0.000000010	0.000000118
Uranus................	0.0000000051	0.0000000146
Neptune..	0.0000000032	0.0000000038

9. The obvious meaning of this result is that if the planets and satellites were projected along the tangents of their present orbits, with such small squared velocities as shown in the table, they not only would not pursue their present paths, but would in each case fall towards the centers and describe orbits with semi-axes just about one half. In other words, the projected satellites would pursue very elongated ellipses having their aphelia at the existing orbits, but their perihelia penetrating the central masses about which they now revolve. *In no case would over half a revolution be possible without collision, because the projected satellites would fall almost straight to the center and be absorbed in the central mass.*

If we compute the densities of the sun when expanded to fill the orbits of the planets, and of the several planets when expanded to fill the orbits of their respective satellites, we shall get the results given in the last column of the table. In considering this table it is sufficient to recall that an atmospheric pressure of 0.1 of a millimeter or a density of 1/7,600 that of air is a very high vacuum;

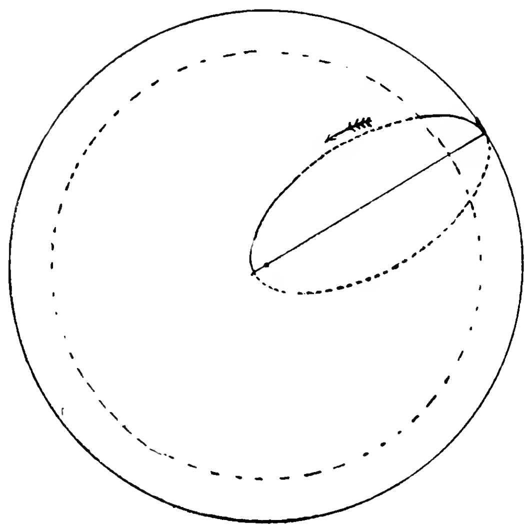


FIG. 1. Illustration of the motion of a particle projected along the inner edge of Saturn's dusky ring with velocity corresponding to planet's axial rotation when expanded to fill the ring. Pointed circle shows present dimensions of planet.

and since no greater hydrostatic pressure than this could be exerted from the center outward in case of most of the planets and many of the satellites as they fall unsustained by centrifugal force towards their dominant central masses, we see that these attendant bodies would in all cases fall practically without obstruction, and collision would in every case occur at the end of half a revolution. Could a more complete overthrow of the traditional detachment hypothesis of Laplace be imagined?

10. This affords an impressive illustration of the fallacy of the Laplacean theory of the origin of the planets and satellites; and

the only way we can explain the failure to detect this contradiction long ago is by the fact that Babinet's criterion seems to have been totally overlooked till taken up by the writer in 1908.

TABLE OF DATA RELATING TO THE SATELLITE SYSTEMS.

Planet.	Satellite.	Centrifugal Force, calculated from data of Babinet's criterion present orbital centrifugal force being unity.	Density of Central Body, when expanded to fill orbit, that of atmospheric air at sea level being unity.
The Earth	The Moon	0.00005657	0.01965
Mars	Phobos	0.001612	1151.
	Deimos	0.000644	73.05
Jupiter	V	0.034408	58.93
	I	0.014694	4.66
	II	0.009277	1.15
	III	0.005820	0.285
	IV	0.003323	0.0523
	VI	0.0005416	0.000232
	VII	0.0005217	0.000208
	VIII	0.0002254	0.0000169
Saturn {	Inner Ring	0.1435 }	
	Mimas	0.048254 }	16.45
	Enceladus	0.037651	7.61
	Tethys	0.030436	4.11
	Dione	0.023772	1.96
	Rhea	0.017017	0.717
	Titan	0.0073449	0.0576
	Hyperion	0.0060716	0.0324
	Iapetus	0.0025205	0.00232
	Phoebe	0.0006962	0.000049
Uranus	Ariel	0.0055888	2.40
	Umbriel	0.0040111	0.88
	Titania	0.0024454	0.200
	Oberon	0.0018287	0.082
Neptune	Satellite	0.0017177	0.43

11. Since the attendant bodies could not be thrown off by rotation, and could not form where they now revolve out of mere scattered dust, owing to the feebleness of the mutual gravitation of such particles, under the powerful dispersive tidal action of the adjacent central masses, it follows incontestably that the planets and satellites have all been captured and added on from without. And we have explained the method of capture by simple and general considerations which make it certain that this process represents the true law of nature.

12. Any one who calculates the moment of momentum of orbital motion of a satellite such as the moon, which is 4.8 times larger than the present total moment of momentum of the earth's axial rotation, will perceive how powerfully the rotation of a planet may be accelerated by the impact of satellites against its surface. To be sure, the case of the moon is by far the most extreme in the solar system, owing to its large mass, but the tendency is the same everywhere, and really rapid rotations can be acquired only by the gathering in of quantities of satellites in the sun's field of force by large planets, as in the observed cases of Jupiter and Saturn.

The planets were formed at great distances from the sun, where the field of attraction would be much feebler than in their present situations; and hence nebulous matter collecting to such secondary centers in the outer parts of our nebula would give but feeble rotations of these masses about their axes; so that at no time in the past history of the solar system could a planet have rotated with sufficient rapidity to develop an appreciable tendency to detach a satellite. Such an hypothesis is wholly untenable, because it is found by calculation that the rapid rotations develop only when the planets are comparatively near the sun, and the relative moment of momentum of impinging particles therefore large.

Accordingly the simple considerations here adduced confirm from another independent point of view the results already obtained in second volume of my "Researches"; namely, that the moon and other satellites are merely captured planets, originally describing independent orbits about the sun; and show that the Capture Theory unquestionably is an ultimate law of nature.

U. S. Naval Observatory,
Mare Island, California,
October 17, 1910.

THE TRUE ATOMIC WEIGHTS OF OXYGEN AND SILVER.

By DR. GUSTAVUS HINRICHS.

(*Read December 2, 1910.*)

Chemists have been trying to determine the atomic weights of the chemical elements for now fully a century. Dalton introduced the idea in 1804, while Berzelius made the first reliable determinations as far back as 1814 and for a third of a century kept ahead of all others in this work.

While the laboratory work required for atomic weight determinations has been greatly improved since the time of Berzelius, the methods of reduction of the same by mathematical calculation have remained almost the same as those used by Berzelius a century ago. The work I have done in this line has hardly become known in this country, where the work done in one of the scientific departments at Washington, published by the Smithsonian Institution and disseminated under the official frank, continues to be upheld as the standard through the Committee on Atomic Weights of the American Chemical Society. But at the opening of the volume for the present year it is declared that "there is confusion and uncertainty throughout the table of atomic weights."[1]

It therefore seems desirable that this question be considered independent of the dominant chemical school by those who, like electricians and physicists, come in contact with the broad question of the constitution of matter which involves that of the atomic weights of the chemical elements.

In the old Berzelian calculations the atomic weight of oxygen is assumed as a fixed constant (100 by Berzelius, 16 at present) and all other atomic weights are referred to this standard. This system has led to the now "official" value 107.88 for silver, a value which I have repeatedly shown to be in conflict with the most renowned

[1] *Journal Am. Chem. Soc.*, 1910, p. 4.

recent determinations to the extent of over one tenth of one per cent.

This is not the place for renewing the discussion in detail; the publications in question are accessible to all. We would rather take up the question in the broadest way and try to decide it by a sort of crucial test. *We will show that the evidence on which the value 16 for oxygen rests is exactly the same as that which gives the value 108 for silver.*

It is a fact that all chemical reactions are approximately exact only and that all laboratory work is subject to the same limitation. It is therefore incorrect to assume that in the reactions and in the laboratory work there is involved no error whatever on the part of the oxygen and that all errors are due to the other elements associated with oxygen. It seems sufficient to state this to have it admitted.

But if oxygen be supposed free from all material imperfections, as is assumed in the common calculations, its actual shortcomings are not blotted out thereby—they are simply placed to the account of the other elements which are unfortunate enough to be in reaction with the supposedly perfect element, assumed to be immune from error by the school. Hence the errors of the associated elements will be correspondingly magnified. In this way, the dominant school now has arrived at the value 107.88 for silver.

Let us try to see how this has been brought about and how the question may be put in the above crucial form for decision.

It is agreed by all and not even denied by the American School that the atomic weight, for O = 16, is very near the round numbers: Ag 108, Cl 35.5, C 12, N 14, etc. These values we have called the *absolute atomic weights.*

Accordingly, the *true atomic weights* can differ from these absolute values by small fractions only; this we call the *departure* and designate by the letter epsilon (ϵ).

Hence the real mathematical problem is the *determination of the departure for each element in every reaction used.* We determine the departure in thousandths of the unit. Thus, if the American Chemical School be right in declaring that the atomic weight of silver is 107.88, the departure for silver would have the enormous value

— 120, while for the element oxygen in all reactions and in all determinations made by all chemists at all times and under all conditions the departure was zero always.

But is this introduction of the departure for the entire atomic weight not merely a formal matter? Not to the man versed in higher mathematics; for he knows that even the most complex functions permit a simple solution by proportional parts for all cases in which the increment of the variable is sufficiently small. Thus this solution would even hold true, with fair approximation, for the departure of 120 thousandths of a unit, above indicated. We may therefore express the difference between the Berzelian reduction maintained to the present day by the dominant school of chemistry and our own as corresponding to the difference between common algebra and higher mathematics.

While we have worked with the departures instead of the entire atomic weight for over twenty years, we had not been able to determine the relation between the different departures of the different elements in a given chemical reaction until we discovered the equation of condition in 1907. Since then we have determined the departures for each element in all the three hundred chemical reactions that have been used for the determination of atomic weights during the entire century. The results have been put into five tables, each giving sixty reactions. The first two of these tables have been published, the other three have been ready for publication since the close of 1909.

Now we may return to the point at issue, the value of the atomic weight of silver: is it 107.88 or is it 108, on the scale of 16 for oxygen?

Our tables show that oxygen occurs in 158 of the reactions used for the determination of atomic weights, while silver occurs 115 times. Consequently the atomic weight of oxygen has been determined 158 times and that of silver 115 times. Of course, the dominant school will declare that they have done no such thing; but that will not prevent us from using the data they have published although we will accept their declaration as made in good faith.

The following table gives the results obtained, as stated repeatedly: departures (expressed in thousandths of the unit) from the absolute values 16 and 108.

TABLE OF DEPARTURES ε (IN THOUSANDTHS).

Departures.	0–10	11–20	21–100	0–20	21–40	41–100	0–100	101–
OXYGEN.								
No. of cases	89	30	33	119	17	16	152	6
Mean ε	1	5	— 1	2	14	—17	1	
SILVER.								
No. of cases	39	22	36	61	22	14	97	18
Mean ε	—1	—5	—23	—2	—5	—54	—10	
No. per cent.								
Oxygen	58	20	22	78	11	11	100	4
Silver	40	23	37	63	23	14	100	18
Mean	49	21	30	70	17	13	100	--
All 656								
Determ. for all elements	49	21	30	70	18	12	100	7

An extended study of this table would be most instructive, but a brief summary must answer here.

It is noted that we have given two distinct sets of returns: by intervals of 10 and of 20 thousandths of the departure. In each set we have given only the first two intervals, putting all the others into a third column. The table itself shows the reason for these modes of record in *the great predominance of the small departures.*

This very predominance of the small departures means that the real departures are minute and the actual departures here tabulated as the results of the experimental work done during the century prove the absolute atomic weights to be the real atomic weights of nature. The question of a minute weight of the chemical valence, touched upon in our recent publications, does not come under consideration here.

The table shows that 50 per cent. of all cases give departures below 10 thousandths (0.01) and 70 per cent. below 20 thousandths (0.02) on all the 656 atomic weight determinations made by all the 300 reactions employed. This proves that the determinations have for their limit the absolute atomic weights themselves and that the departures represent mainly the imperfections of the experimental work.

The relatively few larger departures (only about 12 per cent.)

above 40 thousandths (0.04) are mainly due to the fact that we have taken all reactions used without excluding those known to be imperfect and which are generally excluded by others.

It will also be noticed that the mean departure for the greatest number of cases is only one or a few thousandths; it is without significance in the question here considered.

This question can now be fully answered. Bearing in mind that increased atomic weight necessarily brings a slight increase in the departures for experimental reasons, we must admit that *the departures for oxygen and for silver are essentially alike, so that the values* $O = 16$ *and* $Ag = 108$ *stand and fall together.* If oxygen is 16, then silver is 108, with nearly the same degree of precision. Again, if the value 108 is denied for silver, then the value 16 for oxygen is equally untenable.

Finally, it appears to me that it has been fully demonstrated that the assumption of immunity from error for oxygen is as false in fact as it is absurd in philosophy; possibly that accounts for the tenacity with which the school has clung to the same.

St. Louis, Mo.

THE PROPAGATION OF LONG ELECTRIC WAVES ALONG WIRES.

By E. P. ADAMS.

(*Read December 2, 1910.*)

In the usual deduction of the equations of propagation of electric waves along wires the notion of the electric constants per unit length is introduced. While there is no difficulty involved in this as far as resistance and leakage are concerned, the legitimacy of the extension of this notion to self-induction and capacity is not obvious. In order to determine the exact meaning to be attached to these terms it is convenient to consider a line in which the electric properties are localized in a finite number of coils, condensers and leaks, joined by ideal conductors of no resistance, self-induction and capacity. For the special case of long electric waves the solution can readily be obtained by means of the calculus of finite differences. On passing to the limit, by letting the number of coils, etc., increase indefinitely while their electric constants decrease indefinitely, the equations of propagation and their solution for a uniform line are at once obtained. There appears to be a considerable advantage in the use of this method in respect to its simplicity, particularly where the terminal conditions are at all complicated. Two problems are worked out in this paper; the first that of the free vibrations of a line earthed at both ends, and the second that of the forced vibrations when a periodic impressed electromotive force is applied to the circuit.

Consider a line of length l, in which are inserted at equal intervals n coils each of resistance R' and self-induction L. At points between each pair of coils one plate of a condenser of capacity S is connected, the other plate being earthed; and at the same points leaks to earth, each of conductance K', are introduced. The current in the kth coil is C_k, and the potential at a point between the coils k and $k+1$ is V_k. For the first problem we then have $V_o=V_n=0$. Let L_1 be the coefficient of mutual induction between any coil and one of its nearest neighbors; L_2 the coefficient of mutual induction between the same coil and its next nearest neighbor but one, and so on. Similarly, let S_1 be the electric induction coefficient between any condenser and one of its nearest neighbors; S_2 the induction coefficient between two alternate condensers, etc. We can then write for the kth coil:

$$V_{k-1}-V_k=\frac{d}{dt}\Big(LC_k+L_1\,C_{k-1}+L_1\,C_{k+1}+L_2\,C_{k-2}+L_2\,C_{k+2}+\cdots\Big)+R'C_k \tag{1}$$

and for the kth condenser:

$$C_k-C_{k+1}=\frac{d}{dt}\Big(SV_k+S_1\,V_{k-1}+S_1\,V_{k+1}+S_2\,V_{k-2}+S_2\,V_{k+2}+\cdots\Big)+K'V_k \tag{2}$$

Now in the case of long electric waves the currents in any coil and its near neighbors will be very nearly the same. The terms in the series in (1) containing currents in distant coils become relatively unimportant on account of the diminution of their coefficients. In this special case it will therefore be legitimate to replace the series by a single term and we can therefore write:

$$V_{k+1}-V_k=L'\frac{d}{dt}C_k+R'\,C_k, \tag{3}$$

in which L' may be termed the effective coefficient of self-induction of any one of the coils. When we pass to the limit by increasing indefinitely the number of coils, etc., and at the same time decreasing indefinitely all the electric constants, the limiting value which the product of L' by the number of coils in a unit length approaches will be the self-induction per unit length of the uniform line. Equation (2) modified in an analogous manner reduces to:

$$C_k-C_{k+1}=S'\frac{d}{dt}V_k+K'\,V_k \tag{4}$$

Subtracting the equation for the coil $k+1$ from (3) and substituting from (4), we get:

$$V_{k-1}-(2+h)V_k+V_{k+1}=0, \tag{5}$$

where

$$h=R'K'-L'S'p^2+ip(L'K'+R'S'), \tag{6}$$

in which it is assumed that the potentials and currents all vary as e^{ipt}. (5) is a linear difference equation of the second order. By the usual method we put

$$V_k=A_k\alpha^k e^{ipt},$$

and find:

$$\alpha=\frac{2+h}{2}\pm\tfrac{1}{2}\sqrt{4h+h^2}\cdot \tag{7}$$

Let these two values be α and β, the former with the positive sign of the radical. In general α and β are different, and so we get the two distinct solutions required by an equation of the second order. But for $h=0$ or $h=-4$ α and β have the same values. The complete solution of (5) is therefore:

$$V_k=(A_1+B_1k)e^{ip_1t}+(A_2+B_2k)e^{ip_2t}+(A_3+B_3k)(-1)e^{ip_3t}$$
$$+(A_4+B_4k)(-1)^k e^{ip_4t}+\sum(A_p\alpha^k+B_p\beta^k)e^{ipt}.$$

Since $V_0=V_n=0$, whatever t, $A_1=A_2=A_3=A_4=B_1=B_2=B_3=B_4=0$; $A_p+B_p=0$ and

$$\alpha^n-\beta^n=0. \tag{8}$$

Let now

$$h=-4\sin^2\theta, \tag{9}$$

(7) reduces to

$$\alpha,\beta=\cos 2\theta\pm i\sin 2\theta, \tag{10}$$

and (8) gives

$$\theta=\frac{m\pi}{2n}, \tag{11}$$

where m is any integer. $m=0$ and $m=n$ are excluded because these give $h=0$ and $h=-4$, which are already disposed of. If we take $m=n+1$, $n+2$, etc., we get the same series of values obtained from $m=1$ to $m=n-1$. We thus get:

$$V_k = \sum_{m=1}^{m=n-1} A_m \sin \frac{km\pi}{n} e^{ip_m t}. \qquad (12)$$

A_m and p_m are complex quantities. Writing $p_m = p_m' + ip_m''$, we get from (6), (9) and (10):

$$p_m' = \sqrt{\frac{4 \sin^2 \frac{m\pi}{2n}}{L'S'} - \frac{(L'K' - R'S')^2}{4L'^2S'^2}}, \qquad (13)$$

$$p_m'' = \frac{L'K' + R'S'}{2\,L'S'} = p''. \qquad (14)$$

p_m'' is thus independent of m. The real part of (12) may now be written

$$V_k = e^{-p''t} \sum_{1}^{n-1} A_m \cos (p_m't - \phi_m) \sin \frac{km\pi}{n}, \qquad (15)$$

where A_m and ϕ_m are new arbitrary real constants.

The currents in the several coils may be obtained from (3) combined with (12). Taking the real part we find

$$C_k = e^{-p''t} \sum_{1}^{n-1} B_m \cos (p_m't - \psi_m) \cos (2k - 1)\frac{m\pi}{n} \qquad (16)$$

where B_m and ψ_m are known in terms of A_m and ϕ_m in (15).

The last four equations give the complete solution of the problem. The constants A_m and ϕ_m may be determined by Fourier's method when the initial conditions are known.

Now let n increase indefinitely while R', L', S' and K' all decrease indefinitely. Let $L =$ limit $L'n/l$, and similarly for the others. Let δx be the distance between two coils, so that $n\delta x = l$. Measuring x from the end of the line corresponding to $k=0$, we have $k = nx/l$ and we get in the limit:

$$V = e^{-p''t} \sum_{1}^{\infty} A_m \cos (p_m't - \phi_m) \sin \frac{m\pi x}{l}, \qquad (17)$$

$$C = e^{-p''t} \sum_{1}^{\infty} B_m \cos (p'_m t - \psi_m) \cos \frac{m\pi x}{l}, \qquad (18)$$

$$p_m' = \sqrt{\frac{m^2\pi^2}{l^2LS} - \left(\frac{LK - RS}{2LS}\right)^2}, \tag{19}$$

$$p'' = \frac{LK + RS}{2LS}, \tag{20}$$

which are the well-known solutions for the free vibrations of a uniform line for long waves.

The differential equation of which (17) is the solution is obtained by passing to the limit in the difference equation (5). We thus get

$$\frac{\partial^2 V}{\partial x^2} = LS\frac{\partial^2 V}{\partial t^2} + (LK + RS)\frac{\partial V}{\partial t} + RKV.$$

Equations (3) and (4) on passing to the limit give:

$$-\frac{\partial V}{\partial x} = L\frac{\partial C}{\partial t} + RC,$$

$$-\frac{\partial C}{\partial x} = S\frac{\partial V}{\partial t} + KV.$$

For the second problem, that of a periodic impressed electromotive force applied to one end of a line, the other end being earthed, we have to solve equation (5) subject to the conditions:

$$\begin{aligned} k &= 0, \quad V_k = Ee^{i\nu t}, \\ k &= n, \quad V_k = 0. \end{aligned} \tag{21}$$

The resulting solution may of course be applied to a closed circuit with the periodic force $Ee^{i\nu t}$ introduced in it at any point. After the free vibrations have been damped out, the solution will be

$$V_k = (A\alpha^k + B\beta^k)Ee^{i\nu t},$$

where A and B are arbitrary constants and α and β are given by (10). Determining A and B by means of (21) we get

$$V_k = \frac{\sin 2(n - k)\theta}{\sin 2n\theta} Ee^{i\nu t}. \tag{22}$$

θ is a complex angle defined by (6) and (9) if ν is written for p

in (6). Putting $\theta = \theta' + i\theta''$, we get as the real part of (22)

$$V_k = \frac{E}{2(\cosh 4n\theta'' - \cos 4n\theta')} \{e^{2(2n-k)\theta''} \cos(\nu t + 2k\theta') + e^{-2(2n-k)\theta''} \cos(\nu t - 2k\theta') - e^{2k\theta''} \cos(\nu t + 4n\theta' - 2k\theta') - e^{-2k\theta''} \cos(\nu t - 4n\theta' + 2k\theta')\}, \tag{23}$$

which together with

$$4(\sin^2\theta' \cosh^2\theta'' - \cos^2\theta' \sinh^2\theta'') = \nu^2 L'S' - R'K' \tag{24}$$

$$-4\sin 2\theta' \sinh 2\theta'' = \nu(L'K' + R'S') \tag{25}$$

gives the complete solution.

Now on passing to the limit as before, we can replace $\sin\theta$ by θ, $L' = L\delta x$, etc., and we find

$$2\theta' = -Q\delta x,$$
$$2\theta'' = P\delta x,$$

where

$$P,\ Q = \frac{1}{\sqrt{2}} \{(\nu^2 L^2 + R^2)(\nu^2 S^2 + K^2) \pm (RK - \nu^2 LS)\}^{\frac{1}{2}},$$

We thus have

$$4n\theta'' = 2Pl, \quad 4n\theta' = -2Ql,$$
$$2k\theta'' = Px, \quad 2k\theta' = -Qx;$$

(23) thus reduces to

$$V = Ee^{-Px}\cos(\nu t - Qx) + \frac{Ee^{-Pl}}{2(\cosh 2Pl - \cos 2Ql)^{\frac{1}{2}}} \times \{e^{Px}\cos(\nu t + Qx + \phi) - e^{-Px}\cos(\nu t - Qx + \phi)\},$$

where

$$\tan\phi = \frac{\sin 2Ql}{e^{-2Pl} - \cos 2Ql},$$

which is the solution for this case as given by Heaviside,[1] except that leakage is here considered and the real impressed force is $E\cos\nu t$ instead of $E\sin\nu t$.

[1] "Electrical Papers," Vol. 2, p. 62.

PRINCETON UNIVERSITY.

INDEX.

MAGELLANIC PREMIUM

FOUNDED IN 1786 BY JOHN HYACINTH DE MAGELLAN, OF LONDON

1911

THE AMERICAN PHILOSOPHICAL SOCIETY

HELD AT PHILADELPHIA, FOR PROMOTING USEFUL KNOWLEDGE

ANNOUNCES THAT IN

DECEMBER, 1911

IT WILL AWARD ITS

MAGELLANIC GOLD MEDAL

TO THE AUTHOR OF THE BEST DISCOVERY, OR MOST USEFUL INVENTION, RELATING TO NAVIGATION, ASTRONOMY, OR NATURAL PHILOSOPHY (MERE NATURAL HISTORY ONLY EXCEPTED) UNDER THE FOLLOWING CONDITIONS:

1. The candidate shall, on or before November 1, 1911, deliver free of postage or other charges, his discovery, invention or improvement, addressed to the President of the American Philosophical Society, No. 104 South Fifth Street, Philadelphia, U. S. A., and shall distinguish his performance by some motto, device, or other signature. With his discovery, invention, or improvement, he shall also send a sealed letter containing the same motto, device, or other signature, and subscribed with the real name and place of residence of the author.
2. Persons of any nation, sect or denomination whatever, shall be admitted as candidates for this premium.
3. No discovery, invention or improvement shall be entitled to this premium which hath been already published, or for which the author hath been publicly rewarded elsewhere.
4. The candidate shall communicate his discovery, invention or improvement, either in the English, French, German, or Latin language.
5. A full account of the crowned subject shall be published by the Society, as soon as may be after the adjudication, either in a separate publication, or in the next succeeding volume of their Transactions, or in both.
6. The premium shall consist of an oval plate of solid standard gold of the value of ten guineas, suitably inscribed, with the seal of the Society annexed to the medal by a ribbon.

All correspondence in relation hereto should be addressed

TO THE SECRETARIES OF THE

AMERICAN PHILOSOPHICAL SOCIETY

NO. 104 SOUTH FIFTH STREET

PHILADELPHIA, U. S. A.

The American Philosophical Society

Announces that an Award of the

HENRY M. PHILLIPS PRIZE

will be made during the year 1912; essays for the same to be in the possession of the Society before the first day of January, 1912.

The subject upon which essays are to be furnished by competitors is:

The Treaty-making power of the United States and the methods of its enforcement as affecting the Police Powers of the States.

The essay shall contain not more than one hundred thousand words, excluding notes. Such notes, if any, should be kept separate as an Appendix.

The Prize for the crowned essay will be $2,000 lawful gold coin of the United States, to be paid as soon as may be after the award.

Competitors for the prize shall affix to their essays some motto or name (not the proper name of the author, however), and when the essay is forwarded to the Society it shall be accompanied by a sealed envelope, containing within, the proper name of the author and, on the outside thereof, the motto or name adopted for the essay.

Essays may be written in English, French, German, Dutch, Italian, Spanish or Latin; but, if in any language except English, must be accompanied by an English translation of the same.

No treatise or essay shall be entitled to compete for the prize tht has been already published or printed, or for which the author has received already any prize, profit, or honor, of any nature whatsoever.

All essays must be typewritten on one side of the paper only.

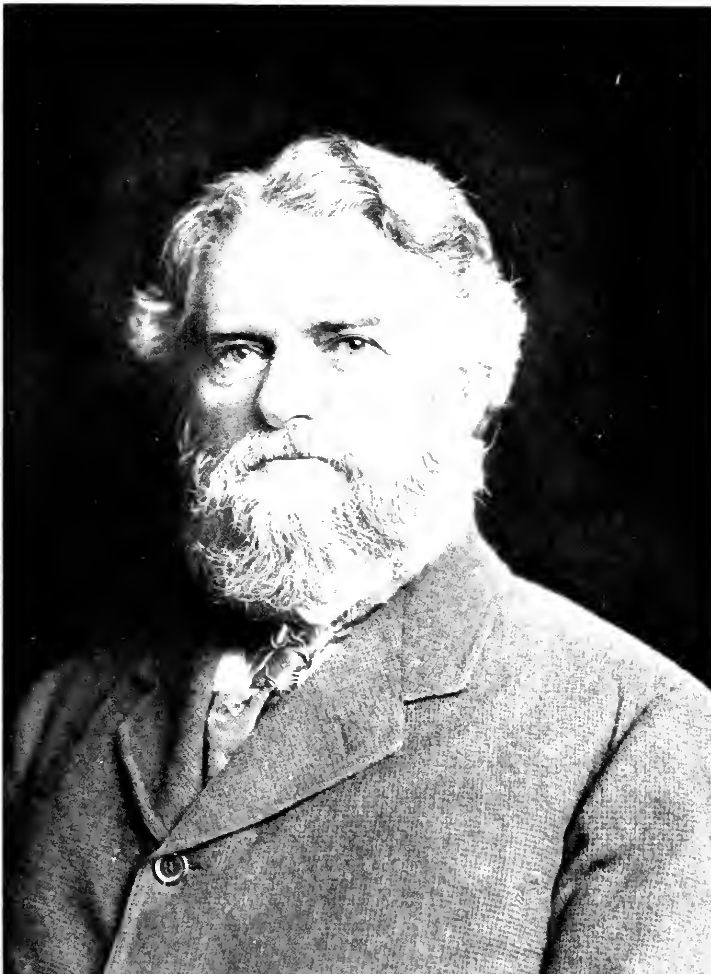
The literary property of such essays shall be in their authors, subject to the right of the Society to publish the crowned essay in its Transactions or Proceedings.

The essays must be sent, addressed to

The President of the American Philosophical Society,

No. 104 South Fifth Street, Philadelphia, Penna., U. S. A.

OBITUARY NOTICES OF MEMBERS DECEASED.



Simon Newcomb

SIMON NEWCOMB, F.R.S., LL.D., D.C.L.

(Read April 23, 1910.)

The subject of this sketch, Simon Newcomb, furnishes a conspicuous instance of a career carved out by the man himself. Born March 12, 1835, in what seems to us a remote region of Nova Scotia, with nothing in his early history to suggest much beyond a perpetual struggle for existence, he, nevertheless, by the exercise of great natural talent, with undaunted pluck and perseverence, succeeded in attaining a place in the front ranks of his favorite science, that science which some of us, at least, consider the noblest of all—namely astronomy.

Though born in Nova Scotia, Professor Newcomb's ancestors were of that sturdy New England stock which his furnished so large a proportion of our distinguished men, and which has contributed so powerfully toward making our nation what it is to-day.

In his remniscences, Professor Newcomb says:

> So far as the economic conditions of society and the general mode of thinking were concerned, I might lay claim to have lived in the time of the American Revolution. A railway was something read or heard about with wonder, a steamer had never ploughed the waters of Wallice Bay. Nearly everything necessary for the daily life of the people had to be made on the spot and even at home.

It was in this environment of Arcadian simplicity that young Newcomb passed his early years. His father's occupation was that of school teacher—not a lucrative one, and made even less so by the fact that he had ideas of his own on the subject of education which were not in strict harmony with those of his contemporaries. Wealth and poverty are, however, relative terms, and this early period which, from our point of view, would no doubt appear to be one of considerable privation, was probably not so regarded by those immediately concerned.

Such were the surroundings in which the first sixteen years of young Newcomb's life were passed. Where everyone labored from daylight to dark, his lot could not be expected to form an exception.

The future seemed to have only the vaguest prospects of any other lot. His father indeed spoke of an ambition to see him a prominent lawyer, while it was his mother's wish that he should become a clergyman, although she feared that he would never be good enough. In his own day dreams, he was a farmer, driving his own team, although he appears never to have found himself in harmony with this mode of life, being greatly mortified at his want of skill, particularly in the art of driving oxen.

At the age of sixteen, an opportunity presented itself which seemed to offer a solution of the problem of a career for the future, more in keeping with the tastes of a young man like Mr. Newcomb, than the life of a farmer. This was an arrangement with a quack doctor, Hershay by name, by which Newcomb was to live with the former until of age, assisting about the house and office in whatever way he could, in return for which the doctor was to teach him what he knew of medicine. It cannot be said that the doctor failed to carry out this part of the contract, but as a guide and example at this formative period of a young man's life, nothing could be more pernicious. On the only occasion when the doctor expressed himself freely to his pupil, he gave his views as to the true secret of success as follows, "The world is all a humbug and the biggest humbug is the best man." As for Newcomb, it seems very unfortunate that he should have wasted two years of his life with the petty drudgery of this position, when he was so obviously receiving nothing in return. The relation was, however, terminated by Newcomb, two years before the expiration of the contract by the simple and effectual process of running away. In due time, he joined his father at Salem, Massachusetts, who had decided to try his fortune in the states, his mother having died some years before. Before very long the young man found himself in charge of a country school, at a place called Massey's cross roads, and later at Sudlersville, Kent County, on the east shore of Maryland.

This may be considered as the beginning, though an humble one, of a new order of things, the outcome of which was to be the distinguished scientist as we know him, the recipient of the highest honors and distinctions which the world of science and culture had to bestow.

In Newcomb's accounts of the early years of his life we are impressed by the small amount of encouragement and assistance which he found in his vague aspirations toward something better. He seems to have read everything that came within his reach. This included a number of very uninviting works on geometry and algebra, navigation and the like, which very few boys would have the courage to attempt without a teacher. But the entire absence of anyone capable of directing him in his studies, is perhaps even more marked than the absence of books. Everyone in this primitive region where his early life was passed, seems to have been so absorbed in the struggle for existence that very little energy remained for anything else. If during the first eighteen years of his life, he ever met anyone who had seen a college, I have found no mention of the fact.

In 1856 Mr. Newcomb found himself teaching in the family of a planter named Byran, fifteen or twenty miles from Washington. He could, therefore, readily visit this place at frequent intervals. He tells us that, up to this time, he is not aware that he had ever seen a real live professor. He had, however, had a little correspondence with Professor Henry, which is so characteristic of the kindly and genial nature of the latter as to be worth relating. While teaching at Sudlersville, Mr. Newcomb made what appears to have been his first serious attempt at an original mathematical investigation, viz., "A New Demonstration of the Binomial Theorem." This he sent to Professor Henry, asking whether he considered it suitable for publication. Instead of consigning it to the waste paper basket and giving it no further attention, as many a man in Professor Henry's position would have done, he gave a negative reply, but as mathematics was not his specialty, offering to submit the document to some one better informed than himself in such matters. As was to have been expected, an adverse report came to hand in due time, which Professor Henry transmitted, accompanied by a pleasant note from himself to the effect that, although not so favorable as might have been expected, it was sufficiently so to encourage further effort. Soon after this Mr. Newcomb took what was for him the bold step of calling on Professor Henry, who not only received him with characteristic kindness, but in due time introduced him to Professor Hilgard, of the Coast Survey, who in turn introduced him to Professor Winlock, of the "Nautical Almanac."

At this time the headquarters of this publication were at Cambridge, Mass. Encouraged by a note from Professor Henry, Mr. Newcomb started for Cambridge in December, 1856, hoping to find employment. After some delay, he was finally installed as computer at a salary of thirty dollars a month. Humble as this may appear to us, the day of receiving the appointment was probably the happiest of his life up to this time. Professor Newcomb speaks of his first arrival at this destination as follows:

> I date my birth into the world of sweetness and light on one frosty morning in January, 1857, when I took my seat between two well-known mathematicians, before a blazing fire in the office of the Nautical Almanac at Cambridge, Mass. The men beside me were Professor Joseph Winlock, the superintendent, and Mr. John D. Runkle, the senior assistant in the office.

Nearly five years were passed here amid surroundings which were very pleasant and agreeable, the more so by contrast with Mr. Newcomb's former life. They were years of education and development, the opportunities being peculiarly favorable. No fixed hours of attendance were required at the office. If the work assigned was due satisfactorily, that seems to have fulfilled every requirement. This made it possible for Mr. Newcomb to become a student in the Lawrence Scientific School, from which he graduated in due time. His course was naturally largely mathematical under the direction of the well-known Benjamin Peirce. The number of students at this time, following this line of work was naturally small, but Professor Peirce's abstruce lectures found at least one appreciative listener. We naturally feel considerable pride in the prominent place which American astronomy and astronomers occupy in the world today. Matters were, however, very different in this respect fifty-five years ago. It was about this time that the situation was summed up as follows by a German "Gelehrte" who had recently visited this country, "You have one astronomer, Professor Peirce, and no mathematicians." The prediction of Alexis de Tocqueville, that the conditions of life in America would never be favorable to the development of eminence in science, seems to have been tacitly accepted by many as practically disposing of the matter.

The beginnings of physical science in America have been compared to the birth of Minerva as she sprang fully armed from the brain of Jove. The simultaneous appearance, here in Philadelphia, of Franklin, Rittenhouse and William Smith, at once brought this country to the notice of the devotees of science in Europe. The interest taken in the transit of Venus in 1769, for observing which elaborate preparations were made in Philadelphia, brought astronomy to the front, with plans for a first class astronomical observatory with, presumably, Rittenhouse as director. This project was probably premature, but in any case the approaching revolution soon absorbed all the energies of the country, and three quarters of a century was destined to elapse before anything like a general revival of interest in this subject took place. At the time of which we are speaking, however, the movement was well under way, and the men with whom Mr. Newcomb was associated at Cambridge including more than one name destined to achieve international reputation. A number of observatories were in more or less active operation but, as yet, little had been produced which could bear comparison with the best work in Europe.

The work, however, was well begun, but was no doubt destined to be somewhat retarded by the Civil War—the preliminary skirmishes were even now being fought in Kansas and elsewhere. But the times were ripe for an advance and it could not again be suppressed.

On July 17, 1860, occurred a total eclipse of the sun. The path of totality passed across British North America, touching southern Greenland, thence across the Atlantic to Spain. Mr. Newcomb was one of a party organized for observing the eclipse. The point chosen was on the Saskatchewan River, about as inaccessible and remote from civilization at that time as central Africa is today. The journey lay from St. Paul across Minnesota by stage, thence down the Red River by steamer, across Lake Winnipeg and up the Saskatchewan by birch bark canoe. Unexpected delays and difficulties seemed likely to prevent the party from reaching its destination in time for the eclipse. By heroic exertion on the part of the half breeds who paddled the canoe, however, they arrived at the place selected on the evening before the eclipse was to take place. Clouds, however, covered the sky so that nothing could be done, a not uncommon experience in such cases.

In 1861 Mr. Newcomb was informed of a vacancy in the corps of professors of mathematics attached to the Naval Observatory at Washington, with the suggestion that he should apply for the place. Although the desirability of some position of greater prominence and with a better outlook for the future was not new, still the surroundings and attractions at Cambridge were so congenial that the thought of severing them was not an agreeable one. After some hesitation, however, Mr. Newcomb made formal application for the professorship and, as he confesses, was greatly surprised to receive, a month later, his commission, duly signed by Abraham Lincoln.

The duties at the Washington Observatory were of a character entirely new to Mr. Newcomb. The use of instruments in practical work was entirely outside his experience. In fact, with the exception of two or three visits to the Cambridge Observatory, he had never been inside such an establishment. Nor had he any particular liking for this kind of work, which, it must be confessed, involves a great amount of drudgery, and interferes sadly with continuous theoretical investigation.

The Washington Observatory was at that time practically the only place in the country where continuous observation was carried on. The principal instruments consisted of a mural circle in charge of Professor Yarnall, with the necessary clocks and subsidiary apparatus. In point of accuracy and precision, these were not what we should call first class instruments. But the methods followed were even worse than the instruments. Each observer pursued his own plan, observing what he pleased and when he pleased, with no uniformity of program, employing no uniform system of reduction, so that anything like homogeneity of results was out of the question. To add to the difficulty, the observatory was situated in a malarial district near the Potomac, far from the resident quarter of the city, so that the observers were compelled to walk from one to three miles through muddy streets in going and returning from work.

Mr. Newcomb was at first assigned to duty as assistant to Professor Yarnall on the transit instrument. The results of this system or want of system, were afterwards published, forming the Washington catalogue of 10,964 stars, commonly known as "Yarnall's catalogue." Though by no means worthless, this is far from possessing the value which it should have had. The work done at Greenwich, for instance, at the same time, was much superior.

It is to be remembered that, at the period of which we are now speaking, the Civil War was raging at its greatest fury. Washington was the center of gigantic military operations which seemed to overshadow everything else. But those in authority took a certain pride in having this work kept up without interruption during the conflict. On one occasion only does the serenity, supposed to attend scientific pursuits, seem to have been seriously disturbed. This was on the occasion of the noted raid of General Early in 1864. The defeat by the latter of General Lew Wallace seemed to leave the way open to Washington. It is an open question whether the city might have been taken by a rapid dash on Early's part at this time. Under these conditions, all who were in the service of the war and navy departments were ordered out to assist in manning the entrenchments which were the only defence of the city. The detachment to which Professor Newcomb was assigned was ordered to Fort Lincoln, where for two days they waited an attack by Early. Meanwhile reinforcements arrived from Fort Monroe and Early abandoned any design which he may have had for an attack.

Captain Giliss, the superintendent of the observatory, was an astronomer of distinction. Previous to his taking charge of this work, he had made many thousands of observations. He was naturally much interested in the improvement of the unsatisfactory state of affairs, but it was not an easy matter, in these exciting times, to accomplish much. At length, in 1863, he obtained authority to procure a meridian circle of the highest order of excellence, which was finally completed and ready for active service on January 1, 1866. Professor Newcomb was placed in charge with three assistants. An elaborate program of fundamental work was adapted to be carried out on a uniform plan for three years. This involved continuous attendance on the part of one or another of the observers, both day and night whenever the weather permitted observation. Much was expected in the way of results, superior perhaps to anything before attained, but these anticipations were not fully realized. The instrument itself was not what had been expected and the mounting proved unstable, so that the results, instead of the superiority which had been looked for, proved inferior to those reached at the European observatories.

When a large telescope was installed at Washington toward the end of 1875, Professor Newcomb was placed in charge of the instrument, which at his request was turned over to Professor Hall two years later. This closed Professor Newcomb's activities in the way of systematic observation.

In 1869 he made application to be transferred to the office of the "Nautical Almanac," with the understanding that his time should be devoted to an investigation of the moon's motion, a problem in which he had become greatly interested. Doubtless there is room for honest difference of opinion as to the relative importance of the two branches of astronomical work, but if the lunar problem was to have been taken hold of at this time, there is no question as to whom it should have been entrusted. Men could be found in plenty who were capable of reaching valuable results in the field of observation, but those who were capable of attacking with success the most intricate of all astronomical problems, the lunar theory, have always been extremely few. The transfer to the "Nautical Almanac" was not made, but the arrangement which resulted was probably even more satisfactory. A few years before Newcomb began his work on the moon, the lunar tables of Hansen had been published. They were based on a part only of the Greenwich observations from 1750 to 1850, and represented with practical accuracy the moon's motions for this period of a full century. But, in the course of a very few years, the actual position was found to deviate very appreciably from those given by the tables, the deviations increasing from year to year. What the state of things previous to 1750 had been was a very interesting question, but one not easy of solution, as very little material accurate enough for such an investigation was known to exist.

The ordinary meridian determinations, before the time of Bradley, 1750, were of very little use for a refined investigation like this. Another class of observations, however, furnished data comparable in accuracy with the best determinations made today, that is, the occultations of stars and planets by the moon. Though but few such had ever been published, it occurred to Professor Newcomb that among the unpublished work at the European observatories, possibly enough such data might be found to repay the labor involved in the search. The result exceeded his most sanguine expectations. At the Paris observatory, in particular, data were brought to light which carried the period of accurate lunar observation back nearly a century, so that now instead of a lunar theory, which like that of Hansen depended on one hundred years of observation, we have available two hundred and fifty years of accurate data.

It is interesting to know that this work at the Paris Observatory was carried on while the struggle with the Commune was at its height, the windows frequently rattling with the reports of cannon.

The lunar theory, as it is called, seems to have been the problem of Professor Newcomb's predilection. Besides the researches already mentioned which involved a great amount of time and labor he wrote, together with other papers, an elaborate memoir dealing with the action of the planets on the moon. He did not attempt, however, a complete revision of the subject. His most important services in this direction were in what has been styled the border land between the theoretical and practical, viz., that of assembling all available data and comparison with theory, thus exhibiting in a concise manner precisely what remains to be accomplished in order to bring the two into harmony.

The last of his published papers is found in the *Monthly Notices of the Royal Astronomical Society* for January, 1909, exhibiting the deviations of the mean longitude of the moon from the position given by theory. The explanation of these discrepancies now constitutes one of the most interesting of the unsolved problems of astronomy.

The theoretical researches in the lunar theory have been greatly extended by the classic work of Mr. George W. Hill, while the prodigious labor involved in applying the latter to the derivations of new tables of the moon's motion is being well taken care of by our fellow member, Professor Ernest W. Brown.

In September, 1877, occurred an event which had been long anticipated, viz., the retirement of Professor Coffin from the directorship of the "Nautical Almanac," and the appointment of Professor Newcomb to this position. Meanwhile, in 1875, the directorship of the Harvard College Observatory became vacant by the death of Professor Winlock. Soon after, Professor Newcomb was surprised to receive from President Eliot a letter offering him the position. Although, as has been said already, the practical work of an observatory was not the line of activity which appealed most strongly to Professor Newcomb's tastes, a professorship at Harvard, with all that this implies, was not to be lightly disregarded. There was, moreover, opportunity for escape from the political atmosphere with all its petty annoyances, attendant on a government position at Washington.

I do not know that Professor Newcomb was ever fully convinced that he had chosen the better course in declining this offer, though he disposes of the matter very modestly as follows: "No one who knows what the Cambridge observatory has become under Professor Pickering can feel that Harvard had any cause to regret my decision."

The directorship of the "Nautical Almanac" now gave Professor Newcomb the long-wished-for opportunity to take up seriously the herculean task of a complete revision of the entire subject of exact astronomy. Only those who have had some experience in these matters can form any adequate conception of what this involved. The vast field of stellar astronomy, of the planetary and lunar motions, had been cultivated since the time of Hipparchus by many of the ablest minds which the world has produced. As, however, each investigator usually carried on his work independently of the others, there was great want of consistency and homogeneity in the mass taken as a whole. Professor Newcomb may not at first have planned so large an undertaking, but this is the form which it assumed. For twenty years, during which he remained at the head of the "Nautical Almanac," assisted by a small army of aids and computers, including at least one man of international reputation, and at one time or another a dozen line officers of the navy, this work went steadily forward.

All who are in any way interested in the professional work of astronomy are familiar with some part at least of the eight quarto volumes of astronomical papers of the "American Ephemeris" which contain the most important results of this undertaking. This is not the place for an analysis of the contents of these volumes or for more than the briefest outline of the work attempted. It may be said that it involved a complete investigation of the orbits of the principal planets, on a uniform plan employing a strictly homogeneous system of constants derived from practically all existing data. For the latter purpose, the number of observed positions of the sun, Mercury, Venus and Mars alone numbered 62,030, made at thirteen different observatories. The investigations for these inner planets, and the two outer ones, Uranus and Neptune, were made by Professor Newcomb himself or under his personal direction. That of Jupiter and Saturn was entrusted entirely to Mr. George W. Hill. A man more competent could not have been found on either side of the Atlantic. Without his valuable assistance, it would hardly have been possible to bring the task to a successful close. Redeterminations of the solar parallax, the constants of precession, nutation and abberration were involved directly or indirectly in the undertaking, together with an elaborate investigation of the places of the fixed stars, on which, in the last analysis, all else depends.

This work, which has been outlined, was near completion when the time for retirement under the age limit arrived. Doubtless, Professor Newcomb would have preferred to remain at his post some time longer, but the law was inexorable.

His retirement did not, however, imply cessation from activity. Arrangements were made by which this great work was brought to a practical completion. The lunar researches were provided for by a grant from the Carnegie fund, and were completed only a short time before his death, under conditions of physical suffering, such that very few would have had energy for any purpose.

Besides the activities which have been briefly outlined in what precedes, Professor Newcomb was more or less directly connected with a large number of scientific undertakings, particularly with many astronomical enterprises of importance in which this country was concerned. He was secretary of the Transit of Venus Commission in 1874 and 1882, and was in charge of a party in 1882 for observing the transit at the Cape of Good Hope. This event attracted much attention at the time and there seems to have been no difficulty in obtaining from Congress appropriations aggregating $375,000 for the purpose. Yet a small sum of perhaps $5,000 for preparing the results for publication has never been forthcoming. The work has consequently never been published and there is little prospect that it ever will be.

Professor Newcomb also took part in several eclipse expeditions, one of which, that of 1860, has already been mentioned. He was, to a great extent, responsible for the planning and installing of the large telescope at the Washington Observatory, at that time the largest refracting telescope in the world. The details of location, construction and equipment of the Lick Observatory were settled, for the most part, by his advice in coöperation with Professor Holden, its first director.

Professor Newcomb was appointed professor of mathematics and astronomy at the Johns Hopkins University in 1884, as successor to Professor Sylvester. His duties as teacher closed in 1894. In 1900 he was made professor emeritus. He was editor of the *American Journal of Mathematics* from 1884 to 1894, and during 1899 to 1900.

Professor Newcomb was president of the American Association for the Advancement of Science in 1876 and 1877, of the Astronomical and Astrophysical Society of America from its beginning in 1899 until 1905, of the American Mathematical Society, the Society for Psychic Research, and chairman of a great number of scientific assemblages and congresses, the most important of which was perhaps the International Congress of Arts and Sciences, held at St. Louis in 1906 in connection with the exposition, the complete success of which was due more than anything else to his world-wide reputation and to the untiring energy with which he planned and carried out the undertaking.

Professor Newcomb was a recipient of honorary degrees from seventeen American and foreign universities; academies and scientific organizations throughout the world honored him with membership, decorations and medals, until the supply was almost exhausted. Among others, he was one of the eight foreign associates of the Institute of France, a distinction which had come to no other American scientist since the time of Franklin. He was commander of the Legion of Honor of France. From the German Emperor he received the highest honor which he could bestow, viz., Knighthood for Merit in Science and Art, a distinction held by no other native American. The complete list is too long for this time and place.

He was presented to Emperor William, to King Edward, the kings of Italy and of Sweden, and the president of the French Republic.

A complete bibliography of Professor Newcomb's writings embraces about four hundred titles. A large variety of subjects, practically every phase of astronomical science, received some attention. Many of these works are elaborate treatises embodying the labor of years. There are papers on pure mathematics, on political economy, in which he was greatly interested, series of astronomical and mathematical text-books, and many books and magazine articles of a popular or semi-popular nature. The following are a few specimen titles: "Reminiscences of an Astronomer," "Sidelights on Astronomy," The A. B. C. of Finance," "Principals of Political Economy," "A Plain Man's Talk on the Labor Question," "Popular Astronomy," "Astronomy for Everybody." Translations of his books are to be found in the German, Russian, Dutch, Norwegian, Bohemian and Japanese languages.

Professor Newcomb's ability to concentrate his attention strictly on the matter in hand was a very important factor in accomplishing what he did. This perhaps gave an impression of indifference and unsociability in the minds of those who knew him only casually. Naturally he had little patience with the too numerous class of charlatans and cranks who are always ready to waste the time of a man of any prominence in expounding their peculiar views, but those who sought his advice and encouragement in reference to a matter of any importance found him more than ready to give such assistance as lay in his power.

His busy life naturally limited his social activities. He cared little for fashionable gatherings, but he greatly enjoyed the company of congenial minds, and many men of science, residents of Washington, and visitors from other places, can testify to the hospitality with which they were entertained at his home. He was very fond of history and poetry, his favorite poems seem to have been memorized with very little effort. Addison's Ode, "The Spacious Firmament on High," especially the closing lines, appears to have given him great pleasure:

"What though in solemn silence all
Move round the dark terrestrial ball?
What though no real voice nor sound
Amid their radiant orbs be found?
In reason's ear they all rejoice
And utter forth a glorious voice
Forever singing as they shine,
The hand that made us is divine!"

Professor Newcomb was always religiously inclined, though he never became a church communicant. For many years he attended the Presbyterian Church with his wife and family. And he was a firm believer in a future life. One of the great pleasures to which he looked forward was that of meeting such men as Hipparchus, Copernicus, Newton and others who had gone before.

Professor Newcomb was the oldest of seven children. Two brothers and two sisters survived him.

In 1863, he married Miss Mary Caroline Hassler, daughter of Dr. Hassler, of the U. S. Navy, who lost his life in the wreck of the steamer *Atlantic*. Her grandfather was Ferdinand Rudolph Hassler, founder and first superintendent of the U. S. Coast Survey.

A widow, three daughters and eleven grandchildren survive him. An only son died in infancy. After a protracted and very painful illness, Professor Newcomb passed away July 13, 1909, leaving as a heritage to his family and his country, the memory of

> One of those few immortal names
> That were not born to die.

I now have the pleasure of inviting your attention to this portrait, by C. H. L. Macdonald, in which you will recognize the features of our late vice-president. Its presence here is due to the liberality of a number of our fellow members who believe that in thus honoring the memory of Professor Newcomb they are at the same time honoring themselves.

Mr. chairman, in behalf of the committee having this matter in charge, I beg to present this portrait to the Philosophical Society, not doubting that it will be considered worthy of a place beside those of Franklin and Rittenhouse and of these other distinguished men who, in various activities, have contributed so greatly to the glory of this society and of the nation.

C. L. Doolittle.

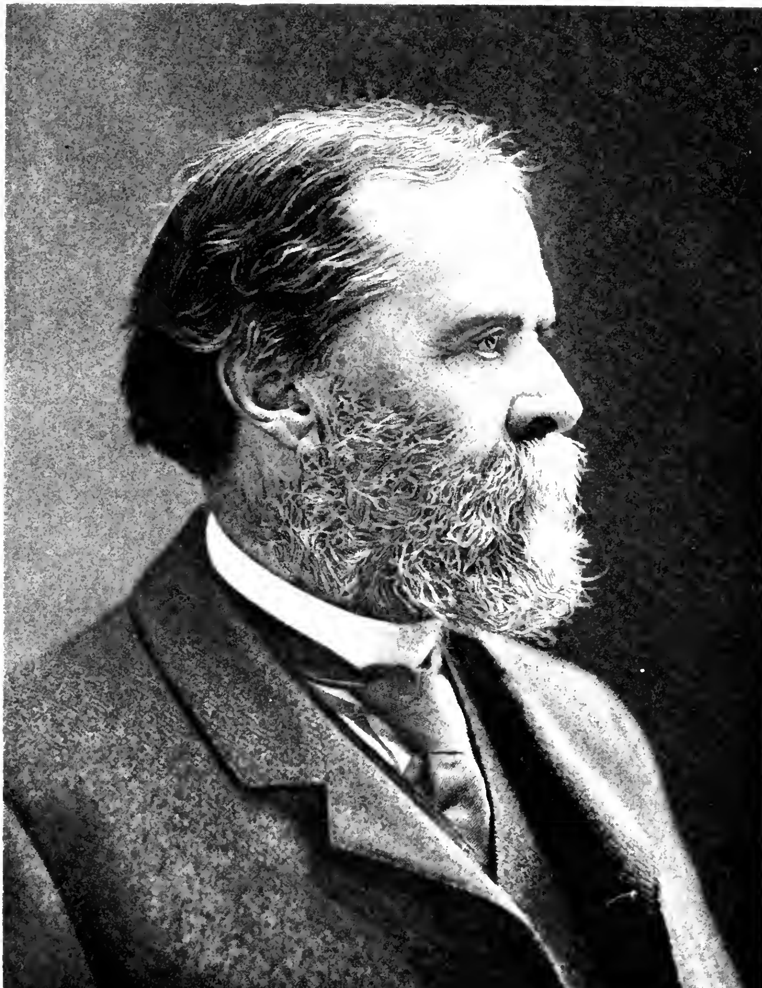
Vice-President Pickering, in receiving the portrait, spoke substantially as follows:

It is difficult to add to the statement just made, and to those of others, any new facts regarding the life of Simon Newcomb. I could only tell you of personal recollections which would not be in place here. Our friendship, extending over forty years, was enlivened by many differences of opinion, but never marred by hard words or unkind feelings. It was never safe for anyone to make an absolute statement to Newcomb unless they were prepared to defend it by established facts.

A striking characteristic of the work of Newcomb was its versatility. Both astronomers and mathematicians regarded him as a leader, while his contributions to philosophy, to political economy and to other sciences were numerous and valuable. It was curious to see how, after devoting a life to the older astronomy, he became deeply interested in astrophysics, at an age when many men cease to do useful work.

The excellent portrait before you is especially welcome, since it is the gift of many, not of a few, of his admirers. As it hangs on the walls of this room it should serve as a model to us all. What happier lot can be asked for a man who, after retirement for age from the service of the United States, could continue his work with the greatest vigor, could live to see the greater part of it completed, and who retained his intellectual powers to the end?

In the name of the American Philosophical Society, I accept this gift to it, and tender the thanks of the members to the donors.



Wolcott Gibbs

OLIVER WOLCOTT GIBBS, 1822–1908.[1]

(*Read May 20, 1910.*)

The father of Wolcott Gibbs, Col. George Gibbs, was a man of some wealth. Possessed of much talent, and of much culture, brilliant in conversation, of polished manners, and with a wide experience of men and life, he was one of the marked men of his day. His beautiful place at the northwest angle of Long Island, with its front on East River at one of its most picturesque points, was one of the landmarks of the river. This large mansion at Sunswick Farms, near Hellgate Ferry, was the seat of an elegant hospitality somewhat rare in this country at the time in question: within was a fine library and a collection of minerals, for Col. Gibbs was an enthusiastic mineralogist. He was for many years a vice-president of a geological society which met at New Haven, and his donations of valuable specimens to that as well as to other mineralogical societies were frequent. During travel in Europe, he purchased a large collection of minerals, which, in the years from 1810 to 1812, was arranged at New Haven. The use of this collection was freely given to Yale College up to the year 1825, when the college purchased it. Once meeting the elder Silliman in the steamer *Fulton*, on Long Island Sound, he suggested to the latter the establishment of the *American Journal of Science*, and urged it with a zeal which was successful. To the first and second volumes of the new journal, he contributed four notes and brief papers. The mineral Gibbsite was named in his honor, and his name is preserved in Rafinesque's curious "Manual of the Grape, etc." as that of a public benefactor who had, before 1825, established one of the earliest vineyards in the attempt to introduce European wine-grapes into this country; and it appears also in lists of agricultural competitions.

[1] The writer, not now having convenient access to a chemical library, has depended for facts upon Clarke's "Wolcott Gibbs Memorial Lecture" to an unusually large extent.

Laura (Wolcott) Gibbs, the mother of Wolcott Gibbs, was a daughter of Oliver Wolcott, Secretary of the Treasury from 1795 to 1800, under Washington and Adams; afterwards justice of the United States Circuit Court, and, for the last eleven years of his life, governor of Connecticut. Her grandfather, Oliver Wolcott was brigadier general of Connecticut militia, member of the Continental Congress, and a signer of the Declaration of Independence, and lieutenant governor and governor of Connecticut. Her great-grandfather, Roger Wolcott, was major general of the army which captured Louisberg in 1745, being second in command at the siege, and was afterwards governor of the colony of Connecticut.

An older brother of Wolcott Gibbs was named George Gibbs. He early acquired a taste for natural history, so that, before he was twenty, he had made and mounted a collection of birds. After some years of travel, he studied law, and opened an office for the practice of the law in New York. He was for many years the librarian of the New York Historical Society; in 1846 he published "Memoirs of the Administrations of Washington and John Adams, edited from the papers of Oliver Wolcott, Secretary of the Treasury." He was appointed collector for Astoria in 1854. Residence here naturally gave him an opportunity for geological and for ethnologic and linguistic studies; in 1857, he accompanied an exploring expedition as botanist and geologist. Later, he served on a boundary commission. He edited, for the Smithsonian Institution, a large collection of documents relating to the ethnology and philology of certain Indian tribes.

Wolcott Gibbs was born in New York City, February 21, 1822. His parents gave him the name Oliver Wolcott, but he dropped the first name on entering adult life. His early days were spent mostly at Sunswick Farms. At the age of seven years, he was sent to a private school at Boston, where he was in the care of a maiden aunt. Some part of his vacations were spent at Newport, R. I., in the house of the distinguished Unitarian divine, William Ellery Channing. When he was about eleven years in age, his father died; his mother survived her husband by more than thirty-five years, and her strong character, and the great abilities which she had inherited from the remarkable Connecticut family from which she was descended, furnished the lad with the guidance and the home influences suitable for his healthy development. At the age of twelve years, he returned to New York and prepared for college. In three years, he entered the freshman class of Columbia College, and was graduated in 1841, at the age of nineteen years. In the year before his graduation, he published the description of a new form of galvanic battery, in which carbon was used for the inactive plate; the young undergraduate publishing the discovery in the same year as Cooper and Schoenbein. The impulse towards such studies is to be found in the early life on his father's estate and in a father's example and other influence; but from the teaching of Renwick, then professor of physics and chemistry at Columbia College, a student like young Gibbs doubtless obtained much which was of value.

In the class of 1841, in which Wolcott Gibbs was graduated, there were at some time forty-nine men, of whom thirty-one took their bachelor's degree. Among these thirty-one, the most distinguished, after Wolcott Gibbs were: Duffield, senator for the Third Senatorial District of Michigan, brigadier general, United States Volunteers, 1863–1864, and superintendent of the United States Coast and Geodetic Survey, 1894–1898; and Emott, mayor of Poughkeepsie, justice of the Superior Court of New York, 1855–1863, and the judge of the Court of Appeals till his death in 1884. Among those who did not take the final degree was William H. Vanderbilt, who died in 1885.

After graduation, Wolcott Gibbs served for some months as assistant to Robert Hare, the inventor of the compound blowpipe, who was professor of chemistry in the Medical School of the University of Pennsylvania. After obtaining here an experience certain to be useful in fitting him for such a professorship, he entered the College of Physicians and Surgeons in New York, and received the degree of doctor of medicine in 1845. There is not much reason to suppose that Gibbs ever expected to practice medicine, for his next step shows that the study of chemistry had become the main purpose of his life. He went to Germany to secure such instruction in chemistry as was not then organized in this country. He first spent some months with Rammelsberg in Berlin, then studied for a year under Heinrich Rose, and then for some months under Liebig at Giessen. Lectures by Laurent, Dumas and Regnault marked the close of his days of travel and study, and in 1848, he returned to America. Among all these great teachers, it was Rose, whom he greatly admired, who seems most to have put his impress on Gibbs, and it may well be that from Rose came the preponderant inclination towards analytical and inorganic chemistry. But the young student who put himself under the instruction of the great leaders of organic chemistry, and of inorganic chemistry, and of theoretical chemistry and of physical chemistry, had already qualities from which were easily developed the breadth of view and of interest in very various kinds of research which always marked Gibbs.

He was soon appointed professor of chemistry in the New York Free Academy, now the College of the City of New York. The teaching required was elementary, and his activities accordingly overflowed in various channels. In the year 1851 he became associate editor of Silliman's *American Journal of Science,* to the establishment of which his father had contributed. To the succeeding forty-five volumes of this journal, Gibbs contributed 472 pages containing abstracts of 605 investigations on chemical and physical matters which had been published in Europe. The careful selection and the clear and accurate reports of these papers were a great service to American science. In 1852, Gibbs discovered a salt of xanthocobalt, a new cobaltamine; which led to important work, published in 1857, in collaboration with Genth. In 1861, he published the first of his papers on the analytical chemistry of the platinum metals. The considerable amount of the works, and especially the masterly ability shown in the paper on the cobalt bases, put Gibbs easily in the front rank of American chemists.

In 1863, Gibbs was appointed Rumford Professor of the Application of Science to the Useful Arts. He was expected to lecture on heat and light, and also to take charge of the chemical laboratory of the Lawrence Scientific School. As this was a position in which little elementary instruction was required, the opportunity for usefulness to his students and to science generally were great. Association with such men as Louis Agassiz, the zoölogist, and Asa Gray, the botanist, and Jeffries Wyman, the comparative anatomist, and Benjamin Peirce, the mathematician, and Josiah Parsons Cooke, the chemist, together with their equals in other fields than scientific, made the position altogether delightful.

Dr. Gibbs remained in charge of the chemical laboratory of the Lawrence Scientific School for eight years. During this time his researches were naturally in great part directed to analytical methods, for he was training men who were to become chemists, some of whom were to gain a livelihood by being analytical chemists. The number of students in the laboratory was not large, some of these were qualified to assist Gibbs in his experimental researches. There was an assistant to take the burden of much routine work, and lectures on thermodynamics cost but little effort. The position was accordingly specially advantageous for one who would devote himself to chemical research.

Some one of the many distinguished chemists who were pupils of Gibbs in the Lawrence Scientific School could have spoken from personal knowledge of him as a teacher; but the choice of your president fell upon me, and it would only be some serious disability which would justify any American chemist in declining to voice the honor in which all of them hold Wolcott Gibbs. Since some expression of opinion and of feeling from those who came in close personal contact with him ought not to be omitted, it is fitting that the words in which one of the most distinguished of his pupils should be cited here. Clarke, chief chemist of the United States Geological Survey says:

Most of the students had already gained some elementary knowledge of chemistry; their work began with the usual practice in analytical methods and chemical manipulations, and as the men showed capacity they were admitted to the confidence of their master and aided him in his investigations. This procedure may seem commonplace enough today, but in the years of which I speak it was new to American institutions and was looked upon doubtfully by some. . . . The real examinations under Gibbs were daily interviews, when he visited each student at his laboratory table and questioned him about his work. This, together with the reported analyses, gave the teacher a clear conception of the true standing of each man. The fewness of the pupils was a distinct advantage, for all worked together in one room, beginners and research students often side by side. The result was that they learned much from one another, and there were many discussions among them over the chemical problems of the day. The men were taught to think for themselves, laying thereby a foundation for professional success which was pretty substantial. The course of instruction had no definite term of years prescribed for it, and graduation came whenever the individual had done the required amount of work, and submitted an acceptable original thesis. The final examination was usually oral, each man alone with his teacher, and was conducted in an easy conversational way which tended to establish the confidence of the candidate from the very beginning. In my own case, I remember that the questions covered a fairly broad range of chemical topics, and at the end of it, Dr. Gibbs drew me into a sort of discussion or argument with him over the then modern doctrine of valence. I now see that his purpose was not merely to ascertain what I had read on the subject, but what I really thought about it, if indeed I was entitled to think at all. Gibbs invariably treated his students, not as so many vessels into which knowledge was to be poured, but as reasonable beings, with definite purposes, to whom his help must be given. The research work in which the advanced students shared, and for which they received public credit, served to teach them that chemistry was a living and growing subject, and to train them in the art of solving unsolved problems.

What was there at all unusual in his teaching? Nothing, perhaps, from a modern point of view, but much that was new in America in the middle sixties. It was Gibbs's merit that he, more than any other one man, introduced into the United States the German conception of research as a means of chemical instruction, a conception which is now taken as a matter of course without thought of its origin. Gibbs worked with small resources and with no help from the outside; he was a reformer who never preached reform; his students rarely suspected that they were doing anything out of the ordinary; but they had the utmost confidence in their master, and took it for granted that his methods were sound. . . . The success of his students is perhaps the best monument to his memory.

In 1871, the chemical instruction in the Lawrence Scientific School was consolidated with that in Harvard College. This elicited vigorous protests from leading scientific journals, from scientific men, and from Professor Gibbs himself, but to no purpose. His duties henceforth were limited to his lectures on the spectroscope and on thermodynamics. He no longer had a laboratory in which to work, nor students, some of whom could assist in his researches. Contact with a great teacher was no longer a source of inspiration to students ready to profit by it. He himself had more time for private research, but fewer facilities for it. Fortunately he had means enabling him to establish a small private laboratory, and to employ an assistant. It was in this laboratory, first at Cambridge and afterwards at Newport, that he carried out that one of all his researches which was the most important and elaborate and extensive.

The equipment of this laboratory was modest and very suitable for the work done in it. An ordinary kitchen stove served conveniently for the drying of precipitates in its oven, and the ignition of crucibles buried in the burning coal, and the heating or evaporation of liquids on its top. These processes were going on for much of the time during many years. There are kinds of work for which refined and elaborate apparatus barely accomplishes what is needful; if Gibbs had been occupied with work of this character, he would have provided it. There are not many things remaining which can be done with the iron spoon and gun-barrel which so well served Priestley; it happened that Gibbs, in his great work on the complex inorganic acids, was busy in a region very different from the determination of residuals which demands the utmost instrumental refinement, and his keen good sense suited the equipment to its purpose.

After the closing of the chemical laboratory of the Lawrence Scientific School, Dr. Gibbs lectured to small classes on the spectroscope and on thermodynamics till 1887, when he retired as professor emeritus. After this, he lived at Newport, where he had before spent summer vacations. His private laboratory was reëstablished here, and he continued his researches a long as health and strength sufficed. Some of his hours of recreation were spent in his flower garden, and his roses were much admired. He passed away on the ninth of December, 1908, at the age of eighty-six years nine and a half months. His wife, whose name was Josephine Mauran, died several years earlier, leaving no children.

It remains to speak briefly of Gibbs's scientific work. Fitly to describe it, even to an audience of chemists, would take more time than our traditions in the American Philosophical Society allow, so it is the esteem in which the work is held rather than the details of the work which will occupy us.

The infinite variety of careers for which variety of native powers and accidental advantages and opportunity open before us commonly involves at the beginning of active life a period of effort which is more or less tentative. Gibbs was no exception to this rule, but the period of tentative effort was brief, including, perhaps, only the first half dozen of his scientific papers. It is worthy of note that these show a somewhat wide range of interest and capacity.

His first scientific paper, published while in the junior class of Columbia College, was entitled "Description of a New Form of Magneto-Electric Machine, and an Account of a Carbon Battery of Considerable Energy." While a student in the medical college, he published a discussion of the theory of compund salt radicals. While a student in Germany, he published several mineral analyses. Just before becoming professor of chemistry in the New York City Free Academy, he showed that color changes produced by heat are in the direction of the less refrangible end of the spectrum. In 1852, he published the first of his papers on analytical methods. In 1853, he prepared an arsenical derivative of valeric acid. None of this work was of commanding importance, but it was of good quality, and it well illustrates Gibbs's knowledge of, and power of interesting himself in, somewhat widely varied departments of his chosen science.

The work to be mentioned next was worthy of the powers of any chemist in the world, and gave to him an established reputation and an honorable place among the leaders of chemistry. During the years to which allusion has just been made, he had been at work on a new cobaltamine, and, in 1856, he published a great research which commanded general recognition of the abilities of a master. A salt of the first known of the cobaltamines had been prepared by Gmelin in the year in which Gibbs was born, but it was some time before its true nature was understood. In 1847, Genth, then a student in Germany, prepared other related compounds and was the first rightly to understand their composition. Two French chemists working independently came to conclusions like those of Genth; he had established the composition of the two cobaltamines now called luteocobalt and roseocobalt. Gibbs in 1852 established the composition of a third and new cobaltamine now called xanthocobalt, and the two American chemists naturally were led to work in concert. In 1856 they published their celebrated memoir, describing no less than 35 salts of these three cobaltamines, together with some of those of a fourth called purpureocobalt. They gave adequate analyses of these salts, together with crystallographic measurements, by J. D. Dana, of eleven of them. Purpureocobalt was then first distinguished from roseocobalt, though the conception then attained of the precise nature of the difference was not final. Gibbs attempted a discussion of the constitution of these bases, but prematurely. Adequate theories of structure were yet to be developed; when such development came, the facts established by Gibbs and Genth formed a solid foundation for the brilliant superstructure. Eleven years after this paper, Gibbs himself made an attempt to establish the theory of the structure of these amines. The conception utilized by Gibbs was also utilized two years later by one of the leaders in the establishment of the doctrines of structural chemistry; these views have now given way to other views which harmonize better with a wider range of facts; but the discussion of tentative possible explanations of facts is one of the steps by which the truth is finally discovered. Gibbs utilized his hypothesis of the structure of the cobaltamines in further papers in 1876 and 1877, in which he described many more salts, some of these being salts of a fifth amine called croceocobalt. The whole was a great piece of valuable and most fruitful work, carried on with extraordinary ability and success.

The work on the platinum metals, published from 1861 to 1864, related mainly to analytical methods. In 1871, he published a brief note on a remarkable compound of iridium, and in 1881, he described a new basic compound of osmium. But these researches had to be discontinued for lack of suitable facilities.

Gibbs was a man fertile in varied suggestions; in many a conversation he would lavish freely material almost sufficient for the working capital of a teacher who taught largely by inspiring and directing research. It was natural, therefore, that besides the great work on the cobalt bases and another greater work yet to be mentioned, he should publish many papers of less extent. From the hand of such a master, these papers are valuable and important. Many relate to analytical methods; many relate to the analytical methods of the platinum metals, or to new compounds of the platinum metals. These papers are numerous; most of them are too technical for presentation here, but one of them is especially worthy to be mentioned and to be mentioned in this city. The electrolytic determination of copper was first published from the laboratory of the Lawrence Scientific School and the whole field of electro-chemical analysis, nowhere cultivated more successfully than in Philadelphia, was opened by Gibbs. In collaboration with E. R. Taylor, he devised filters composed of insoluble powders like glass and sand; then Munroe, another student, invented porous porcelain cones for filtration, and Gooch, an assistant of Gibbs, invented perforated crucibles with filters composed of layers of asbestos fibre made into a felt, in place of which we now often use spongy platinum. So the use of filters which do not change weight on ignition and which do not require us to heat reducible substances in contact with carbon, is due to Gibbs.

The most remarkable work of Gibbs is contained in his series of researches on the complex inorganic acids, whose publication began in 1877 and was continued till after 1890. Some complex inorganic acids were already well known; for instance, phosphomolybdates are in common use. But, in his first paper, Gibbs showed that, far from being exceptional compounds, they were members of an extensive class; that the formation of complex acids was characteristic of tungstates and molybdates to an extraordinary degree; and that the possible number of such compounds was vast. After this preliminary announcement, Gibbs determined the true composition of the sodium tungstates. Then he prepared phosphotungstates and phosphomolybdates, and similar compounds with arsenic in place of phosphorus. From this beginning, the work developed in directions which cannot be well described except to an audience of chemists; taking in all the degrees of oxidation of phosphorus, with all the known variations in the amount of replaceable hydrogen in its numerous acids. Then were described complex acids with not two but three acid radicals in the complex, like that of the stannophosphotungstates. Next, other elements like platinum, selenium, tellurium, cerium, uranium, were drawn into like complex acids. On salt, phosphovanadiovanadicotungstate of barium, $60WO_3$, $3P_2O_5$, V_2O_5, VO_2, $18BaO$, $150H_2O$, had an atomic weight of 20,066 and a complication the interpretation of which seems almost incredibly difficult. Salts of over fifty such complex acids were described, and all this immense volume of work was accomplished in a small private laboratory with only one assistant.

In his address as president of the American Association for the Advancement of Science in 1898, Gibbs spoke on the complex inorganic acids. That the views then presented were final was not to be expected. Even to establish a simple matter like the composition of water, expressed in three characters, required, if we may agree with Kopp, no less than the joint efforts of three men. In the case which we have to consider, one man was working alone in a wilderness to be compared, both for extent and for complexity, not to a simple problem in inorganic chemistry, but to some great section of organic chemistry. But though working alone, he mapped out the wilderness so that he who will may now survey it at his ease. We are now in possession of methods which would have been of great service in this reconnoissance, had they been developed early enough. The cryoscopic or the ebullioscopic method of determining molecular weights would have helped to ascertain whether a given body was a compound of a basic radical with a single complex acid radical or with two simple acid radicals. Electrical methods might have assisted in ascertaining the composition of a complex ion. But these methods by means of which physical chemistry has made so great conquests were not ready to be used when Gibbs worked, and, accordingly, his survey does not include some facts and some conclusions which they might establish. So Newton had no spectroscope to use. But that the work of Gibbs was less valuable for this reason, few chemists would be willing to admit. He put before us a great and difficult problem and he did, towards its solution, more than almost any other man could have done.

It has been indicated that Gibbs had a wide range of insight and interest. He did no considerable work in organic chemistry, but he did not entirely neglect it. Lecturing as he did upon heat and light, and writing as he did for twenty-three years the abstracts of physical researches which appeared in the *American Journal of Science*, he had a knowledge of and an interest in, physical subjects which was expressed in several papers on optical matters. Serious work on atomic weights was carried on in his laboratory and under his direction, where three important determinations were made; he also devised a method of determining some atomic weights which had before been rather difficult to obtain. He published this method after he was seventy years of age, and the method has since been applied by others with good success. Processes requiring refinement and consummate accuracy were attractive to him, as well as some in which refinement and final accuracy are to be attained by some future generation. In an important study of the physiological effects of isomeric organic compounds on animals, he utilized his early medical training.

All Gibbs's activities were actuated by very high ideals. He was little known by the public at large, even by the best part of the public, but was greatly honored among scientific men. He was one of the founders and original members of the National Academy of Sciences and for some years was its president. He was president of the American Association for the Advancement of Science in 1897–1898. He was an honorary member of the three great chemical societies of the world and of the Prussian Academy of Science, and several universities gave him honorary degrees. He was a devoted scholar, glad to give his best efforts to the world, highly valuing unsought approval, and never seeking other reward. He was during the civil war, for several years an earnest and active member of the Sanitary Commission. It was he who first suggested that the ideas on which the Sanitary Commission was founded ought to take the form of a club, and it was at a meeting in his house that the Union League Club was established.

In the words of Clarke:

Gibbs was a man of striking personality, tall, erect and dignified. As with most men of positive character, he had strong likes and dislikes, but the latter never assumed unworthy form. To his friends he was warmly devoted, and always ready to help them in their work with manifold suggestions. His breadth of mind is indicated by the range of his researches, and his liberality by the way in which he encouraged his students to develop his ideas. More than one important investigation was based upon hints received from him, and was carried out under his supervision, to appear later under another name. Gibbs never absorbed the credit due even in part to others, nor failed to recognize the merits of his assistants in the fullest way. Had he been more selfish, his list of publications would have been lengthened; but his sense of justice was most keen, and therefore he held the esteem and confidence of his co-workers. No man, not even among his opponents, for such there were, could ever accuse him of unfairness. He deserved all honor, and his name will long live in the history of that science to which his life was given.

EDWARD W. MORLEY.

MINUTES.

MINUTES.

Stated Meeting, January 7, 1910.

William W. Keen, M.D., LL.D., President, in the Chair.

A communication was received from the Committee of Organization of the 3^me^ Congrès International de Botanique announcing that the Congress will be held in Brussels from the fourteenth to the twenty-second of May and inviting the Society to be represented thereat.

The decease was announced of:

Dr. Enrico Hillyer Giglioli, at Florence, on December 16, 1909, æt. 64.

Mr. Israel W. Morris, at Philadelphia, on December 18, 1909, æt. 76.

Dr. Charles Benjamin Dudley, at Altoona, Pa., on December 21, 1909, æt. 67.

Prof. J. C. Branner read a paper on "The Geology of the Black Diamond Regions of Bahia, Brazil."

The judges of the annual election of officers and councillors held on this day, between the hours of two and five in the afternoon, reported that the following named persons were elected, according to the laws, regulations and ordinances of the Society, to be the officers for the ensuing year.

President:

William W. Keen.

Vice-Presidents:

William B. Scott, Albert A. Michelson, Edward C. Pickering.

Secretaries:

I. Minis Hays, James W. Holland,
Arthur W. Goodspeed, Amos P. Brown.

Curators:

Charles L. Doolittle, William P. Wilson, Leslie W. Miller.

Treasurer:

Henry La Barre Jayne.

Councillors:

(*To serve for three years.*)

Edward L. Nichols, Ernest W. Brown,
Samuel Dickson, Morris Jastrow, Jr.

Stated Meeting, January 21, 1910.

William W. Keen, M.D., LL.D., President, in the Chair.

Dr. W. W. Keen read a paper on "Modern Antiseptic Method and the Rôle of Experiment in its Discovery and Development."

Stated Meeting, February 4, 1910.

William W. Keen, M.D., LL.D., President, in the Chair.

The decease was announced of Hon. Edward Patterson, at New York, January 28, 1910, æt. 70.

The following papers were read:

"Notes on some Pseudomorphs, Petrifactions and Alterations," by Austin F. Rogers, Ph.D. (Introduced by Prof. J. C. Branner.)

"The Influence of Mental and Muscular Work on Nutritive Processes," by Prof. Francis B. Benedict. (Introduced by Dr. W. W. Keen.)

Stated Meeting, February 18, 1910.

William W. Keen, M.D., LL.D., President, in the Chair.

The decease was announced of Prof. Dr. Friedrich Kohlrausch, æt. 70.

Mr. J. Vipond Davies (introduced by Dr. W. W. Keen) read a paper on "The Tunnel Construction of the Hudson and Manhattan Railroad Company."

Stated Meeting, March 4, 1910.

William W. Keen, M.D., LL.D., President, in the Chair.

Dr. Samuel W. Stratton read a paper on "The Work of the National Bureau of Standards."

Stated Meeting, March 18, 1910.

William W. Keen, M.D., LL.D., President, in the Chair.

The decease was announced of Dr. Wharton Sinkler, at Philadelphia, on March 16, 1910, æt. 65.

Dr. Jay F. Schamburg (introduced by Dr. W. W. Keen) read a paper on "Vaccination and the Ravages of Small-pox among Royal and Noble Families."

Stated Meeting, April 1, 1910.

William W. Keen, M.D., LL.D., President, in the Chair.

The decease was announced of Prof. Alexander Agassiz, at sea, on March 28, 1910, æt. 74.

Mr. David Fairchild read a paper entitled "A New World for Exploration."

General Meeting, April 21, 22 and 23, 1910.

Thursday, April 21. Opening Session—2 o'clock.

William W. Keen, M.D., LL.D., President, in the Chair.

Invitations were received:

From the XI^e Congrès Géologique International, to be represented at the Congress to be held at Stockholm on August 18–25, 1910.

From the President of the Argentine Scientific Society to be represented at the International American Scientific Congress to be held at Buenos Aires from July 10–25, 1910.

The decease was announced of:

Prof. George L. Vose, at Brunswick, Me., on March 30, 1910, æt. 79.

Prof. Robert Parr Whitfield, at New York, on April 6, 1910, æt. 82.

The following papers were read:

"The Great Japanese Embassy of 1860; The Forgotten Chapter in the History of International Amity and Commerce," by Patterson DuBois, of Philadelphia.

"The Government of the United States in Theory and Practice," by C. Stuart Patterson, of Philadelphia.

"Early Greek Theories of Sound and Consonance," by Wm. Romaine Newbold, Professor of Philosophy in the University of Pennsylvania.

"New Fields of German-American Research," by M. D. Larned, Director of the Institution of German-American Research, University of Pennsylvania.

"The Real Meaning of the Controversy concerning Pragmatism," by Albert Schinz, Associate Professor of French Literature in Bryn Mawr College, Pa. (Introduced by Mr. Harrison S. Morris.)

"Magical Observances in the Hindu Epic," by E. Washburn Hopkins, Professor of Sanskrit in Yale University, New Haven, Conn.

"The Bearded Venus," by Morris Jastrow, Jr., Professor of Semitic Languages in the University of Pennsylvania.

"Roman Mysticism in the Fourteenth Century," by Kuno Francke, Curator of the Germanic Museum, Harvard University, Cambridge.

"A German Monk of the Eleventh Century," by A. C. Howland, Assistant Professor of Mediæval History in the University of Pennsylvania. (Introduced by Professor Edward P. Cheyney.)

"The New Shakespeare Discoveries," by Felix E. Schelling, Professor of English Literature in the University of Pennsylvania.

Friday Morning, April 22. Executive Session—10 o'clock.

WILLIAM W. KEEN, M.D., LL.D., President, in the Chair.

Mr. Edwin Swift Balch, on behalf of the Committee on Antarctic Exploration, presented the following report:

At the annual meeting of the American Philosophical Society, held on April 22, 1909, resolutions were passed, requesting the coöperation of the scientific and geographical societies of the United States to urge on the government to make sufficient appropriations to send a vessel under the direction of the Secretary of the Navy to reëxplore Wilkes Land.

These resolutions were embodied in a circular letter under date of May 20, 1909, sent to a number of scientific organizations, and they elicited favorable response by the following societies:

The American Academy of Arts and Sciences, Boston.
The American Geographical Society, New York.
The California Academy of Sciences, San Francisco.
The New York Academy of Sciences.
The Franklin Institute, Philadelphia.
The Geographical Society of Philadelphia.
The American Museum of Natural History, New York.
The Buffalo Society of Natural Sciences.
The Geological Society of America.
The Association of American Geographers.
The American Alpine Club.

At the meeting of the American Philosophical Society held on November 5, 1909, a Committee on Antarctic Exploration was appointed consisting of the following:

Edwin Swift Balch, *Chairman.*
Henry Grier Bryant.
Dr. Hermon C. Bumpus.
Professor William Morris Davis.
Rear Admiral George W. Melville, U.S.N.

Professor Henry Fairfield Osborn.

Dr. Charles D. Walcott.

The Committee met on Friday, November 19, 1909. Dr. Bumpus suggested that the "Albatross" might be assigned to work in the Antarctic by the Bureau of Fisheries. Admiral Melville urged trying to get either the "Bear" or the "Thetis" detailed for this service. The Committee finally decided to approach the Secretary of the Navy directly.

Dr. Walcott arranged for a meeting of the Committee with Admiral Pillsbury, representing the Secretary of the Navy, and at this meeting, on December 20, 1909, Admiral Pillsbury advised writing a letter, explaining the matter fully, to the Secretary of the Navy. This was done, and on January 4, 1910, this letter, signed by all the members of the Committee, was sent to the Secretary of the Navy. On January 15 an answer was received from the Secretary of the Navy, stating that he would bring the matter to the attention of the President at an early date.

Admiral Pillsbury, at the direction of the Secretary of the Navy, prepared estimates as to the expense of outfitting an expedition to Wilkes Land. He made inquiries on the Pacific coast and in Newfoundland about whaling ships, but found none suitable for the purpose. He also got estimates for building a ship.

About this time it was reported that the "Roosevelt" was for sale; and from unofficial information received, it seemed possible that her owners might be willing to loan her or rent her. The chairman, then, on January 24, 1910, sent a letter to her owners, the Peary Arctic Club, asking whether the club would be willing to loan her to the Government for an expedition to Wilkes Land. No direct answer to this letter, however, was ever received.

The American Geographical Society cordially coöperated to induce the Government to send this expedition to Wilkes Land. Messrs. Archer M. Huntington, Chandler Robbins, Hamilton Fish Kean and Cyrus C. Adams, especially, urged the matter on the Secretary of the Navy.

It was finally decided by the President that it was not advisable at this time to ask Congress for an appropriation for this purpose.

The main object of the resolutions of the American Philosophical Society, therefore, has not been accomplished as yet. Nevertheless the resolutions have borne fruit.

The National Geographic Society and the Peary Arctic Club are preparing jointly an expedition to the Antarctic. The National Geographic Society did not acquiesce in the request to urge the Government to send an expedition to Wilkes Land. But General Greely informed your chairman last summer that, after the resolutions of the American Philosophical Society were received, he brought the matter up at a Board meeting; that it was agreed that it was of national importance; that the sentiment was against approaching Congress for an appropriation; that it was decided to take steps to raise a national fund; and that General Greely himself was appointed chairman of the committee for the purpose. Nothing further appears to have been done after this until the Peary Arctic Club suggested a joint expedition. While this latter expedition therefore is entirely independent of the one suggested in the resolutions of April, 1909, and has been evolved on different lines; nevertheless it is all an expression of a national desire and a part of the same movement for Antarctic exploration, and your Committee wishes heartily for its complete success.

An important direct result of the movement has been the getting of certain nomenclature about the Antarctic placed on the official charts of the Hydrographic Office. Admiral Pillsbury brought a preliminary drawing of a circumpolar chart of the southern hemisphere to the conference with your Committee on December 20, 1909. The Chairman of the Committee had some correspondence with the hydrographer, Captain A. G. Winterhalter, about this chart, and suggested placing the names "West Antarctica," "East Antarctica," "Wilkes Land" and "Palmer Land" on it. These suggestions were accepted by the Navy Department, and the names placed on a "Circumpolar Chart of the Southern Hemisphere" published on February 21, 1910.

Perhaps the most important result of the movement, however, is the arousing of interest in America about the Antarctic. Since the return of the expedition led by Lieutenant Wilkes, American interest in the south polar regions has been dormant. Now many scientific men and societies are awake and interested. Let us hope this interest will grow and, perhaps, in the years to come, lead to the gathering of more scientific and geographic data about the still unknown south, and to the sending of an American expedition to Wilkes Land, to explore it and establish beyond question the geographical discoveries reported by Lieutenant Wilkes, in command of the U. S. Exploring Expedition of 1838–1843. It would seem wise to continue the committee with instructions to prepare a renewed application to the Government and continue the dissemination of interest in this great undertaking.

The report was accepted and the Committee continued.

Morning Session, 10.05 o'clock.

William W. Keen, M.D., LL.D., President, in the Chair.

The following papers were read:

"Physical Notes on Meteor Crater, Arizona," by William F. Magie, Professor of Physics at Princeton University.

"The Conversion of the Energy of Carbon into Electrical Energy on Solution in Iron," by Paul R. Heyl, Assistant Professor of Physics in the Central High School, Philadelphia. (Introduced by Prof. Harry F. Keller.)

"The One Fluid Theory of Electricity," by Francis E. Nipher, Professor of Physics in Washington University, St. Louis.

"The Past and Present Status of the Ether," by Arthur Gordon Webster, Professor of Physics at Clark University, Worcester.

"The Ether Drift," by Augustus Trowbridge, Professor of Physics at Princeton University.

"The Effects of Temperature on Fluorescence and Phosphorescence," by E. L. Nichols, Professor of Physics at Cornell University, Ithaca.

"Infra-red and Ultra-violet Landscapes."

"New Optical Properties of Mercury Vapor."

"Newton's Rings as Zone-plates," by Robert Williams Wood, Professor of Experimental Physics at Johns Hopkins University, Baltimore.

"New Surgery of the Viscera of the Chest," by Alexis Carrel, Associate Member of the Rockefeller Institute for Medical Research, New York.

"Experimental Poliomyelitis in Monkeys," by Simon Flexner, Director of Laboratories of Rockefeller Institute for Medical Research, New York.

"Description of the Brain of an Eminent Chemist and Geologist (a member of this society)—a note Concerning the Size of the Callosum in Notable Persons," by E. A. Spitzka, Professor of General Anatomy in Jefferson Medical College, Philadelphia.

"A Brain of about One Half the Average Weight from an Intelligent White Man," by Burt G. Wilder, Professor of Neurology and Vertebrate Zoölogy in Cornell University, Ithaca, N. Y.

Afternoon Session—2 o'clock.

William B. Scott, Ph.D., LL.D., Vice-President, in the Chair.

The following papers were read:

"Characteristics of Existing Continental Glaciers," by William H. Hobbs, Professor of Geology in University of Michigan, Ann Arbor.

"Dermal Bones of Paramylodon from the Asphaltum Deposits of Rancho la Brea, near Los Angeles, Cal."

"The Restored Skeleton of *Leptauchenia decora*," by William J. Sinclair, Instructor in Geology in Princeton University. (Introduced by Professor W. B. Scott.)

"Correlation of the Pleistocene of the New and Old Worlds," by Henry Fairfield Osborn, President of the American Museum of Natural History, New York.

"The Primates of the Old and the New Worlds, together with Man," by Giuseppe Sergi, Professor of Anthropology in the University of Rome, Italy.

"A Note on Antarctic Geology."

"The Italian Riviera—a Study in Geographical Description," by William Morris Davis, Professor of Geology in Harvard University, Cambridge.

"Some Recent Results in Connection with the Absorption Spectra of Solutions," by Harry C. Jones, Professor of Physical Chemistry in Johns Hopkins University. (Introduced by Dr. James W. Holland.)

"The Propagation of Explosions in Mixtures of Petroleum Vapor with Air in Tubes," by Charles E. Munroe, Professor of Chemistry in George Washington University, Washington, D. C.

"What Constitutes a Species in *Agave*," by William Trelease, Director of the Missouri Botanical Garden, St. Louis.

"The Suppresion and Extension of Sporogenous Tissue in *Piper Betel*," by D. S. Johnson, Professor of Botany in Johns Hopkins University, Baltimore. (Introduced by Professor John W. Harshberger.)

"A Method of Using the Microscope," by N. A. Cobb, Crop Technologist in charge of Agricultural Technology, Bureau of Plant Industry, Dept. of Agriculture, Washington. (Introduced by Professor John W. Harshberger.)

"The Use of the Hydrometer in Phytogeographic Work," by John W. Harshberger, Assistant-Professor of Botany in University of Pennsylvania, Philadelphia.

Friday Evening, April 22.—8.15 P. M.

Prof. George E. Hale, F.R.S., Director of the Solar Observatory of the Carnegie Institution, at Pasadena, Cal., gave an illustrated lecture on "The Work of the Mount Wilson Solar Observatory."

Saturday, April 23. Executive Session—9.30 o'clock.

William W. Keen, M.D., LL.D., President, in the Chair.

Candidates for membership were balloted for, and the tellers, Secretary Holland and Dr. Charles Sedgwick Minot, reported the election of the following:

Residents of the United States.

Simeon Eben Baldwin, LL.D., New Haven.
Francis G. Benedict, Ph.D., Boston.
Charles Francis Brush, Ph.D., LL.D., Cleveland, Ohio.
Douglas Houghton Campbell, Ph.D., Palo Alto, Cal.
William Ernest Castle, Ph.D., Belmont, Mass.
George Byron Gordon, Philadelphia.
David Jayne Hill, LL.D., American Embassy, Berlin.
Harry Clary Jones, Ph.D., Baltimore.
Leo Loeb, M.D., Philadelphia.
James McCrea, Ardmore, Pa.
Richard Cockburn Maclaurin, F.R.S., LL.D. (Cantab.), Boston Mass.
Benjamin O. Peirce, Ph.D., Cambridge, Mass.
Harry Fielding Reid, Ph.D., Baltimore.
James Ford Rhodes, LL.D., Boston, Mass.
Owen Willans Richardson, M.A. (Cantab.), D.Sc. (Lond.), Princeton, N. J.

Foreign Residents.

Adolf von Baeyer, Ph.D., M.D., F.R.S., Munich.
Madame S. Curie, Paris.
Sir David Gill, K.C.B., Sc.D., LL.D., F.R.S., London.
Edward Meyer, Ph.D., LL.D., Berlin.
Charles Emile Picard, Paris.

Morning Session—10 o'clock.

Edward C. Pickering, LL.D., F.R.S., Vice-President, in the Chair.

The following papers were read:

"Solar Activity and Terrestrial Magnetic Disturbances" (second communication).

"Magnetic Results of the first Cruise of the 'Carnegie,' 1909–1910," by L. A. Bauer, Director of Department of Terrestrial Magnetism, Carnegie Institution, Washington.

"The New Tide-predicting Machine of the U. S. Coast and Geodetic Survey," by O. H. Tittman, Superintendent of the Survey.

"Spectra of Recent Comets," by Edwin B. Frost, Director of Yerkes Observatory, Williams Bay, Wis.

"On the Distances of Red Stars," by Henry Norris Russell, Assistant-Professor of Astronomy, Princeton University. (Introduced by Professor W. F. Magie.)

"A Standard System of Photographic Stellar Magnitudes," by Edward C. Pickering, Director of Harvard College Observatory, Cambridge.

"The Existence of Planets about the Fixed Stars."

"Results of Recent Researches in Cosmical Evolution," by T. J. J. See, U. S. Naval Observatory, Mare Island, Cal.

"Some Interesting Double Stars," by Eric Doolittle, Assistant-Professor of Astronomy in University of Pennsylvania.

"Radio-action in the Heavenly Bodies."

"Radio-action the Cause of Hale's Anomalous Solar Spectrum," by Monroe B. Snyder, Director of the Philadelphia Observatory.

"Certain Irregularities in the Problem of Several Bodies," by Edgar Odell Lovett, President of the William M. Rice Institute, Houston, Texas.

"Groups Generated by Two Operators, Each of which Transforms the Square of the Other into a Power of Itself," by George A. Miller, Professor of Mathematics in the University of Illinois. (Introduced by Professor C. L. Doolittle.)

"Obituary Notice of Simon Newcomb, F.R.S., LL.D., D.C.L., late Vice-President of the American Philosophical Society," by C. L. Doolittle, Director of the Flower Observatory of the University of Pennsylvania.

Presentation of a portrait of the late Vice-President Newcomb. Acceptance on behalf of the Society, by Vice-President Pickering.

Afternoon Session—2 o'clock.

William W. Keen, M.D., LL.D., President, in the Chair.

Dr. Harry C. Jones, a newly-elected member, subscribed the laws and was admitted into the Society.

The following papers were read:

"The Origin of Our Alphabet and the Race of the Phenicians," by Paul Haupt, Professor of Semitic Languages in the Johns Hopkins University.

Symposium on Experimental Evolution:

"Inheritance in Non-sexual and Self-fertilized Organisms," by Herbert S. Jennings, Professor of Experimental Zoölogy in Johns Hopkins University, Baltimore.

"Germinal Analysis through Hybridization," by George H. Shull, Resident Investigator, Station for Experimental Evolution, Carnegie Institution of Washington, Cold Spring Harbor, N. Y. (Introduced by Dr. Charles B. Davenport.)

"New Views about Reversion," by Charles B. Davenport, Director of Station for Experimental Evolution, Carnegie Institution, Cold Spring Harbor, N. Y.

"Experimental Modification of the Germ Plasm," by William L. Tower, Assistant-Professor of Embryology in the University of Chicago. (Introduced by Dr. Henry H. Donaldson.)

Stated Meeting, May 6, 1910.

William W. Keen, M.D., LL.D., President, in the Chair.

Letters accepting membership were read from:

Simeon Eben Baldwin, LL.D., New Haven.
Francis G. Benedict, Ph.D., Boston.
Charles Francis Brush, Ph.D., LL.D., Cleveland, Ohio.
Douglas Houghton Campbell, Ph.D., Palo Alto, Cal.
George Byron Gordon, Philadelphia.
Harry Clary Jones, Ph.D., Baltimore.
Leo Loeb, M.D., Philadelphia.
James McCrea, Ardmore, Pa.
Richard Cockburn Maclaurin, F.R.S., LL.D. (Cantab.), Boston, Mass.
Benjamin O. Peirce, Ph.D., Cambridge, Mass.
Harry Fielding Reid, Ph.D., Baltimore.

James Ford Rhodes, LL.D., Boston, Mass.

Owen Willans Richardson, M.A. (Cantab.) D.Sc. (Lond.), Princeton, N. J.

The decease was announced of Mr. John H. Converse, at Rosemont, Pa., on May 3, 1910, æt. 69.

The following papers were read:

"On the Radio-Active Substances," by Prof. Harry F. Keller.

"Further Notes on Burial Customs, Australia," by R. H. Mathews.

Special Meeting May 20, 1910.

William W. Keen, M.D., LL.D., President, in the Chair.

Mr. James McCrea, a newly-elected member, signed the laws and was admitted into the Society.

Letters accepting membership were received from

Dr. William Ernest Castle.

Dr. Adolf von Baeyer.

Sir David Gill.

Dr. Eduard Meyer.

Prof. Charles Émile Picard.

The decease was announced of Sir William Huggins, K.C.B., F.R.S., at London on May 12, 1910, æt. 86.

Prof. Edward W. Morley read an obituary notice of Prof. Oliver Wolcott Gibbs.

Prof. Robert H. Hough read a paper on the "Principle of Relativity and its Significance."

Stated Meeting, October 7, 1910.

William W. Keen, M.D., LL.D., President, in the Chair.

Letters were received accepting membership from:

Prof. Francis G. Benedict,

Hon. David Jayne Hill, and

Mme. S. Curie.

Inviting the Society to be represented at the

Tenth International Geographical Congress, to be held at Rome in October, 1911.

Centenary of the Königliche Friedrich-Wilhelms Universitet, Berlin, in October, 1910.

First Universal Races Congress, to be held at London, in July, 1911.

Inauguration of Frank LeRend McVey, Ph.D., LL.D., as President of the University of North Dakota, in September, 1910.

The decease was announced of:

Prof. Mathaeus Much, at Vienna, on December 17, 1909, æt. 78.

Prof. William Phipps Blake, at Berkeley, Cal., on May 21, 1910, æt. 84.

Prof. George F. Barker, at Philadelphia, on May 24, 1910, æt. 74.

Mr. Joseph S. Harris, at Philadelphia, on June 2, 1910, æt. 74.

Prof. Giovanni Schiaperelli, at Milan, on July 4, 1910, æt. 75.

Hon. Craig Biddle, at Andalusia, Pa., on July 26, 1910, æt. 87.

Sig. Paolo Mantegazzo, at Spezia, on August 28, 1910, æt. 79.

Prof. Frederick Augustus Genth, at Lansdowne, Pa., on September 2, 1910, æt. 55.

Prof. William A. Lamberton, at Point Pleasant, N. J., on September 8, 1910, æt. 61.

Mr. Richard Wood, at Philadelphia, on September 29, 1910, æt. 76.

The following papers were read:

"On Suicide," by J. Chalmers Da Costa, M.D., discussed by Prof. L. M. Haupt.

"The Origin of Primitive Man in America," by Albert S. Ashmead.

Stated meeting, November 4, 1910.

William W. Keen, M.D., LL.D., President, in the Chair.

An invitation was received for the Society to be represented at the Twenty-fifth Anniversary of Bryn Mawr College, on October 21 and 22, 1910.

The decease was announced of:

Mr. Arthur Erwin Brown, at Philadelphia, on October 29, 1910, æt. 60.

The following papers were read:

"Further Considerations on the Origin of the Zone of Asteroids and on the Capture of Satellites," by Dr. T. J. J. See.

"Modern Physics," by President Ernest Fox Nichols, discussed by Profs. Trowbridge, Snyder, Doolittle and Goodspeed.

Stated meeting, December 2, 1910.

William Berryman Scott, ScD., LL.D., Vice-President, in the Chair.

The decease was announced of:

Prof. A. Marshall Elliott, at Baltimore, on November 9, 1910, æt. 66.

The following papers were read:

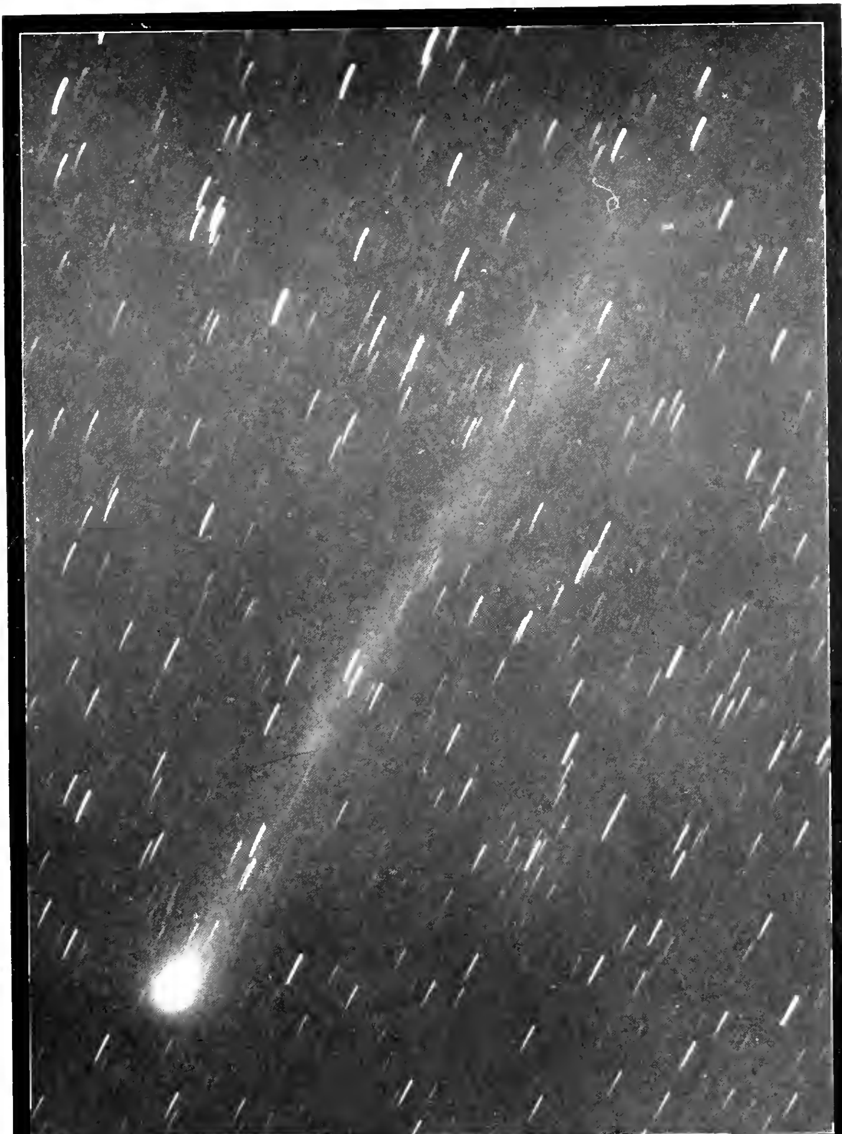
"Some Aspects of the Scientific Work of the U. S. Bureau of Fisheries," by Dr. Hugh M. Smith, discussed by Mr. Willcox, Dr. Holland, Mr. Jayne, Dr. Jastrow and Dr. Marshall.

"The True Atomic Weights of Oxygen and Silver," by Dr. Gustavus Hinrichs. (Communicated by Dr. J. W. Holland.)

"Propagation of Long Electric Waves Along Wires," by Prof. E. P. Adams. (Communicated by Prof. W. F. Magie.)

PLATE I

West



Comet 1907 *d* (Daniel) on July 11, 1907, at 13^h 58^m C.S.T.
Exposure 2^h 12^m. Scale: 1 inch = 46′
10-inch Bruce Portrait Lens. Yerkes Observatory.

PLATE II

West

Comet 1907 *d* (Daniel) on July 15, 1907, at 13h 52m C.S.T.
Exposure 2h 20m. Scale: 1 inch = 32′
10-inch Bruce Portrait Lens. Yerkes Observatory.

PLATE III

West

Comet 1907 *d* (Daniel) on July 17, 1907, at 13[h] 50[m] C.S.T.
Exposure 2[h] 10[m]. Scale: 1 inch = 50′
10-inch Bruce Portrait Lens. Yerkes Observatory.

PLATE IV

West

Comet 1907 *d* (Daniel) on July 19, 1907, at $13^h\ 54^m$ C.S.T.
Exposure $2^h\ 18^m$. Scale: 1 inch = 49′
10-inch Bruce Portrait Lens. Yerkes Observatory.

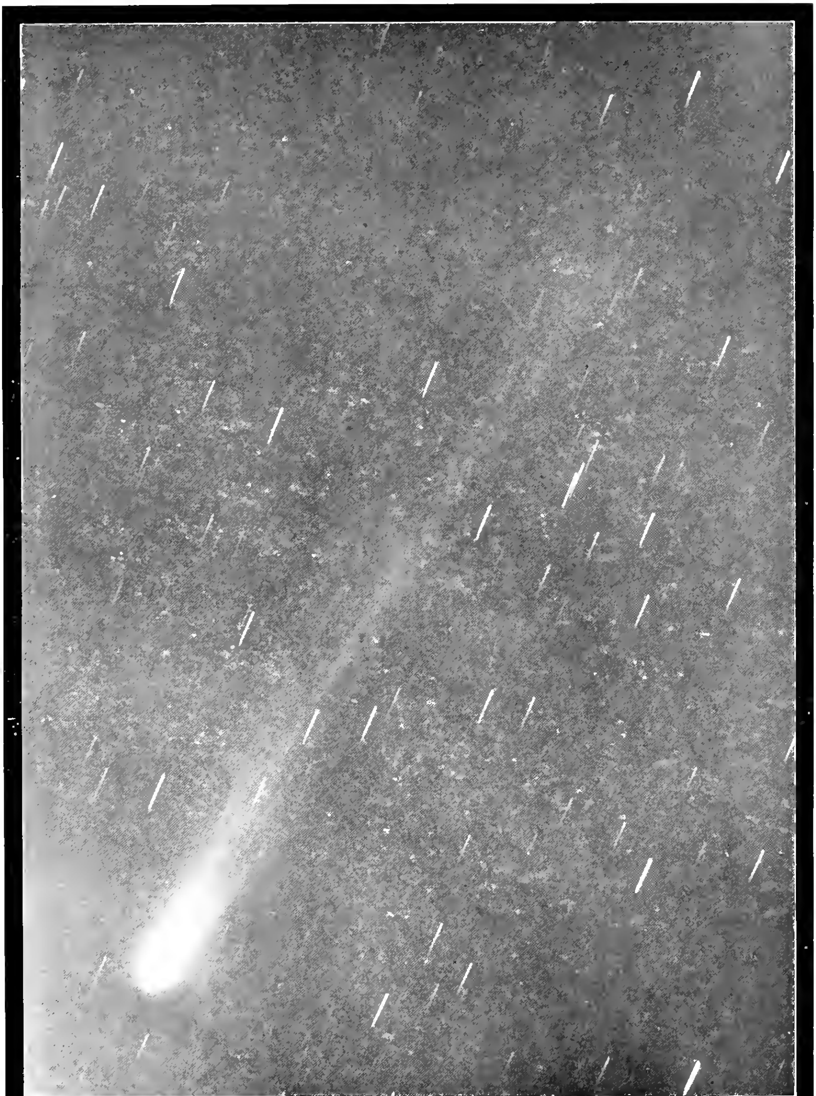
PLATE V

West

Comet 1907 *d* (Daniel) on July 29, 1907, at $14^h\ 41^m$ C.S.T.
Exposure $1^h\ 40^m$. Scale: 1 inch = 44′.
10-inch Bruce Portrait Lens. Yerkes Observatory.

PLATE VI

West



Comet 1907 *d* (Daniel) on August 3, 1907, at 14^h 53^m C.S.T.
Exposure 1^h 40^m. Scale: 1 inch = 40′
10-inch Bruce Portrait Lens. Yerkes Observatory.

PLATE VII

West

Comet 1907 *d* (Daniel) on August 5, 1907, at $15^h\ 8^m$ C.S.T.
Exposure $1^h\ 6^m$. Scale: 1 inch = 42′
10-inch Bruce Portrait Lens. Yerkes Observatory.

PLATE VIII

West

Comet 1907 *d* (Daniel) on August 6, 1907, at $15^h\ 3^m$ C.S.T.
Exposure $1^h\ 21^m$. Scale: 1 inch = 39′
10-inch Bruce Portrait Lens. Yerkes Observatory.

PLATE IX

West

Comet 1907 *d* (Daniel) on August 8, 1907, at 15^h 30^m C.S.T.
Exposure 0^h 33^m. Scale: 1 inch = 44′
10-inch Bruce Portrait Lens. Yerkes Observatory.

PLATE X

West

Comet 1907 *d* (Daniel) on August 9, 1907, at 15^h 1^m C.S.T.
Exposure 1^h 32^m. Scale: 1 inch = 50′
10-inch Bruce Portrait Lens. Yerkes Observatory.

Plate XI

West

Comet 1907 *d* (Daniel) on August 10, 1907, at 14^{h} 57^{m} C.S.T.
Exposure 1^{h} 43^{m}. Scale: 1 inch = 42′
10-inch Bruce Portrait Lens. Yerkes Observatory.

PLATE XII

Comet 1907 *d* (Daniel) on August 11, 1907, at 14^h 50^m C.S.T.
Exposure 1^h 42^m. Scale: 1 inch = 44′
10-inch Bruce Portrait Lens. Yerkes Observatory.

PLATE XIII

West

Comet 1907 *d* (Daniel) on August 11, 1907, at 14^h 59^m C.S.T.
Exposure 1^h 42^m. Scale: 1 inch = 129′
3.4-inch Bruce Portrait Lens. Yerkes Observatory.

PLATE XIV

West

Comet 1907 *d* (Daniel) on August 12, 1907, at 15h 7m C.S.T.
Exposure 1h 43m. Scale: 1 inch = 51′
10-inch Bruce Portrait Lens Yerkes Observatory

PLATE XV

Comet 1907 *d* (Daniel) on August 13, 1907, at $15^h\ 8^m$ C.S.T.
Exposure $1^h\ 11^m$. Scale: 1 inch = 41′
10-inch Bruce Portrait Lens. Yerkes Observatory.

PLATE XVI

West

Comet 1907 *d* (Daniel) on August 14, 1907, at 15^h 11^m C.S.T.
Exposure 1^h 20^m. Scale: 1 inch = 53′
10-inch Bruce Portrait Lens. Yerkes Observatory.

Plate XVII

West

Comet 1907 *d* (Daniel) on August 14, 1907, at $15^h\ 11^m$ C.S.T.
Exposure $1^h\ 20^m$. Scale: 1 inch = 141′
3.4-inch Bruce Portrait Lens. Yerkes Observatory.

PLATE XVIII

West

Comet 1907 *d* (Daniel) on August 17, 1907, at 15^h 10^m C.S.T.
Exposure 1^h 30^m. Scale: 1 inch = 40′
10-inch Bruce Portrait Lens. Yerkes Observatory.

PLATE XIX

West

Comet 1907 *d* (Daniel) on August 19, 1907, at 15^h 40^m C.S.T.
Exposure 0^h 40^m. Scale: 1 inch = 36′
10-inch Bruce Portrait Lens. Yerkes Observatory.

PLATE XX

West

Comet 1907 *d* (Daniel) on August 20, 1907, at 15ʰ 27ᵐ C.S.T.
Exposure 1ʰ 5ᵐ. Scale: 1 inch = 53′
10-inch Bruce Portrait Lens. Yerkes Observatory.

PLATE XXI

West

Comet 1907 *d* (Daniel) on August 21, 1907, at 15^h 36^m C.S.T.
Exposure 0^h 50^m. Scale: 1 inch = 56′
10-inch Bruce Portrait Lens. Yerkes Observatory.

PLATE XXII

West

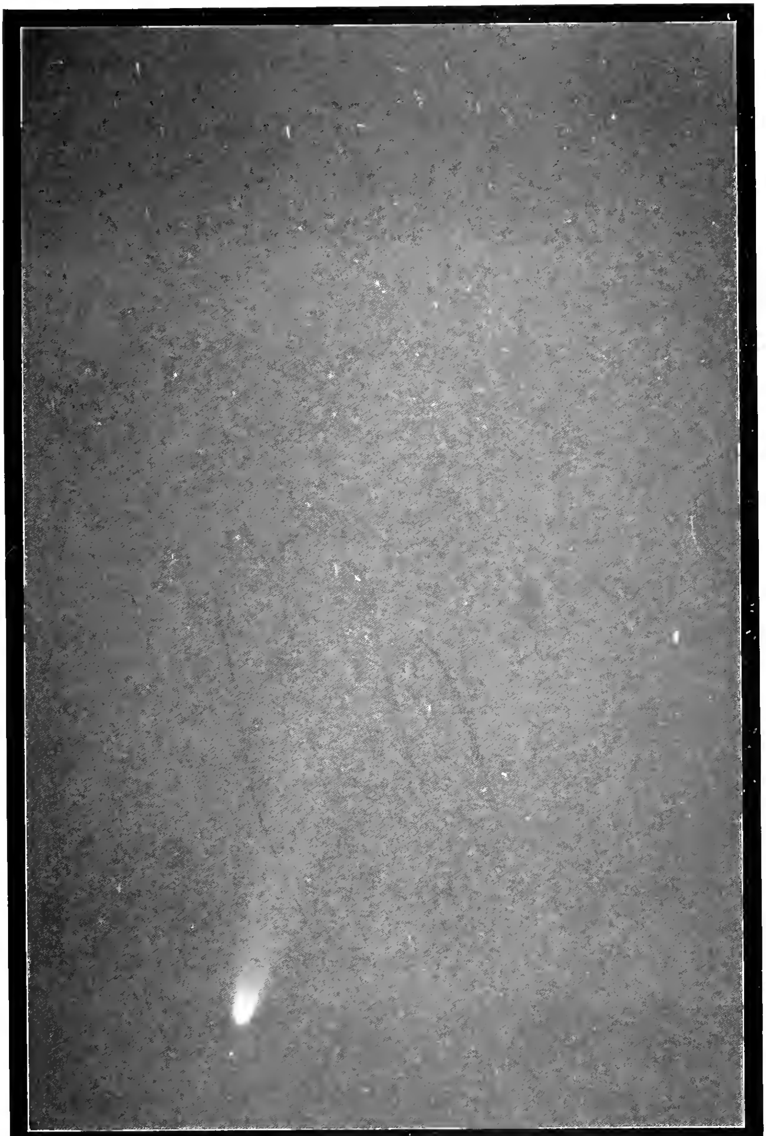
Comet 1907 *d* (Daniel) on August 24, 1907, at $15^h\ 48^m$ C.S.T.

Exposure $0^h\ 34^m$. Scale: 1 inch = 39′

10-inch Bruce Portrait Lens. Yerkes Observatory.

PLATE XXIII

West



Comet 1907 *d* (Daniel) on September 2, 1907, at 15^h 51^m C.S.T.
Exposure 0^h 18^m. Scale: 1 inch = 30′
10-inch Bruce Portrait Lens. Yerkes Observatory.

PLATE XXIV

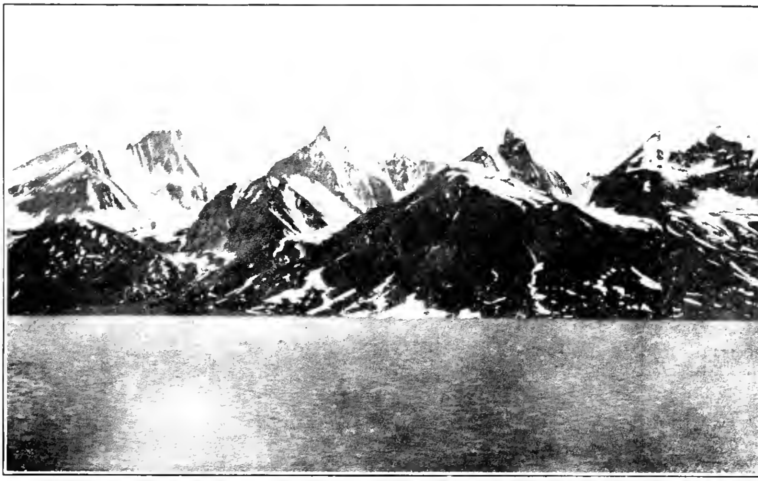
West

Comet 1907 *d* (Daniel) on September 5, 1907, at 16h 0m C.S.T.
Exposure 0h 37m. Scale: 1 inch = 38′.
10-inch Bruce Portrait Lens. Yerkes Observatory.

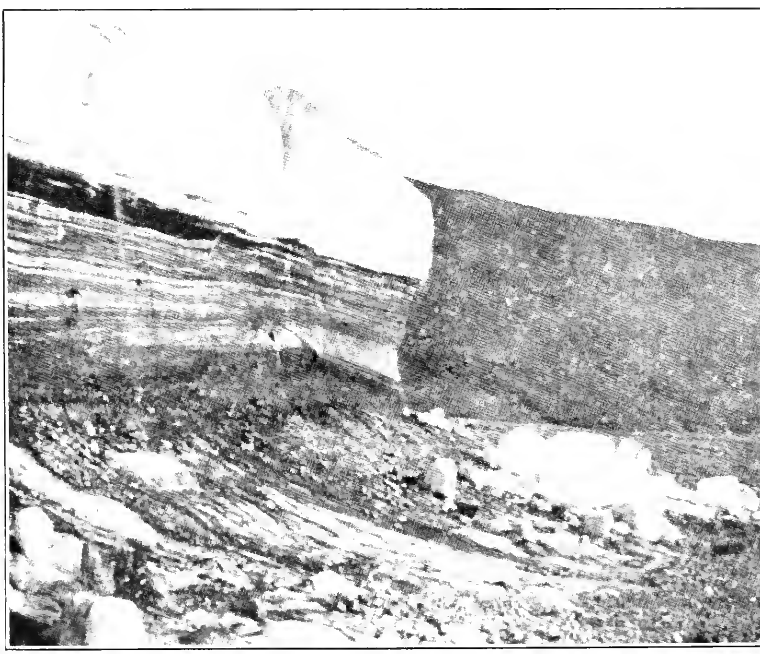
PLATE XXV

West

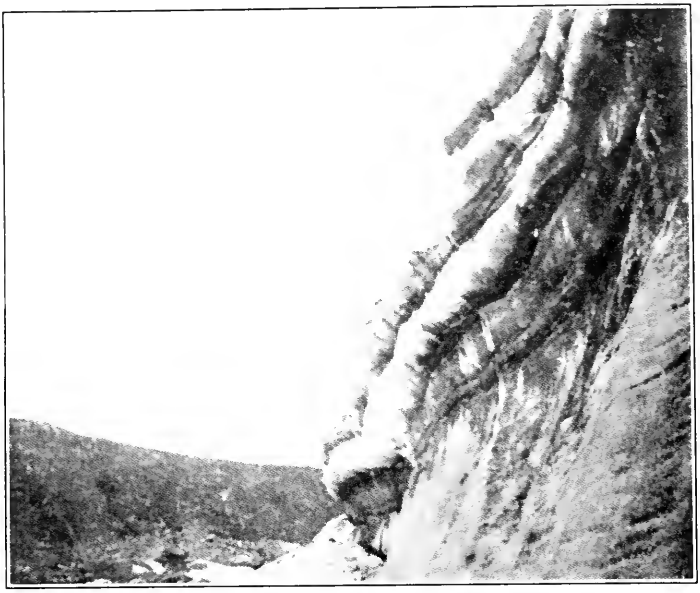
Comet 1907 *d* (Daniel) on September 8, 1907, at 16^h 17^m C.S.T.
Exposure 0^h 5^m. Scale: 1 inch = 90′
6¼-inch Bruce Portrait Lens. Yerkes Observatory.



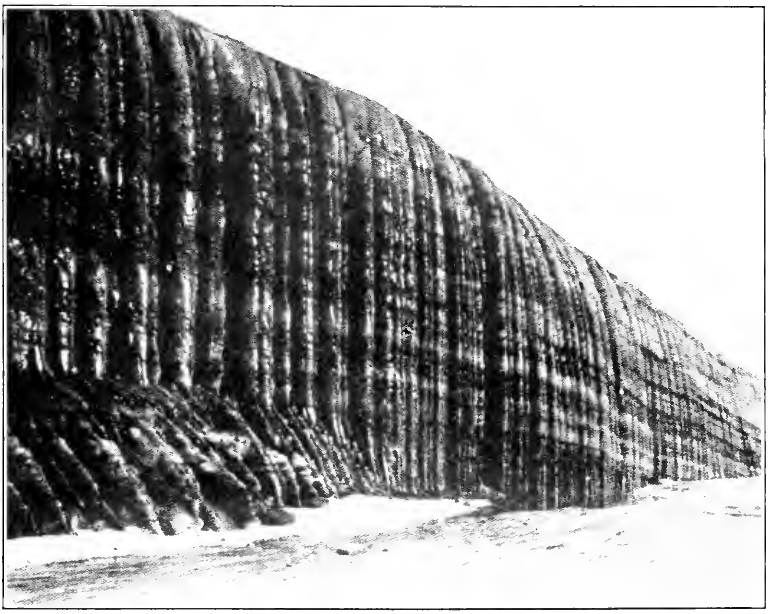
A. Fretted upland carved by mountain glaciers about King Oscar's Fjord, eastern Greenland. The highest points are from 1,360 to 1,570 meters above the sea (after Nathorst).



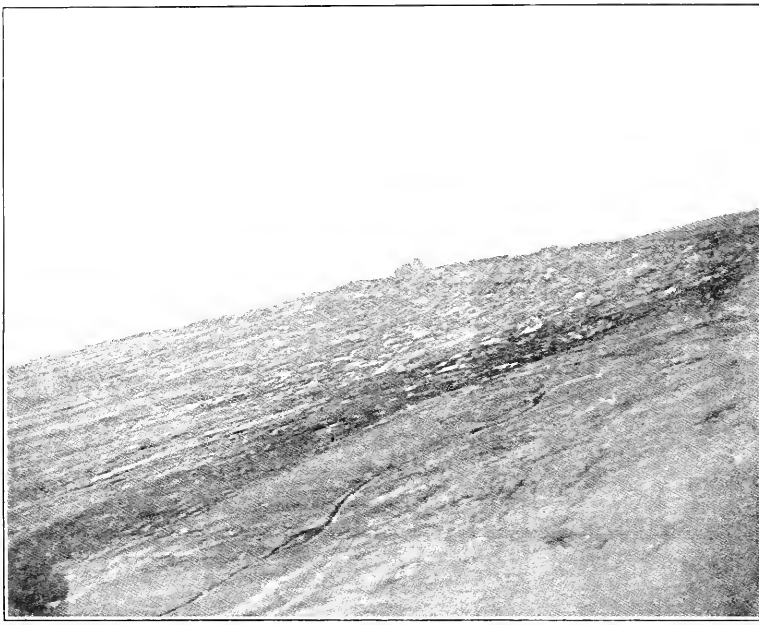
B. Front of the Bryant glacier tongue showing the vertical wall and stratification of ice. It also shows the absence of rock débris from the upper layers (after Chamberlin).



A. Portion of the southeast face of the Tuktoo glacier tongue showing the projection of the upper layers apparently as a result of overthurst (after Chamberlin).

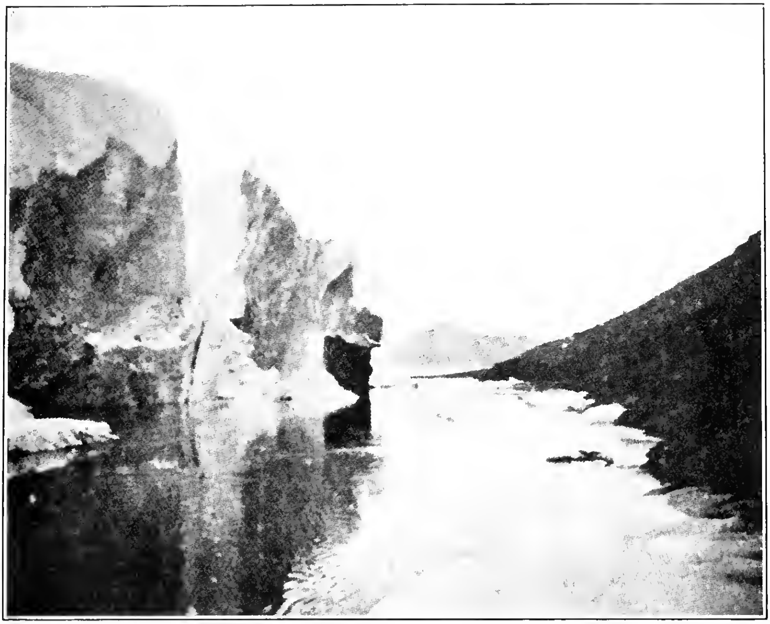


B. Ice-face at eastern margin of the inland-ice of Greenland in latitude 77° 30′ N. (after Trolle).

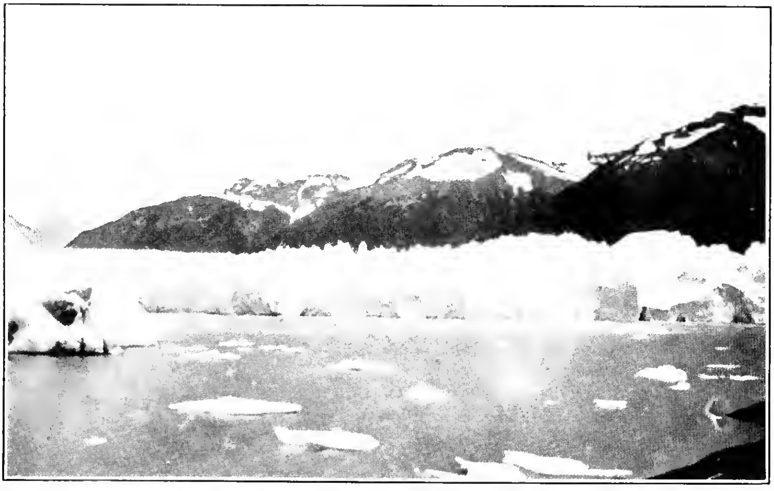


A. Normal slope of the inland-ice at the land margin near the Cornell tongue (after Tarr).

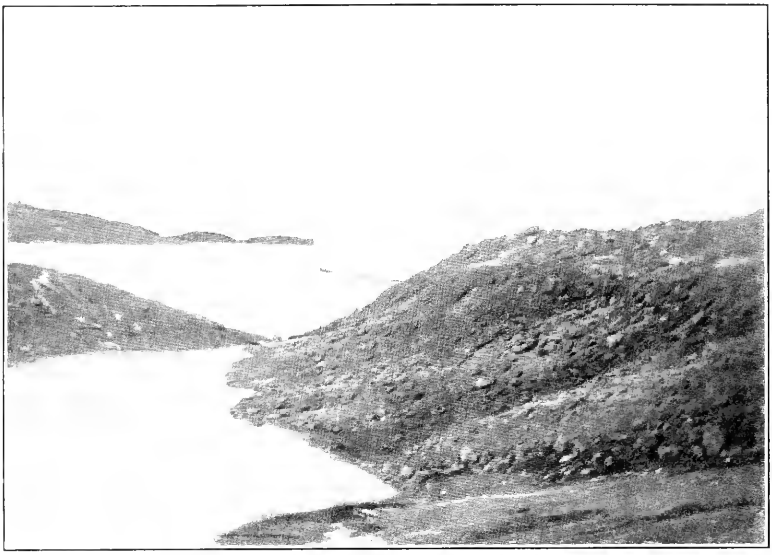
B. Hummocky moraine on the margin of the Cornell glacier tongue (after Tarr).



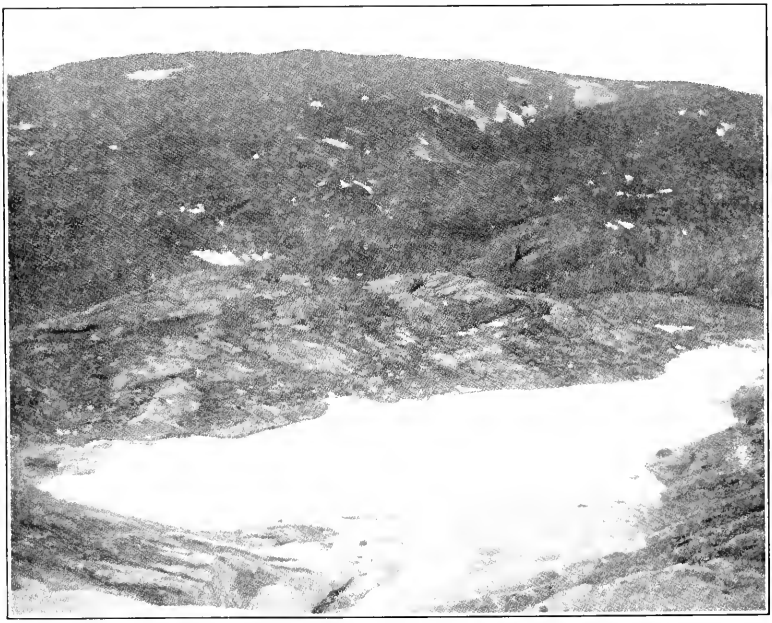
A. Lateral glacial stream flowing between ice and rock, Benedict glacier tongue (after Peary).



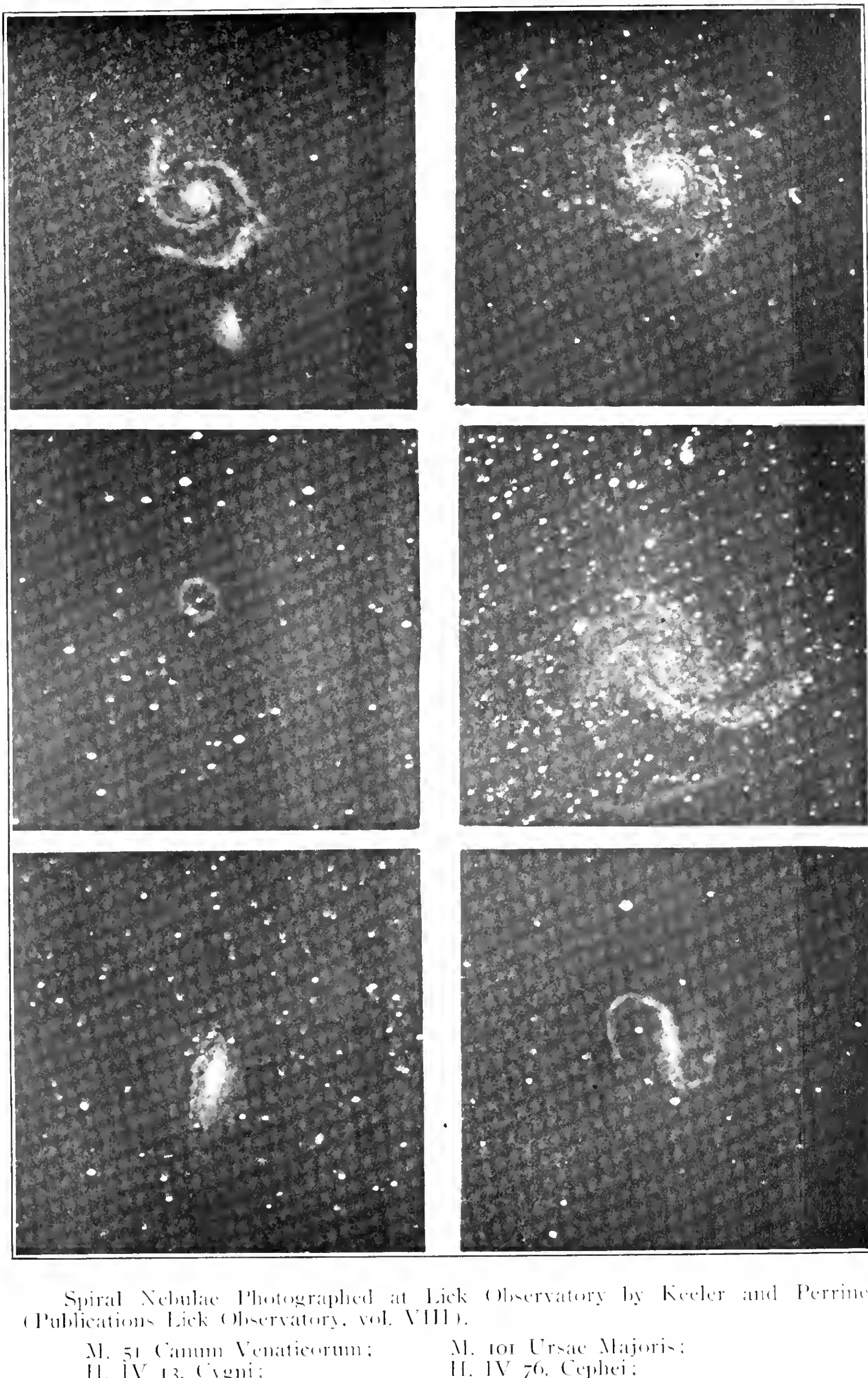
B. The ice-dammed lake Argentino in Patagonia (after Sir Martin Conway).



A. Ice-dammed lakes on the margin of the Cornell tongue of the inland-ice (after Tarr).



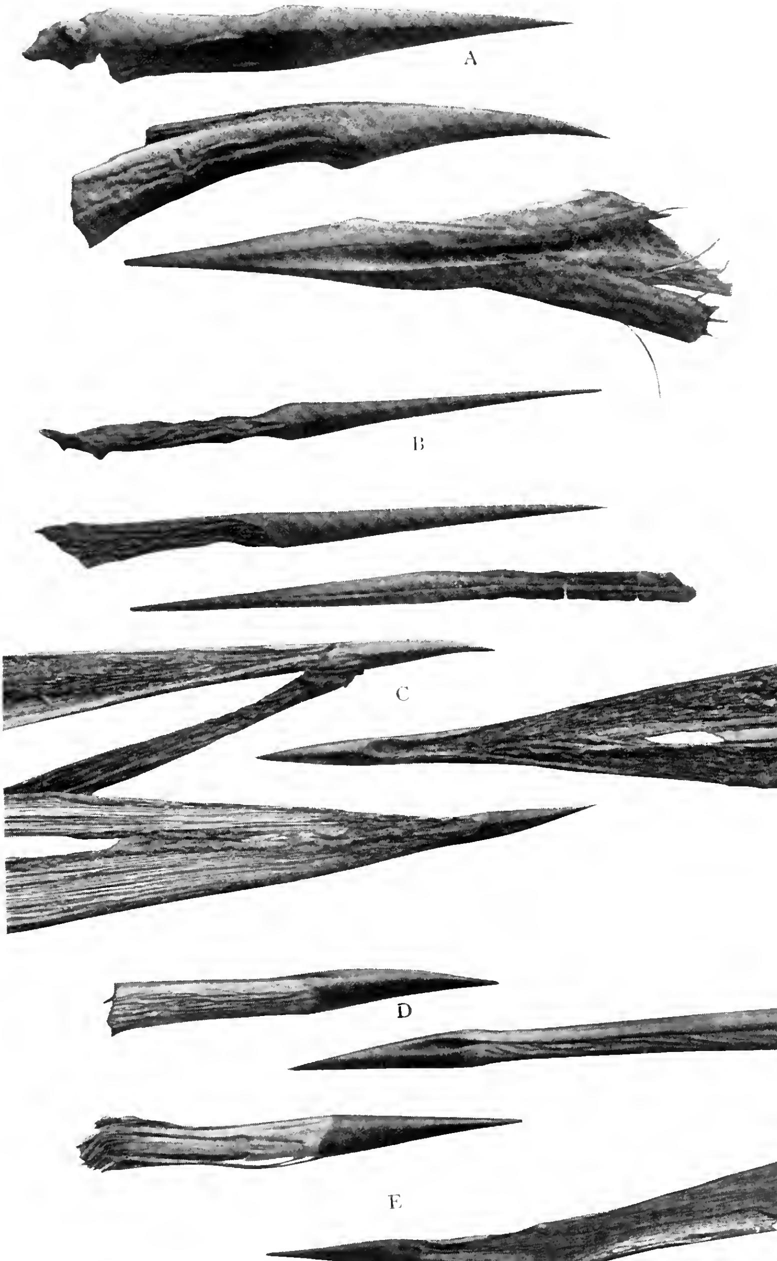
B. Delta in one of the marginal lakes to the Cornell glacier tongue (after Tarr).



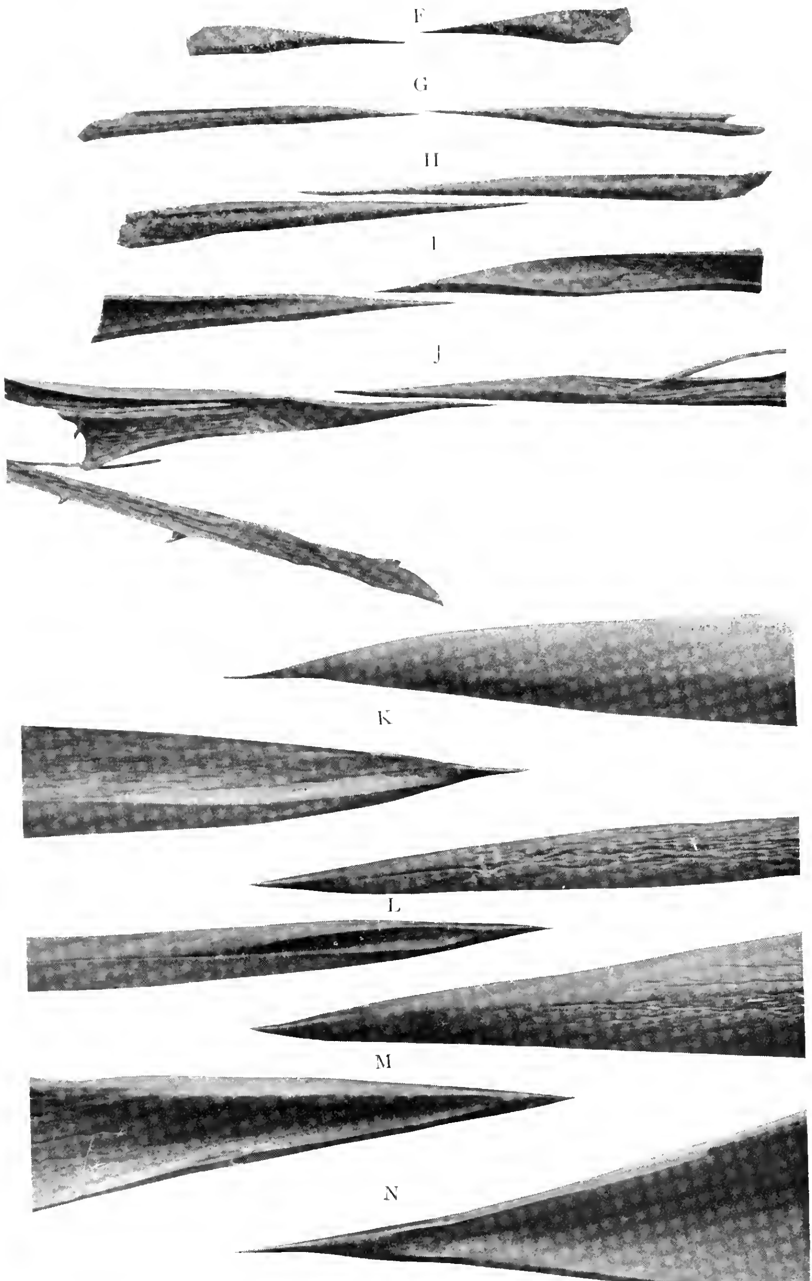
Spiral Nebulae Photographed at Lick Observatory by Keeler and Perrine (Publications Lick Observatory, vol. VIII).

M. 51 Canum Venaticorum;	M. 101 Ursae Majoris;
H. IV 13, Cygni;	H. IV 76, Cephei;
H. I 53, Pegasi;	H. I 55, Pegasi.

These photographs show that the attendant bodies are not thrown off by rotation, but begin forming in the distance and are gathered in towards the center as the whirlpool settles under its own gravitation.



SPINES OF AGAVE.



SPINES OF AGAVE.

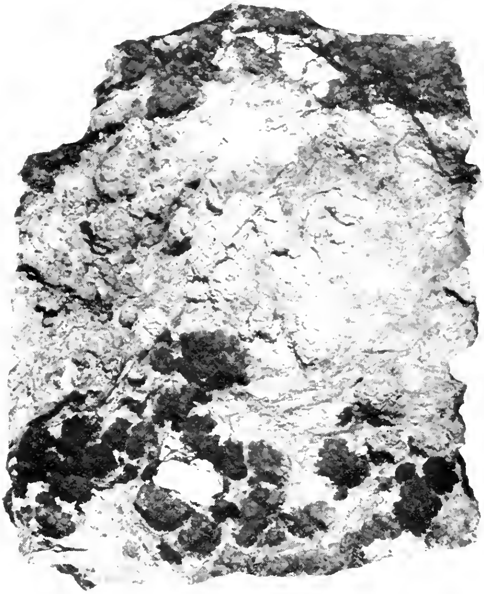


FIG. 1. Serpentine decomposing and giving rise to hydromagnesite. From Sulphur Creek. (Reduced ⅓ of diameter.)

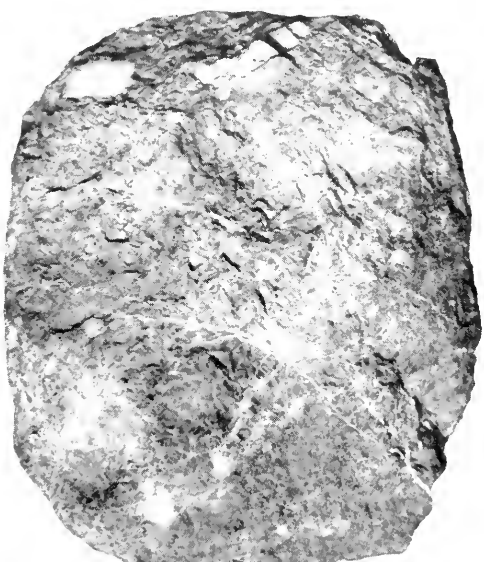


FIG. 2. Bastite, pseudomorphous after pyroxenite. From the Knoxville area. (Reduced ⅓ of diameter.)

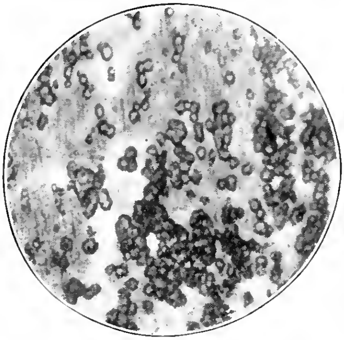


FIG. 3

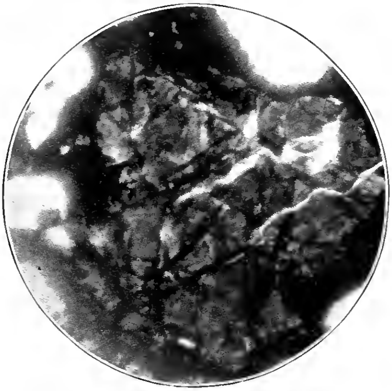


FIG. 4

FIG. 3. Grains of spinel in a bastite matrix. (Magnified 35 diameters.)
FIG. 4. Picotite with opaque chromite. (Magnified 35 diameters.)

FIG. 5. Serpentine with veins of chrysotile stained by iron oxide. From the Oak Hill area, near San José. (Reduced ⅓ of diameter.)