PROCEEDINGS

OF THE

American Philosophical Society

HELD AT PHILADELPHIA

FOR

PROMOTING USEFUL KNOWLEDGE

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VOL. XLV JANUARY–APRIL, 1906. No. 182.

Stated Meeting January 5, 1906.

President SMITH in the Chair.

An invitation was received from the Royal Geographical Society of Australasia, announcing that it would celebrate in June, 1906, the twenty-first anniversary of its foundation and inviting this Society to participate. On motion the President was authorized to appoint a delegate to represent the Society thereat.

The Judges of the Annual Election of Officers and Councillors reported that an election had been held on the afternoon of this day and that the following named members had been elected to be officers for the ensuing year:

President

Edgar F. Smith.

Vice-Presidents

George F. Barker, William B. Scott, Simon Newcomb.

Secretaries

I. Minis Hays,
Edwin G. Conklin,
Arthur W. Goodspeed,
Morris Jastrow, Jr.

Curators

Charles L. Doolittle, William P. Wilson, Albert H. Smyth.

Treasurer

Henry La Barre Jayne.

Councillors to Serve for Three Years

Patterson Du Bois,	Ernest W. Brown,
Samuel Dickson,	William Keith Brooks.

The Annual Report of the Treasurer was presented.

Stated Meeting January 19, 1906.

President Smith in the Chair.

Mr. Burnet Landreth read a paper entitled "A Case of Persistent Vitality in Seeds" (see page 5), which was discussed by Dr. W. P. Wilson, Professor Conklin and Dr. Ravenel.

Dr. John Marshall made some "Observations on the Venom of the Rattlesnake," which was discussed by Dr. W. P. Wilson, Professor Willis, Mr. J. E. Whitfield, Dr. Ravenel and Dr. Fetterolf.

A CASE OF PERSISTENT VITALITY IN SEEDS.

By BURNET LANDRETH.

(*Read January 19, 1906.*)

Mr. Watson, Curator Royal Botanic Gardens of Kew, published in the *Gardeners' Chronicle* of the eleventh of February, 1905, his opinion that seeds hermetically sealed were injured in vitality. No doubt he is correct to a degree, but commercially he is wrong, as those merchants practically engaged in shipping seeds through, or to, damp climates, as via the Suez Canal to India, have had just the opposite experience as compared with seeds not hermetically sealed.

The advantage of air-tight containers for the transportation through, or the keeping of seeds, in tropical countries, has also been proven nearer home, as, not only in Central America and Mexico, but in our own states bordering on the Gulf of Mexico, where it is well known that seeds not hermetically sealed will lose 50 per cent. to 60 per cent., and even 70 per cent., of vitality in a single summer; in the language of the southern seedsman, "they sweat to death."

But just here is a novel record as respects exposed seeds in a dry and arctic climate, an incident without any paralleling features as to a prolongation of vitality.

The seeds referred to, if kept at Bloomsdale Farm, where they were grown, through the period of sixteen years, would not have possessed any vitality, while in this case, under a continuously low temperature, one of the two varieties saved, the radish, grew up to 50 per cent. upon being returned to the United States; this, I infer, from the complete arresting of transpiration in the dry and cold atmosphere of the very far north.

This is a record of scientific interest, a contribution to the store of vegetable physiology, a demonstration never before attainable, and not likely ever again to be repeated. It is, and will remain, unique.

The following is the statement of Dr. Dedrick of the Peary Expedition of 1901:

"WASHINGTON, NEW JERSEY,
"15th November, 1905.

"MR. BURNET LANDRETH,

"*Dear Sir:*—The incidents of the finding of certain seeds abandoned by Lieutenant Greely at Fort Conger, 490 miles from the pole, were as follows:

"In January, 1899, the expedition of Lieutenant Peary, of which I was surgeon, discovered Fort Conger, 81° 44′, or about 490 miles from the pole. This station was abandoned in 1883, sixteen years, and among the articles reclaimed by the Peary party were a lot of seeds in packages bearing your name. These seeds were sealed up in the usual flat paper packets as issued by your establishment (I send you some of the identical seed packets) and were found in an open box in the loft or attic of Fort Conger, where they had rested sixteen years, well sheltered from rain and snow, but exposed to a winter temperature of 60° to 70° below zero.

"In April, 1899, with the seeds in my possession, we journeyed by sledge over the ice some 300 miles south to our ship, from whence I sent the seeds home that season, where they remained unplanted until the spring of 1905.

"The seeds from two of these packets, one of lettuce and one of radish, I planted in my garden at Washington, New Jersey. The lettuce seed failed entirely to germinate, but about one half of the radish seeds germinated and reached perfection in size, and even reproduced seed. The photograph which I enclose was taken from one of the roots, grown during the summer of 1905, after it had reached full development, and the seeds sent you are the produce of the same roots.

"This retention of germinative force, under the conditions of sixteen winters of exceedingly low temperature (we found it 70° below zero at Fort Conger) and during the five years subsequently when brought back to the United States, twenty-one years in the total, is to me most extraordinary, and this record is another added to the many valuable contributions to science made by Lieutenant Peary's four years' expedition.

"THOMAS S. DEDRICK, M.D."

Explanatory of the taking of the seeds to the far north, is the following letter from General A. W. Greely, now Chief Signal Officer of the Army:

"WAR DEPARTMENT,
"WASHINGTON, D. C.,
"October 23, 1905.

"MR. BURNET LANDRETH, Bristol, Pennsylvania.

"*Dear Sir:*—Referring to your letter of October 21, I have to state that I took north with me in 1881, in connection with the Lady Franklin Bay Expedition, various seeds. It is my impression, although I am not certain on this point, that they came from your own seed farm. Attempts were made at Fort Conger, 81° 44′ north, 64° 45′ west, to raise crops of lettuce, cabbage, radish, etc., with the desire of adding fresh vegetables to the dietary of the expedition, for their antiscorbutic qualities.

"Despite considerable care these efforts were not successful, the general opinion being that the soil was too strongly saturated with alkalies to suit these crops.

"Judging from the experience of my own expedition, the seeds which were brought back by Surgeon Thomas Dedrick must have been subjected nearly every winter since 1883 to temperatures of 60° below zero, Fahrenheit, and probably during the summer to temperatures approximating 60° above zero, Fahrenheit.

"Yours truly,
"A. W. GREELY."

No seeds, so far as on record, ever had such a prolonged or severe test as to their vitality as these which Lieutenant Greely took to the very far north, seeds which laid at Camp Conger, with other abandoned property, for sixteen years, or until 1899, when the north polar expedition under Lieutenant Peary found the camp.

Experiments in the laboratory have been made by exposing seeds to the influences of liquid air in temperatures 40° and 50° below zero, but never has the opportunity existed, and possibly there never will, to reclaim seeds after seventeen years' exposure to an arctic temperature every winter of 60° below zero.

The following are some extracts upon the subject of the retention of germinative force of radish seeds:

1. From the Seed Division of the United States Department of Agriculture: "We have not conducted any experiments along these lines."

2. From the Seed Division of Cornell University: "Our director of twenty years ago, Dr. Sturtevant, made some experiments to determine the longevity of radish seeds, and reported that starting with one-year-old seed at 71 per cent., it was at two years 57 per cent., at three years 49 per cent., at five years 37 per cent., six years 12 per cent., seven years 3 per cent., twelve years 0 per cent."

3. From the Department of Agriculture, Ottawa, Ontario: "We have no experiments to report. Our observations are that a dry cold atmosphere has little or no influence upon northern-grown seed. The effect of cold depends upon the amount of moisture surrounding the embryo. Northern-grown glutinous wheats are very resistant to cold."

The summary of these communications, and many others, being to the effect that never before has been presented any similar opportunity under which can be observed the effect of intensely low and prolonged cold as preserving and greatly extending the germinative force of seeds. These seeds certainly were harvested the summer of 1880 or earlier, consequently were twenty-three years old when the crop was grown. Just here the thought occurs to me, can it be possible that the electrically charged atmosphere, so constant in far northern regions, has the effect of prolonging germinative force. All arctic explorers observe that the atmospheric electric currents add quite one hundred per cent. to the rapidity of plant growth, and to the development of a miraculous brilliancy of color and strength of perfume.

BRISTOL, PENN.

Stated Meeting February 2, 1906.

President SMITH in the Chair.

Dr. Charles H. Frazier, a recently elected member, was presented to the Chair and took his seat in the Society.

An invitation was presented from the Academy of Science of St. Louis, inviting the Society to participate in the Commemoration of the Fiftieth Anniversary of its foundation, on March 10 next, and the President appointed Dr. William Trelease to represent the Society on this occasion.

The decease of the following members was announced:

Professor A. Thury, of Geneva, Switzerland, on January 19, 1905, aged 82 years.

Professor Peter R. v. Tunner, of Leoben, Austria, on June 8, 1897, aged 89 years.

The following papers were read:

"The Uses of Plants by the American Indian," by Mr. Frederick V. Coville, which was discussed by Mr. Jayne.

"Some Results from the Drift-Cask Experiment in the Arctic Ocean," by Mr. Henry G. Bryant.

Stated Meeting February 16, 1906.

President SMITH in the Chair.

The decease was announced of Professor Fredolin Sandberger, of Wurzburg, Bavaria, on April 11, 1898, aged 71 years.

Professor William A. Lamberton read a paper entitled "Some Points in the Work of St. Paul," which was discussed by Mr. Goodwin and Mr. Wood.

Stated Meeting March 2, 1906.

President SMITH in the Chair.

The decease was announced of Prof. Samuel P. Langley, on February 27, 1906, æt. 71, at Aiken, S. C.

Dr. David L. Edsall read a paper on "Some Recent Studies of Digestion," which was discussed by Prof. E. G. Conklin.

Stated Meeting March 16, 1906.

President SMITH in the Chair.

The decease was announced of Mr. Clarence H. Clark, at Philadelphia, on March 13, 1906, æt. 73.

Dr. Edward A. Spitzka read a paper entitled "The Anatomy of the Human Brain, with Special Reference to the Mental Functions —as illustrated by the Brains of Men Notable in the Professions, Arts and Sciences." Discussed by Dr. Dercum and President MacAlister.

Stated Meeting April 6, 1906.

President SMITH in the Chair.

The decease of the following members was announced:

Mr. William W. Jefferies, at New York, on February 23, 1906, æt. 86.

Mr. John Vaughan Merrick, at Philadelphia, on March 28, 1906, æt. 78.

Dr. Henry M. Chance read an obituary notice of Prof. J. Peter Lesley.

Mr. B. H. E. Groth presented a paper on "The History, Structure and Growth of the Sweet Potato," which was discussed by Mr. Goodwin and Mr. Landreth.

Dr. J. M. Macfarlane exhibited some species and hybrids of *Sarracenia* in flower from the Botanical Garden of the University of Pennsylvania.

General Meeting April 17, 18, 19 and 20, 1906.

April 17—Evening Session.

President SMITH in the Chair.

The opening session was devoted to the celebration of the two hundredth anniversary of the birth of Benjamin Franklin, and the proceedings will be found in the *Franklin Bi-centenary Volume.*

April 18—Morning Session.

The following papers were read:

"The Statistical Method in Chemical Geology," by Frank Wigglesworth Clark, Sc.D., of Washington, which was discussed by Mr. Joseph Wilcox.

"On a Possible Reversal of the Deep Sea Circulation and its Effect on Geological Climates," by Prof. Thomas C. Chamberlin, of Chicago, which was discussed by Professors Newcomb, Morris Davis, Chamberlin and others.

"Elementary Species in Agriculture," by Prof. Hugo deVries, of Amsterdam, Holland, which was discussed by Professors Conklin, Osborn and Dr. McDugal.

"An International Southern Observatory," by Prof. Edward C. Pickering, of Cambridge, Mass., which was discussed by Professor Michelson and Dr. Brashear.

"The Figure and Stability of a Liquid Satellite," by Sir George Darwin, K.C.B., F.R.S., of Cambridge, England.

Executive Session—12.30 o'clock.

The pending nominations for membership were read and spoken to, and the Society proceeded to an election.

Afternoon Session.

The tellers of election reported that the following candidates had been elected to membership:

Residents of the United States.

Hon. Joseph Hodges Choate, LL.D., D.C.L. (Oxon.), New York.

Henry Herbert Donaldson, Ph.D., Philadelphia.

Russell Duane, Philadelphia.

David Linn Edsall, M.D., Philadelphia.

John W. Harshberger, Ph.D., Philadelphia.

Charles S. Hastings, Ph.D., New Haven, Conn.

William Francis Hillebrand, Ph.D. (Heidelberg), Washington.

Charles Rockwell Lanman, LL.D., Cambridge, Mass.

Franklin Paine Mall, M.D., LL.D., Baltimore.

Ernest Fox Nichols, D.Sc., New York City.

Hon. Elihu Root, LL.D., Washington.

Thomas Day Seymour, LL.D., New Haven, Conn.

Edward Bradford Titchener, M.A., Oxford; Ph.D., Leipsic, Ithaca, New York.

Otto Hilgard Tittmann, Washington.

Arthur Gordon Webster, Ph.D. (Berlin), Worcester, Mass.

Foreign Residents.

Adolf Engler, Ph.D., Berlin.

Dr. Hendrik Antoon Lorentz, Leyden, Holland.

Dmitri Ivanovitch Mendeleff, St. Petersburg.

Theodor Nöldeke, Ph.D., Strassburg.

August Weismann, Freiburg.

The following papers were presented:

"Form Analysis," by Prof. Albert A. Michelson, of Chicago.

"The Present Position of the Problem concerning the First Principles of Scientific Theory," by Prof. Josiah Royce, of Cambridge, Mass.

"The Human Harvest," by President David Starr Jordan, of Stanford University, Cal.

"On Positive and Negative Electrons," by Prof. H. A. Lorentz, of Amsterdam.

Evening Session.

Vice-President Barker in the Chair.

(See Franklin Bi-centenary Volume.)

April 19.

(See Franklin Bi-centenary Volume.)

April 20—Morning Session.

(See Franklin Bi-centenary Volume.)

Afternoon Session.

The following papers were presented:

"The Elimination of Velocity-Head in the Measurements of Pressures in a Fluid Stream," by Prof. Francis E. Nipher, of St. Louis.

"Old Weather Records and Franklin as a Meteorologist," by Prof. Cleveland Abbe, of Washington.

"Was Lewis Evans or Benjamin Franklin the first to recognize that our North-east Storms come from the South-west?" by Prof. William Morris Davis, of Cambridge, Mass.

"Notes on the Production of Optical Planes of large Dimensions," by Dr. John A. Brashear, of Allegheny, Pa.

"Repetition and Variation in Poetic Structure," by Prof. Francis Barton Gummere, of Haverford, Pa.

"The Herodotean Prototype of Esther and Sheherazade," by Prof. Paul Haupt, of Baltimore, Md.

"Heredity and Variation, Logical and Biological," by Prof. Wm. Keith Brooks, of Baltimore.

"Notes on a Collection of Fossil Mammals from Natal," by Prof. William B. Scott, of Princeton.

"The Use of Dilute Solutions of Sulphuric Acid as a Fungicide," by Prof. Henry Kraemer, of Philadelphia.

Prof. Arthur G. Webster and Prof. Charles R. Lanman, newly elected members, were presented to the Chair and took their seats in the Society.

Letters of acceptance of membership were read from Dr. Henry A. Donaldson, Mr. Russell Duane, Dr. David L. Edsall and Prof. Franklin P. Mall.

It was ordered that a message of condolence be sent to Madame Curie, on the death of her husband.

The following resolution, offered by Dr. Hays, was unanimously adopted:

As the Society has heard with profound grief of the appalling disaster that has befallen San Francisco and its neighboring towns, it desires to express its deep sympathy for their citizens, and particularly for its sister society, the California Academy of Sciences, and as a slight evidence of its sympathy, the Secretaries be instructed to send to the California Academy of Sciences a complete set of the Transactions and the Proceedings of the American Philosophical Society, so far as the same may be available, so soon as the California Academy of Sciences is prepared to re-form its library.

THE STATISTICAL METHOD IN CHEMICAL GEOLOGY.

By F. W. CLARKE.

(*Read April 18, 1906.*)

In an essay upon the relative abundance of the chemical elements, published some sixteen years ago,[1] I attempted to apply the statistical method to ascertaining the average composition of the earth's crust. Since that time the data have been repeatedly revised, both by myself and by others, and the results obtained have found applications which I did not anticipate. The composition of the lithosphere has furnished, so to speak, a sort of base-line to which other computations could be referred; the figures, therefore, have acquired a peculiar importance, and it has become desirable to determine, more critically than heretofore, the degree of their validity. To discuss the nature of the averages and to consider how far they may be utilized is the purpose of the present communication. In order to accomplish this purpose I must restate, very briefly, the main points of the original argument.

As a first step in the discussion, it was necessary to assume a definite mass of matter as available for statistical analysis. That mass included the atmosphere, the ocean and a certain portion of the lithosphere; the portion, namely, that may be supposed to lie within our reach. For the last item it was assumed that a shell having a thickness of ten miles below sea-level would represent known material; in other words, that it would consist of rocks essentially identical in general character with those which we can study at the surface. How much thicker the rocky crust of the earth may be is another question, upon which we do not need to enter. It is only the *known* material which concerns us now; and some of that is brought to us by volcanoes from depths far below any to which we can penetrate directly. The eruptive rocks enable us to determine what sort of matter lies below the immediately observable surface.

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[1] *Bull. Philos. Soc. Washington*, vol. 11, p. 131, Oct. 26, 1889. Also in U. S. Geol. Survey Bull. 78, p. 34.

I am indebted to Dr. R. S. Woodward for data relative to the volume of matter which is thus taken into account. The volume of the ten-mile rocky crust, including the mean elevation of the continents above the sea, is 1,633,000,000 cubic miles; and to this material we may assign a mean density not lower than 2.5, nor much higher than 2.7. The volume of the ocean is put at 302,000,000[1] cubic miles, and I have given it a density of 1.03, which is a trifle too high. The mass of the atmosphere, so far as it can be determined, is equivalent to that of 1,268,000 cubic miles of water, the unit of density. Combining these data, we get the following expression for the composition of the known matter of our globe.

	Density of Crust. 2.5.	Density of Crust. 2.7.
Percentage of atmosphere	.03	.03
Percentage of ocean	7.08	6.58
Percentage of solid crust	92.89	93.39
	100.00	100.00

In short, we can regard the surface layer of the earth, to a depth of ten miles, as consisting very nearly of 93 per cent. solid and 7 per cent. liquid matter, treating the atmosphere as a small correction to be applied when needed.[2] The figure thus assigned to the ocean is probably a little too high, but its adoption makes an allowance for the fresh waters of the globe, which are too small in amount to be estimable directly. Their insignificance may be inferred from the fact that a section of the ten-mile crust having the surface area of the United States, represents only about 1.5 per cent. of the entire mass of matter under consideration. Even the mass of Lake Superior thus bcomes a negligible quantity.

The composition of the ocean is easily determined from the data

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[1] Sir John Murray, *Scottish Geograph. Mag.*, 1888, p. 39, estimates the volume of the ocean at 323,722,150 cubic miles. Karstens, more recently, puts it at 1,285,935,211 cubic kilometres, or 307,496,000 cubic miles. "Eine neue Berechnung der mittleren Tiefen der Oceane," *Inaug. Diss.*, Kiel, 1894. Karstens gives a good summary of previous estimates, which vary widely. To change the figure given in my original essay would be a straining after unattainable precision.

The adoption of Murray's figure for the volume of the ocean would raise its percentage to from 7.12 to 7.88, according to the density, 2.5 or 2.7, assigned to the lithosphere.

given by Dittmar in the Report of the Challenger Expedition.[1] The maximum salinity observed by him amounted to 37.37 grammes of salts in a kilogramme of water, and by taking this figure instead of a lower average value, we can allow for saline masses enclosed within the solid crust of the earth, and which would not otherwise appear in our final estimates. Combining this datum with Dittmar's figures for the average composition of the oceanic salts, we get the second of the subjoined columns. Other elements, contained in sea water, but only in minute traces, need not be considered here. No one of them could reach 0.001 of 1 per cent.

Composition of Salts.	
$NaCl$	77.76
$MgCl_2$	10.88
$MgSO_4$	4.74
$CaSO_4$	3.60
K_2SO_4	2.46
$MgBr_2$	.22
$CaCO_3$	.34
	100.00

Composition of Ocean.	
O	85.79
H	10.67
Cl	2.07
Na	1.14
Mg	.14
Ca	.05
K	.04
S	.09
Br	.008
C	.002
	100.00

It is worth while at this point to consider how large a mass of matter these oceanic salts represent. The *average* salinity of the ocean is not far from 3.5 per cent.; its mean density is 1.027, and its volume is 302,000,000 cubic miles. The specific gravity of the salts, as nearly as can be computed, is 2.25. From these data it can be shown that the volume of the saline matter in the ocean is a little over 4,800,000 cubic miles, or enough to cover the entire surface of the United States, excluding Alaska, 1.6 miles deep. In face of these figures, the beds of rock salt at Stassfurt and elsewhere, which seem so enormous at close range, become absolutely trivial. The allowance made for them by using the maximum salinity of the ocean instead of the average, is more than sufficient; for it gives them a total volume of 325,000 cubic miles. That is, the data used for computing the average composition of the ocean, and its average

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[1] In Volume 1, "Physics and Chemistry."

significance as a part of all terrestrial matter, are maxima; and tend therefore to compensate for the omission of factors which could not well be estimated directly.

The average composition of the lithosphere is very nearly that of the igneous rocks alone. The sedimentary rocks represent altered igneous material, from which salts have been leached into the ocean, and to which oxygen, water and carbon dioxide have been added from the atmosphere. For these changes, corrections can be applied, and their magnitude and effect, as will be shown later, is surprisingly small. The thin film of organic matter upon the surface of the earth can be neglected altogether. In comparison with the ten-mile thickness of rock below it, its quantity is too small to be considered. Even beds of coal are negligible, for their volume also is relatively insignificant. Practically, we have to consider at first, only ten miles of igneous rock; which, when large enough areas are studied, averages much alike in composition all over the globe. This point was established in my original memoir, in which groups of analyses, representing rocks from different regions, were compared. The essential uniformity of the averages was unmistakable, and it has been still further emphasized by later computations by others as well as by myself. The following averages are now available for comparison.

A. My original average of 880 analyses, of which 207 were made in the laboratory of the U. S. Geological Survey and 673 were collected from other sources. Many of these analyses were incomplete.

B. The average of 680 analyses from the records of the Survey laboratories, plus some hundreds of determinations of silica, lime and alkalies. The Survey data up to January 1, 1897.

C. The average of 830 analyses from the Survey records, plus some partial determinations. The Survey data up to January 1, 1900.

D. An average of all the analyses, partial or complete, made up to January 1, 1904, in the laboratories of the Survey.[1]

E. An average, computed by A. Harker,[2] of 397 analyses of

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[1] See U. S. Geological Surv. Bull. 228, p. 17, for details.

[2] *Geol. Mag.*, 1899, p. 220.

igneous rocks from British localities. Many of these analyses were incomplete, especially with respect to phosphorus and titanium.

F. An average of 1,811 analyses, from Washington's tables.[1] Calculated by H. S. Washington. The data represent material from all parts of the world.

Now, omitting minor constituents, which rarely appear except in the more modern analyses, these averages may be tabulated together, although they are not absolutely comparable. The comparison assumes the following form:

	A. Clarke.	B. Clarke.	C. Clarke.	D. Clarke.	E. Harker.	F. Washington.
SiO_2	58.59	59.77	59.71	60.91	58.75	58.239
Al_2O_3	15.04	15.38	15.41	15.28	15.64	15.796
Fe_2O_3	3.94	2.65	2.63	2.63	5.34	3.334
FeO	3.48	3.44	3.52	3.46	2.40	3.874
MgO	4.49	4.40	4.36	4.13	4.09	3.843
CaO	5.29	4.81	4.90	4.88	4.98	5.221
Na_2O	3.20	3.61	3.55	3.45	3.25	3.912
K_2O	2.90	2.83	2.80	2.98	2.74	3.161
H_2O at 100°	1.96	....	....	.41	2.23	.363
H_2O above 100°		1.51	1.52	1.49		1.428
TiO_2	.55	.53	.60	.73	.12	1.039
P_2O_5	.22	.21	.22	.26	.02	.373
	99.66	99.14	99.22	100.61	99.56	100.583

Although these six columns are not very divergent, there are differences between them which may be more apparent than real. Differences of summation are partly due to the omission of minor constituents; but the largest variations are attributable to the water. In two columns hygroscopic water is omitted; in two it is not distinguished from combined water; in two a discrimination is made. By rejecting the figures for water and recalculating to 100 per cent., the averages become more nearly alike, as follows:

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[1] U. S. Geological Surv. Professional Paper 14, p. 106.

	A.	B.	C.	D.	E.	F.
	Clarke.	Clarke.	Clarke.	Clarke.	Harker.	Washington.
SiO_2	59.97	61.22	61.12	61.71	60.36	58.96
Al_2O_3	15.39	15.75	15.77	15.48	16.07	15.99
Fe_2O_3	4.03	2.71	2.69	2.68	5.48	3.37
FeO	3.56	3.53	3.60	3.50	2.46	3.93
MgO	4.60	4.51	4.46	4.18	4.20	3.89
CaO	5.41	4.93	5.02	4.94	5.12	5.28
Na_2O	3.28	3.69	3.63	3.49	3.34	3.96
K_2O	2.97	2.90	2.87	3.02	2.83	3.20
TiO_2	.56	.54	.61	.74	.12	1.05
P_2O_5	.23	.22	.23	.26	.02	.37
	100.00	100.00	100.00	100.00	100.00	100.00

Of these averages, only "D" and "F" need be considered any further, for they include the largest masses of trustworthy data. "A" was only a preliminary computation, "B" and "C" are included under "D"; Harker's average contains too many imperfect analyses. "D" and "F," however, are not strictly equivalent. Washington's average relates only to analyses which were nominally complete, and made in many laboratories by very diverse methods. My average represents the homogeneous work of one laboratory, and includes, moreover, many partial determinations. For the simpler salic rocks determinations of silica, lime and alkalies are generally all that is needed for petrographic purposes. The femic rocks are mineralogically more complex, and for them full analyses are necessary. The partial analyses, therefore, chiefly represent salic rocks, and their inclusion in the average tends to raise the percentage of silica and to lower the proportions of other elements. The salic rocks, however, are more abundant than those of the other class, and so the higher figure for silica seems more probable. This conclusion is in line with a criticism by Mennell,[1] who thinks that the femic rocks received excessive weight in my earlier averages. Mennell has studied the rocks of southern Africa, where granitic types are predominant; and he believes that the true average should approximate to the composition of a granite. A wider range of

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[1] *Geol Mag.*, ser. 5, vol. 1, p. 263. For other discussions of the data given in my former papers, see De Launay, *Revue Gén. des Sciences,* April 30, 1904; and Ochsenius, *Zeitsch, prakt. Geol.*, May, 1898.

observation would probably modify that opinion, which, however, is entitled to some weight.

So far, my final average has only been partly given; the minor constituents of the rocks remain to be taken into account. In the laboratory of the Geological Survey the analyses of igneous rocks have been unusually elaborate, and many things have been determined which are too often ignored. The complete average is given in the next table, with the number of determinations to which each figure corresponds. In the elementary column, hygroscopic water does not appear; but an allowance is made for a small amount of iron which was reported in the analysis as FeS_2. When a "trace" of anything is recorded, it is arbitrarily reckoned as 0.01 per cent., and when a substance is known to be absent from a rock, by actual determination of the fact, it is assigned zero value in making up the averages.

	Number.	Average.	Reduced to 100.	In Elementary Form	
SiO_2	1,358	60.91	59.87	O	47.09
Al_2O_3	912	15.28	15.02	Si	28.23
Fe_2O_3	961	2.63	2.58	Al	7.99
FeO	962	3.46	3.40	Fe	4.46
MgO	1,027	4.13	4.06	Mg	2.46
CaO	1,215	4.88	4.79	Ca	3.43
Na_2O	1,268	3.45	3.39	Na	2.53
K_2O	1,265	2.98	2.93	K	2.44
H_2O-	770	.41	.40	H	.17
H_2O+	832	1.49	1.46	Ti	.43
TiO_2	870	.73	.72	Zr	.026
ZrO_2	185	.03	.03	C	.14
CO_2	469	.53	.52	P	.11
P_2O_5	884	.26	.26	S	.11
S	575	.11	.11	Cl	.07
Cl	234	.07	.07	F	.02
F	73	.02	.02	Ba	.089
BaO	617	.11	.11	Sr	.034
SrO	520	.04	.04	Mn	.084
MnO	899	.10	.10	Ni	.023
NiO	243	.03	.03	Cr	.034
Cr_2O_3	246	.05	.05	V	.02
V_2O_3	40	.03	.03	Li	.01
Li_2O	550	.01	.01		100.000
		101.74	100.00		

In this computation the figures for C, Zr, Cl, F, Ni, Cr and V are only very rough approximations. They show, however, that these elements exist in igneous rocks in determinable quantities. The elements not included in the calculation represent minor corrections, to be applied whenever the necessity for doing so may arise. For estimates of their probable amounts, the papers by Vogt[1] and Kemp[2] can be consulted. It is probable that no one of them would reach 0.01 per cent. The elements not mentioned in the table cannot amount to more than 0.5 per cent. altogether; and even that small figure is likely to be an overestimate.

	Shale. A.	Sandstone. B.	Limestone. C.
SiO_2	58.38	78.66	5.19
Al_2O_3	15.47	4.78	.81
Fe_2O_3	4.03	1.08	.54
FeO	2.46	.30	
MgO	2.45	1.17	7.90
CaO	3.12	5.52	42.61
Na_2O	1.31	.45	.05
K_2O	3.25	1.32	.33
H_2O at 110°	1.34	.31	.21
H_2O above 110°	3.68	1.33[3]	.56[3]
TiO_2	.65	.25	.06
CO_2	2.64	5.04	41.58
P_2O_5	.17	.08	.04
S	......	......	.09
SO_3	.65	.07	.05
Cl	......	trace	.02
BaO	.05	.05	none
SrO	none	none	none
MnO	trace	trace	.05
Li_2O	trace	trace	trace
C, organic	.81	......	......
	100.46	100.41	100.09

Before we can finally determine the composition of the lithosphere, the sedimentary rocks must be taken into account; and to do this we must ascertain their relative quantity. First, however, we may consider their composition, which has been determined by

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[1] *Zeitsch. prakt. Geol.*, 1898, pp. 225, 314, 377, 413, and 1899, pp. 10, 274.

[2] *Science*, Jan. 5, 1906, and *Economic Geology*, vol. 1, pp. 207.

[3] Includes organic matter.

means of composite analyses. That is, instead of averaging analyses, average mixtures of many rocks were prepared,[1] and these were analyzed once for all. The results appear in the preceding table.

A. Composite analyses of 78 shales; or more strictly, the average of two smaller composites, properly weighted.

B. Composite analysis of 253 sandstones.

C. Composite analysis of 345 limestones.

In attempting to compare these analyses with the average composition of the igneous rocks, we must remember that they do not represent definite substances, but mixtures shading into one another. The average limestone contains some clay and sand, the average shale contains some calcium carbonate. Furthermore, they do not cover all of the products derived from the decomposition of the primitive rock, for the great masses of sediments on the bottom of the ocean are left out of account. The analyses of the latter are too few to give conclusive averages, but the data published in the Challenger Reports[2] indicate a difference between them and the terrigenous deposits. The "red clay," for example, which covers 51,500,000 miles of the ocean floor, at its greatest depths, is much richer in iron than the average shale. In twenty-one analyses of this sediment a mean of 13.61 per cent. of ferric oxide was found. The thickness of this deposit is quite unknown. There are also metamorphic rocks to be considered, such as amphibolites and serpentines; although their quantities are presumably too small to seriously modify the final averages. They might, however, help to explain a deficiency of magnesium which appears in the sedimentary analyses. Partly on account of these considerations, and partly because the sedimentary rocks contain water and carbon dioxide which have been added to the original igneous material, we cannot recombine the composite analyses so as to exactly reproduce the composition of the primitive matter. To do this it would be necessary also to allow for the oceanic salts, which represent, in part at least, losses from the land; but that factor in the problem is perhaps the least

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[1] These mixtures were prepared by G. W. Stose, under the direction of G. K. Gilbert. The analyses were made by H. N. Stokes in the laboratory of the U. S. Geological Survey. See Bull. 228, p. 20.

[2] "Volume on Deep Sea Deposits," p. 198.

embarrassing. Its magnitude is easily estimated, and it gives a measure of the extent to which the igneous rocks have been decomposed.

If we assume that all of the sodium in the ocean was derived from the leaching of the primitive rocks, and that the average composition of the latter is correct, it is easy to show that the marine portion is very nearly one thirtieth of that contained in the ten-mile lithosphere. That is, the complete decomposition of a shell of igneous rock, one third of a mile thick, would yield all the sodium in the ocean. Some sodium, however, is retained by the sediments, and the analyses show that it is about one third of the total amount. That is, the oceanic sodium represents two thirds of the decomposition, and the estimate must, therefore, be increased one half. On this basis, a rocky shell one half mile thick, completely enveloping the globe, would slightly exceed the amount needed to furnish the sodium of the sea and the sediments. No probable change in the composition of the lithosphere can modify this estimate very considerably; and since the ocean may contain primitive sodium, not derived from the rocks, the half mile must be regarded as a maximum allowance. If the primeval rocks were richer in sodium than those of the present day, a smaller mass of them would suffice; if poorer, more would be needed to account for the salt in the sea. Of the two suppositions, the former is the more probable; but neither assumption is necessary. If, however, we assume that our igneous rocks are not altogether primary, but that some of them represent re-fused or metamorphosed sedimentaries, we must conclude that they have been partly leached and have, therefore lost sodium. That is, the original matter was richer in sodium, and the half-mile estimate is consequently too large.

From another point of view, the thinness of the sediments can be simply illustrated. The superficial area of the earth is 199,712,000 square miles, of which 55,000,000 are land. According to Geikie,[1] the mean elevation of all the continents is 2,411 feet. Hence, if all of the land now above sea level were spread uniformly over the globe, it would form a shell about 660 feet thick. If we

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[1] "Textbook of Geology," 4th ed., vol. 1, p. 49.

assume this matter to be all sedimentary, which it certainly is not, and add to it any probable allowance for the sediments at the bottom of the sea, we shall still fall far short of the half-mile shell, which, on chemical evidence, is a maximum. In the following calculation this maximum will be taken for granted.

The relative proportions of the different sedimentary rocks within the half-mile shell can only be estimated approximately. Such an estimate is best made by studying the average igneous rock, and determining in what way it can break down. A statistical examination of about 700 igneous rocks, which had been described petrographically, leads to the following rough estimate of their mean mineralogical composition:

Quartz	12.0
Feldspars	59.5
Hornblende and pyroxene	16.8
Mica	3.8
Accessory minerals	7.9
	100.0

The average limestone contains 76 per cent. of calcium carbonate, and the composite analyses of shales and sandstones correspond to the subjoined percentages of component minerals:

	Shale.	Sandstone.
Quartz[1]	22.3	66.8
Feldspars	30.0	11.5
Clay[2]	25.0	6.6
Limonite	5.6	1.8
Carbonates	5.7	11.1
Other minerals	11.4	2.2
	100.0	100.0

If now we assume that all of the igneous quartz, 12 per cent., has become sandstone, it will yield 18 per cent. of that rock, which is evidently a maximum. Some quartz has remained in the shales. One hundred parts of the average igneous rock will form, on decomposition, less than 18 parts of sandstone.

The igneous rocks contain, as shown in the last analysis cited,

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[1] The total percentage of free silica.

[2] Probably sericite in part. In that case the feldspar figure becomes lower.

4.79 per cent. of lime. This would form 8.55 per cent. of calcium carbonate, or 11.2 per cent. of the average limestone. But at least half of the lime has remained in the other sediments, so that its true proportion can not reach 6 per cent., or one third the proportion of the sandstones. The remainder of the igneous material, plus some water and minus oceanic sodium, has formed the siliceous residues which are grouped under the vague title of shale. Broadly, then, we may estimate that the lithosphere, within the limits assumed in this memoir, contains 95 per cent. of igneous rock, with 5 per cent. of sedimentaries. If we assign 4.0 per cent. to the shales, 0.75 per cent. to the sandstones, and 0.25 per cent. to the limestones, we shall come as near to the truth as is possible with the present data.[1] On this basis, the average composition of the lithosphere may be summed up as follows. The analyses of the sedimentary rocks are recalculated to 100 per cent.:

	95 Per Cent. Igneous.	4 Per Cent. Shale	0.75 Per Cent. Sandstone	0.25 Per Cent. Limestone.	Average.
SiO_2	59.87	58.10	78.33	5.19	59.79
Al_2O_3	15.02	15.40	4.77	.81	14.92
Fe_2O_3	2.58	4.02	1.07	.54	2.63
FeO	3.40	2.45	.30	....	3.33
MgO	4.06	2.44	1.16	7.89	3.98
CaO	4.79	3.11	5.50	42.57	4.82
Na_2O	3.39	1.30	.45	.05	3.28
K_2O	2.93	3.24	1.31	.33	2.96
H_2O	1.86	5.00	1.63	.77	1.98
TiO_2	.72	.65	.25	.06	.71
ZrO_2	.03	....	....	....	.03
CO_2	.52	2.63	5.03	41.54	.74
P_2O_5	.26	.17	.08	.04	.25
S	.11	....	....	.09	.10
SO_3	....	.64	.07	.05	.02
Cl	.07	....	....	.02	.07
F	.02	....	....	....	.02
BaO	.11	.05	.05	....	.10
SrO	.04	....	....	....	.04
MnO	.10	....	....	.05	.09
NiO	.03	....	....	....	.03
Cr_2O_3	.05	....	....	....	.05
V_2O_3	.03	....	....	....	.02
Li_2O	.01	....	....	....	.01
C	....	.80	....	....	.03
	100.00	100.00	100.00	100.00	100.00

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[1] Van Hise, "A Treatise on Metamorphism," U. S. G. S. Monograph 47, p. 940, divides the sedimentary rocks into 65 per cent. shales, including all pelites and psephites, 30 per cent. sandstones, and 5 per cent. limestones.

The final average differs from that of the igneous rocks alone, only within the limits of uncertainty due to experimental errors, and the assumptions made as to the relative proportions of the sedimentaries. If the work were ideally exact, the last column of figures should differ from the first symmetrically, being higher in water and carbon dioxide and lower in all other constituents. Lime and potash, however, show small gains, which are abnormal, and indicative to some extent of the errors above mentioned. It is possible that excessive weight has been assigned to the limestones, but on that theme it is hardly worth while to speculate. The values chosen for the sediments are approximations only, and nothing more can be claimed for them. They seem to be near the truth; as near as we can approach with data which are necessarily imperfect, and so they may be allowed to stand without further emendation.

Now, with the help of this new average, we are in a position to compute the relative abundance of the chemical elements in all known terrestrial matter. For this purpose, the column is restated in elementary form, with an arbitrary allowance of 0.5 per cent. for all of the elements not specifically included in it. As for the atmosphere, it is represented in the final result by 0.02 per cent. of nitrogen, which is a little too low. The mean composition of the lithosphere, the ocean and the atmosphere, then, is as follows:

	93 Per Cent. Lithosphere.	7 Per Cent Ocean	Average, Including Nitrogen.
Oxygen	47.07	85.79	49.77
Silicon	28.06	....	26.08
Aluminum	7.90	....	7.34
Iron	4.43	....	4.11
Calcium	3.44	.05	3.19
Magnesium	2.40	.14	2.24
Sodium	2.43	1.14	2.33
Potassium	2.45	.04	2.28
Hydrogen	.22	10.67	.95
Titanium	.40	....	.39
Carbon	.20	.002	.18
Chlorine	.07	2.07	.21
Bromine	....	.008	....
Phosphorus	.11	....	.10
Sulphur	.11	.09	.10
Barium	.09	....	.09
Manganese	.07	....	.07
Strontium	.03	....	.03
Nitrogen	....	....	.02
Fluorine	.02	....	.02
All other elements	.50	....	.50
	100.00	100.00	100.00

It would be foolish to ascribe any high degree of accuracy to these figures, for the data are confessedly of very unequal value. They do show, however, clearly and conclusively, the order of magnitudes with which we have to deal. We may claim to know, for example, that oxygen forms about one half of all known terrestrial matter and silicon about one fourth. Next in order of abundance comes aluminum, then iron, and then calcium, followed by magnesium, sodium and potassium in nearly equal proportions. So much is established, and we are also able to say that certain other elements appear in minor, but determinable, amounts. In its general drift, the table is satisfactory; but its details are subject to revision. The question now is, what legitimate uses can be made of it? What problems can it help us to solve?

My answers to these questions, I fear, can hardly be satisfactory. The more closely I scrutinize the uses which have been made of the averages, the more questionable I find them to be. For instance, it is possible to compute from the average chemical or mineralogical composition of the igneous rocks their average physical properties; and as the component data in such a calculation are usually quantities of similar magnitude, the results obtained will probably be quite near the truth. Mr. W. H. Emmons,[1] for example, has determined, from one of my averages, the average mineralogical composition to which it corresponds, using for that purpose the norms of the new quantitative classification. Then, from the known coefficients of expansion of the minerals, he has calculated the mean coefficient for the igneous crust of the earth. The value found is 0.0000199, and its uncertainty cannot be very large. But, after all, what can be done with the figure? If we use it to discuss the swellings and shrinkings of the lithosphere, we are limited to surface phenomena alone, in which the disturbances due to cracks and crevices in the rocks are exceedingly large. If we go below the surface, to a point where the rocks are presumably continuous, this particular coefficient of expansion ceases to be applicable, for it has been modified by changes in temperature and by increased pressure. In other words, our constant is only a constant under

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[1] In Chamberlin and Salisbury's "Geology," vol. 1, p. 546.

surface conditions, and at five or ten miles below the surface it must assume a different and unknown value. The coefficient becomes larger as the temperature rises, but the influence of increased pressure is undetermined. In short, the data as they stand to-day are inapplicable to discussions of this kind; although it is conceivable that future discoveries may enable us to eliminate the difficulties that now exist. When the physical properties of rocks and minerals shall have been measured under widely differing conditions of temperature and pressure, we may be able to apply the data to such averages as I have given; and so assist in the solution of geophysical problems.

Professor Van Hise[1] has attempted to combine my third average for the igneous rocks with the composite analyses of the sedimentaries, in order to determine the redistribution of the elements during the processes of metamorphism. In so doing he has assumed that shales, sandstones and limestones exist respectively in the proportions of 65, 30 and 5 per cent. of the sedimentary rocks; and upon recombining the data with allowances for matter contained in the ocean, he found various excesses and deficiencies. These differences from the parent rock he seeks to explain; but it seems to me that his efforts are premature. The data are not yet sufficiently precise to justify so elaborate a discussion, as a comparison of Mr. Washington's average with mine will show. Furthermore, as I have already observed, several important factors in the problem remain to be determined. The metamorphosed sediments and the oceanic deposits are not covered by the composite analyses, and that relative to the shales is otherwise imperfect. It represents only 78 rocks, a number which is quite inadequate. A much larger amount of more varied material must be studied before the analyses of the igneous and sedimentary rocks can be well fitted together. Only the broadest relations are ascertainable now, as I have sought to show in the preceding pages. We can determine, roughly, the *maximum* relative mass of the sedimentaries, and also something of their proportional quantities; but much farther than that we are hardly yet ready to go. One suggestion, however, merely as a sug-

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[1] "A Treatise on Metamorphism," pp. 947–1002.

gestion, may be worth considering. The quantity of carbon dioxide locked up within the lithosphere is, as shown by the data now before us, about equal to twenty-five times the mass of the atmosphere. To that quantity must be added at least three more atmospheres, and perhaps a much larger quantity of oxygen which has been consumed in transforming the ferrous compounds of the igneous rocks into the ferric oxide of the sediments. We may well ask whether all of this material was actually absorbed from the atmosphere, and if so, whence was it derived? Did the primeval atmosphere contain it all at once, or was it drawn from cosmical sources, or expelled first from the earth's interior? These questions I shall not attempt to answer, for I do not care to enter the realm of speculation. It is enough for me to point out the order of the quantities involved in the problems thus suggested, problems which have been the themes of many writers, and leave them as crude data for possible future use.

One other phase of geochemical statistics remains to be mentioned; namely, the attempt to use chemical evidence in the measurement of geological time. At least two such efforts have been made; the one by T. Mellard Reade,[1] the other by Professor Joly.[2] Mr. Reade, from a study of the soluble substances contained in the surface waters of England and Wales, estimates that their removal from the rocks and soil would lower the level of those countries at the rate of one foot in 12,978 years. This means a transfer to the ocean of dissolved matter alone equivalent to 143.5 tons per annum from each square mile of land; and to this must be added the solid sediments. From various data relative to the drainage basins of Europe, and to some large rivers in other parts of the world, Reade calculates that the average denudation of all the land of the globe amounts to about 800 tons annually per square mile. This figure, combined with the supposition that the sediments represent a thickness of ten miles, gives a period of 526,000,000 years since the process of sedimentation began. The assumed thickness is evidently many times too great, and the figure is, therefore, excessive.

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[1] "Chemical Denudation in Relation to Geological Time," London, 1879.

[2] *Sci. Trans. Royal Dublin Soc.*, ser. 2, vol 7, p. 23. Alsa a note in *Chem. News*, vol. 83, p. 301.

In another calculation, Reade has estimated that the limestones are equal in bulk to a zone 528 feet thick completely enveloping the globe. Then, from the rate at which lime salts are carried from the land into the sea, he computes the time required to form the limestones as equal to 600,000,000 years. I may note here, in passing, that if my own estimate of the mass of the limestones is correct, this quantity should be divided by four, giving 150,000,000 years as the time needed for their formation. I do not care now to criticize Reade's calculations in detail, for his data were in many respects defective, and the conjectural element in his reasoning was very large. In spite of these obvious objections, however, Reade's work has shown certain statistical possibilities, and has pointed out a line of investigation that may be profitably followed. Previous to the appearance of Reade's memoir the mechanical sediments had been used in estimating geological time, but the dissolved matter, which is of nearly equal importance and much more easily measurable, was neglected.

Professor Joly's line of attack upon the time problem was analagous to Reade's, but different. Taking as his fundamental datum the quantity of sodium in the ocean, and then estimating the annual amount of sodium brought in by waters from the land, he was able to compute the time required for the oceanic accumulation. This method of calculation is simple, direct and clear, provided the rate of supply has been constant, and that all corrections are known and applied. Uncorrected, the time needed for the observed accumulation is 99,400,000 years, which is evidently a maximum. Corrected, by a questionable allowance for pre-sedimentary sodium in the ocean, and for cyclic salts lifted by winds from the sea and returned to it again, the estimate is reduced to 89,300,000 years. If we admit that Joly's data are correct, we may round off his figures to between ninety and one hundred millions of years, and feel reasonably confident that the time of sedimentation is a quantity of that order.[1]

There are two weak points in Joly's calculation. First, the mag-

[1] For discussions of Joly's memoir see Mackie, *Trans. Edin. Geol. Soc.*, vol. 8, p. 240; Fisher, *Geol. Mag.*, 1900, p. 124; Ackroyd, *Chem. News*, vol. 83, p. 265; 84, p. 56; and *Geol. Mag.*, Aug. and Oct. 1901. See also Sollas, "The Age of the Earth," p. 21.

nitudes of his corrections are uncertain, and secondly, our knowledge of the contribution made by rivers to the sea is most imperfect. Joly has used Sir John Murray's estimate of the composition of river water,[1] and that rests upon insufficient data. Murray has averaged together the analyses of nineteen rivers, which are not named, but which were presumably, for the most part, European. Data are lacking for the great African and Asiatic rivers, the Nile excepted, and the other available material is incomplete. A river varies in composition from time to time and from place to place; so that a single analysis of it may be misleading in the highest degree. And yet many of the published analyses are of that character; that is, they represent single samples of water from river systems in which the local and annual variations may be very large. Furthermore, Murray's average is mainly, if not wholly, of waters from the temperate zone; from which, in all probability, tropical and arctic waters may differ considerably. I speak thus advisedly, for I have compared, and reduced to uniform standards, more than a hundred analyses of river waters, and have noted their wide variations. For a very few streams the average annual composition is known, and from such averages, weighted in accordance with the areas drained, the final estimate must be made. The data are now being gathered; the nature of the work to be done is well understood, and in a few years it may be possible to replace Murray's average with one of a more definite character. Then, and not till then, can Joly's method be profitably applied to the discussion of geological time. His results may or may not be seriously modified, but the conclusions reached will be more definite than any we can attain to now. At all events, the present rate of chemical denudation is a measurable quantity, and it will form an important statistical datum for use in the investigation of various problems.

In conclusion I may be permitted to urge upon chemists and geologists the importance of the statistical method in the investigation of large geochemical problems. The method is evidently applicable in many cases, and leads to conclusions of positive value. We need, however, better material to work with than we have now,

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[1] *Scottish Geographical Magazine*, 1887, p. 76.

and I have indicated some of the desiderata. From chemical evidence we can draw deductions relative to the volumes and masses of the sediments, and also gain something towards the measurement of geological time. Even if the conclusions to be reached by these methods are not final, they are at least helpful. In order to be convincing, the chemical evidence must follow lines convergent with other testimony, and when it does so we may be satisfied that we are approaching the truth.

ON A POSSIBLE REVERSAL OF DEEP-SEA CIRCULATION AND ITS INFLUENCE ON GEOLOGIC CLIMATES.[1]

BY THOMAS C. CHAMBERLIN.

(Read April 18, 1906.)

Among the multitude of subjects that drew illuminating thought from the cosmopolitan philosopher to whom we pay our homage, were the phenomena of the atmosphere and the ocean. Aside from atmspheric electricity, certain climatic phenomena were subjects of his special study, as we are to learn more fully in the course of these memorial exercises. But Benjamin Franklin was above all a student of human affairs and his physical inquiries were instinctively correlated with human interests. If, therefore, in treating a phase of oceanic circulation and its bearings on geologic climates, I associate my subject frankly with the interests of our race, I trust it may be assigned to a desire to bring my contribution into harmony with the spirit of the distinguished American philosopher of the eighteenth century.

The control of secular climates is obviously a condition prerequisite to biologic continuity. The preservation of a narrow range of temperature and a limited variation of atmospheric constituents throughout the millions of years of the biologic past was absolutely essential to organic evolution. Continued preservation for millions of years to come seems equally a condition precedent to an intellectual and spiritual evolution commensurate with the physical and biological evolutions that have preceded it. Only such a prolonged evolution of the intellectuality now just dawning gives full moral satisfaction to our conception of the sum-total of terrestrial history.

The narrowness of the range to which temperatures must be confined to permit progressive organic and intellectual evolution takes on its true meaning only when we recall that the natural tem-

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[1] Presented by permission of the President of the Carnegie Institution under whose auspices these studies have been prosecuted.

perature-range on the earth's surface is sixteen times as great as this, while that affecting the solar family is at least sixty times as great. For a hundred million years, more or less, this narrow range of temperature has been maintained quite without break of continuity, unless geologists and biologists are altogether in error in their inductions. On the further maintenance of this continuity hang future interests of transcendent moment.

So too the maintenance of a narrow range of atmospheric constitution, notably in the critical element carbon dioxide, has been equally indispensable. These two critical limitations of temperature and of constitution seem also to have been interdependently correlated with one another.

The climatic problem is as difficult as it is important. The factors are so many, so elusive, so imperfectly determined, perhaps even so imperfectly determinable, that the utmost patience and assiduity are a duty of the investigator, and the utmost charity of judgment an obligation of fellow scientists. I am persuaded, however, that tentative analyses of the tangle of factors are an indispensable aid to the future solution of the problem. One of the gravest difficulties confronting us to-day is the imperfection of observations and the inconclusiveness of experimentation; and this arises in no small degree from the lack of such patient preliminary analyses of the problem as shall bring into sharp recognition the occult things that are to be observed and the precise experimental determinations which alone can really aid in the solution. If the little contribution of this half hour shall have any value at all, it will lie in its suggestive relations to the larger problem of secular climates, past and prospective.

As this larger problem has recently assumed, with some of us, a phase much at variance with its more familiar aspects, it may need to be briefly sketched. It has been customary to assign to the primitive earth a climate quite beyond the Miltonian conception of Gehenna in its fiery intensity, and to predict an impending refrigeration scarcely inferior in antithetic supremacy. The familiar conception of the sum-total of atmospheric history as a decline from one excess to another as the sequence of thermal wastage, is a logical deduction from the hypothetical derivation of the earth from a gaseous or quasi-gaseous nebula through gravitative condensation. To some of us, however, such a derivation seems inconsistent with the dynamics of the present solar system, and an alternative hypothesis has been formulated to meet the supposed requirements of existing phenomena. The acceptance of this requires a reconstruction of the whole conception of geologic climates. The new view discards the primitive molten state as a necessary condition and presents the alternative of a slow growth of the earth by planetesimal accessions. This alternative involves a slow growth of the atmosphere also, until it reached a volume similar to the present, when its growth is assumed to have been arrested and thereafter limited by the interplay of opposing agencies. These agencies are thought to have held it ever since within so narrow a range of oscillation as to foster organic evolution. A continuance of the same control offers ground for hope of a perpetuation of conditions congenial to organic and intellectual life, through a period to which no definite limits can now be set beyond the presumption that there must ultimately be a limit. The inevitable cooling of a once white-hot earth plays no part in this prognosis. The agencies of atmospheric *maintenance* and *control* thus force themselves upon consideration as factors of supreme importance.

The assigned agencies of atmospheric *restraint* are molecular velocities, chemical combination and condensation. By virtue of the first, the lighter constituents are reduced to a minimum and all constituents are restricted within certain large limits. By virtue of the second, the chemically active factors are kept down to states of dilution compatible with organic evolution, while the inert elements have probably been permitted to increase steadily. By the third, the excess of water-vapor has been condensed into the ocean, which has probably increased rather than diminished through the ages.

The postulated agencies of atmospheric *supply* are accessions from without and emanations from within, of which Vesuvius is just now giving us an impressive illustration.

To the interplay of these opposing agencies of loss and gain is assigned the maintenance of the requisite narrow range of atmospheric constitution, of temperature, and of associated conditions. Under this general resetting of fundamental conceptions, the question of climatic regulation takes on very concrete aspects and presents specific lines of study.

Subsidiary to these narrow limitations, the recognition of pronounced variations is forced upon us by a growing mass of geologic evidence. Throughout most of the well-known geologic periods, the poleward distribution of life implies warm climates, even as high as 70° and 80° of latitude. How life of sub-tropical types could have survived the long polar nights is one of the most obdurate puzzles of the earth's climatology. It becomes all the more strenuous if we cast aside all resort to an early fervid state and a molten interior. Quite irrespective of primitive conceptions, however, the edge of the problem has sharpened as we have been forced to recognize that *between* the warm polar stages there were episodes of glaciation in strangely low latitudes. It appears necessary now to accept as demonstrative the evidences of extensive glaciation in India, Australia and South Africa in the midst of the later coal-forming stages of the Paleozoic era. The glacial beds lie even between coal beds of Permian or Permo-Carboniferous age; while, strangely enough, the areas of glaciation approach and even overlap the tropics of Cancer and Capricorn. And yet, figs and magnolias have grown in Greenland since, and mild polar climates are as well authenticated after as before this climateric glaciation. Less complete evidences from China[1] and Norway imply a very much earlier glaciation, falling in the oldest Cambrian or perhaps even pre-Cambrian times.

The climatic student seems therefore compelled to face oscillations within the known geologic periods ranging from sub-tropical congeniality within the polar circles, on the one hand, to glacial conditions in low latitudes, on the other, and these *in alternating succession:* while neither of these oscillations was permitted to swing across the narrow limital lines of organic endurance. There is little doubt that the ocean, the daughter of the atmosphere, is one of the most potential agencies in controlling these oscillations. It is one of its possible functions in such regulation that invites our present attention.

Some of the regulating functions of the ocean have long been

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[1] Willis, "Third Year-Book Carnegie Institution," 1904, p. 282.

recognized. Certain less familiar ones have been brought under study in recent years by a few students independently. Schlœsing was perhaps the first to clearly recognize that the carbon dioxide of the ocean is an important agency in the regulation of the atmospheric content of this critical factor. As early as 1880[1] he advanced the view that the carbon dioxide of the atmosphere is in equilibrium not only with the free carbon dioxide absorbed in the sea water but through dissociation with the second equivalent of carbon dioxide in the oceanic bicarbonates. The sum-total of such free and loosely combined carbon dioxide available at present as a possible supply for the atmosphere may be some twenty-five times the present atmospheric content. Schlœsing held that any depletion of the atmospheric content would be followed by emanation from the ocean, and any excess acquired by the atmosphere would be followed by oceanic absorption, and hence great changes in the atmospheric content would only be brought about by reducing or increasing the large sum-total of atmospheric and oceanic supply. This was a contribution of the first order to the problem of atmospheric regulation. It is necessary for a geologist, however, to recognize that the exchange, and even the equilibrium itself, are dependent on geological and physical conditions. At periods in which the oceanic bicarbonates were most abundant, the amount of free and loose carbon-dioxide in the ocean may perhaps have reached thirty or forty times the present atmospheric content, while on the other hand it may have fallen to a very low figure when the ocean was depleted of carbonates. It is necessary also to recognize that the diffusion of gases in water, so far as it is covered by experiment, is a slow process, and computation seems to show that the supply of carbon dioxide to the atmosphere might be much too slow to offset its consumption under certain geologic conditions, unless effectively aided by oceanic circulation. The active superficial circulation immediately assignable to the winds would aid somewhat but its competency is limited. It was in an attempt to determine the functions of the deep-sea circulation in this interchange that the conceptions of this paper arose.

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[1] "Sur la constance de la proportion d'acide carbonique dans l'air," *Comp. Rend.*, 1880, t. 90, p. 1410.

In an endeavor to find some measure of the rate of the abysmal circulation, it became clear that the agencies which influenced the deep-sea movements in opposite phases were very nearly balanced. From this sprang the suggestion that if their relative values were changed to the extent implied by geological evidence there might be a reversal of the direction of the deep-sea circulation and that this might throw light on some of the strange climatic phenomena of the past and give us a new means of forecast of climatic states in the future.

That the deep-sea circulation is now actuated dominantly by polar agencies is clear from the low temperatures of the abysmal waters, even beneath the tropics. It is a firm inference that cold waters creep slowly along the depths from the polar seas equatorward where they gradually rise to the surface and return on more superficial routes. This is not, however, yet a matter of observation and the courses pursued are unknown. It is perhaps more probable that they are gyratory or spiral and complex than that they are simple and direct.

The agencies that affect oceanic circulation include at least: (1) wind, (2) atmospheric transfer, (3) differences of salinity, and (4) differences of temperature, including freezing and thawing. The earth's rotation of course modifies the currents but does not actuate them.

1. The effect of the wind is superficial and familiar, and need only be considered here in so far as it affects the deep-sea circulation. Its currents constitute horizontal circuits, and their frictional effect upon the deep currents is probably slight and of a gyratory phase in the main. In so far as they are strictly horizontal, they doubtless favor equally poleward and equatorward movement in the abysmal waters. If there is a component of their sum-total that favors the piling up of waters in the polar regions, it must favor the present deep circulation. If the opposite is true, it must antagonize it. There seems no way at present to measure the relative amounts of these opposing tendencies. It is plausible enough to reason that the cold air from the polar regions would flow more largely at the base of the atmosphere than would the warmer air from the equatorial regions and that the polar winds would thus

antagonize the present abysmal circulation. But theoretical deductions are rarely sure-footed in these complex subjects. The balance of influence, whatever it may be, is probably so slight as to be negligible.

2. We cannot here attempt to follow empirically the transfers of water by evaporation and precipitation, but general inspection seems to indicate the nature of the average effect. The saturation point of the atmosphere falls progressively from the equator to the poles, and the actual humidity runs roughly parallel to it on the grand average. Poleward movement of the atmosphere leads therefore to a lower content of moisture; equatorward movement to a higher. As the acquisition of moisture lags behind the capacity to hold it, it is a rather firm inference that precipitation exceeds evaporation in the high latitudes and that evaporation exceeds precipitation in the low latitudes, on the grand average. The bearing of observational data is of the same import. The result of these ratios of precipitation and evaporation is a raising of the ocean surface by fresh waters in the polar regions and a lowering of it in the low latitudes accompanied there by concentration of saline constituents. Considered alone and ideally, this should give a slight equatorward gradient and a flow of fresh surface waters in that direction. These fresh waters, however, mingle with the superficial sea waters and involve a movement of these also toward the equator. So far as these affect abysmal movement, they antagonize the present circulation.

3. In so far as evaporation exceeds precipitation in the low latitudes, it results in an increased salinity of the superficial waters and a tendency of these to sink and flow poleward to replace the salt waters carried equatorward by the fresh waters as just observed. If these were the only factors it seems clear that the deep circulation would be poleward.

4. On the other hand, the lower temperatures of the high latitudes increase the density of the water and tend to cause it to sink and flow equatorward. But the low temperatures affect primarily the superficial stratum which is freshened by the superior precipitation of the high latitudes, and both computation and observation show that cold fresher waters may float upon warmer saline waters.

A large part of this cold superficial water flows away in surface currents to lower latitudes.

In view of these complications, the precise mode by which polar agencies control the deep circulation is much less obvious than it might at first seem. There is ground to suspect that the formation and melting of ice is an important factor. In freezing, the salt and gases of the surface layer are largely forced out into the underlying layer. If the surface layer has an average degree of salinity, the underlayer is super-charged, and being also cold, must tend to sink. On the borders of the ice-covered tracts where the precipitation and melting are considerable and where adjacent polar lands pour in much fresh water, the surface layers are so much fresher than the average sea-water that the concentration of salinity by freezing does not overbalance the original freshness. But in those polar regions where there is no inflowage from the land, where precipitation is slight and almost wholly snow, which accumulates on a previously frozen surface and absorbs most of its own summer melting, and where the ice is borne away to lower latitudes and the waters arising from it do not redilute the concentrated waters, it is believed that a sufficient degree of saline concentration, combined with depression of temperature takes place to cause an effective downward movement. This is believed to cooperate with diffusion and conduction in giving the lower body of polar waters the superior gravity which actuates the abysmal circulation. The sea immediately bordering Antarctica and that lying northwest of Greenland seem to furnish these conditions. Moss[1] and Krogh[2] independently have found that at times of northwesterly wind, the air west of Greenland contains about double the usual content of carbon dioxide. This I have suggested may come from waters overcharged with it by the freezing of the overlying layer.

It is not to be inferred, however, that the deep-sea waters derived from the polar regions exceed in salinity the waters of the evaporating tracts of low latitudes, but merely that by this concentration through freezing conjoined with low temperature and modified by

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[1] Moss, "Notes on Arctic Air," *Proc. Roy. Dublin Soc.*, Vol. II, 1880.

[2] Krogh, "Abnormal CO_2 Percentage in the Air of Greenland," etc., *Meddelelser om Gronland*, Vol. XXVI, 1804, pp. 409–411.

diffusion and mechanical mixture, water of superior gravity is derived and that this controls the abysmal circulation.

Dr. Otto Pettersson, in an elaborate article, supports by experiment and observation the theory of Bjerknes, that the *melting* of the polar ice also promotes circulation, both superficial and deep-seated, but I can only make reference to this here.[1]

A survey of the existing temperatures and salinities of the ocean also makes it clear that the battle between temperature and salinity is a close one and that no profound change is necessary to turn the balance. The combined results of the many polar expeditions have shown that in the high latitudes of both hemispheres there is a superficial sheet of water two hundred to three hundred meters deep that is colder, but lighter, than that below, because it is fresher. It floats upon a warmer, more saline body of water below. This has been specially demonstrated by the investigations of Nansen.[2] This layer of coldest water moves to lower latitudes superficially in the main, showing that coldness alone is not determinative.

In the open Pacific and Indian oceans hydrostatic equilibrium must be very closely maintained, because of the slight resistance to adjustment. It is shown by the charts of Dr. Alexander Buchan[3] that the concentrated warm saline waters form inverted cone-like masses that reach down some four thousand feet or more. It thus appears that they lie in the same horizons as great masses of colder waters which their salinity must counterbalance. Less striking phenomena of similar import mark the evaporating areas of the north and south Atlantic. The equatorial tracts of freshened waters arising from high precipitation are scarcely traceable to half the depth. This seems to imply that in the low latitudes increased density due to evaporation is more potent than freshening by precipitation, in harmony with theory as already set forth, and that the density due to salinity is not greatly over-matched by the low tempera-

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[1] "On the Influence of Ice-melting on Oceanic Circulation," *Geog. Jour.*, XXIV., 1904, pp. 285–333.

[2] "The Norwegian North Polar Expedition, 1893–1896, Scientific Results," Fridjof Nansen, Vol. II, Oceanography of the North Polar Basin.

[3] "Challenger Reports," Summary of Results, Pt. II, Appendix, Rept. on Oceanic Circulation.

ture density of the Antarctic regions from which the Pacific and Indian oceans are not separated by appreciable barriers.

An interesting illustration of the close balance between salinity-density and temperature-density is presented by the saline waters that issue from the Mediterranean in which evaporation is in excess of combined precipitation and inflow from adjacent lands. As a result, the concentrated waters that form the deeper body of the Mediterranean creep out through the bottom section of the Straits of Gibraltar, while the upper section is occupied by a compensating inflow from the Atlantic. Although the straits are shallow, the out-creeping current does not appear in the upper horizons of the adjacent Atlantic waters, according to Buchan's charts, but descends to depths of three thousand to five thousand feet before it finds a horizon of density-equilibrium. It then spreads westerly in a great spatulate wedge across the north Atlantic and occupies the larger part of its area between the depths of four thousand and five thousand feet. (See the maps of Buchan.) It is warmer and more saline than the normal oceanic waters at its horizon, and lies on colder but less saline waters below.

These and similar phenomena point to a notable closeness of the balance between the density effects of salinity and of temperature respectively. More saline but warmer waters both overlie and underlie less saline but colder waters. On the whole, however, at present, the temperature effects are dominant and cold waters occupy the abysmal depths of all the great oceans.

A comparative computation of salinity-effects and of temperature-effects on density, from such data as are now available, leads to a similar conclusion relative to the closeness of balance between the opposing agencies, but this cannot be entered upon here.

Now, as previously remarked, the geological record gives good evidence that in the majority of known periods the temperatures in the polar regions were subtropical or warm temperate. Freezing must apparently have been a trivial factor, if not quite absent, and low temperature was robbed of its chief densifying effects. Evaporation in the zones of descending air currents in low latitudes must apparently have been operative, in some degree at least, to furnish the geological agencies which the record implies. Deposits of salt and gypsum in not a few periods testify directly to regional aridity.

The most marked of these are, to be sure, referable to the periods of glaciation, but many of them have no such assignable association.

In these periods of warm polar temperature there is reason to believe that the high-latitude temperature-effects fell below the low-latitude concentration-effects and that therefore the deep oceanic circulation was actuated by the dense waters of the evaporating tracts. These may then be supposed to have slowly descended and crept poleward, acquiring a trivial amount of heat from the earth's interior and loosing some to the waters above, but substantially maintaining their temperatures until they rose to the surface in the polar regions and gave their warmth to the atmosphere. Aided by the enshrouding mantle of vapors that must have arisen from such a body of water, it is conceived that the mild temperatures requisite for the maintenance of the recorded life through the polar nights may have been thus maintained.

If this be granted, however, it is wise to note that this is not a radical solution of the climatic problem, for a fundamental cause for the conditions that brought on freezing at one period and prevented it at others is prerequisite to the postulated influence of these in the reversal of the abysmal circulation. At the best, our suggestion offers only an auxiliary agency in the control of secular climates. Some more fundamental agency or agencies must be sought.

UNIVERSITY OF CHICAGO,
April 16, 1906.

AN INTERNATIONAL SOUTHERN TELESCOPE.

By EDWARD C. PICKERING.

(*Read April 18, 1906.*)

It is difficult to find a department in the arts or sciences which was not studied by that eminent and practical man, Benjamin Franklin. As his interests were mainly in the practical side of life, it is surprising that one of the least practical subjects, the study of the appearance of the heavenly bodies, should have attracted him. Yet we find that he was probably the first to bring a reflecting telescope to this country. It illustrates the widespread and keen desire of man to probe more and more deeply the sidereal universe. It is also remarkable that the reflector, after falling into disuse for many years, should now appear to be the form of telescope best adapted to this end. The object of the present paper is to propose a practical plan by which a telescope of the largest size should be so constructed and used as to lead to results of the greatest astronomical value.

So careful a study has been made of astronomy, during the last half century, that it is not easy to secure a real advance. We must learn from the success attained in industrial enterprises, and spare no pains to secure the best possible conditions in every respect, however trivial. The best location, the best form, the cost, the method of administration, and the discussion of the results will be considered in turn. It is in the last of these that the greatest advance may be expected. An attempt will be made to show how these results can be discussed, not by an individual or single institution, but by the astronomers of the world, and how numerous departments of astronomy may thus be advanced to a higher plane.

LOCATION.

If we take a map of the world and mark upon it the principal observatories, we shall find that nearly all of them are in locations especially unsuited to good astronomical work. Almost all are near large cities, capitals of countries, or great universities. These are centers of civilization, since the climate is temperate and frequent rains promote agriculture, inland navigation, and the support of large populations. The very conditions that have rendered man's progress successful are those most unfavorable to good astronomical work. Besides these, smoke, electric lights, and jars, all fatal to the most careful study of the stars, accompany the growth of large cities. If we divide the earth into cloudy and clear halves, nine tenths of the observatories will lie in the cloudy regions.

There are three extensive clear regions upon the earth. The first and largest includes nearly all of the interior of northern Africa. There is no large observatory in that region. The second is in South Africa. The only large observatory there is in Capetown, an exceptional cloudy part of that region. The third region is the interior of Australia. The principal observatories are on the coast, at Melbourne and Sydney.

If we arrange observatories according to latitude, we find that six sevenths of them are between latitudes + 35° and + 60°, or the latitudes of Spain and Scotland. A large part of the southern sky, containing many of the most interesting objects, can never be seen from the observatories of the United States or Europe. If we are to erect the greatest telescope in the world it will have a much wider field of usefulness if placed in the southern hemisphere, where comparatively neglected objects can be studied.

A location should be selected at a considerable elevation, to avoid the dust and haze of the lower atmosphere. These form the greatest obstacles to the use of a large telescope, and their effect is thus reduced to a minimum. In this respect no place is comparable with South America, where one railway attains an elevation of 17,000 feet.

Two locations suggest themselves, the west coast of South America and South Africa. The Harvard College Observatory after careful study, selected a point near Arequipa, Peru. For the last seventeen years it has maintained a station there, at an elevation of 8,000 feet. It is doubtful whether a better location can be found, although it is open to two objections. It is so near the equator that objects near the south pole are always low, and clouds are much more frequent during the summer, from November to March, than during the remainder of the year. If we go further south, the pole is higher, but the weather is more cloudy. Sir David Gill, Director of the Cape Observatory, recommends Blomfontein. It is one of the most promising locations. It is thirty degrees south of the equator, and the pole is accordingly at that height.

FORM OF INSTRUMENT.

In the time of Franklin, mainly through the triumphs of Sir William Herschel, the reflector was considered the best form of telescope. This form has been frequently used ever since in England, but until recently, it was seldom employed on the continent or in this country. Dr. Henry Draper, nearly half a century ago, recognizing the advantages of the reflector, constructed and used with success one of the largest yet made in this country. One firm, Alvan Clark & Sons, revolutionized public opinion regarding the best form of telescope. The desire to possess the largest telescope in the world has been a common one. There is perhaps no form of memorial which has been more widely known and admired. Five times the Clarks filled an order for the largest telescope in the world, and, in each case except the first, the previous record was their own. They accomplished this by making successively for the Mississippi, Washington, Pulkowa, Lick, and Yerkes Observatories telescopes of 18, 26, 30, 36, and 40 inches aperture. In each case the telescope was a complete success. They proved one of their principles, that whenever they could see an error they could correct it. The limit of size of telescopes of this form seems, however, to be nearly reached. The cost is very great, and the engineering difficulties become serious in the largest instruments.

The genius of one man, the late James E. Keeler, Director of the Lick Observatory, again revolutionized the views of astronomers regarding the advantages of the reflector over the refractor. Having secured a three-foot reflector which previously had done but little work, he obtained with it photographs of extraordinary perfection. Similar results have since been obtained with the two-foot reflector of the Yerkes Observatory. It has thus been shown that, in certain departments of astronomy, especially in photographing faint stars and nebulæ, results could be obtained far beyond those which had been secured with any form of refractor. One great advantage of the reflector is its low price. The cost of a mirror is about one tenth of that of a lens of the same size. The great defect of large refractors, the color of the images, due to chromatic aberration, is not present in reflectors. The loss of light by absorption increases rapidly with the size of the refractor, and not at all with a reflector. The difference in focus of rays of different colors is so great with a large refractor that the small dispersion needed for photographing the spectra of faint stars cannot be used. No such difference exists with a reflector. On the other hand, the reflector is much more sensitive to changes of temperature or flexure, and the silver surface becomes tarnished and must be renewed at intervals. Nevertheless, in a very large instrument the advantages of a reflector far outweigh those of a refractor.

It is therefore proposed that the telescope shall be a reflector having an aperture of about seven feet, and a focal length of forty-four feet, thus giving images on a scale of 15″ to the millimeter.

Let us imagine the instrument completed, and describe its probable construction. The polar axis is enclosed in an iron cylinder, resembling a boiler, and resting in water according to the method adopted by Mr. Common in the construction of the sixty-inch reflector now at Cambridge. There will be no difficulty from freezing, as the instrument will doubtless eventually be erected in a location having a warm climate. It would be better, if possible, to counterpoise the telescope, taking a large part of the weight off the bearings of the polar axis by a series of ball bearings, if motion of sufficient uniformity can thus be secured. Electric motors furnish abundant power for the motions in right ascension and declination, and a motor controlled by a clock is used for following. This method was employed with entire success in the Harvard telescope, 135 feet long, sent to Jamaica in 1901, and in other telescopes. (See *Astrophysical Journal,* XV, 202.)

The photographic plate is placed at the principal focus of the telescope. For visual work, this is replaced by an inclined mirror which reflects the beam of light to the side of the tube. It then falls upon an astronomical objective of five or six inches aperture, and after undergoing a second reflection, is brought to a focus in the prolongation of the declination axis of the telescope. It is here enlarged by an eyepiece in the usual way. All objects on the meridian, or at the same hour angle, are thus viewed by the observer without changing his position. He and his recorder are enclosed in a small observing room which protects them from the wind, and which may be warmed if desired. When the object is near the meridian, the observer is looking horizontally, and east or west. As the object moves, the inclination of the line of sight gradually changes, about fifteen degrees an hour. It is probable that the instrument will be used principally for photographic work, and the same method will be employed for following. The possibility of observing a distant object in this way has been established at Harvard, since 1870, with the two eight-inch collimating telescopes of the meridian circle. The images compared in this case are nearly forty feet apart. An important use of the instrument will be in photographing the spectra of faint stars. These will be taken in two ways. A concave and convex lens are inserted near the focal plane of the telescope, and between this plane and the principal mirror. Their positions are such that between them the cone of rays of each star is parallel. A prism is inserted as described more fully in Harvard Circular 108. For measuring the approach and recession of faint stars, as described in Harvard Circular 110, a similar device is employed. As a large dispersion is required, the prism has such an angle that the cone of rays is inclined, and comes to a focus outside the tube of the telescope. The photographic plate, therefore, does not intercept any part of the incident rays. The reversed spectrum is formed by turning the lenses, prism, and plate 180°.

Cost.

To establish an observatory of the first class is a costly operation. The expenditure for plant, land, buildings, and instruments should be two or three hundred thousand dollars. The annual income of the Greenwich, Paris, Pulkowa, and Harvard observatories is about fifty thousand dollars, in each case. To secure this permanently at four per cent. the sum of $1,250,000 would be required. Accordingly, the total cost would be $1,500,000. To duplicate the resources of the U. S. Naval Observatory would involve an expenditure of at least double this sum, or $3,000,000. A sixth part of the last named sum, or $500,000 would suffice to carry out the plan proposed in this paper. Figures can be given with a good deal of confidence since, at Harvard, we have had experience of a nearly similar character. A reflecting telescope of two feet aperture and its mounting have recently been constructed at a cost of less than $4,000. For a larger telescope we may assume that the cost of drawings and plans will be proportional to the first power, the cost of the machine- and handwork to the square, and of the material, to the cube of the dimensions. At this rate, telescopes of six, seven, and eight feet aperture would cost $42,900, $63,000, and $87,400, respectively. The five-foot telescope at Harvard cost us much less than this rule would imply, but the conditions under which it was acquired were exceptional. Assuming the cost to be proportional to the cube of the dimensions, we have the cost in the three cases $108,000, $171,500, and $256,000. The actual cost would probably lie between these rather wide limits, but it is believed that a telescope of seven-feet aperture and mounting could be constructed for $150,000.

The current expenses can be closely estimated since, for seventeen years, the Harvard Observatory has conducted an auxiliary observing station in South America. Ten thousand dollars a year would be needed to carry on the proposed station satisfactorily. To produce this sum, allowing four per cent. interest, $250,000 would be required. With the income, three or four assistants could be maintained, who would keep the telescope at work throughout every clear night, and perhaps some smaller instruments. A certain amount of the income would be available for publication and for subsidies paid to astronomers here or in other countries, for assistants who would aid them in measuring and discussing the photographs. Before the large telescope is completed the interest on the principal would defray the expenses of the preliminary work of testing locations with smaller instruments, erecting houses for the observers and similar work. If the fund had an independent foundation, an additional $100,000 would be required for an executive for the management of the fund, etc. This would be saved if the superintendence would be undertaken without charge by the Harvard or some other existing observatory. The entire amount required would, therefore, be $400,000 or $500,000, which at most would only be a third of that required for an observatory of the first class and of the usual form.

Method of Administration.

The administration and management of the fund would, of course, rest with the donor. If it were left to me, I should at once write to the principal makers of glass for estimates of the cost and time required to furnish a disk of glass seven feet in diameter and one foot thick. An expedition to South Africa would next be planned, equipped with the two-foot reflector of the Harvard Observatory. This instrument would be mounted in the best available location, and regular work undertaken which would test the steadiness and other qualities of the atmosphere. Tests would also be made of various adjacent localities, with refracting telescopes of four, five or six inches aperture. Meanwhile, correspondence would be opened with all those astronomers likely to give useful advice, and a committee would be formed of such astronomers as would attend a meeting at an early date. Thus, no time would be lost. The form of mounting would be the principal subject to be discussed at the first meeting, and the work of construction would be begun as soon as this point was settled. The results of the first expedition would probably serve to determine whether a better location could be found in South Africa than that we now occupy in Peru.

Discussion of Results.

Not only from its size, but from its exceptional location, this telescope ought to give better results than those previously obtained in almost every department of astronomical science. Its principal use will be in photography, determining the positions, brightness, and spectra of faint stars, especially novæ and variables, in depicting clusters and nebulæ, in studying the distribution of faint stars, in discovering and following faint satellites and asteroids, in measuring parallaxes and proper motions, and, in general, in studying all the properties of stars beyond the reach of smaller instruments.

In visual work, very high powers could be employed, without the difficulties usually encountered from diffraction when a very small emergent pencil is used. Owing, also, to the great light gathering power, it is probable that visual observations of the surfaces of the outer planets, especially Uranus and Neptune, could be made to great advantage.

Evidently the material accumulated photographically would greatly exceed what could be properly discussed by a single individual or institution. Especial pains should be taken to place this material in the hands of any astronomers qualified to use it. The entire collection of photographs should eventually be kept together, where it could be consulted, but copies or enlargements of any portion or of the whole should be furnished at cost to any one desiring them. Qualified astronomers, ready to discuss any portion of the work, should be offered the use of the original negatives, given copies, and in every way aided in discussing and preparing the results for publication. It is desirable that they should be published in a separate series of quarto volumes. The fundamental principle should be that the results are for the world and not for a single individual, and every concession should be made to secure the widest use of the material collected. The telescope should be kept at work throughout every clear night. A scheme of work should be prepared every year by the aid of an international committee of astronomers, which should provide for a proper division of the time of the telescope, secure assistance and advice in discussing the results, and, in general, aid in obtaining the best administration. For instance, such a committee might spend several days together in New York, travelling and hotel expenses being paid, and care being taken that at least one European astronomer should be present each year. A German delegate might report that in his country a particular astronomer desired to study the distribution and brightness of the stars in globular clusters. A hundred hours might be assigned to this work and five photographs of each of ten clusters would be taken with exposures of two hours each. Contact prints would be made of these photographs and the originals sent to the German astronomer, who might be furnished with means for paying the salary of an assistant who would make the measures under his direction if the work was considered of sufficient importance. When the research was completed, the original negatives would be returned and added to the rest of the collection. The results would be printed in the series of annals, giving the author as many copies as he could usefully distribute. On special occasions, as during an opposition of Mars, a specialist might be invited to the observatory and the telescope placed, for the time, at his disposal. It would be difficult to find useful work for the telescope when the moon was full. Such researches as photometric measures of the relative brightness of the components of close double stars and studies of the moon and planets could be made at such times.

Conclusions.

To sum up the results of this paper, it may be said that the desire to have the largest telescope in the world and to carry our knowledge of the stars farther than has ever been done before, has been very widespread. It would be unwise to construct a refracting telescope much larger than those already made. A million and a half dollars would be required to duplicate one of our present observatories of the first class. A reflecting telescope of seven feet aperture, larger and more powerful than any hitherto constructed, could be made at a moderate price. It should be mounted in the best possible location as regards climate, and preferably in the southern hemisphere, to permit the study of neglected regions. Such an instrument would produce, by photography, results in quality much better than can be obtained elsewhere and in such quantity that no single institution could discuss and publish them. These photographs should be distributed throughout the world, and astronomers of all countries would thus be furnished with better material for study than they could possibly obtain themselves. They would also be offered every aid in discussing and publishing their conclusions.

The estimated cost of carrying out this plan is not more than half a million dollars, or one third of that of an observatory of the usual form as now constructed. Not only would results be obtained superior to those now secured anywhere else, but the work would be planned, not by a single astronomer, but by an international committee of astronomers, and the results would be discussed by the most distinguished specialists in each department. In this way, following the example of the great industrial enterprises of the country, the plan of work would be improved continually in every detail.

It will be difficult during the twentieth century to make as great an advance in science as was done during the life of Franklin in the eighteenth century or after his death in the nineteenth century. How could the name of Franklin be more highly honored than by initiating this undertaking at his bi-centennial? Were he living, is there any way that would be more in accordance with his wishes and aims in life than to advance a science, not only in a direction, but by a method, which would bring together, as here proposed, experts from all parts of the world in a single field of work.

It is not easy for a man who by life-long work and skill has accumulated a large fortune to expend it wisely in science and to his own satisfaction. It is hard for him to see it wasted or yielding inadequate results. Money thus given should be expended, as it has been acquired, by careful management and the use of strict business methods, in order to obtain the greatest return for every disbursement.

The name of a donor could in no way be better immortalized than by associating it with such a real advance in the greatest problem to the solution of which the mind of man has aspired,—the study of the sidereal universe.

THE HUMAN HARVEST.

By DAVID STARR JORDAN.

(Read April 18, 1906.)

Science is wisdom set in order. It is known as science by its orderly arrangement, but above and beyond all matters of arrangement the wisdom itself must take rank. Wisdom is the essence of human experience, the contact of mind with the order of nature. Of all men of his time, Benjamin Franklin was preëminently a man of wisdom. By the same token the first leader in science in America, he still takes rank with the greatest.

So in this time of historic recognition, it is proper that a speaker of to-day should find his message in the words of Benjamin Franklin, and the message I choose is one for which this City of Philadelphia has always stood and from which it has taken its Greek name, the name which in classical phrase says with a single word that men are brothers worthy of our love. It is a message for which the State of Pennsylvania has always stood, for the same principle was embodied in the life of William Penn. This has always been a Quaker City, and the Quakers, the Friends, have been our best apostles of the gospel of "peace on earth, good will towards men," the culmination of social and political wisdom.

Benjamin Franklin once said, "All war is bad; some wars worse than others." Then, once again, in more explicit terms, referring to the dark shadow of war cast over scenes of peace, the evil of the standing army, Franklin said to Baynes:

"If one power singly were to reduce its standing army it would be instantly overrun by other nations. Yet I think there is one effect of a standing army which must in time be felt so as to bring about the abolition of the system. A standing army not only diminishes the population of a country, but even the size and breed of the human species. For an army is the flower of the nation. All

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[1] Parton's "Life of Franklin," II, p. 572.

the most vigorous, stout, and well-made men in a kingdom are to be found in the army, and these men in general cannot marry."[1]

What is true of standing armies is far more true of armies that fight and fall; for as Franklin said again, "Wars are not paid for in war times: the bill comes later."

In the discussion of the principles involved in Franklin's words, I must lay before you four fragments of history, three stories told because they are true, and one parable not true, but told for the lesson it teaches. And this is the first: Once there was a man strong, wealthy and patient, who dreamed of a finer type of horse than had ever yet existed. This horse should be handsome, clean-limbed, intelligent, docile, strong and swift. These traits were to be not those of one horse alone, a number of a favored equine aristocracy, they were to be "bred in the bone" so that they would continue from generation to generation, the attributes of a special common type of horse. And with this dream ever before his waking eyes, he invoked for his aid, the four twin genii of organic life, the four by which all the magic of transformism of species has been accomplished either in nature or in art. And these forces once in his service, he left to their control all the plans included in his great ambition. These four genii or fates are not strangers to us, nor were they new to the human race. Being so great and so strong, they are invisible to all save those who seek them. Men who deal with them after the fashion of science give them commonplace names, variation, heredity, segregation, selection.

Because not all horses are alike, because in fact no two were ever quite the same, the first appeal was made to the genius of Variation. Looking over the world of horses, he found to his hand Kentucky race horses, clean-limbed, handsome and fleet, some more so and others less. So those which had the most of the virtues of the horse which was to be were chosen to be blended in new creation. Then again, he found thoroughbred horses of Arabian stock, hardy and strong and intelligent. These virtues were needed in the production of the perfect horse. And here came the need of the second genius, who is called Heredity. With the crossing of the racer with the thoroughbred, all qualities of both were blended in the progeny. The next generation partook of all desirable traits and again of undesirable ones as well. Some the one, and some the other, for sire and dam alike had given the stamp of its own kind and for the most part in equal degree. But again never in a degree quite equal, and in some measure these matters varied with each sire and each dam, and with each colt of all their progeny. It was found that the progeny of the mare called Beautiful Bells excelled all others in retaining all that was good in fine horses, and in rejecting all that a noble horse should not have. And like virtues were attached to the sires called Palo Alto, Electricity and Electioneer.

But there were horses and horses; horses not of the chosen breed, and should these enter the fold with their common blood it would endanger all that had been already accomplished. For the ideal horse mating with the common horse controls at the best but half the traits of the progeny. If the strain were to be established, the vulgar horse flesh must be kept away, and only the best remain in association with the best. Thus Segregation, the third of the genii was called into service lest the successes of this herd be lost in the failure of some other.

Under the spell of Heredity all the horses partook of the charm of Beautiful Bells and of Electricity and of Palo Alto, for firmly and persistently all others were banished from their presence. There were some who were not strong, some who were not sleek, some who were not fleet, some who were not clean-limbed, nor docile, nor intelligent. At least, they were not so to the degree which the dream of fair horses demanded. By the force of Selection, all such were sent away. Variation was always at work making one colt unlike another; Heredity made each colt a blend or mosaic of traits of sire and of grandsires and granddams; Selection left only good traits to form this mosaic, and the grandsire and granddam, sire and dam, and the rest of the ancestry lived their lives again in the expanding circle of descent.

Thus in the final result, the horses who were left were the horses of their owner's dream. The future of the breed was fixed, and fixed at the beginning by the very framing of the conditions under which it lived. It is variation which gives better as well as worse. It is heredity which saves all that has been attained—for better or for worse. It is selection by which better triumphs over worse, and it is segregation which protects the final result from falling again into the grasp of the general average. In all this, selection is the vital moving changing force. It throws the shaping of the future on the individual chosen by the present. The horse who is left marks the future of his kind. The history of the steed is an elongation of the history of those who are chosen for parentage. And with the best of the best chosen for parentage, the best of the best appears in the progeny. The horse-harvest is good in each generation. As the seed we sow, so shall we reap.

And this story is true, known to thousands of men. And it will be true again just as often as men may try to carry it into experiment. And it will be true not of horses alone, for the four fates which guide and guard life have no partiality for horses but work just as persistently for cattle or sheep, or plums or roses, or calla or cactus, as they do for horses or for men. From the very beginning of life they have wrought untiringly—and in your life and in mine—in the grass of the field, the trees of the forest—in bird and beast, everywhere we find the traces of their energy.

And this brings me to my second story, which is not true as history, but only in its way as parable.

There was once a man—strenuous no doubt, but not wise, for he did not give heed to the real nature of things and so he set himself to do by his own unaided hand the work which only the genii can accomplish. And this man possessed also a stud of horses. They were docile, clean-limbed, fleet, and strong and he would make them still more strong and swift. So he rode them swiftly with all his might—day and night, always on the course, always pushed to the utmost, leaving only the dull and sluggish to remain in the stalls. For it was his dream to fill these horses with the spirit of action, with the glory of swift motion, that this glory might be carried on and on to the last generation of horses. There were some who could not keep the pace, and to these and these alone he assigned the burden of bearing colts. And the feeble and the broken, the dull of wit, the coarse of limb, became each year the mothers of the colts. The horses who were chosen for the race-course he trained with every care, and every stroke of discipline showed itself in the flashing eyes and straining muscles, such were the best horses. But the other horses were the horses who were left. From their loins came the next generation and with these then was less fire and less speed than the first horses possessed in such large measure. But still the rush went on—whip and spur made good the lack of native movement. The racers still pushed on the course, while in the stalls and paddocks at home, the dull and common horses bore their dull and common colts. Variation was still at work with these as patiently as ever. Heredity followed, repeating faithfully whatever was left to her. Segregation, always conservative, guarded her own, but could not make good the deficiencies. Selection, forced to act perversely, chose for the future the worst and not the best, as was her usual fashion. So the current of life ran steadily downward. The herd was degenerating because it was each year an inferior herd which bred. Each generation yielded weaker colts, rougher, duller, clumsier colts, and no amount of training or lash or whip or spur made any permanent difference for the better. The *horse-harvest* was bad. Thoroughbred and race-horse gave place to common beasts, for in the removal of the noble the ignoble always finds its opportunity. It is always the horse that remains which determines the future of the stud.

In like fashion from the man who is left flows the current of human history.

This tale then is a parable, a story of what never was. but which is always trying to become true.

Once there was a great king—and the nation over which he bore rule lay on the flanks of a mountain range, spreading across fair hills and valleys green and fertile across to the Mediterranean Sea. And the men of his race, fair and strong, self-reliant and self-confident, men of courage and men of action, were men "who knew no want they could not fill for themselves." They knew none on whom they looked down, and none to whom they regarded themselves inferior. And for all things which men could accomplish, these plowmen of the Tiber and the Apennines felt themselves fully comptent and adequate. "Vir," they called themselves in their own tongue, and *virile,* virilis, men like them are called to this day. It was the weakling and the slave who was crowded to the wall; the man of courage begat descendents. In each generation and from generation to generation the human harvest was good. And the great wise king who ruled them; but here my story halts—for there was no king. There could be none. For it was written, men fit to be called men, men who are *Vires,* "are too self-willed, too independent, and too self-centred to be ruled by anybody but themselves." Kings are for weaklings, not for men. Men free-born control their own destinies. "The fault is not in our stars, but in ourselves that we are underlings." For it was later said of these same days: "there was a Brutus once, who would have brooked the Eternal Devil to take his seat in Rome, as easily as a king." And so there was no king to cherish and control these men his subjects. The spirit of freedom was the only ruler they knew, and this spirit being herself metaphoric called to her aid the four great genii which create and recreate nations. Variation was ever at work, while heredity held fast all that she developed. Segregation in her mountain fastnesses held the world away, and selection chose the best and for the best purposes, casting aside the weakly, and the slave, holding the man for the man's work, and ever the man's work was at home, building the cities, subduing the forests, draining the marshes, adjusting the customs and statutes, preparing for the new generations. So the men begat sons of men after their own fashion, and the men of strength and courage were ever dominant. The Spirit of Freedom was a wise master, cares wisely for all that he controls.

So in the early days, when Romans were men, when Rome was small, without glory, without riches, without colonies and without slaves, these were the days of Roman greatness.

Then the Spirit of Freedom little by little gave way to the Spirit of Domination. Conscious of power, men sought to exercise it, not on themselves but on one another. Little by little, this meant banding together, aggression, suppression, plunder, struggle, glory, and all that goes with the pomp and circumstance of war. The individuality of men was lost in the aggrandizement of the few. Independence was swallowed up in ambition, patriotism came to have a new meaning. It was transferred from the hearth and home to the trail of the army.

It does not matter to us now what were the details of the subsequent history of Rome. We have now to consider only a single factor. In science, this factor is known as "reversal of selection." "Send forth the best ye breed!" That was the word of the Roman war-call. And the spirit of Domination took these words literally, and the best were sent forth. In the conquests of Rome, *Vir,* the real man, went forth to battle and to the work of foreign invasion, *Homo,* the human being, remained in the farm and the workshop and begat the new generations. Thus "Vir gave place to Homo." The sons of real men gave place to the sons of scullions, stable-boys, slaves, camp-followers, and the riff-raff of those the great victorious army does not want.

The fall of Rome was not due to luxury, effeminacy, corruption, the wickedness of Nero and Caligula, the weakness of the train of Constantine's worthless descendants. It was fixed at Philippi, when the spirit of domination was victorious over the spirit of freedom. It was fixed still earlier, in the rise of consuls and triumvirates and the fall of the simple sturdy self-sufficient race who would brook no arbitrary ruler. When the real men fell in war, or were left in far-away colonies, the life of Rome still went on. But it was a different type of Roman which continued it, and this new type repeated in Roman history its weakling parentage.

"It is puerile," says Charles Ferguson, "to suppose that kingdoms are made by kings. The kings could do nothing if the mob did not throw up its cap when the king rides by. The king is consented to by the mob, because of that in him which is mob-like. The mob loves glory and prizes, so does the king. If he loved beauty and justice, the mob would shout for him while the fine words were sounding in the air, but he could never celebrate a jubilee or establish a dynasty. When the crowd gets ready to demand justice and beauty, it becomes a democracy and has done with kings."

Thus we read in Roman history the rise of the mob and of the emperor who is the mob's exponent. It is not the presence of the emperor which makes imperialism. It is the absence of the people, the want of men. Babies in their day have been emperors. A wooden image would serve the same purpose. More than once it has served it. The decline of a people can have but one cause, the decline in the type from which it draws its sires. A herd of cattle can degenerate in no other way than this, and a race of men is under the same laws. By the rise in absolute power, as a sort of historical barometer, we may mark the decline in the breed of the people. We see this in the history of Rome. The conditional power of Julius Cæsar, resting on his own tremendous personality, showed that the days were past of Cincinnatus and of Junius Brutus. The power of Augustus showed the same. But the decline went on. It is written that "the little finger of Constantine was thicker than the loins of Augustus." The emperor in the time of Claudius and Caligula was not the strong man who held in check all lesser men and organizations. He was the creature of the mob, and the mob, intoxicated with its own work, worshipped him as divine. Doubtless the last emperor, Augustulus Romulus, before he was thrown into the scrap-heap of history, was regarded in the mob's eyes and his own as the most godlike of them all.

What have the historians to say of these matters? Very few have grasped the full significance of their own words, for very few have looked on men as organisms, and on nations as dependent on the specific character of the organisms destined for their reproduction.

So far as I know, Benjamin Franklin was the first to think of man thus as an inhabitant, a species in nature among other species and dependent on nature's forces as other animals and other inhabitants must be.

In Otto Seeck's great history of "The Downfall of the Ancient World" (Der Untergang der Antiken Welt), he finds this downfall due solely to the rooting out of the best ("Die Ausrottung der Besten"). The historian of the "Decline and Fall of the Roman Empire" or any other empire is engaged solely with the details of the process by which the best men are exterminated. Speaking of Greece, Dr. Seeck says, "A wealth of force of spirit went down in the suicidal wars." "In Rome, Marius and Cinna slew the aristocrats by hundreds and thousands. Sulla destroyed the democrats, and not less thoroughly. Whatever of strong blood survived, fell as an offering to the proscription of the Triumvirate." "The Romans had less of spontaneous force to lose than the Greeks. Thus desolation came to them sooner. Whoever was bold enough to rise politically in Rome was almost without exception thrown to the ground. *Only cowards remained and from their brood came forward the new generations.* Cowardice showed itself in lack of originality and in slavish following of masters and traditions."

The Romans of the Republic could not have made the history of the Roman Empire. In their hands it would have been still a republic. Could they have held aloof from world-conquering schemes, Rome might have remained a republic, enduring even to our own day. The seeds of destruction lie not in the race nor in the form of government, but in the influences by which the best men are cut off from the work of parenthood.

"The Roman Empire," says Seeley, "perished for want of men." The dire scarcity of men is noted even by Julius Cæsar. And at the same time it is noted that there are men enough. Rome was filling up like an overflowing marsh. Men of a certain type were plenty, "people with guano in their composition," to use Emerson's striking phrase, but the self-reliant farmers, the hardy dwellers on the flanks of the Apennines, the Roman men of the early Roman days, these were fast going, and with the change in the breed came the change in Roman history.

"The mainspring of the Roman army for centuries had been the patient strength and courage, capacity for enduring hardships, instinctive submission to military discipline of the population that lined the Apennines."

With the Antonines came "a period of sterility and barrenness in human beings." "*The human harvest was bad.*" Bounties were offered for marriage. Penalties were devised against race-suicide. "Marriage," says Metellus, "is a duty which, however painful, every citizen ought manfully to discharge." Wars were conducted in the face of a declining birth rate, and this decline in quality and quantity of the human harvest engaged very early the attention of the wise men of Rome.

"The effect of the wars was that the ranks of the small farmers were decimated, while the number of slaves who did not serve in the army multiplied" (Bury).

Thus "*Vir* gave place to *Homo*," real men to mere human beings. There were always men enough such as they were. "A hencoop will be filled, whatever the (original) number of hens," said Benjamin Franklin. And thus the mob filled Rome. No wonder the mob-leader, the mob-hero rose in relative importance. No wonder "the little finger of Constantine was thicker than the loins of Augustus." No wonder that "if Tiberius chastised his subjects with whips, Valentinian chastised them with scorpions."

"Government having assumed godhead took at the same time the appurtenances of it. Officials multiplied. Subjects lost their rights. Abject fear paralyzed the people and those that ruled were intoxicated with insolence and cruelty." "The worst government is that which is most worshipped as divine." "The emperor possessed in the army an overwhelming force over which citizens had no influence, which was totally deaf to reason or eloquence, which had no patriotism because it had no country, which had no humanity because it had no domestic ties." "There runs through Roman literature a brigand's and barbarian's contempt for honest industry." "Roman civilization was not a creative kind, it was military, that is destructive." What was the end of it all? The nation bred real men no more. To cultivate the Roman fields "whole tribes were borrowed." The man of the quick eye and the strong arm, gave place to the slave, the scullion, the pariah, the man with the hoe, the man whose lot does not change because in him there lies no power to change it. "Slaves have wrongs, but freemen alone have rights." So at the end the Roman world yielded to the barbaric, because it was weaker in force. "The barbarians settled and peopled the barbaric rather than conquered it." And the process is recorded in history as the fall of Rome.

"Out of every hundred thousand strong men, eighty thousand were slain. Out of every hundred thousand weaklings, ninety to ninety-five thousand were left to survive." This is Dr. Seeck's calculation, and the biological significance of such mathematics must be evident at once. Dr. Seeck speaks with scorn of the idea that Rome fell from the decay of old age, from the corruption of luxury, from neglect of military tactics or from the over-diffusion of culture.

"It is inconceivable that the mass of Romans suffered from over-culture." "In condemning the sinful luxury of wealthy Romans, we forget that the trade-lords of the fifteenth and sixteenth centuries were scarcely inferior in this regard to Lucullus and Apicius, their waste and luxury not constituting the slightest check to the advance of the nations to which these men belonged. The people who lived in luxury in Rome were scattered more thinly than in any modern state of Europe. The masses lived at all times more poorly and frugally because they could do nothing else. Can we conceive that a war force of untold millions of people is rendered effeminate by the luxury of a few hundreds?"

"Too long have historians looked on the rich and noble as marking the fate of the world. Half the Roman Empire was made up of rough barbarians untouched by Greek or Roman culture."

"Whatever the remote and ultimate cause may have been, the immediate cause to which the fall of the empire can be traced is a physical not a moral decay. In valor, discipline and science the Roman armies remained what they had always been and the peasant emperors of Illyricum were worthy successors of Cincinnatus and Caius Marius. But the problem was, how to replenish those armies. Men were wanting. The Empire perished for want of men" (Seeley).

Does history ever repeat itself? It always does if it is true history. If it does not we are dealing not with history but with mere succession of incidents. Like causes produce like effects, just as often as may may choose to test them. Whenever men use a nation for the test, poor seed yields a poor fruition. Where the weakling and the coward survives in human history, there "the human harvest is bad," and it can never be otherwise.

The finest Roman province, a leader in the Roman world, was her colony of Hispania. What of Spain in history? What of Spain to-day? "This is Castile," said a Spanish writer, "she makes men and wastes them." "This sublime and terrible phrase," says another writer, "sums up Spanish history."

In 1630, according to Captain Calkins, the Augustinian friar, La Puente, thus summed up the fate of Spain:

"Against the credit for redeemed souls, I set the cost of armadas and the sacrifice of soldiers and friars sent to the Philippines. And this I count the chief loss: for mines give silver and forests give timber, but only Spain gives Spaniards, and she may give so many that she may be left desolate and constrained to bring up strangers' children instead of her own."

Another of the noblest of Roman provinces was Gallia, the favored land, in which the best of the Romans, the Franks and the Northmen have mingled their blood to produce a nation of men, hopefully leaders in the arts of peace, fatally leaders also in the arts of war.

To-day we are told by Frenchmen that France is a decadent nation. This is a confession of judgment, not an accusation of hostile rivals. It does not mean that the slums of Paris are destructive of human life. That we know elsewhere. Each great city has its great burdens, and these fall hard on those at the bottom of the layers of society. There is degradation in all great cities, but the great cities are not the whole of France. It is claimed that the decadence is deep-seated, not individual. It is said that the birth-rate is steadily falling, that the average stature of men is lower by two inches at least than it was a century ago, that the physical force is less among the peasants at their homes. Legoyt tells us that "it will take long periods of peace and plenty before France can recover the tall statures mowed down in the wars of the republic and the first empire." What is the cause of all this? Intemperance, vice, misdirected education, bureaucracy and the rush toward ready made careers? These may be symptoms. They are not causes. Demolins asks in that clever volume of his: "In what constitutes the superiority of the Anglo-Saxon?" Before we answer this, let us inquire in what constitutes the inferiority of the Latin races? If we admit this inferiority exists in any degree, and if we answer it in any degree, we find in the background the causes of the fall of Greece, the fall of Rome, the fall of Spain. We find the spirit of domination, the spirit of glory, the spirit of war, the final survival of subserviency, of cowardice and of sterility. The man who is left holds in his grasp the history of the future. The evolution of a race is always selective, never collective. Collective evolution among men or beasts, the movement upward or downward of the whole as a whole, irrespective of training or selection does not exist. As Lepouge has said, " It exists in rhetoric, not in truth nor in history."

The survival of the fittest in the struggle for existence is the primal moving cause of race progress and of race changes. In the red stress of human history, this natural process of selection is sometimes reversed. A reversal of selection is the beginning of degradation. It is degradation itself. Can we see the fall of Rome in the downfall of France? Let us look again at the history. A single short part of it will be enough. It will give us the clue to the rest.

In the Wiertz gallery in Brussels is a wonderful painting, dating from the time of Waterloo, called Napoleon in Hell. It represents the great marshal with folded arms and face unmoved descending slowly to the land of the shades. Before him, filling all the background of the picture with every expression of countenance are the men sent before him by the unbridled ambition of Napoleon. Three millions and seventy thousand there were in all—so history tells us, more than half of them Frenchmen. They are not all shown in one picture. They are only hinted at. And behind the millions shown or hinted at are the millions on millions of men who might have been and are not—the huge widening human wedge of the possible descendants of the men who fell in battle. These men of Napoleon's armies were the youth without blemish, " the best that the nation could bring," chosen as " food for powder," " ere evening to be trampled like the grass," in the rush of Napoleon's great battles. These men came from the plow, from the work-shop, from the school, the best there were—those from eighteen to thirty-five years of age at first, but afterwards the older and the younger. " A boy will stop a bullet as well as a man "; this maxim is accredited to Napoleon. " The more vigorous and well born a young man is," says Novicow, " the more normally constituted, the greater his chance to be slain by musket or magazine, the rifled cannon and other similar engines of civilization." Among those destroyed by Napoleon were " the élite of Europe." "Napoleon," says Otto Seeck, "in a series of years seized all the youth of high stature and left them scattered over many battle fields, so that the French people who followed them are mostly men of smaller stature. More than once in France since Napoleon's time has the military limit been lowered."

I need not tell again the story of Napoleon's campaigns. It began with the United States, the justice and helpfulness of the Code Napoléon, the prowess of the brave lieutenant whose military skill and intrepidity had caused him to deserve well of his nation.

The spirit of freedom gave way to the spirit of domination. The path of glory is one which descends easily. Campaign followed campaign, against enemies, against neutrals, against friends. The trail of glory crossed the Alps to Italy and to Egypt, crossed Switzerland to Austria, crossed Germany to Russia. Conscription followed victory and victory and conscription debased the human species. *"The human harvest was bad."* The first consul became the emperor. The servant of the people became the founder of the dynasty. Again conscription after conscription. "Let them die with arms in their hands. Their death is glorious, and it will be avenged. You can always fill the places of soldiers." These were Napoleon's words when Dupont surrendered his army in Spain to save the lives of a doomed battalion.

More conscription. After the battle of Wagram, we are told, the French began to feel their weakness, the Grand Army was not the army which fought at Ulm and Jena. "Raw conscripts raised before their time and hurriedly drafted into the line had impaired its steadiness."

On to Moscow,[1] "amidst ever-deepening misery they struggled on, until of the 600,000 men who had proudly crossed the Niemen for the conquest of Russia, only 20,000 famished, frost-bitten, unarmed spectres staggered across the bridge of Korno in the middle of December."

"Despite the loss of the most splendid army marshalled by man, Napoleon abated no whit of his resolve to dominate Germany and discipline Russia. ". . . He strained every effort to call the youth

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[1] These quotations are from the "History of Napoleon," I, by J. H. Rose.

of the empire to arms . . . and 350,000 conscripts were promised by the Senate. The mighty swirl of the Moscow campaign sucked in 150,000 lads of under twenty years of age into the devouring vortex." "The peasantry gave up their sons as food for cannon." But "many were appalled at the frightful drain on the nation's strength." "In less than half a year after the loss of half a million men a new army nearly as numerous was marshalled under the imperial eagles. But the majority were young, untrained troops, and it was remarked that the conscripts born in the year of Terror had not the stamina of the earlier levies. Brave they were, superbly brave, and the emperor sought by every means to breathe into them his indomitable spirit." "Truly the emperor could make boys heroes, but he could never repair the losses of 1812." "Soldiers were wanting, youths were dragged forth." The human harvest was at its very worst.

And the sequel of it all is the decadence of France. In the presence of war—of war on such a mighty ruthless and ruinous scale—one does not have to look far to find in what constitutes the superiority of the Anglo-Saxon. And we see the truth in Franklin's words, the deeper truth of their deeper wisdom: "Men do not pay for war in war time; the bill comes later."

Another wise man, Ralph Waldo Emerson, has used these words: "Man has but one future, and that is predetermined in his lobes." "All the privilege and all the legislation in the world cannot meddle or help. How shall a man escape from his ancestors or draw off from his veins the black drop?"

It is related that Guizot once asked this question of James Russell Lowell, "How long will the republic endure?" "So long as the ideas of its founders remain dominant," was the answer. But again we have this question: "How long will the ideas of its founders remain dominant?" Just so long as the blood of the founders remains dominant in the blood of its people. Not necessarily the blood of the Puritans and the Virginians alone, the original creators of the land of free states. We must not read our history so narrowly as that. It is the blood of free-born men, be they Roman, Frank, Saxon, Norman, Dane, Goth or Samurai. It is a free stock which creates a free nation. Our republic shall endure so long as the human harvest is good, so long as the movement of history, the progress of peace and industry leaves for the future not the worst but the best of each generation. The Republic of Rome lasted so long as there were Romans, the Republic of America will last so long as its people, in blood and in spirit, remain what we have learned to call Americans.

By the law of probabilities as developed by Quetelet, there will appear in each generation the same number of potential poets, artists, investigators, patriots, athletes and superior men of each degree.

But this law involves the theory of continuity of paternity, that in each generation a percentage practically equal of men of superior force or superior mentality should survive to take the responsibilities of parenthood. Otherwise Quetelet's law becomes subject to the operation of another law, the operation of reversed selection, or the biological "law of diminishing returns." In other words, breeding from an inferior stock is the sole agency in race degeneration, as selection natural or artificial along one line or another is the sole agency in race progress.

And all laws of probabilities and of averages are subject to a still higher law, the primal law of biology, which no cross-current of life can overrule or modify: *Like the seed is the harvest.*

HEREDITY AND VARIATION; LOGICAL AND BIOLOGICAL.

By WILLIAM KEITH BROOKS.

(Read April 20, 1906.)

One need know little of the current literature of biology to be aware that many hypotheses have been proposed to account for the resemblance of offspring to parent. This resemblance is commonly held to be due to the transmission of a substance of inheritance, and we are told that this substance is the residence of the species and the bearer of its qualities.

Reproduction is the transmission of living matter of some sort, and it is part of the legitimate work of biology to discover, by the scientific method of observation and experiment, what it is in the transmission of which reproduction consists; but it by no means follows that there is meaning in our words when we call that which is thus transmitted in reproduction the substance of heredity, or the bearer of the species.

So far as the word is used inductively in biology, heredity is the resemblance of child to parent, of offspring to ancestor, while the difference between child and parent is called variation. These words are also used metaphorically to designate the cause or the explanation of the resemblances and differences between descendants and ancestors, just as gravitation is used metaphorically to designate that which makes things gravitate, geotropism that which makes roots grow downwards, and selection that which brings about survival in the struggle for existence. In what I have to say I shall restrict myself to the inductive meaning of the words, for I know that your thoughts are so free from the bonds of metaphysics that you know we accomplish nothing by saying that heredity makes beings inherit, or that variation makes them vary, or that selection selects.

Let us consider the word inheritance as a term to designate the resemblance between child and parent. You know that while the descendant does, on the average, resemble its ancestors and collateral relatives more than it resembles anything else in nature, it is never identical with them. We say, in our careless way, that organisms exhibit specific identity behind or in spite of their individual diversity, when all we mean is that, while they resemble their parents, they are never identical with them. This diversity in unity is common to all natural objects, but it is most impressive in familiar living beings, in our friends and acquaintances, in our dogs and horses, and in the plants that we tend with our own hands. We may think of the casual stranger in the crowded street, or the unknown citizen of Timbuctoo, or the stalks in the cornfield that we pass in the train, as representatives of species and nothing more, but all the living beings we know practically we know as individual members of their kind.

If we are permitted to reason from the living beings we know best to those that concern us less, we must conclude that every living being is a unique member of its kind. It is more like its kind than like anything else in nature, but it is unique for there is nothing else in nature just like it. Reproduction is not the generation of like by like in any literal or mathematical sense. It is, rather, the generation of unique beings that are, on the average, more like their allies than they are like anything else in nature. We may for our own purposes, and in our minds, consider their kinship apart from their individuality, but this does not show that their kinship is separated from their individuality in fact. Living beings do not exhibit unity and diversity, but unity in diversity. These are not two facts but one. The delight of intimate acquaintance with animals is due to the inseparableness of their specific unity from their individuality, and our attempts to separate in our minds what is not separable in fact lead us to two narrow and imperfect views of the facts, two crude and unfinished mental concepts, neither of which corresponds to anything in nature.

All this is familiar, but I ask you to reflect upon it, to decide for yourselves whether it does not mean that inheritance or resemblance to ancestors, and variation or difference from ancestors, are only imperfect mental concepts; crude ideas, and not facts; whether the fact is not the individuality in kinship of living beings. Each of you must answer this simple question for himself. I cannot regard them as facts, as they seem to me to be only imperfect ideas of facts, mental states which have arisen through a partial and uncritical view of our experience, to the neglect of that which has not interested us nor seemed to concern us.

If you agree with me that resemblance to ancestors does not exist in nature separated from individuality or difference from ancestors, that inheritance is not a fact but an imperfect idea of facts, admitting of improvement by comparison with nature, and in no other way,—if you agree to this, what becomes of the notion of a substance of inheritance? There is, no doubt, a material equivalent for every mental concept, and the material equivalent of heredity may be in the brain of the speculative philosopher, for I cannot find it in living beings nor in germ cells nor in chromatin.

I hope you will not accuse me of opposing the scientific study of inheritance and variation, for nothing is farther from my intention. The resemblances and differences between ancestors and descendants are as worthy of study as arithmetic, which has been of inestimable value to mankind although there is in nature no quantity without quality.

We cannot make progress in natural knowledge without specializing; picking out what interests us and ignoring what does not seem to concern us; but specialization is not an unmixed benefit, and if it blinds our eyes to the real world that lies before them it may prove to be an unmitigated evil; leading the modern scientific man into the forlorn agnosticism of the ancient philosophers who held that we can never know anything because no real thing exists abstractly. Things do not cease to be because we fail to note them, and when we fix our attention upon some partial and imperfect conception of nature to the neglect of that which does not interest us, we may forget the reality of that which we have failed to consider, and we may thus be led to opinions which seem to be the logical conclusions of sound reasoning when they are but new illustrations of the threadbare fallacy of the undistributed middle—the fallacy which comes from mistaking a part for a whole.

The paradoxes into which the biologists fall in their efforts to locate the substance of inheritance remind me of the perplexity of the school boy, who, having tried to add together six horses and nine cows and five apples, wonders whether the result is horses or cows or apples. If he were to attribute the virtue of his arithmetic to a substance of numeration and to wonder whether it resides in cows or apples, he would be still more like those who speculate about the location of the substance of inheritance, and think they have put their finger on it when they have called it idioplasm.

If you choose to declare that my contention, that inheritance is not a fact, is a metaphysical subtilty, I cannot help it. Call me a metaphysician if you will. But may it not be the speculative biologist who hunts in germ cells and in their chromatin for the physical basis of the crudity of his ideas who is the true metaphysician, and not I, who plead for nothing but the correction of our scientific concepts and their reduction to exactness by comparison with nature?

Science is making marvellous revelations of the order that pervades the apparent disorder of nature, showing us, by the method of analysis and comprehension, the most wonderful and admirable evidence of regularity in the course of events that had seemed to be chaotic, but this statistical method deals with averages while the natural world is concrete. No living being is a statistical average, and it is the peculiar task of biological science to recall our attention to the diversity of the statistical data, thus making equally marvellous and equally instructive revelations of the inexhaustible variety and boundless wealth of nature, for science deals with progress and discovery, not with finality, and the test of truth is nature and not logic.

Statistical science shows that there is, on the average, about one chance in some thousands that the average human being will commit murder or suicide within the year, but my friend is not a two hundred thousandth of a murderer, and I prize him because there is no one like him.

The biometrician tells us of a standard or norm, from which living beings recede by variation, and to which they approximate by heredity, but the normal or average living being does not exist in nature. The student of statistical science talks glibly of the normal man as if he were a public character, the familiar acquaintance of men of intellect, and a well known face to even the common herd. The biologist declares that he knows no such person;

that all men are particular men, concrete and unique; that the normal man is a fictitious character, a statistical average, reached by ignoring all that is distinctive of each human being.

One can easily see why the notion that species is in germ cells has come to prevail. Nothing in nature, except the human mind, is easier to contemplate as an independent, self-sustaining, self-sufficient whole than is an egg. The symbolical comparison of the universe to an egg appeals to all, for nothing is easier than to think of an egg as a metaphysical thing in itself, a self-centred and self-sufficient microcosm. For many of the practical purposes of the scientific embryologist it is convenient and legitimate to regard it as a compete and self-sufficient being, but one must not forget what these practical purposes are, for the use of a concept for a practical purpose is apt to end in belief that it is true in general and useful for all purposes, and thus to entangle one in unforeseen paradoxes.

Every reflective biologist must know that no living being is self-sufficient, or would be what it is, or be at all, if it were not part of the natural world, although no truth is easier to lose sight of. Living things are real things, and we can never know too much about them, but their reality is in their interrelations with the rest of nature, and not in themselves.

Surely, this is good sense and good science. No physiologist who studies the waste and repair of living bodies; no naturalist who knows living beings in their homes; no experimental embryologist who studies the influence of conditions, internal and external, upon development, should, for an instant, admit that a living being is self-sustaining or self-sufficient, or that its being is in itself; for the line we draw, for our own convenience, between living things and the external world, is not one that we find in nature, but one that we make for our own purposes.

The external world of a living being is as essential to it as its histological structure. If the environment of its body, or of any cell within its body had been different, neither cell nor body would be what it is, and if they had no environment they would not be at all, for neither seeds nor eggs nor desiccated rotifers exist abstractly. A self-sufficient and self-sustaining living organism, whose being is in itself, is as fabulous as a griffin or a centaur, but no naturalist thinks, for an instant, that this truth casts any doubt upon the real existence of living things. While living things are real their reality or being is not absolute but dependent and relative.

One modern school of embryologists tells us that while the development of the egg into an individual organism is due to the reciprocal interaction between the germ and its environment, the species is in the germ as it is in itself; because, if it were not, like could not produce like. Like never does produce like, in any literal or absolute sense. If what has come about once may come about again under like conditions; it is among the possibilities of nature that a new animal kingdom, as rich and diversified as the one we know, might arise, in course of ages, from a starting point in the germ cells of some modern animal; for we know of nothing in the architecture of germ plasm that forbids.

If I venture at this late day to point out that ancestral development may be as epigenetic, from beginning to end, as individual development, and that the species for which we are seeking is not, and cannot be in the germ, I do so because the discovery is neither new nor original with me. It is so old that " up to date " zoologists tell us it is antiquated, abandoned, no longer worthy the attention of advanced thinkers.

According to this view, the species is not in chromatin, nor in germ cells, nor in differentiated cells, nor in gemmules, nor in idioplasm, nor in biophores nor in allelomorphs, nor in living beings at any stage of their existence, nor in the conditions of existence, because it is in that reciprocal interaction between the living being and the natural world, of which it is a part, which has been called the struggle for existence. Neither the stability of species nor the mutability of species is in living beings, because it is through extermination in the struggle for existence that the type is kept true to its kind, and also through this struggle that it becomes slowly changed.

You will note that it is as great an error to locate species in the external world as it is to locate it in germ cells, or in chromatin. It neither exists in the organism nor in the environment, because it is in the reciprocal interaction between the two. The biological types of which the biometricians tell us are neither external standards to which living beings approach and from which they recede by variation, nor are they standards fixed in living beings by heredity. Inheritance and variation are not two things, but two imperfect views of a single process, for the difference between them is neither in living beings nor in any external standard of extermination, but in the reciprocal interaction between each living being and its competitors and enemies and sources of food and the others conditions of life.

If the being of the individual organism is not in itself, but in the reciprocal interaction between it and its environment, and if the being of species is not in germ cells but in the reciprocal interaction between living beings and their environment, then the being of the canine species is of the same sort as the being of a dog, and that of everything else in nature.

Is it as a self-sufficient thing in itself, or as part of the universe, that the stone exhibits gravitation? "When Sir Isaac Newton made his speech about the child and the pebble: "Did he mean," asks Dr. Holmes, "to speak slightingly of a pebble? A body which knows all the currents of force that traverse the globe; which holds fast by invisible threads to the ring of Saturn and the belt of Orion." "This is certain," says Locke, "things however absolute and entire they seem in themselves, are but retainers to other parts of nature, for that which they are most taken notice of by us. Their observable qualities, actions, and powers, are owing to something without them; and there is not so complete and perfect a part that we know of nature, which does not owe the being it has, and the excellencies of it, to its neighbours; and we must not confine our thoughts within the surface of any body, but look a great deal farther, to comprehend perfectly those qualities that are in it."

Since these things are true, is it not time to have done, once for all, with the metaphysical, pre-Darwinian notion of species, as something that resides in germ cells and is handed down by a substance of heredity?

THE ELIMINATION OF VELOCITY EFFECTS IN MEASURING PRESSURES IN A FLUID STREAM.

By FRANCIS E. NIPHER.

(Read April 20, 1906.)

In determining either velocities or pressures in a current of air or water within a pipe, it has always been found exceedingly difficult to obtain a value which has any physical meaning.

A straight tube thrust through the side of the pipe, terminating at some point within, and connected to a guage, will transmit the pressure, but on account of the draught of the fluid across the open end of the tube, the fluid will also be drawn out, by an atomizer action.

If on the other hand the tube is in the form of a Pitot tube with its mouth directed towards the advancing current, it transmits both static and dynamic pressure to the gauge.

In the case of a building or other structure in a stream of air, if it is desired to find the variation of pressure on the surface due to the wind, any form of barometer gives misleading results by reason of the compressions and rarefactions around the instrument itself. Moreover the wind sweeps across or into various openings leading to the mercury surface or flexible diaphragm. This introduces disturbances or errors, which cannot be corrected. Engineers are accustomed to combine with the Pitot tube, a similar tube with its opening at right angles to the stream lines in a pipe, but they usually give warning that the atomizer action of the air blowing across the opening is likely to give misleading results, if the velocity is great.

In order to study the Pitot tube in a manner that would eliminate statical pressure, a long series of experiments was made from the window of a car. The tube was so mounted that the mouth of the tube could be directed at any angle, the position being determined on a graduated circle. The mouth of the tube could thus be directed towards the head of the train, towards the sky, the rear of the train, towards the ground and then towards the head of the train again. The setting could be made for each ten degrees. The zero angle was taken when the mouth of the tube was directed towards the head of the train, the pressure being then a maximum. The tube had a diameter of one inch, and the walls of the tube were of thin metal. For angles between 0 and 60°, the pressures were positive. At 60°, the pressure observed was zero. At an angle of 90°, the decrease in pressure was greater than the increase when the angle was zero.

The values observed are here given, the pressures being in grams per square centimeter. These values may, of course, be represented by an harmonic series. It requires about ten terms.

a	*P*	*a*	*P*
0	1.34		
10	1.30	100	—1.29
20	1.26	110	—1.03
30	1.21	120	—0.93
40	1.03	130	—0.89
50	0.63	140	—0.86
60	0.00	150	—0.86
70	—0.63	160	—0.79
80	—1.10	170	—0.66
90	—1.44	180	—0.54

These values are plotted in a polar diagram in Fig. 1.

The pressures were measured with a water manometer, having its tube inclined to a slope of one in twenty. The actual readings were, therefore, twenty times the numbers above given.

These values are the means of twenty measnrements. The average velocity of the train as determined by simultaneous measurements with another Pitot tube was 41.8 miles per hour. The relative velocity of the air with respect to the train at the point where the measurements were made was 32.5 miles per hour. This lesser relative velocity was due to the fact that air was dragged along with the train.

It is therefore evident that in such a stream of air, a tube which is to eliminate the velocity effects, must be set with its mouth directed at an angle of 60° with the stream lines, and not at an angle of 90° as is the usual custom.

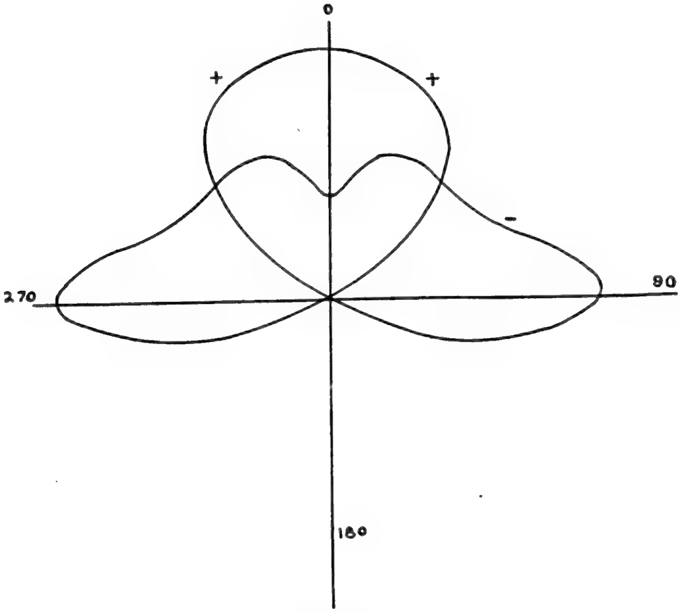

FIG. 1.

Two tubes rigidly connected and having their axes at an angle of 60° inserted into a pipe through which air is flowing will separate velocity pressure from staticial pressures. One is to be so turned in the stream that the reading of its gauge is a maximum. It will indicate both pressures. The other indicates only the actual pressure due to resistance of the pipe in advance of that point, if the discharge is into the air. Such statical pressure differences at two points along the pipe, will correspond to voltmeter readings between the points on a wire carrying an electric current.

Another means of eliminating velocity effects is given by a device which I here present.

A tube to carry the pressure to the gauge, terminates at the point where the pressure is to be collected, in a thin circular disk, placed edge-wise in the stream. A parallel disk is secured to it by soldered or brazed rivets, with four or five circular sheets of wire cloth loosely clamped between the disks. The bundle of wire cloth sheets projects to some distance beyond the metal disks. On either side of the disk-collector so formed, a couple of sheets of wire cloth are laid, thus hiding the metal disks in the wire-cloth bundle.

With this arrangement, the pressure at the mouth of the collecting tube is wholly unaffected by the motion of the current of air. The compression and rarefaction around the tube are prevented from affecting this pressure by the two disks. The compression and rarefactions around the edges of the wire cloth bundle are also eliminated by the projecting margin around the edges of the disks, so that these compression and rarefaction effects do not get between the disks. They do get between the disks and affect the reading in a way that cannot be corrected, if the wire cloth layer does not project beyond the metal disks.

The layers of wire cloth on the outside of the disks is a recent improvement, which is necessary for very high velocities. It serves to smooth away irregularities in outline and prevents rarefactions due to these irregularities in the projecting layer of wire cloth from affecting the air pressure between the disks.[1]

When this disk collector stands edge-wise in free air in a current of air from a ten inch pipe delivering 96 cubic feet per second, the gauge connected with it is not in the least affected.

For comparatively small velocities and uniform pressures, as in pipes connected with blowers, the tube piercing one disk of the collector may be very small for a distance of an inch, and then widen in order to give necessary stiffness. The wire cloth layer need not then be over an inch in diameter, and the metal disks need not be over half an inch in diameter.

In measuring wind pressures on buildings where the pressures vary rapidly the tube should be a quarter of an inch in diameter, and the disks should be three and one-half inches and the wire cloth layers from five to five and one-half inches in diameter. The disks are placed flatwise near the wall.

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[1] *Trans. Acad. of Sc. of St. Louis,* VIII.: 1.

These disk collectors may also be used with perfect results in water pipes, if the velocity is not great enough to produce discontinuities where air would show rarefactions. No tests have yet been made under these conditions. For high velocities the action of the Pitot tube deserves a complete re-investigation. It seems probable that the tube inclined at an angle of 60° with a Pitot tube will be the best method of eliminating velocity effects in a current of water at very high velocity.

In measuring wind pressures on buildings by means of inclined water gauges, the open end of the tube must be connected with a large reservoir, which is connected to a horizontal disk collector, far above the building. The disk is to be horizontal, so that the air current strikes it edgewise. These disks are now being mounted on the physics building of Washington University. Nearly a mile of piping is being used in connecting the various collectors, each

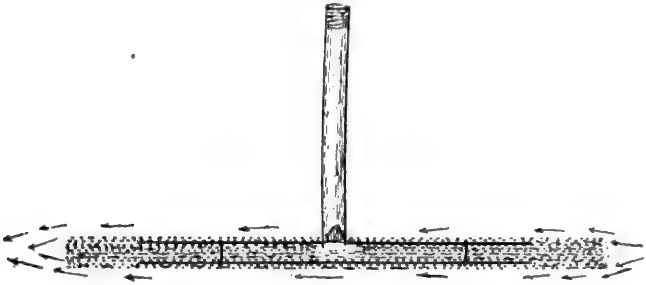

FIG. 2.

with its own gauge. The details of the collector are shown in Fig. 2, in cross section.

The two collectors here shown are presented to the society.

THE PRESENT STATE OF THE QUESTION REGARDING THE FIRST PRINCIPLES OF THEORETICAL SCIENCE.

By JOSIAH ROYCE.

(*Read April 19, 1906.*)

I venture to use this opportunity to call attention to the existence and to the spirit of certain researches in which a good many well-equipped students of science are now taking part, and in which I myself, although very ill prepared for the work, have already tried in a very modest way to take some part myself. These researches interest primarily logicians, and to some extent mathematicians. They have a relation, however, not only to general philosophy, but also to the interests of a good many students of the special sciences. Let me try briefly to indicate the problems which give rise to such researches.

I.

Science, as we all know, has two aspects, namely, that aspect which is concerned with discovering and reporting facts, and that aspect which is concerned in constructing and applying theories. A scientific theory is a body of assertions connected together by processes of logical reasoning, and so chosen as to be of use in displaying the rational connections of facts, and in predicting facts which have not yet been observed. The extent to which theories are of use for the work of a given science varies very greatly with the stage of evolution which the science has reached, with the character of the subject matter and with the interests which control our study of the facts in question. Celestial mechanics furnishes an instance of a very highly developed theoretical branch of science. The extent to which theory is significant and successful in any one science, as for instance in biology or in chemistry, is in case of each such branch of scientific inquiry a kind of test of the stage which the science in question has reached in its evolution. In the history of a science the premature prominence of theoretical constructions leads to a neglect of facts or to a too easy contentment with an insufficient collection of facts. When a science, however, is already highly developed but is also rapidly growing, the search for new facts is commonly guided by more or less highly developed theoretical interests, and is directel by presuppositions, by hypotheses, by questions, which have come to mind in consequence of reasonings due to theories.

In the normal case of a science in which theories play an important part, a scientific theory takes the form, first, of the statement of a set of principles, or of relatively fundamental propositions, which the theory treats, at least provisionally, as true. Secondly, the theory consists of the logical development of a set of consequences, which follow from these principles and so will be true in case the latter are true. These consequences may be reached, and in the case of the most highly developed theories, are reached, by mathematical computations. In its application to the work of the science, the theory becomes useful, in so far as its results can be compared with the particular facts of experience, or can be tested by seeing how far they lead to successful predictions.

A theory whose results disagree with facts has to be amended accordingly, but in many cases may be adjusted to the facts by alterations which leave its main principles intact, and which involve only minor modifications. When a theory succeeds up to a certain point, but leaves some facts indeterminate, it frequently gives rise to hypotheses concerning phenomena as yet unobserved, and in this sense may prove a guide to investigation. There are well known and important cases where a theoretical computation disagrees for a time with actually observed facts, but where the discrepancy can be shown to be due to the non-recognition of certain facts which, so soon as you take them into account, enable the original theory to apply with reasonable accuracy to the whole system of facts in question. In some cases improved methods of calculation, or other purely logical developments of theory itself, suffice to remove discrepancies; and such cases furnish very persuasive tests of the value of the theory in question.

If you look towards the world of facts, as experience shows them to you, the principal use of a theory seems to lie in two things. First, a theory, if successful, enables you to give an economical description of a vast number of facts. Secondly, a theory usefully guides your search after new facts, and in particular your predictions, and the practical activities by means of which you apply your science to the study of new cases. The common mind often opposes theory and practice. But every enlightened student is aware how large a part theory plays, in those cases where theory is possible, as a means towards guiding the practical applications of a science to the various arts. Thus without astronomical theory the application of astronomy to navigation would remain very limited; because one who has to apply observations of the heavenly bodies to the work of the navigator must accomplish his application by means of definite processes of computation. Such computations can be reduced to precise rules only by means of considerations which belong to the theoretical side of the science. So long as, for the Babylonian astrologers, astronomy remained a mysterious branch of empirical natural history, computations could have only a limited scope. It is astronomical theory, not to be sure the whole of astronomical theory, but a certain limited portion of it, which gives to the navigator's computations a uniform and controlable character. Economical description, controlable application to the search for new facts, and to the practical uses of a science, these are the characters which a scientific theory must possess in order to meet the requirements which the real world makes upon it.

But these requirements cannot be met unless the theory possesses a certain coherent logical structure. This structure might in general be possessed, almost or quite equally, by a great number of different theories whereof only one happened to be true to the facts. Nevertheless, although the internal logical finish of the structure of a theory is by itself no guarantee that the theory is useful in describing or predicting facts, such logical structure is a condition sine qua non of a good theory. A natural question arises as to what constitutes this internal logically coherent structure to which a highly developed theory must conform. The question so stated may appear at first sight very vague and indeterminate. A theory, you will say, must make use of principles which it provisionally assumes to be true. It must then develop the consequences of these principles. It must be logically accurate, and as full in its development of consequences as the application which is to be made of the theory seems to require. This, it would seem, can in general be said. But with this vague generality the theory of what constitutes a good theory would appear at first sight to be completed.

Yet a moment's thought will show, that we all pretend to know more about the structure towards which highly developed theories tend, than this first generalization would make manifest. For instance, it is a comment which has become commonplace, that, wherever quantitative conceptions are possible, theories whose first principles can be expressed in quantitative form, have a formal advantage over theories which have to be expressed in non-quantitative terms. Some portions of our empirical world are subject to measurement. Measurement in practice gives results which vary within the limits of error, and which are therefore inexact. A theory which is to be just to any highly advanced state of knowledge regarding measurable facts, must make use of principles which involve provisionally assumed relations of quantities. One advantage which a quantitative theory can then possess lies in the very fact that its provisionally assumed principles may be stated with an exactness which empirical measurements never reach. In other words, the very incapacity of our theory to account for the variations, and for the inexactness of any single process of measurement, may be an advantage in the development and in the further application of the theory. Assuming exact relations, and invariant relations, where the actual measurements of observers show a considerable range of uncontrollable variation, the theory may enable computations to be made, in terms of which the work of measurement may be guided, and the essential and unessential elements of experience may be distinguished. Cases of this sort suggest that the structure of theories is subject to certain logically definable laws which are somewhat independent of the precise degree to which in a given case the theory in question can be verified. In other words, while the true theory, in the sense of the theory that agrees with the observed facts, is indeed the ideal, one may be able to judge the value of a theory in advance of knowing whether it is true of not, in so far, for instance, as a quantitative theory is preferable to a non-quantitative one, and in so far as exact theoretical interpretations are preferable to inexact ones.

This very commonplace instance suggests where lies the logical problem regarding the internal structure of theories. What does one mean, for instance, by a quantitative theory? In order to answer this question one must know what one means by quantity. Why are quantitative ideas more useful than non-quantitative ideas? Wherein lies the logical difference between conceptions of quantity and other conceptions? Is the notion that quantitative conceptions stand alone amongst possible scientific conceptions, in their peculiar possession of exactness, and of a capacity to be submitted to precise and entensive processes of deduction, is this presupposition itself well founded? Are there other concepts which are logically as exact as the quantitative concepts, which are as capable of being subjected to elaborate processes of a deductive character? If so, are there other regions than those of the sciences of measurement in which highly developed theoretical finish is possible? May the science of the future come to use other than quantitative theories in dealing with regions of nature or of mind where measurement proves to be unattainable, or inexact? How will the non-quantitative theories, in so far as they can be developed, stand related to the quantitative theories? What is it that makes certain concepts adapted to furnish a wide range of unexpected results, which can be reached deductively, and by exact devices of thinking, although these results cannot readily be seen at a glance, by merely inspecting the conceptions in question? How can mere deduction lead to an infinite number of unexpected results, as is often the case in the exact sciences? Do the possible conceptions which the human mind can frame, and can lay at the basis of theoretical constructions, form anything like a closed system? In other words, is the range over which our theoretical constructions vary simply limitless, and indeterminate, or is it, even if infinite, still in some way itself determinate, so that one can name certain fundamental concepts which every theory must use, or from which every theoretical construction must make a selection, even in defining its provisionally assumed principles? In other words, are there first principles of scientific theory? Are the ideas which we can use in defining our provisional hypotheses, in initiating processes of logical deduction, ideas of which some general and thoroughgoing account is possible, so that, although we cannot predict the facts of the natural world, we can predict the forms in terms of which we shall always be obliged to think the rational connections of these facts in case we form any theory at all? Are, then, the internal conditions of theoretical science, the logical possibilities upon which such a science depends, of a determinate range, and of a knowable character? Such are the problems which are suggested when we begin to inquire as to the logical position which quantitative theories hold amongst the various types of theories which are logically possible.

The questions thus suggested are obviously of the most fundamental importance for any one who is interested in understanding the workings of science. Science depends upon finding facts; it certainly also aims at the controlling of facts. The control which is here in question may either mean the technical mastery of facts, the power to produce them at will, or it may mean the prediction of facts. But either kind of control is possible only in so far as we possess something of the nature of a theory. And a theory involves the construction and control, and logical linking of concepts which have to be of our own making. Therefore, the study of the types of concepts which we can construct and control and link, the study of the forms and linkages which the nature of our thought makes possible, is surely as serious a study, as the direct study of the facts which we can hope to control through the use of our intelligence. The pursuit of useful knowledge surely includes in the end a knowledge of those logical processes of thought whereby we come to make an intelligent use of facts.

II.

The result of such considerations is that a science is needed which I may provisionally call the morphology of theories. This science is a branch of logic. And it is to this science that I now call your attention.

So far it is easy to define our problem, and to see that if solvable, it must be an important problem. What the student unacquainted with modern logic will find doubtful may be the assertion that such a problem can at present be fruitfully studied. If you define this study of the first principles of theoretical science as a branch of what is called logic, it was until recently the fashion to say that since Aristotle logic has made no progress; that that marvellous thinker had already seen nearly all of what the human mind can see regarding the structure of our thinking processes, and regarding the way in which we can use principles provisionally assumed, for the purpose of drawing conclusions from them. The principal addition that was supposed to have been made to logic since Aristotle was confined, according to this view, to a study of that inductive logic which is concerned rather with the application of our thinking processes to the discovery, the collection and the arrangement of facts, than with the structure of our thinking process itself. I wish to call attention on this occasion to the fact that this familiar assertion concerning logic and concerning its stagnation since Aristotle, in no longer true. We are today in the midst of a very vigorous and many-sided movement which interests the students of several different sciences, and which involves a rapid advance towards an answer to those very questions which I have just enumerated. We are today in a way to grow very rapidly in our comprehension of the range, of the varieties, and of the logical nature, of the fundamental conceptions upon which all theoretical science depends. We are no longer confined to the commonplace observations just cited regarding the peculiarly advantageous character of quantitative concepts and theories. We begin to know *why* the concept of quantity has the logical usefulness that it possesses. And as we come to know this, we see that the concept of quantity is one only amongst the exact and definable fundamental concepts upon which scientific theory depends. We discover that even the quantities get their logical usefulness for purposes of scientific theory from certain characters which they share with a very large number of other concepts, namely from their character of being capable of serial arrangements. and from their further character of constituting what is now called a group, with reference to certain specific operations. The series concept and the group concept thus obtain a logical place amongst fundamental concepts which permits us at once to view the quantitative theories as a special instance only amongst an infinite, but again perfectly determinate range of possible theories, some of which have already their place in certain of the sciences, while other exact, and equally fruitful, although non-quantitative theories, are likely to become of definite use in the science of the future. We are, therefore, already on the way vastly to enlarge, but on the other hand much more precisely to define our concept of what constitutes an exact scientific theory. We are on the way towards understanding why some theoretical concepts permit of such a vast range of deduction, while others are less significant in this respect. We are becoming able to face as never before the logical question as to what we mean when we define facts as being quantitative at all. And as our view of the forms of conceptual structure which are possible for the human mind not only enlarges, but becomes more exact, we are coming nearer to the point where we can profitably study what the conditions are upon which the formation of exact concepts depends.

III.

The researches to which I refer are well known to all students of modern logic. They have come, to a considerable extent, from the mathematical side. They have been suggested, however, not only by mathematical science, but by the logical analysis of the exact physical sciences, and to some extent by the analysis of the concepts which lie at the basis of the study of the humanities, and of the historical sciences. The interest in formal logic which received a new impetus from the researches of Boole, has added itself to these other motives. As examples of inquiry of the type that I here have in mind, one may mention the well known works of Mach, and of Pearson, on the concepts and methods of physical and of statistical science, the recent books of Ostwald and of Poincarè, the various lectures on the concepts and methods of science, which were called out by the St. Louis Congress, the varied and extensive investigations of our principal American logician, Mr. Charles Peirce, the great literature which has now grown up about the theory of assemblages wihch Cantor initiated, the investigations of Dedekind upon the concepts of arithmetic, the lectures and essays of Helmholtz regarding the concepts of the exact natural science, the extensive inquiries into principles of geometry, the modern effort to formulate the concepts and purposes of historical science, the manifold controversies concerning the office and conceptions of recent psychology, the whole range of researches in modern group theory; and in brief, all the more enlightened types of recent reflection upon the principles of science. Although myself a student of philosophy, I lay here no stress upon the contributions to this research which have in my opinion been due to the progress of modern philosophy viewed as such. There is no reason to consider the philosophers in this field as either a privileged or a dangerous class, or as for that matter easily a separable class. Cantor and Dedekind are philosophers amongst the mathematicians. I suppose it might be fair to call Mr. Bertrand Russell mathematician amongst the philosophers. I am certain that Mr. Charles Peirce is a philosopher. I am certain that Boole, although a mathematician, was guided by profoundly philosophical instincts. My interest at this moment is in laying stress upon the fact that the modern study of this subject is confined to no one branch of students, and on the other hand has so far developed that in this field one is no longer confined to the chance observations of this or of that introspective philosopher concerning what he happens to have noted regarding his personal thinking processes. The science which now deals with the morphology of theories, which seeks for their fundamental concepts, which tries to detect what unity there is amongst these concepts, which endeavors to show wherein lies the advantage which certain concepts possess for the purposes of theoretical construction, this whole science, I say, is now no longer a matter of merely private scrutiny, and of personal opinion. It is full of still unsolved problems; but it has a definite method of work. This method, like that of other sciences, is itself at once empirical and theoretical. Empirically the student of logic treats scientific theories as themselves facts which the history of science presents for his inspection. He analyzes these theories to see what their conceptual structure is. A comparative study of theories shows him the prevalence and the importance of certain types of concepts, such for instance as the concept of quantity itself. The student hereupon undertakes to analyze these various concepts into their elements, to detect what their structure is, to describe them as one would describe organisms or solar systems. He then proceeds to ask in what ways the structure of such theories is determined by the nature of human thinking. To this end he uses means for the analysis of the thinking process which have become accessible only within the last generation. They are the means furnished by a new and now rapidly progressive science called symbolic logic. Not all the actual students of our topic have as yet made use of this instrument of research. Comparatively few are well acquainted with it. But there can be, to the initiated, no doubt of the fundamental importance of this instrument. By means of this and of other instruments of analysis, the modern student is endeavoring to trace thought to its sources, or in more exact language, to see in just what relations we place objects and ideas before us, whenever we undertake to think about such objects and ideas. The thinking process is by no means as monotonous an affair as the ordinary traditional textbooks of logic have depicted. It is worth while to add that the analysis of concepts in which the student of logic is interested is from this point of view very different indeed from a psychological analysis of thinking or from any analysis that could be carried out either by means of direct introspection or by means of the study of language. Whoever is disposed, as some psychologists are, to imagine that logic is a special branch of psychology, may well be invited to make an excursion into modern logic long enough to consider that analysis of the relations amongst the concepts: *and, or,* the concepts of *implication,* and the concept of *negation,* which the recent methods include. Such psychologists are then invited to endeavor to discover by what psychological analysis of the thinking process they could ever detect these relations.

When the analysis of the thinking process is accomplished, so far as that is yet possible, the student of modern logic is next interested in surveying the range of variation to which our theoretical concepts may be subjected. For it is a notable fact that however wide the range of liberty that we give to our thoughts, however free the range of creative activities over which we let ourselves roam, the results in the way of conceptual structure which appear to be accessible, are remarkably limited as to the number of generically distinct types which appear to be open for our consideration. Each one of these types appears, indeed, to involve, as we have already indicated, an infinitude of various exemplifications. But with all this wealth, the definite structure, the determinate range of variation of fundamental concepts, the distinctly limited list of categories with which the logician apparently has to deal, together constitute one of the most striking results of the investigation. The thought forms, the kinds of conceptual structures which are possible, are certainly not yet thoroughly known, and their range may prove to be very far greater than we yet suspect. But the notable fact is that they appear to be built up upon a few fundamental types, which remind one by analogy of some such natural types as the vertebrate skeleton, or as the type of the insects. With endless variations in detail, each of these great types is built up in its own way, and preserves its morphological identity through its variations. The thought-types are thus not spread out in endless profusion, but apparently have a well-knit organization of their own, wherein a limited range of fundamental types spring from a common root. For instance, I have already referred to the type of structures which modern group theory defines. This type has, to be sure, an infinity of exemplifications; but all these conform to certain simple and fundamental laws. The one theory of groups consequently includes, in a sense, a very large portion of the theory of those conceptual structures which are prominent in modern mathematics. Yet there are systems whose structure is not that of the mathematical group. Their forms, again, vary in ways which we are only just beginning to understand, but which do not seem to exhibit any merely capricious variety. Unity in variety is, then, peculiarly well exhibited in the world of forms.

IV.

A few of the problems which such a survey of the morphology of the conceptual world, seems to present, may now be mentioned more in detail. That the forms of possible existence which our thought necessarily recognizes, are indeed limited in number, and depend upon as well as exhibit the necessary constitution of our thought, this philosophers long since came to feel. But the effort to enumerate such fundamental types is greatly hindered by our incapacity directly to analyze through any introspective process what the logical structures of our concepts may be. For a concept, that is a fashion of thinking, expresses a characteristic way of behavior of the mind, a fashion of reacting to our environment. And no simple introspection can tell what such a way of behavior involves. For just as personal character cannot be discovered by looking within, but must express itself in a long and active life, before it can be fathomed, so with the forms of thought. They are methods of activity. A direct reflection does not discover their constitution and relations. These must be judged through an examination of consequences, and through a development of extensive thinking processes. For instance, if you ask a plain man how he gets the idea of number, he will reply, by counting. And he supposes that he knows by direct introspection what counting is. A psychological analysis made under experimental conditions may in many ways further dissect the mental processes which go on when we count. But how remote any such analysis is from a logical comprehension of the form of thought used in counting will become evident only after one has read such discussions as those of Dedekind in his famous essay on whole numbers, or such as Russell's and Whitehead's recent analyses of the relation between the cardinal and ordinal numbers. The relation between the number concept and the concept of quantity is again wholly inaccessible to direct introspection or to psychological experiment. Only an elaborate process of what one might call logic experimentation brings out the relation between the two concepts. The analysis of Peano, or the recent papers of my colleague, Professor Huntington, are instances of such logical experimentation. The process of experimentation in question consists of undertaking to discover what assumptions, or what various sets of assumptions, are sufficient, or are both necessary and sufficient, in order that one may be able to deduce from them the consequences which are already known to be characteristic of whatever concept one happens to be analyzing. Only by such experimentation can one dissect the thought form with which one is dealing.

It follows from our inability to detect by any direct mental inspection what ones of our concepts are the fundamental ones, it follows, I say, that the older philosophers, including Aristotle, were indeed frequently very profound and as far as they went accurate in some of their logical analyses, but could never be exhaustive, in their account of the first principles of our theoretical thinking.

Some of Aristotle's anaylses of such principles do show in fact a wonderful instinct for the essential, a logical depth of comprehension, which remains permanently marvelous as well as instructive. So, for instance, his brief but penetrating analysis of the concept of the Continuum touches upon a problem which brings him into close touch with the inquiries even of a Dedekind; and this fact about Aristotle's view of continuity has well been pointed out by Mr. Peirce.

But at the best these older analyses labored under one presupposition which was long prominent in philosophical textbooks, which was, however, long since rejected by at least one of the most famous modern philosophers, namely Hegel, and which has now become, as I think, a definitively exposed error, deeply rooted as it still is in the popular mind. This was the presupposition that the first principles of theoretical science, the fundamental concepts upon which all theoretical construction depends, are or can be known to the mind in the form of a list of self-evident principles, or of simply unavoidable and obviously necessary concepts. I say the older analyses of theoretical science mainly depend upon supposing a list of self-evident principles to be discoverable, and a list of self-evident concepts to be attainable. Even Locke, empiricist as he was, regarded the self-evident concepts and principles as indeed psychologically due to our experience, but as coming to our consciousness, after once our experience had been matured, in a shape which made them shine by their own light. But the modern logician has learned to see, that the feeling of self-evidence which frequently attends the enunciation of a principle, is commonly an indication that one has not yet learned to analyze the principle. In other words, self-evidence is a suspicious sign. It warns you that you do not yet understand the topic. If you cut a strip of paper and bring the two ends of its together to make a ring, it appears self-evident that any strip of paper must have two sides, and that in order to get from the inside of the ring to the outside of the ring by a movement which keeps your finger, or a pencil, in contact with the paper, you have either to go through the paper, or to go over the edge of the paper. All this seems self-evident or to many people may seem so, until someone shows you the now well-known one-sided paper ring, made of an ordinary strip of paper, but so made that the two sides form but a single side. In this case the very strip of paper which has but one side, now has but one edge. And thus a universal principle which might, but for such an example, have seemed self-evident, namely the principle that a ring strip of paper must have two sides and two edges, becomes in the light of this principle, simply false; and one's geometrical ideas are hereby enlarged. So long, then, as it is self-evident to you that any ring strip of paper must have two sides, you simply do not understand the forms in question. Another case now very familiar in discussion, another case, I say, of a principle long regarded as self-evident, is the principle that the whole of a collection, must exceed in multitude any part of that collection which may be formed by leaving some of the members of the collection out. But the modern theory of infinite collections is founded upon supposing this principle to be, as it actually is, false for such collections. Thus there are as many powers of two as there are whole numbers, an assertion which follows directly from the definition of a power of two, and from the definition of whole numbers. Yet the powers of two are themselves whole numbers, and are but a portion of the whole numbers, and may be viewed as an extremely small portion in case one judges its size merely by considering what whole numbers are omitted from this collection. Upon self-evidence, then, no theory of the scope of theoretical science can be built up. I do not hesitate to say that there are no self-evident principles. And as myself, in philosophy, what is called an absolutist, that is a believer in the existence of absolute truth, I utter this assertion not in the interests of skepticism, but in the interests of truth. Single truths do not possess self-evidence, just because there are many truths which form a system, wherein each element is dependent for its nature upon its relations to the others. In general the assertion of the self-evidence of single principles has repeatedly been a foe to the progress of civilization, as it is hostile to a genuinely logical understanding of the nature of truth. The assertion of self-evidence has been used to defend almost any bulwark of tyranny from the questionings of beneficent reformers. Not upon self-evidence, therefore, nor upon a list of fundamental verities, each of which shines merely by its own separate light, can the logical theory of science be founded. In general what we call first principles are such merely in some certain respect, or from some special point of view. Otherwise viewed, these same principles may appear as derived. And to discuss the various ways in which such derivation may be brought to light, is one of the principle problems of modern logical theory.

V.

The relative accomplishment of such a task in the case of any particular branch of logical theory involves a sort of study which the recent discussion of the logic of geometry, as well as of the logic of number theory, often exemplifies. Instead of setting forth certain self-evident axioms of geometry, or of arithmetic, the modern logical investigator undertakes to do what Russell and Couturat call defining a certain type of space, or a certain type of numbers. This process of definition, also often called the process of definition by postulates, consists substantially in saying: "I am going to describe to you the properties of a certain class of ideal entities. I do not say that these entities exist in the physical world, just as I do not deny that they exist there. But I am going to treat them simply as the entities which conform to the following definition. The definition I will state in the form of a set of principles given in order as first, second, third, and so on. I state the principles, and I define the entities in question as a set of entities such that they conform to these principles. If the principles involve no mutual contradictions, such entities are possible." Thus Dr. Veblen, in his recent essay on the so-called axioms of geometry, states twelve different principles to which certain abstract entities named points are to conform. He does not assert that these principles are self-evident. Since he is talking about purely abstract entities, which are the creatures of his definition, the principles could not be self-evident. They are true only in the sense that the entities defined are said by definition to conform to them. Dr. Veblen then shows that the laws of our ordinary geometry can be deduced from these principles as laws which hold for the defined entities. These principles, then, are sufficient as a basis for geometrical science.

A similar procedure has now become so common in discussions of this modern type, that it needs no further characterization for those who have examined any such researches. Their interest lies not in the founding of scientific theories upon any set of self-evident principles. The interest lies in showing the connection which exists amongst various concepts and principles, and in bringing to pass a logical analysis of the theory in question and of the concepts which this theory involves. No exclusive significance can be attached to any one such investigation. There are numerous, probably very numerous different sets of principles, upon which geometrical science could be founded. How far our experience of space bears out any of these principles by confirming their truth is a matter for the science of nature. Why our experience of space has these characters is ultimately a matter for philosophy. What set of geometrical principles it is convenient to use for the purposes of a textbook of geometry, is a pedagogical matter. Geometry is not deducible from self-evident axioms, since there are no self-evident axioms. Geometry is a theoretical science, since we are not confined to particular observations for our knowledge of space relations, but are acquainted with laws which enable to describe and predict our spacial experiences in general terms. The first principles of this theoretical science can be variously stated. The logical problem lies in understanding the relations that exist amongst these various statements.

Nevertheless, when a large number of theoretical sciences have been treated in this way, when their various concepts have been analyzed from various points of view, and when as is the case the forms or types of concepts which they contain have been shown to be variations of a comparatively limited number of types, such as the series type, the group type, or to speak of a more special instance, the type of the ordinary real numbers, or of the ordinary complex numbers, one is brought in the presence of a further problem which is indeed at the present time the central and characteristic problem of logical theory. It is the problem as to the unity of these forms. Fundamental ideas, in the sense of self-evident concepts and principles, do not exist in scientific theory. On the other hand, the various inter-dependent truths and concepts of theoretical science appear to form a relatively closed system, where the special forms are infinitely numerous, but where the main types or species are comparatively few. The question in which all students of science ought to be interested, and in which students of philosophy are explicitly interested, is the question as to what common tendency of human activity it is which differentiates itself into all these forms. What a thinker does when he puts facts together, and forms a theory, depends of course upon the nature of the facts, in so far as he is trying to describe them, but it also depends upon the nature of his thought, in so far as he can only do for the purposes of thinking, what appeals to his rational interest, and what solves a thoughtful problem. A thinker, however faithful to his facts he means to be, has his needs as a thinker, and his forms of thought are his ways of satisfying his needs. He cannot merely report facts. He must interpret them. His theories are his interpretations. His world of science is his world as interpreted. It cannot be understood therefore apart from his needs as a thinker. The structure of his theories is the embodiment of these requirements of his own nature as a thinker. That quantitative science, that the principles of geometry, in whatever form they may be stated, that group theory, and that number systems, apply to his world in the regions of which he has a theoretical understanding, all this is due not merely to any outer world, which can exist wholly apart from the thinker,—not merely to such a world, I say,—but certainly also to the nature of the thinker himself. Our study of theoretical science has to be interpreted, then, as a kind of science of a thinker's ways, as an inquiry into what sort of ideal he has, as a study of the meaning of his thoughtful life, of its internal meaning, and of truth, in so far as truth is related to this internal meaning of the thinker. When we find, as we do, that the forms of thought are not endlessly variable, but are reducible to a certain range of generically different conceptual structures, we are therefore led to this question which now we face. To what are these thought forms due? What is their unity?

VI.

In an address which I was privileged to make before the St. Louis Congress I pointed out a contribution to this problem which had been suggested and in part carried out by Mr. A. B. Kempe. I have since further pursued the research which Mr. Kempe has initiated, and have published my results in a paper entitled "The Relation of the Principles of Logic to the Foundations of Geometry," printed in the *Transactions of the American Mathematical Society*. This is no place to discuss the issues involved in that paper. I want simply to indicate in a very general way one point regarding the kind of result which seems to me to be already in sight, although the matter is still very incompletely worked out. The different characteristic forms of thought to which I have referred, are distinguished by the various types of relations which these various forms exemplify. Thus the characteristic ordinal relation of descriptive geometry is the relation called "between"; and Dr. Veblen has shown how in terms of this single relation, and of the assumption of the existence of appropriate objects or entities, one could state all the principles that are needed as the foundation for geometry. The characteristic relation of the world of quantity, the relation of "greater and less," is a relation which in combination with the triadic relation that is involved in the ordinary operation of addition, is sufficient to give form to the principles of algebraic analysis. In brief, then, each theoretical science has its own characteristic set of relationships. When so viewed these relations stand by themselves, as if they were separate facts in the natural history of the forms of thought. Relations may be classified, just as truly as birds, or as bacteria may be classified. There are relations dyadic, triadic, n-adic, there are relations symmetrical, unsymmetrical transitive, intransitive. These varieties of form in the world of relation, when thus viewed, seem ultimate and irreducible. Yet I do not think that anybody finds it self-evident, or axiomatic, that only these relations should be possible. I do not think that we have any warrant for saying on the other hand that the sorts of relations which exist are capable of a simply limitless and a capricious variety. The concept of a relation is to my mind, as to the minds of a good many of my colleagues, something that is intelligible only in terms of the activities of our own thought. We understand relations, because of our own thinking processes we can at once depict, and in a sense reconstruct or create them. The types of our own construction, of our own thoughtful activity, are therefore the relational types. If we are to understand, then, the unity and the system of relational types, we must see how their varieties are related to our own activities as thinkers.

Now, however, relations are known to us not only as existing in teh world of numbers and of geometry, but as present in the purely logical world, the world of classes and propositions, of syllogisms, and of reasonings in general. I have already mentioned what some of the logical relations are. They are relations such as are expressed by the words "*and,*" "*or,*" "*not,*" "*implies,*" and so on. These relations are as fundamental and as simple as are our thinking processes themselves. We learn about them not through our senses, but through our activity as thinkers. Now what Kempe's research suggests, and what my own line of research has tended I hope to bring a very small step further on the road towards definition, and confirmation, is the thought that such geometrical relations as "between," such relations as "greater" and "less," and even such relations as are fundamental in group theory, are capable of being interpreted as instances, as consequences, or as partial views, of the fundamental logical relations themselves. Kempe has shown how a logical class can be viewed as "between" two other classes and how the geometrical "between" can be regarded as a special instance of this logical "between" I have shown how the system of Dr. Veblen's principles of geometry could be brought into definite connection with the relations which characterize a system of logical classes. The whole research in question is still in a very elementary stage, but enough has been done, I think, to make it at least probable that whoever comprehends the most fundamental logical relations, such as a child begins to comprehend when it first says "no," that is, whoever comprehends such relations, as "and" and "or" and "not," and the relation of implication, has already in his hand the means for developing the fundamental concepts of all of the exact sciences, since the relations of these exact sciences are more or less complex variations and recombinations of the fundamental logical relations themselves.

Meanwhile, however, the fundamental logical relations are characteristic not only of our world of thought, but also of our world of action. For will-acts involve acceptance and refusal, affirmation and negation, a consciousness of consequences, a facing of alternatives, a union of various acts in one act; so that the logic of action is in form precisely the same as the logic of abstract thought. In brief, so far as I can see, the trend of the modern study of the principles of theoretical science is at present towards proving that all the forms of conception used in exact science are but expressions of the characteristic types of will activity of which we as voluntary agents are capable. We thus conceive the structure of the world in terms of the structure of our own types of voluntary activity. The forms of our will determine the types of our theoretical concepts. We define facts, so far as we theoretically comprehend them, in terms of the nature of our wills. The view of the logical source, and of the internal structure of our concepts which is thus suggested, is closely akin to what is nowadays called pragmatism. But to my mind any pragmatism rationally thought out becomes philosophically speaking an absolutism. Yet with that philosophical question we have here nothing to do. The result of our modern study of logic is certainly to give us no less respect for facts, than we get from the study of nature. But the facts with which the logican has to deal is the fact that as a man willeth, not only so is he, but such are his theoretical conceptions. The whole trend of his theoretical science consists in his effort to find in the universe, in the end, the expression of his own will. His fancies, his capricious will, his temporary hopes and hypotheses, he learns to resign; and he calls this resignation a submission to external facts. But this submission itself is an action of the will, a rational act, but also his own act. As he proceeds in the work of his thinking, he is, as Kant long ago said, endless interpreting the world in terms of his own thought. But the forms of his thought, these prove to be ultimately the forms of his voluntary activity. Our modern unification of the concepts of theoretical science looks then towards viewing all the fundamental types of relations as identical with the types of the purely logical relations, such as come to mind when we assert, deny, infer, or otherwise deal with the relations expressed by the words "and," "or," "not," "implies" and a few similar terms. But these forms of relations are themselves the forms in which our will embodies itself. So that our theories of the universe tend to be like the other works of our civilization, the result of a long struggle with nature, by means of which, when we win at all, we attain the end of finding our own will expressed in the order of the controllable facts. Some such consideration the modern study of the principles of theoretical science seems to me to enforce; and from this point of view I regard this study as belonging to what Franklin had in mind when he used the term "useful knowledge."

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ON POSITIVE AND NEGATIVE ELECTRONS.

BY H. A. LORENTZ.

(*Read April 17, 1906.*)

The illustrious founder of our society will forever occupy a prominent place in the history of physical science, not only for his experimental researches in electricity and his invention of the lightning rod, but also for his theoretical views. Franklin tried to explain all electrical phenomena that were known in his time by means of a single electric fluid, which he supposed to be present in certain definite quantities in all ponderable bodies, when in their natural or unelectrified state, and in larger or smaller quantities in positively or negatively charged bodies. The rival doctrine was of two electric fluids, which in the days before Maxwell served as the foundation of the mathematical theory of electricity and which was adopted by those physicists who, like Riemann, Weber and Clausius, sought to account for electrostatic and electrodynamic phenomena by one fundamental law for the mutual action of electric particles.

Some twenty-five years ago the relative merits of the different laws of this kind that had been proposed, especially of those of Weber and Clausius, were examined by many physicists and in connection with this, there was much discussion about the motion of the two electricities in an electric current. Whereas, in the applications of Weber's law, they were generally supposed to travel in opposite directions with equal velocities, Clausius could free himself from this restriction, and could even suppose one of the electricities to have no motion at all.

These questions, after having for a time lost much of their interest by the universal spreading of Maxwell's ideas, have again sprung up, and have even become of fundamental importance in the modern theory of electrons. I should therefore like to call attention to them for a few moments, hoping the subject will be thought suitable on the present occasion, because it is somewhat like the old question whether one had to assume Franklin's single fluid or a positive and a negative electricity.

In order to show the connection I may observe that we can never wholly escape from the dualism, the notion of two things with opposite properties, that is forced upon our minds as soon as we come to study phenomena. Indeed, while recognizing but one electricity, the unitarian theory invested ordinary matter with the properties of the missing fluid. It was obliged to assume the existence of a mutual repulsion, not only between the particles of electricity, but also between those of matter and to add to these forces an attraction between a particle of matter and one of electricity. This is not very different from a two-fluids theory; it is even practically equivalent to it, if one of the two fluids is supposed to be permanently fixed to the ponderable matter. After all, we shall have to choose, not, strictly speaking, between one or two electricities, but between one or two *movable* electricities, in modern terms, between one or two kinds of *movable* electrons.

I shall confine myself to the case of metallic bodies and I shall first speak of a phenomenon which at first sight might seem sufficient to lead us to a decision.

Let us consider a very thin rectangular sheet of metal, traversed in the direction of its length, say from left to right, by an electric current, and placed in a magnetic field whose lines of force are perpendicular to the sheet. Let us first suppose the current to consist of a flow of positive electrons, towards the right-hand side, of course. Then, by a well-known rule, each of these electrons will be acted on by a force due to the magnetic field and perpendicular both to the lines of force and to the current. This force will tend to drive the electrons in the direction of the breadth from one edge of the plate towards the other, so that, if two points of these edges, which would be at equal potentials in the absence of a magnetic field, are connected by a conducting wire, a current will be set up in the latter. A similar effect, but in the opposite direction, would be produced if the current were a flow of negative electrons. This is easily seen if we keep in mind that the motion of the negative particles must be supposed to be opposite to the nominal direction of the current and that the force exerted by a magnetic field on a moving electron remains the same if the sign of the charge and the direction of the motion are reversed at the same time. On account of their motion from right to left, the negative particles will therefore be driven towards the same edge of the sheet as the positive ones in the former case; the direction of the current produced in the connecting wire will therefore be reversed.

Having got thus far, we can also see what effect will be caused by the magnetic field if the current we send through the metallic sheet consists of a flow of both positive and negative particles in opposite directions, so that its intensity can be considered as the sum of those of two partial currents, i_1 and i_2. We shall then have a superposition of two opposite effects, either of which may predominate, according to the relative magnitudes of i_1 and i_2.

Now the phenomenon which the foregoing reasoning might lead us to expect, has really been observed. I need scarcely tell you that it was discovered by Professor Hall, then working in the laboratory of the Johns Hopkins University, at a time when there was hardly any question of a theory of electrons. The effect has been investigated for a large number of metals and has been found to have different directions in different substances. This is of especial importance in our discussion, for it seems to prove that we must indeed imagine two kinds of free electrons, the motion of the positive ones predominating in one body and that of the negative ones in the other.

I shall now point out some difficulties which present themselves in the further development of this conception of an electric current as a double stream of electrons. Take for instance the simple case of a current flowing across the junction of two pieces of different metals M and M', say from the former towards the latter. Considering two sections, S and S', of the two metals quite near their surface of separation, I shall denote by n_1 the number of positive particles traveling across S per unit of time in the nominal direction of the current, by n_2 that of the negative ones going the other way, and by n_1', n_2' the corresponding numbers for the section S'. Then, if for the sake of simplicity we suppose all electrons to have equal charges, we shall have

$$n_1 + n_2 = n_1' + n_2',$$

but of course this does not imply that n_1 and n_2 are separately equal to n_1' and n_2'. If the Hall-effect is not the same in the two substances, the ratio between n_1 and n_2 will be different from that between n_1' and n_2'; it may very well be that n_1 is much larger than n_2 and n_1' much smaller than n_2'.

In order to fix our ideas, I shall suppose

$$n_1 > n_1', \quad n_2' > n_2.$$

This means that the number of positive electrons entering the space between S and S' through the first of these sections exceeds the number leaving it through the second, so that the number contained within the space will increase by $n_1 — n_1'$. As there will be an equal increase $n_2' — n_2$ of the number of negative particles the result is a continual accumulation at the junction itself of equal positive and negative charges, or, as we may say, of *neutral* electricity. Conversely, neutral electricity would continually be carried away from the place of contact, if the direction of the current were reversed.

It is further to be noticed that a change in the distribution of neutral electricity would even occur if we had no current at all; if there were two kinds of movable electrons, it would already arise from the causes which produce the phenomenon of contact electricity. As to these causes several hypotheses have been put forth, of which two may be briefly mentioned. In his celebrated paper on the conservation of energy, Helmholtz accounted for the difference of potential between two metals by means of certain attractive forces exerted at very small distances by the material atoms on the particles of the electric fluids, or, as we are to say now-a-days, on the electrons; if, for instance, the positive particles are more attracted by the metal *M′* than by *M,* this will of course tend to produce a positive charge of the first metal. A wholly different explanation that has been proposed by Riecke and Drude is based on the assumption that the free electrons in a metal have their share in the molecular agitation by which we account for the phenomena of heat, going to and fro with velocities whose magnitude is a function of the temperature. The consequence of this heat-motion must be a certain equalization of the density (measured by the number of particles per unit volume) with which the electrons are distributed over adjacent parts of space. Hence, if at the same temperature the metal *M* contains a larger quantity of free positive electrons than *M′*, the first metal will lose and the second will gain a certain number of them and the potential of *M′* will be made to exceed that of *M*.

We need not stop to consider in detail these theories; it will suffice to observe that, according to both, the causes which bring about the difference of potential are confined to a very thin layer near the surface of separation of the two metals. Now, whatever may take place in this layer, it is clear that the transfer of electrons from one body to the other will go on until the causes determining it are balanced by the difference of potential that is established. A state of equilibrium would soon be reached in this way if there were but one kind of free electrons. But if there are two, the case will be different. The causes by which the positive electrons are driven across the junction being quite distinct from those on which the flow of the negative particles depends, the value *P* of the difference of potential which is necessary for preventing a further transfer of the positive electricity will in general differ from the value *Q* that is required for stopping the current of negative electrons. Hence, as there is but one difference of potential, a true state of equilibrium can never exist, unless there be some other process that has not as yet been taken into account. The only state of things that could be attained by the motion of the particles we are now considering would be one in which the difference of potential has such a value, intermediate between *P* and *Q,* that the two kinds of electrons flow in equal numbers towards the same side. It would be a final state inasmuch as there would be no further change in the charges or the potentials, but could not be called a state of equilibrium because there would be a never-ceasing stream of neutral electricity.

The question now arises, in this as well as in our first instance, what will become of the accumulating neutral electricity? We cannot suppose this mixture or combination of positive and negative electrons to be absolutely nothing, so that it might be drawn from a body or heaped up in it for hours or days without any observable change in its properties. We are therefore compelled to imagine some new process by which the neutral electricity is carried back from the places where both positive and negative electrons are concentrated towards those from which they are traveling away. Moreover, it is easily understood that a hypothesis of this kind can only suit our purpose if the moving neutral electricity is *not* composed of free electrons. If it were, all sections of the metallic system would after all be traversed by the same number of positive particles and also by the same number of negative ones and this is precisely what we have begun by denying. Our conclusion must be that the neutral electricity is to be regarded as a real combination, in pairs for instance, of positive and negative electrons, a combination that is formed in one part of the system and is decomposed again in another part towards which it is carried by a kind of diffusion.

Though this is rather complicated, we could be ready to admit it, if in doing so, we could obtain a quite satisfactory theory. Unfortunately, this is by no means the case, for it can easily be shown that the state of things we have now imagined would be in contradiction with the second law of thermodynamics. Indeed, it may be taken for granted that combination of a positive and a negative electron will produce a certain amount of heat and that, conversely, heat will be absorbed if the electrons are separated from each other. If now, as we have been led to assume, neutral electricity were built up in one of two metals which are in contact with each other and decomposed in the other, heat would be continually developed in the first and consumed in the second body. By Carnot's principle this can never be the case in a system that is kept at a constant uniform temperature, as our two metals may be.

If I am right in making this last remark, and if it cannot be invalidated by some new hypothesis, we need no longer continue our comparison of the two theories; we ought surely to give up all attempts to explain phenomena by the assumption of two kinds of movable electrons. I shall only adduce one argument more, which may be drawn from what is known of the so-called canal rays and the α rays of radioactive bodies. The positive electrons which constitute these rays have been found to have a mass of the same order of magnitude as that of the chemical atoms, a fact which lends a strong support to the view that in a metal the positive charges are rigidly fixed to the material atoms and that only the negative electrons can freely move over considerable distances.

As to the Hall-effect, which at first sight seemed to speak so strongly in favor of the two fluids theory, we shall have to examine whether it cannot be accounted for by the motion of negative electrons only. If we succeed in this, as perhaps we can by going somewhat deeper into the mechanism of the phenomenon than we have done in our somewhat superficial discussion of it, we shall after all come to a system of explanations much resembling Franklin's unitarian theory of electricity.

LEIDEN, April, 1906.

FORM ANALYSIS.

By A. A. MICHELSON.

(Read April 18, 1906.)

As a recreation in the midst of more serious work, I have been interested in the analysis of natural forms; and hoping that the results of this somewhat desultory occupation be not deemed too frivolous for so august an occasion, I will venture to present some illustrations[1] and generalizations which have occurred to me. I recognize that the subject is one whose adequate treatment would tax the best efforts of one who combined the insight of the scientist, with the æsthetic appreciation of the painter and the gift of language of the poet—and certainly I am lacking in all three—but especially in the power of adequate expression.

I had hoped that my contribution would at least have the merit of originality, but I find that many abler investigators have found a similar delight in this interesting field, and have expounded their ideas with a wealth of poetic imagery and of exquisite illustration such as I cannot hope to emulate.

Haeckel in particular has treated the subject of General Morphology in so exhaustive a fashion that it would seem futile to attempt to add anything of real interest or importance. This is most assuredly the case as regards luxurious wealth of illustration, and better cannot be done than to employ the "Art Forms in Nature" with grateful thanks to the man who has placed these within our reach.

Regarding his classification of forms there may be room for differences of opinion; and here I may venture to suggest some modifications for such a general scheme as shall include not only natural objects but also physical and mathematical forms such as interference patterns and graphs of analytical expressions.

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[1] The paper was illustrated with lantern slides taken mainly from Haeckel's "Kunstformen der Natur."

CLASSIFICATION OF FORMS.

I. *Symmetrical.*

II. *Unsymmetrical.*

I. SYMMETRICAL FORMS.

Congruence of parts by

- A. Rotation through 180° (Odd Symmetry).
- B. Reflection in plane (Even Symmetry).
- C. Rotation + Reflection (Partial Symmetry).

1. RADIAL Symmetry. (Corresponding points equidistant from radiant.)
2. AXIAL Symmetry. (Corresponding points equidistant from a fiducial line.)
3. PLANE Symmetry. (Corresponding points equidistant from plane.)

1. RADIAL SYMMETRY.
    - *a.* *Central* Symmetry. (Corresponding points on straight line through center.)
    - *b.* *Ovoid* Symmetry. (Radiant in axis but not central.)
    - *c.* *Excentric* Symmetry. (Radiant not in an axis.)
2. AXIAL SYMMETRY.
    - *a.* *Centraxal* Symmetry. (Corresponding points on same perpendicular through axis.)
    - *b.* *Dorsiventral* Symmetry. (Corresponding points not on same perpendicular.)
3. PLANE SYMMETRY.
    - *a.* *Triplanar* Symmetry.
    - *b.* *Biplanar* Symmetry.
    - *c.* *Bilateral* Symmetry.

The foregoing general systems of symmetry might be carried out in greater detail. For instance, the central symmetrical forms might be subdivided into (1) Spherical, (2) spheroidal, (3) ellipsoidal, (4) monoclinal, and (5) triclinal. So also the triplanar symmetrical forms may be classified into (1) Isometric, (2) quadratic, (3) rhombic; and the centraxial forms according to the number of similar parts into which an equatorial section may be divided, ($n = 2, 3, 4, 5 \ldots \infty$).

This, however, may well be left open, so that specialists in the various branches of science may elaborate details in such a way as to best fit their respective needs.

It will be noted that the proposed system extends the idea of radial symmetry to include the vast array of natural forms which *radiate,* though not necessarily from a center of figure and which would otherwise be considered either unsymmetrical, or as having merely axial or plane symmetry.

Another point is the *explicit* recognition of "odd" symmetry. This kind of symmetry is admitted by the mathematician on the same terms as "even"—though not so generally by the biologist or the crystallographer. It is easy to show that the one is quite as logical as the other.

The definition of symmetry is necessarily arbitrary—as in fact is well shown by the varied significance attached to the term by biologists, crystallographers and mathematicians. The last are coming to recognize any form as symmetrical when any admitted essential character is unaltered by any specified operation. So broad a definition would scarcely emphasize what is generally understood by the term; but if the "operations" be restricted to (1) rotation through two right angles, and (2) reflection in a plane, a symmetrical form may be defined as one whose corresponding points are (*a*) equidistant from a point, a line, or a plane; and (*b*) in which congruence may be obtained by a single "operation."

If condition *a* is fulfilled, but not *b* (as in the case of triclinic crystals), or if more than one operation is required for congruence, the form may be said to have *partial symmetry.*[1]

II. Unsymmetrical Forms.

Forms exist in endless variety which do not fall under any of these divisions, whose beauty and interest may be fully as striking as the symmetrical forms.

These may be classified according to the following scheme, which is only provisional.

A. Rhythmic.

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[1] If it were not for condition *a* the helix would have to be considered a symmetrical form; but *b* provides that it have partial symmetry.

B. Spiral.
C. Branching.
D. Scattered.
E. Irregular.

It would far exceed the limits of time and space to develop these in detail, but it may be mentioned that:

Rhythmic forms may appropriately be divided into *Line* rhythms, *Nets* or surface rhythms, and *Space* rhythms.

Further the " element " of the rhythm may be constant or (regularly) variable, and (in the case of linear rhythms) the line may be straight or curved.

Spirals may include flat, cylindrical or conoidal forms.

Branching forms may include all the well-known systems of arrangement of leaves and flowers, as well as the ordinary types of branches, to which may be added a number of arrangements peculiar to some of the lower organisms.

Scattered forms are such as are typified by the arrangement of the stars in space.

Irregular forms may be subdivided into Definite, Indefinite, Distorted.

In many cases the essential character of both symmetrical and unsymmetrical forms may be expressed in mathematical language; and even in the cases where such formulæ are only approximate expressions of the reality, such expressions may be the best and readiest means of ascertaining departures from the law.

Doubtless the biologist will look askance at what appears a grewsome array of mathematical symbols—at least until he finds how much less vicious they are than they look—but for the student of mathematics, there can scarcely be a more interesting and instructive practice for familiarizing himself with the properties of analytical functions, than to exercise his ingenuity in fitting the most appropriate expression to the forms he sees in Nature.

A further possibility in the expression of natural forms in mathematical language is the presentation of an ideal—so to speak—which the object is trying to embody; and the attempt to determine what this form would be under standard conditions would furnish an interesting and fruitful subject for research.

In contemplating the variety of exquisitely graceful forms which are exhibited in such abundance by the lower organisms, it is difficult to realize that their building up may be the result of the action of purely physical causes—and in truth it may be long before the theory of their formation is complete.

It is safe to say, however, that such general characters as symmetry of parts, rhythmic arrangements, systems of branching—which occur in strikingly similar fashion in such widely dissimilar objects as vegetation, protozoa, crytals, even liquids—must have some very general and fundamental explanation.

In many of the simpler cases such explanations are almost self-evident. For instance, the very frequent occurrence of spherical or spheroidal forms may be accounted for by the action of capillary forces; and the regular segmentation figures shown in many protozoan cells may be explained by the geometrical necessities of the case. An experimental study of such geometrical relations for figures in one plane was published by Alfred Mayer, and his results furnish many suggestive analogies which bear on the structure of the molecule as well as that of the micro-organism. An extension of the results to three dimensions would doubtless encounter serious experimental difficulties; but these may be overcome, or at least reduced sufficiently for practical purposes. The problem is really however a geometrical one—though the figures resulting from a purely mathematical investigation would be complicated by considerations of stability.

As an instance in which the cause of regularity is reasonably certain, let us consider the forms produced by a drop of colored liquid falling into a body of liquid of nearly the same density. As the drop descends it flattens out, the edge advancing more rapidly than the flattened surface. The equilibrium, already unstable, is still further disturbed by the momentum acquired by the peripheral parts, which causes them to assume a bell-shape. This form is still unstable, and if any accidental cause starts a thickening into drops, this will be assisted by the thinning of the adjacent parts which then lag behind. The same forces act at other points on the bell, to form "drops," the distance from the other drops being just enough to save being thinned. This distance (which depends on the properties of the liquid) determines the "period" of the rhythm. The drops or loops once formed produce vortex motions in the adjacent liquid which tend to separate them, and the action of these quasi repulsions is the stronger the closer the drops are to each other. These will consequently separate until the "repulsions" are equal—that is, till they are equidistant.

The same general explanation holds if the original drop is transformed into a ring.

The result is a beautifully regular (but inverted) dichotomous (tri- or tetrachotomous) branching, closely resembling vegetation.

The study of symmetry is doubtless of relatively subordinate importance from the point of view of the specialist—though Haeckel rightly insists on its significance in the evolution of the lower organisms.

In the study of crystallography considerations of symmetry are of high value; and a clear conception of relations of symmetry often wonderfully simplify the most abstruse problems in mathematical physics.

In the construction of buildings and bridges and engineering works in general as well as in machinery and scientific apparatus, and even of common tools and utensils,—the very necessities of the case have enforced a more or less complete symmetry of parts and of the whole.

Indeed, it is a common experience that the design of a piece of machinery may be so altered as to make it symmetrical often with a surprising increase in efficiency as well as beauty.

In designing for the sake of decoration, symmetrical forms are everywhere manifest, and the perception of their mutual relations is indispensable to the student of art. Occasionally, however, there is in decoration a deliberate departure from symmetry, and such a variation may greatly enhance the beauty and effectiveness of the design. We tire of too great uniformity even of agreeable kinds, and the element of variety is as important in art as an occasional discord is in music—its purpose being to heighten the effect of the succeeding harmony.

One of the great disadvantages of the modern tendency to extreme specialization in research is the loss of companionship of the sister sciences, with the attendant loss of perspective which a more general survey of the whole field of science should furnish. Should we not, then, utilize every opportunity which promises to further their union?

The geologist, the chemist, the physicist, the mathematician, may and occasionally do meet here on the common ground of crystallography. By a comparatively slight extension, the "ground forms" of organisms—as Haeckel terms them—may also be included with a corresponding extension of our society of sciences to include zoölogy and botany.

Nay, Art will demand a chair at the banquet, and Music and Poetry will also grace the feast.

BENJAMIN FRANKLIN AS METEOROLOGIST.

By PROFESSOR CLEVELAND ABBE.

(*Read April 20, 1906.*)

I have been requested by the chief of the Weather Bureau to represent him at this bi-centennial celebration and to express the profound respect that we have for Benjamin Franklin as the first meteorologist of America. It is true that we have records made by observers of the weather before he began his scientific activity, but the progress of meteorology has been such that we have now learned to put the philosophical investigator, that is to say, the man of research, far above the mere observer and recorder. Considered as a mere chronicle of passing events the study of the weather dates from the earliest ages; but considered as a rational investigation into its ultimate physical causes, or as the logical application of well established principles to the elucidation of unexplained phenomena, or as a system of research checked at every step by observations and experiments, the modern physical meteorology or theoretical meteorology, or dynamic meteorology deals exclusively with force or energy, and dates from the days of Galileo, Sir Isaac Newton, Huyghens, Descartes, Boyle and Gay-Lussac, with whom Benjamin Franklin was a worthy co-laborer.

At the present moment we are apt to apply the term "scientist" to one who devotes himself almost exclusively to some special department of research; but in Franklin's day those who contributed to research were generally occupied most of the time with other work, and this was notably true of him.

It is often remarked that events happening in one's childhood, or even a few years before one's birth, may produce such a profound impression on the whole community as to affect the general trend of one's thought and life; therefore, since Franklin did, from early youth, turn his attention so strongly toward natural phenomena, it may be worth while mentioning the fact that he was born in

January, 1706, and that shortly before this, on the 26th November, 1703, there was a most awful and destructive storm on the coast of England, in which a great number of merchants' vessels and crews were lost, as also thirteen sail of British men-of-war with fifteen hundred and nineteen men. This is the storm celebrated by Defoe's description, and to this day it must arouse the attention of every reader. Franklin was a child when in December, 1708, there occurred the coldest month experienced in New England up to that time, but I believe that we have no thermometric records for that time, as accurate thermometers, those made by Fahrenheit, were first brought to this country in 1720 by Dr. John Lining, of Charleston, S. C.

The various published editions of Franklin's writings have brought to public attention such a great variety of remarkable works that we are apt to think of him as a statesman, a printer, a philanthropist, an electrician and a patriot; but a careful study of his life and an examination of the great mass of unpublished manuscripts have shown that if he had done nothing else but his work in meteorology that alone would have entitled him to the highest rank. On this subject he thought and wrote for sixty years, from his diary of 1726 to his long range forecasts of 1786.

In the course of a study of old Philadelphia records, my attention has often been called to the profound influence exerted throughout this country and Europe by this great man. This present date is in fact the anniversary of his death and burial, yet we celebrate his birth and life, for he cannot be said to be dead and gone while the spirit that animated him still lives; while his influence is still felt; while his example is ever before us; while his words are repeated daily; while his maxims are the mottos of our own lives; while his monuments are everywhere and still increasing around us. He marked out the rules of life that will be followed for all time by reasonable men.

Right here let me stop to controvert an error that I have heard repeated today notwithstanding the most beautiful and truthful oration of yesterday by the Hon. Hampton L. Carson. I am told in effect that Franklin was not a religious man. There could not be a more monstrous error than this; it could only have emanated from those who never knew him personally, but in distant places and distant years had heard repeated some of his pointed satires against superstition, dogmatic theology, priestly craft, hypocrisy and the union of church and state. To all such perversions of pure religion he was a lifelong enemy. Bred for a preacher and theologian; educated for the pulpit of the Church of the Puritans; thrown by his removal to Philadelphia into the companionship of honest Quakers; transferred to England to battle for Freedom from corrupt aristocratic government with its Established Church and persecuting clergy,—he, of all men, had occasion from childhood to investigate the foundations of our faith; he had learned to distinguish between that natural religion that is revealed in the heart of every good man, and the dogmas, forms and ceremonies by which the established union of church and state was controlling a large portion of Europe, England and America. His common sense rebelled against this alliance of error with power; he would have none of it. He believed in the Fatherhood of God; in a religion that should be a blessing to men; in an honest statement of what we know, as distinguished from what we imagine or believe. He searched the foundations of his own faith as carefully as he examined into the phenomena of nature; he applied the same rational logic to them both, and sought by independent thought and clear views to emancipate his friends and countrymen from abject subserviency to dogmatic teachers, who were, as he believed, blinded by their own ignorance, fettered by conservatisms and beguiled by love of power and ease.

Being familiar with every line of the Holy Scriptures, far more so than many of his adversaries, and knowing how they misused the words of the Bible, he turned it against them, and by such satires as his proposed new version of the Bible, in 1760, held up to the severest public contempt the false courtiers that surrounded the king, thus goading them on to further acts of injustice and selfishness. In every case in which he seems to be violating all habits of reverential religious thought, you will, if you understand the circumstances of the occasion, find that he was, as it were, confounding his enemies out of their own mouths. Read his "Shavers and Trimmers" in the *Pennsylvania Gazette* for 1743, or the "Parable against Parsons," or the "Parable on Brotherly Love," and learn that there are shavers and trimmers at court, at the bar, in church, and in state.

Now turn to his "Articles of Belief and Acts of Religion" in the reprint by Smyth, vol. 2, pp. 91–100, and then recall that this elaborate declaration of the very highest form of natural religion, fit to be reproduced in every prayer book and Bible the world over, is the earliest autograph we have from Franklin, being written by him at Philadelphia, November 20, 1728, and was his daily pocket companion to the end of his life! He had been back from London two years and had gone through many trials, "but God was with Joseph." This composition marks an epoch in his early life corresponding precisely to what is frequently spoken of as "conversion"; but it was a conversion from men's devices to the higher and larger spiritual "republic of God." This "Articles" is a document that can be read only with the deepest reverence and affords us the true measure of the religious inner life of the noblest of men,—Benjamin Franklin. It fully explains how it happened that from 1753 to the end of his life Franklin was the most intimate companion and friend of "the good Bishop of Asaph."

If we systematically search for evidences of Franklin's interest in meteorology we shall find that in 1726, on his return voyage from London to Philadelphia, he kept a regular diary including items of wind and weather, noting, for instance, on August 30, 1726, "the full moon with a rainbow in the cloud to the west of us, the first lunar rainbow I have ever seen." Again, on September 14, "about 2 P. M. a solar eclipse covering ten-twelfth of the sun's diameter"; and on September 26th, "a sudden squall and rain, but the wind backed to northeast." Again, on September 28th, "run into gulf weed with barnacles and crabs" such as he seems never to have seen on the seashore, he even kept living specimens to see if more such crabs would hatch out, noting that they resembled the crabs he once saw in salt water at Boston and Portsmouth. On September 30th he sits up to observe the eclipse of the moon, which he says "occurred at 5 A. M. September 30th by the London Calendar, but at 11 P. M. September 29th to 2 A. M. September 30th by local ships' time," hence he calculates "that we are now four and a half hours, or 67 degrees, west of Greenwich." October 2d he noted that the water was changing color as though they were near soundings, it changed back again to blue on the 7th; and on the 11th they anchored at Red Bank, six miles from Philadelphia.

Having become proprietor of the *Pennsylvania Gazette* in October, 1729, he published therein an article on earthquakes (compiled, of course, from many sources, to which he gives full credit), as he says, "because of the interest excited in the subject by our recent earthquakes," alluding to slight local earthquakes of 1725 and 1726, that were felt throughout the allied colonies and roused many queries and fears. His editorial work stimulated his habits of reading and writing. He made notes of everything that could be useful to his readers. . . .

In 1726, August 21st, occurred the destruction of Peruna, Italy. In 1740 occurred the great earthquake at Lima. In 1750, that at Concepcion in Chili. On November 1, 1755, a part of Lisbon was swallowed up by an earthquake, sixty thousand persons perished in a few minutes and the ocean flowed over the site of the disaster. In 1755, October 30th, Damascus was destroyed and twelve thousand lives were lost. In 1760, November 25th, Tripoli was destroyed. These were the great earthquakes to which attention was forcibly directed during his early manhood, and such events must be associated with his own intellectual activity.

From 1732 to 1757 there were 25 annual issues of his "Richard Saunders Almanack." Now while it is true that in these he published conjectures as to the weather during the respective years, yet we are not to think of Franklin as a planetary meteorologist; for the fact is that in every one of these issues he disclaims all knowledge of the weather or astrology and pokes fun at his own predictions as utterly absurd and useless. He gives them, as he says, for what they are worth as food for reflection, as matters that are an essential part of a farmer's almanac,—not based upon any knowledge of nature but simply fanciful conjectures. They served, of course, to make his almanac sell, but they were not put forth in any serious vein of thought, but were really to him a matter of humor and fun. For instance, in his preface to his almanac for 1738 he makes "Mystriss Saunders" apologize for the imperfections of the almanac owing to the sudden departure of the husband, "poor Dick"; she adds, "upon looking over the months I see he has put in abundance of foul weather this year and therefore, I have scattered here and there where I could find room, some fair, pleasant, sunshiny, for the good women to dry their clothes in. If it does not come to pass according to my desire I have shown my good will, however, and I hope they will take it in good part." I believe that all the prefaces to Poor Richard's Almanac have been reproduced in Smyth's edition of the writings of Benjamin Franklin, and their humor is so telling that one may well wonder why the most credulous person in those days should have imagined that there could have been anything serious or reliable in such almanac forecasts. Nothing can be more ironical and comical than the following illustration from the preface for 1739: "Ignorant men wonder how the astrologists foretell the weather so exactly unless we deal with the old black devil, alas! it is as easy as is this. The star gazer peeps at the heavens through a long glass; he sees perhaps Taurus or the great Bull in a mighty chafe stamping on the floor of his house, swaying his tail about, stretching out his neck and opening wide his mouth. It is natural from these appearances to judge that this furious bull is puffing, blowing and roaring. Distance being considered, and time allowed for all this to come down [to the earth]—there you have wind and thunder."

But Franklin's life was devoted to the work of promoting useful knowledge and abolishing ignorance, superstition and credulity. His guiding principles were close observation and logical experimentation, and as these succeeded perfectly in his hands in all manner of business and in his dealings with mankind, he did not see why they should not hold good in dealing with nature. His proposal of May 14, 1743, to form a society of ingenious men from all parts of the colonies, to maintain a constant correspondence, and his offering of himself as secretary to that great undertaking, involved such an expenditure of time, thought and money, that we may well consider him as the father of coöperative systematic research in this country, even though a similar idea had animated the Royal Society of London for many years before. He had freed himself from the trammels of theological connections, he was not afraid to see the truth and state the truth; he recognized the limitations of man, that we can not possibly know for a certainty anything except by personal observation and that what we accept on the testimony of reliable witnesses must be capable of being tested by actual experiment or observation and by logical reasoning at any moment. He proposed that his new society should consider a long list of subjects mentioned by him specifically. This list did not include the weather or the atmosphere except by implication in the last paragraph, which reads thus: "and all phylosophical experiments that let light into the nature of things, tend to increase the power of Man over Matter and multiply the conveniences or pleasures of life." (Smyth, vol. 2, p. 230.)

As we delve into the great store of manuscripts relating to his life that have been preserved for us by his own habitual carefulness, we find new points of contact between him and modern ideas. His long record as a patriot, as an ambassador, as a master of pure English, as an electrician, as a man of the world, as a devoted brother, husband and father, has served to obscure some other equally prominent features of his character and his activity. His brilliant discovery of the identity between lightning and the ordinary electricity of the electric machine was not a so-called stroke of genius, but the inevitable result of his most persistent habit of thought and study, namely the determination that every idea that should occur to him of a philosophical nature should as soon as possible be tested by experiment. Accordingly we find the following memorandum in his daily note book, for November 7, 1749; after enumerating several analogies between lightning and the ordinary electric flash he adds: "The electric fluid is attracted by points; we do not know whether this property is in lightning; but since they agree in all the particulars wherein we can already compare them, is it not probable that they agree likewise in this? Let the experiment be made."

This was in November, 1749, at Philadelphia, at the season when lightning was rare, but he prepared to make the experiment as soon as practicable, doing it quietly in order to avoid attracting public attention, for a crowd was very apt to gather about him wherever he appeared. He at first planned to use a pointed rod on a high building,—afterwards he thought of attaching this rod to a kite. His silk kite had a pointed steel bar attached to the kite line, the latter was insulated by a silken cord and a key was tied at the junction, so that all the details imitated his laboratory experiments. It is evident that he did not expect any very dangerous shock, or shall we rather conclude that knowing the poor conducting power of the wet kite string, he know that it would be unable to bring down any really dangerous amount of electricity, and that he therefore would not be risking his life when he touched his finger to the key and received those first delightful sparks of natural electricity from the clouds.

From boyhood Franklin had been accustomed to drill himself in all the arts of a good debater. Whatever subject was up for discussion he kept it in mind during his daily work and jotted down on opposite sides of a sheet of paper the arguments *pro* and *con;* the considerations that should have weight or none; the reasons for thinking this way or that. After crossing off all unimportant matters he would reduce the whole argument to a few terse sentences arranged in logical order. Precisely the same course was followed when in later years he turned his attention to natural philosophy. He tells us that having become prosperous in business by strict logical attention to every consideration that made for success, he, in 1747, believed himself to be in a position to retire from active business. He therefore engaged Mr. Hall as his partner, to take most of the labor of the concern off his mind. He built himself a new house, bought the electrical apparatus of Dr. Spence, of Boston, and added other pieces and proposed to devote himself to a life of philosophical inquiry, of literature and elegant ease. In this he was disappointed, as he writes many years afterwards. "My neighbors, finding that I was a man of leisure, insisted upon my carrying on many public duties, and finally sent me to the Assembly; so that my philosophical inquiries were greatly interrupted." His scientific reputation has, in fact, rested largely upon the work that he did in Philadelphia and London between 1747 and 1757, much of which was published many years later. At the close of that period his correspondence, observations and notes on experimental work seem to have been boxed up previous to his starting for London on his first mission for the people as against the Proprietors. Eventually his collection was removed for safety to the country outside of Philadelphia; a part of it was lost during the revolutionary troubles, and another portion since then; but much has survived, and by searching over the fragments that remain we find that Franklin was always inquiring into the ultimate causes, or as we now say, the physical causes, of natural phenomena. We sometimes speak of him as an electrician, but before he studied electricity and in fact during his whole life he was seeking for explanations of atmospheric phenomena, so that the records will bear me out in asserting that he was among the first and best of the meteorologists, as distinguished from the climatologists, of his day. He tested the forces of nature and searched the motives of men. As his barometer and three thermometers were always at hand in his library, and as he refers frequently to the atmospheric pressure and temperature I shall not be surprised to find some regular record of these elements among the mass of his manuscripts that have yet to be examined, and I am hopefully hunting for his earliest records of the climate of Philadelphia. All are familiar with his inventions of improvements in the barometer, thermometer, and hygrometer; all know his important position as the discoverer of the progressive movement of the northeast wind and rain advancing from Georgia to New England. We must not forget that as postmaster at Philadelphia, 1737–53, and Postmaster General for all the colonies (1753–1775), he was traveling north or south incessantly, and handling reports from his local postmasters that showed him the conditions of the roads and the weather throughout the whole extent of his postal routes. No man was more familiar than he with all the current features of the country as to weather and crops, with the people and their mutual relations to each other, with the business and resources of the country. His argument as to the movement of the storm of 1743 was not a suspicion or guess, but a perfectly sound conclusion based on special inquiries and reports; it was precisely what we now call a careful research. I have collected many forgotten items that will go far to establish his reputation for solid work as a meteorologist, but perhaps the most interesting paragraph, and the only one I need here quote, relates to what are now called long-range forecasts of the weather and the seasons. Although he had published his own "Poor Richard's Almanac" for twenty years with its conjectures as to the weather, based on planetary configurations, yet in every almanac, without exception, he had ridiculed the whole subject, while at the same time knowing that popular superstition and ignorance would cause his almanac to sell. His own ideas as to such forecasts were far above those of his readers, and honest inquiry in this direction was always near his own heart. In the last years of his life he thought he saw one rational connection between cold winters in Europe and the foggy character of the preceding summers. This led him to write a very conservative memoir dated at Passy, near Paris, May, 1784. It was published in the Memoirs of the Manchester Society and I reproduce it from Writings of Benjamin Franklin (Sparks), Vol. VI, pp. 455–456, (Bigelow) Vol. III, p. 488, as follows:

METEOROLOGICAL IMAGINATIONS AND CONJECTURES.

PASSY, May, 1784.

There seems to be a region high in the air over all countries, where it is always winter, where frost exists continually, since in the midst of summer, on the surface of the earth, ice falls often from above, in the form of hail.

Hailstones, of the great weight we sometimes find them, did not probably acquire their magnitude before they began to descend. The air, being eight hundred times rarer than water, is unable to support it but in the shape of vapor, a state in which its particles are separated. As soon as they are condensed by the cold of the upper regions, so as to form a drop, that drop begins to fall. If it freezes into a grain of ice, that ice descends. In descending, both the drop of water and the grain of ice are augmented by particles of the vapor they pass through in falling, and which they condense by coldness, and attach to themselves.

It is possible, that, in summer, much of what is rain when it arrives at the surface of the earth, might have been snow when it began its descent; but, being thawed in passing through the warm air near the surface, it is changed from snow into rain.

How immensely cold must be the original particle of hail, which forms the centre of the future hailstone, since it is capable of communicating sufficient cold, if I may so speak, to freeze all the mass of vapor condensed round it, and form a lump of perhaps six or eight ounces in weight!

When, in summer time, the sun is high, and continues long every day above the horizon, his rays strike the earth more directly, and with longer continuance, than in the winter; hence the surface is more heated, and to a greater depth, by the effect of those rays.

When rain falls on the heated earth, and soaks down into it, it carries down with it a great part of the heat, which by that means descends still deeper.

The mass of earth, to the depth perhaps of thirty feet, being thus heated to a certain degree, continues to retain its heat for some time. Thus the first snows, that fall in the beginning of winter, seldom lie long on the surface, but are soon melted, and soon absorbed. After which, the winds, that blow over the country on which the snows had fallen, are not rendered so cold as they would have been, by those snows, if they had remained; and thus the approach of the severity of winter is retarded; and the extreme degree of its cold is not always at the time we might expect it, viz., when the sun is at its greatest distance, and the day shortest, but some time after that period, according to the English proverb, which says, "As the day lengthens, the cold strengthens"; the causes of refrigeration continuing to operate, while the sun returns too slowly, and his force continues too weak, to counteract them.

During several of the summer months of the year 1783, when the effects of the sun's rays to heat the earth in these northern regions should have been the greatest, there existed a constant fog over all Europe, and great part of North America. This fog was of a permanent nature; it was dry, and the rays of the sun seemed to have little effect towards dissipating it, as they easily do a moist fog, arising from water. They were indeed rendered so faint in passing through it, that, when collected in the focus of a burning-glass, they would scarce kindle brown paper. Of course, their summer effect in heating the earth was exceedingly diminished.

Hence the surface was early frozen.

Hence the first snows remained on it unmelted, and received continual additions.

Hence perhaps the winter of 1783–4, was more severe than any that happened for many years.

The cause of this universal fog is not yet ascertained. Whether it was adventitious to this earth, and merely a smoke proceeding from the consumption by fire of some of those great burning balls or globes which we happen to meet with in our course round the sun, and which are sometimes seen to kindle and be destroyed in passing our atmosphere, and whose smoke might be attracted and retained by our earth; or whether it was the vast quantity of smoke, long continuing to issue during the summer from Hecla, in Iceland, and that other volcano which arose out of the sea near that island, which smoke might be spread by various winds over the northern part of the world, is yet uncertain.

It seems however worth the inquiry, whether other hard winters, recorded in history, were preceded by similar permanent and widely extended summer fogs. Because, if found to be so, men might from such fogs conjecture the probability of a succeeding hard winter, and of the damage to be expected by the breaking up of frozen rivers in the spring; and take such measures as are possible and practicable, to secure themselves and effects from the mischiefs that attend the last."[1]

Franklin's argument may be condensed as follows: The soil is warmed during summer, and its heat comes back to warm the air during winter. Its effect on the air is felt until snow accumulates

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[1] See Sparks, "Life of Benjamin Franklin," Vol. 6, 455–457.

on the ground, so that the severest cold is delayed until long after December 21st. The fog of the summer of 1783 cut off the sun's heat, and left the earth cold; hence early snows were not melted; hence the cold began earlier and became more intense. If in future years a similar summer fog occur, it may be safe to predict that a similar cold winter will follow.

Two years after this we find the following two letters,[2] from his sister, Mrs. Jane Mecom. The first is dated Boston, December 17, 1786: "Your predictions concerning a hard winter are beginning to be verified in a formidable manner. The snow has been so deep that we might have been buried alive were it not for the care of some good neighbors"; and again, her letter dated March 9, 1787: "Your prediction has held invariable thus far, and as it began in October I don't see why it may not hold until May, for any appearance yet to the contrary." Must we not infer from this that Franklin had actually sent her a prediction for a cold winter beginning with October, 1786? The letter by him containing this prediction has not yet been found, but we may be sure that when found it will have in it nothing of planetary meteorology and nothing but strictly logical conclusions based on some well-established facts. I am hunting for this lost letter, but meanwhile I add to the laurel that crowns him another leaf, as the pioneer of the rational long-range forecasters, and of the physical meteorologists who will, undoubtedly, in the future develop this difficult subject.

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[2] These were first published at page 151 of the "Letters to Benjamin Franklin from his family and friends, 1751–1790," "author, W. D."; published by C. B. Richardson, 348 Broadway, New York, 1859. These letters had passed from William Temple Franklin to Dr. Franklin Becke.

WAS LEWIS EVANS OR BENJAMIN FRANKLIN THE FIRST TO RECOGNIZE THAT OUR NORTHEAST STORMS COME FROM THE SOUTHWEST?

By WILLIAM MORRIS DAVIS.

(*Read April 20, 1906.*)

The account of the "Middle British Colonies in America," prepared by Lewis Evans and published in Philadelphia in 1747, contains a remarkably clear and appreciative description of the main features of New Jersey and Pennsylvania. It is illustrated by a map which, like the text, bears witness to an extraordinary acuteness of observation and as well to an unusual power of generalization on the part of the author, who must be ranked as an early leader among American geographers. The map contains a number of explanatory legends, inserted where topographical details were wanting; and here we find, among various items, a significant statement regarding the movement of storms: "All our great storms begin to leeward; thus a NE storm shall be a day sooner in Virginia than in Boston." This brief statement has been taken to be the first recognition, as it surely seems to be the first published announcement of the progressive movement of storms, on which so much of the modern art of weather prediction depends. The statement is however not easily accessible to citation, for apart from the great rarity of complete copies of the first edition of Evans' essay—the map being lost from some of the few copies known to me—the map in the second edition was amended by replacing some of the legends with newly gathered topographical data; and among the matter thus removed was the statement above quoted concerning the movement of storms.

Evans' publishers were Franklin and Hall, and there is good reason to believe, as has already been pointed out by students of this question, that it was Franklin and not Evans who provided the statement concerning storms, along with some account of lightning and electricity, subjects with which Evans was not particularly concerned, but with which Franklin was much occupied. A reference to the letters in Sparks' "Life of Franklin" leaves no doubt on this point. In the very year of the publication of the first edition of Evans' essay, Franklin wrote to a friend as follows:

"We frequently have along the North American coast storms from the northeast, which blow violently sometimes three or four days. Of these, I have had a very singular opinion for some years, *viz:* that, though the course of the wind is from northeast to southwest, yet the course of the storm is from southwest to northeast; the air is in violent motion in Virginia before it moves in Connecticut, and in Connecticut before it moves at Cape Sable." It is a condensed duplicate of this statement that appears on Evans' map.

A question later arose as to the date when this "very singular opinion" had been formed. It was thus brought out that the observations which led to the opinion were made in connection with an eclipse of the moon, which Franklin failed to see in Philadelphia because the sky was covered with the clouds of a northeast storm, and yet which his brother, Thomas, successfully observed in Boston, where the northeast wind and its clouds did not occur until some hours after the eclipse. From this little hint, which many an investigator might have allowed to pass by without further consideration, Franklin gained the idea that the storm might possibly work its way against the wind; and this idea he confirmed by writing to a correspondent in Virginia, from whom he obtained the report that the storm there had begun sooner than it had in Philadelphia. The date of the lunar eclipse was afterwards determined to be in the year 1743. It would thus appear that Franklin contributed a statement of his discovery to Evans' map, making no claim whatever for recognition or priority; and indeed, suffering the statement to be obliterated without remonstrance, so far as now appears, when the second edition of the map was published. Generous as he thus showed himself, to the point of indifference, it is still fitting that we at this time should take pains to give credit where credit is due.

CAMBRIDGE, MASS., April 18, 1906.

PYRITE FROM CORNWALL, LEBANON COUNTY, PENNSYLVANIA.

By CHARLES TRAVIS.

(*Received June 22, 1906.*)

Several varieties of pyrite have been described from the Cornwall magnetite mines. Dr. Carl Hintze[1] mentions three,—(1) a combination of (111) and (100) without a reference; (2) a combination of (100), (111), (210), (321), (432), (221) containing 2 per cent. cobalt;[2] and (3) another variety for which no forms are given, but whose analysis shows 2.39 per cent. copper.[3]

The present paper is a crystallographic study of two varieties of Cornwall pyrite, which will be called Type I and Type II respectively. Type II is identical with the second variety of Hintze, the cobaltiferous; a rough analysis showed 1 per cent. cobalt, and all of the above given forms except (432) were found. Type I, on the other hand, shows no copper nor cobalt, and cannot be identified with any of the above. Apparently it has not been described before.

Occurrence.—Type II occurs scattered through the ore body itself. Crystals of this type grow in cavities in the magnetite, or in what apparently once were cavities. Byssolite and chlorite are commonly found with them. Where the ore is lean, the pyrite crystals are more numerous and are arranged roughly in layers.

The magnetite rests against limestone, which is altered at the contact. Just beyond the contact, in the limestone, Type I occurs, certain thin layers in the rock near the contact and parallel to it being rich with pyrite of this type. The crystals are so crowded in these layers that they largely interfere with each other's growth and perfect crystals are rare.

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[1] "Handbuch der Mineralogie," Vol. I (1900), p. 964.

[2] Blake in Dana's "Mineralogy" (1868), p. 63.

[3] Booth in Dana's "Mineralogy" (1855), p. 55.

TYPE I.

In habit Type I is a combination of (111) and (210), (111) being more developed. The pyritohedron (210) appears in all degrees of development from tiny triangular faces at the corners of the octahedron to equilibrium with it. The trisoctahedron (221) is common and seems to vary with (210), those crystals that have a small pyritohedron face having little or no trisoctahedron. This is not, however, true in all cases. The cube is comparatively rare.

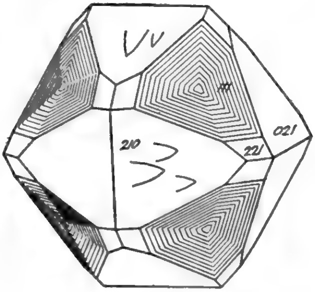

FIG. 1. Type I.

The faces in general are fresh and brilliant. Crystals from the weathered portions show a brown tarnish, which is removed by hot hydrochloric acid, leaving the crystal as bright as fresh material. The octahedron is striated parallel to its intersection with (210) and (221), producing six-sided figures. The pyritohedron has growth figures on its surface, in the form of curved isoceles triangles, whose sides are apparently the intersection of (210) with all possible trisoctahedrons from (221) to (111).

The cube, when present, is the most perfect face on the crystal, and is absolutely without striation. This may be partly due to the fact that it is never very large, and is usually a narrow face beveling the edge of the pyritohedrons. Fig. 1 illustrates the general habit of Type I, and makes an attempt to show the striation and growth figures. The crystals of Type I are, in general, small, few exceeding 0.5 cm. in diameter.

On a series of fifteen crystals of Type I the following forms were observed. Those marked (*) are new for pyrite, and one (14.11.8) has been reported from but one other locality, namely Kotterbach.[1]

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[1] K. Zimanyi in Groth's *Zeitschrift*, Vol. 39 (1903), p. 125.

	Trisoctahedrons.	Diploids.
(100)	(331)	(531)
(111)	*(11.11.4)	(321)
(210)	*(552)	*(753)
	*(773)	(432)
	(221)	(14.11.8)
	*(774)	(421)
	(553)	(13.7.3)
	(332)	*(952)
	*(443)	*(14.9.3)
	*(554)	*(542)
	*(665)	

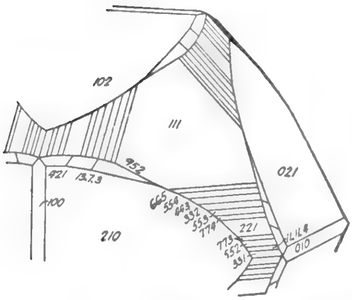

Fig. 2. Type I. Showing trisoctohedrons.

The observations determining these forms are as follows:

(331) was observed on crystal 2 with two faces in the same zone. The better one is about 0.1 mm. wide, and gives the measurement

331 ∧ 221.
Measured .... 6° 07′
Calculated .... 6° 12½′

The second face gives a much poorer image,—

Measured .... 5½° about; signal very poor.

(11.11.4), a new form for pyrite, was observed on crystal 7 with two faces, each about 0.2 mm. wide.

11.11.4 ∧ 111.
Measured .... 20° 48′
Measured .... 20° 51′
Calculated .... 20° 54′

(552), a new form for pyrite, was observed on three crystals, numbers 6, 7 and 10. Numbers 7 and 10 afford the best measurements.

552 ∧ 111.

Measured	19° 25′	(No. 7.)
Measured	19° 10′	(Two faces on No. 10)
Measured	19° 34′	
Calculated	19° 28′	

The measurements on crystal 6 are near enough to identify the form after its presence has been established by the above,—

Measured	19° 03′
Measured	19° 02′

(Two faces on No. 6, measured by direct observation on the face, which was too narrow to give a visible reflection.)

(773) One face of this form (new for pyrite) was observed on crystal 10. The face is quite narrow (0.1 mm.), but is sharp and gives a reasonably good reflection.

773 ∧ 111.

Measured	18° 28′
Calculated	18° 24′

(221) is a common form, and was observed on all the crystals measured,—

221 ∧ 111.

Measured	15° 54′	
Measured	15° 58′	(on various crystals; signal generally good)
Measured	15° 42′	
Measured	15° 46′	
	etc.	
Calculated	15° 47½′	

(This observation shows the degree of accuracy to be expected in the present work.)

(774), a new form for pyrite, was observed on crystal 3, with one narrow face between (221) and (111).

774 ∧ 111.

Measured	13° 05′	(signal rather good)
Calculated	13° 16′	

(553), observed on crystals 1 and 7.

553 ∧ 111.

Measured	12° 22′	(No. 1, signal poor)
Measured	12° 14′	(No. 7)
Calculated	12° 16′	

(332), observed on crystal 1. The zone containing it is somewhat rounded between $(22\bar{1})$ and (111), and gives by reflection a continuous band. The two principal faces in the zone (332) and (553), however, give distinct, measurable reflections.

332 ∧ 111.

Measured	10° 00′	(signal poor)
Calculated	10° 1½′	

This close agreement, however, must be regarded as accidental.

(443), a new form for pyrite, was observed on crystals 6 and 10.

443 ∧ 111.

Measured	7° 28′	(No. 6)
Measured	7° 05′	(No. 10)
Calculated	7° 20′	

(554), a new form, was observed on crystals 1 and 6, with one face on each. The form is small but distinct.

554 ∧ 111.

Measured	5° 46′	(No. 1)
Calculated	5° 46′	

554 ∧ 221.

Measured	10° 2′	(No. 6, mean of two reflections at 9° 49′ and 10° 15′ from 221)
Calculated	10° 1½′	

The agreement, curiously enough, is exact in each case, but this must be accidental.

(665), a new form, was observed as one small face on crystal 10.

665 ∧ 111.

Measured	4° 35′	(signal rather good)
Calculated	4° 45′	

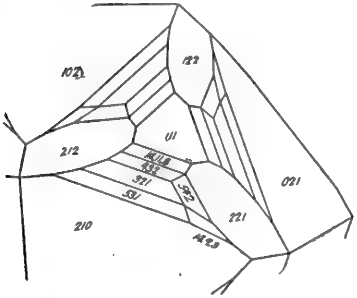

Fig. 3. Type I.

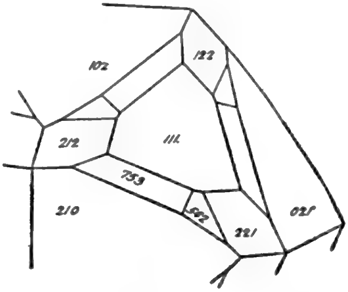

Fig. 4. Type I.

The diploids (421) (13.7.3) and (952) in the zone (112) appear as small, bright, triangular faces between (111) and (210), cutting off the upper corner of the latter face. Considerable difficulty was experienced in measuring them, as they were brilliant only when quite small; the larger faces were poor and rounded.

(421) Only one face was observed; crystal 6 shows a small face whose intersection with (210) is sensibly parallel with the centre line of the latter. It is about 1 mm. long by 0.5 mm. wide.

421 $\wedge$ 111.

Measured	28° 12′
Calculated	28° 6½′

(13.7.3) was found to be common. The form was determined from crystals 10 and 5.

13.7.3 $\wedge$ 210.

	ρ	ϕ	
Measured	8° 30′	11° 30′	(No. 10; signal good)
Measured	7° 55′	11° 49′	(No. 5; signal poor and divided into two. Angle given is mean position)
Calculated	8° 29′	11° 37′	

Other good measurements are

13.7.3 $\wedge$ $2\bar{1}0$.

	ρ	ϕ	
Measured	14° 05′	55° 40′	(No. 8; signal rather bright, but poor)
Calculated	13° 57′	55° 40′	

13.7.3 $\wedge$ 100.

	ρ	ϕ	
Measured	23° 00′	30° 22′	(No. 14)
Calculated	23° 12′	30° 22′	

Besides the above, (13.7.3) was observed on crystal 13.

(952) is a new form for pyrite. It affords smaller faces than (13.7.3) and while bright their smallness prevents any accuracy in measurement. The form was determined from two sets of measurements on crystals 4 and 14.

952 $\wedge$ 210.

	ρ	ϕ	
Measured	11° 40′	11° 15′	(No. 4; signal faint)
Calculated	12° 36′	11° 15′	

As a difference of 1° in ρ is equivalent to 12′ in actual position, the above discrepancy is about 11′.

952 $\wedge$ 001.

	ρ	ϕ	
Measured	28° 45′	79° 06′	(No. 14; signal poor)
Calculated	29° 03′	79° 12′	

(952) was also observed on crystals 5, 9, 10, and 12, giving measurements sufficiently good to identify the form after its occurrence had been established by the above.

The diploids (531), (321), (753), (432) and (14.11.8) in the zone (1$\bar{2}$1) between (210) and (111) were definitely observed on only two crystals, Nos. 11 and 15.

(531), observed on crystal 11 as one face about 0.5 mm. wide by 1.0 mm. long.

531 ∧ 111.

Measured	28° 38′	(signal good)
Calculated	28° 34′	

(321), observed on the same crystal as a comparatively large face, 1 mm. by 1 mm.

321 ∧ 111.

Measured	22° 18′	(signal brilliant and good)
Calculated	22° 12½′	

(753), a new face for pyrite, was observed on crystal 15 only. The face is about 0.5 by 0.2 mm., and is quite good and bright.

753 ∧ 111.

Measured	18° 05′	(signal good)
Calculated	18° 5½′	

(432) was definitely observed on crystal 11. It appears as a narrow face, somewhat rounded, about 0.1 mm. wide, being less than half the width of the face (14.11.8) occurring in the same zone.

432 ∧ 111.

Measured	15° 11′	(signal, rather bright but poor)
Calculated	15° 13½′	

(14.11.8) appears as a good, brilliant face about 0.3 mm. wide, on crystal 11.

14.11.8 ∧ 111.

Measured	12° 32′	(signal bright and fairly good)
Calculated	12° 32′	

(14.9.3), a new form for pyrite, occurs as a good face about 0.5 mm. wide on crystal 11. Its symbol was determined from measurement to two adjacent faces, as follows,—

	14.9.3 ∧ 111.	14.9.3 ∧ 221.	
Measured	15° 01′	27° 28′	(signal bright and good)
Calculated	15° 01′	27° 26′	

(542), a new form for pyrite, was observed on crystals 6, 11, and 15. In each case the face was small but bright.

542 ∧ 111.	ρ	φ	
Measured	33° 55′	21° 16′	(No. 6; signal faint but good)
Calculated	33° 57′	21° 03′	

(In the zone or (221) and (321).)

$542\wedge221$.

Measured	....	6°	(No. 11; signal good)
Measured	....	6° 13′	(No. 15)
Calculated	....	6° 24′	

The faces observed on each crystal are set forth in the following table:

Form.	Crystal No.														
	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
100		*								*			*	*	*
111	*	*	*	*	*	*	*	*	*	*	*	*	*	*	*
210	*	*	*	*	*	*	*	*	*	*	*	*	*	*	*
331		+													
11.11.4							+								
552						*	+			+					
773										+					
221	*	*	*	*	*	*	*	*	*	*	*	*	*	*	*
774			+												
553	+						+								
332	+														
443						+				+					
554	+					+									
665										+					
531											+				
321		?									+				
753															+
432		?									+				
14.11.8											+				
421			+												
13.7.3					+			+		+			*	+	
952				+	*				*	*		*		+	
14.9.3											+				
542						+					+				+

(* indicates that the measurements were used to determine the form and are given above under the description of faces.)

A detailed description of the fifteen crystals measured is unnecessary, but several crystals will be taken up in detail to give an idea of the relative prominence of the observed forms.

Crystal 4 is a typical crystal illustrating (952). It is a fragment, less than half of a complete crystal, about 4 mm. long. It shows one end of a cubic axis, with parts of four octahedron faces and two (210) faces, the larger of the latter being 3 mm. by 1.5 mm. and the other about half that size. One trisoctahedron face is present; if the larger pyritohedron is called (102) this trisoctahedron will be (122). It is about 0.5 mm. wide. The face (529) appears cutting off the corner at the junction of (102), (122) and (111). It is 0.5 mm. by 0.1 mm. and very perfect and bright.

Crystal 10 may be taken as typical of those showing the various trisoctahedrons well developed. It is a fragment about 5 mm. in diameter. It shows two complete pyritohedron faces each about 2 mm. wide. If we put them in the position of (102) and $(\bar{1}02)$ respectively, the other faces developed will be these,—about half of (021) and a small part of (021). The zone $(\bar{2}10)$, $(\bar{1}00)$ and $(\bar{2}\bar{1}0)$ appears, showing a small fragment of these faces. This is the only appearance of the cube on the crystal. The octahedron appears as small portions of the four upper faces. Three trisoctahedron zones appear, (221) being most prominent and giving faces about 1 mm. wide.

One of these zones shows two small faces occupying the position of (552) or (331); they are too minute to give a reflection. The next zone is rich in faces. Starting with the octahedron having the position $(1\bar{1}1)$, we have in order, $(1\bar{1}1)$, $(5\bar{6}6)$, $(1\bar{2}2)$, $(3\bar{7}7)$ on one side, and on the other $(\bar{2}55)$, $(\bar{1}\bar{2}2)$, $(\bar{1}\bar{1}1)$. The two (221) faces are about 0.5 mm. wide; the other trisoctahedrons uniformly about 0.1 mm. The third zone is similar, but shows two faces of (552) instead of one of (552) and one of (773), and a face of (443) instead of (665).

Two diploid faces are present, in the positions (529) and $(7.\bar{3}.13)$. The face of (952) is extremely small and bright, and cuts off the corner formed by (102), (111), and (122). The face of (13.7.3) is larger,—1 mm. long by 0.2 mm. wide,—and rounds off into the adjacent octahedron face, the rounded part being a conical surface. This rounding off was often noticed on (13.7.3) but not on (952).

Crystal 11 shows a rare combination of forms. It diverges remarkably from the type, and indeed might have been supposed to come from another locality, were it not for the perfectly typical markings on the pyritohedron and octahedron. Again, one of the forms it shows, (542), is found on crystals 6 and 15, which are perfectly typical in all respects. It is a rough fragment 1.5 cm. long, showing on one end an octahedron face with two adjacent (221) faces, and one face each of (321), (542), (14.11.8), (432), (14.7.3). A rough curved, irregular surface breaks the (111, 210) zone beyond (321) for the space of 2 mm.; (521) and (210) appear beyond this irregular portion. The octahedron and (221) faces are each a couple of mm. in extent; the face of (321) is 1 mm. each way, and the (14.7.3) and (531) faces are 0.5 mm. wide; (14.11.8) and (542) are about 0.3 mm. wide, and (432) about 0.1 mm. If the pyritohedron be regarded as (102), the observed faces have the positions, (102), $(1\bar{1}1)$, $(2\bar{1}2)$, $(1\bar{2}2)$, $(3\bar{1}5)$, $(2\bar{1}3)$, $(3\bar{2}4)$, $(11.\bar{8}.14)$, $(4\bar{2}5)$, and $(9.\bar{3}.14)$. Fig. 3 shows the combination of forms observed on this crystal.

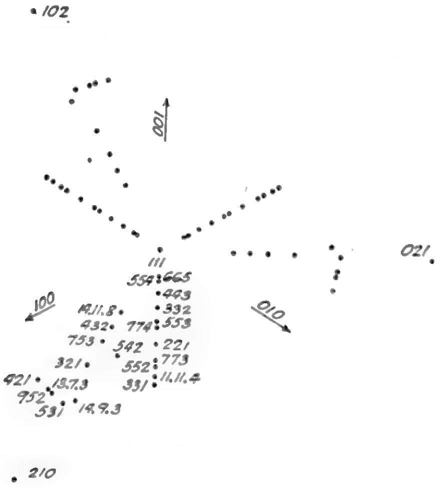

FIG. 5. Type I. Gnomonic projection on plane of octahedron. Illustrating zonal relations.

Crystal 15 shows two octahedron faces, (111) and $(1\bar{1}1)$, and a portion of (102). One face of the trisoctahedron is present, in the position (212). It is a trifle less than 1 mm. wide. (537) lies in the zone of (111) and (102); it is about 0.5 mm. by 0.2 mm. in extent. (425) appears as a nearly equilateral triangle about 0.2 mm. on a side between (212), (537) and $(1\bar{1}1)$. A portion of (210) is present below (212). (425) lies in the zone (210, 212). Fig. 4 shows this combination of forms.

ZONAL RELATIONS OF TYPE I.

The zone of trisoctahedrons, symbol $(1\bar{1}0)$, seems to be the most important in this type. It affords the greatest number of forms, and determines the growth figures on the pyritohedron face and the direction of one set of striations on the octahedrons.

The zone of (111) and (210), symbol $(1\bar{2}1)$, which is usually well developed in pyrite, seems to be of secondary importance in this type. While it determines the second set of octahedral striations, it was only observed on three crystals,—2, 11, and 15. On the last two of these, however, it affords a good series of forms, (531), (321), (753), (432), and (14.11.8).

The zone $(\bar{1}12)$ is important, affording (421), (13.7.3), (952) and (531), but curiously enough only a single face of this zone appears at a time. The primary importance of the zone of trisoctahedrons again appears in the fact that each of the above faces lies between (210) and a trisoctahedron of simple ratio, thus,—

210	210	210		210
421	13.7.3	952	and	531
212	323	434		111

This is also true of the scattering diploids (14.9.3) and (542).

210	210
14.9.3	542
443	332

These trisoctahedrons have the same ratio as the ones above, but are in a different zone.

The occurrence of the zone of trisoctahedrons as an important zone is so rare in pyrite that the writer has been moved to call this type the "trisoctahedral type." While a number of trisoctahedrons have been described on pyrite, their occurrence is usually scattering, one or two from each locality, and not in a complete series, as in the present case.

It will be noticed that the general shape of the crystal as indicated by Fig. 1 approaches a sphere, pointing to growth under great pressure.

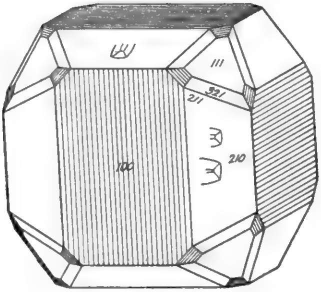

Fig. 6. Type II.

Type II.

This type affords larger and more perfect crystals, which often attain a diameter of 2 or 3 cm. In habit the crystal is a combination of cube and pyritohedron (210) with the octahedron, the diploid (321) and the trapezohedron (211) commonly occurring. The cube is striated in the usual manner for pyrite, parallel to the intersection with (210), and the (211) face is striated parallel to its intersection with the octahedron. Large natural etching figures are common on the cube, approximately square, in diagonal position. There is reason to believe that the striation on (211) is due to natural etching and not to growth. The pyritohedron has growth figures as shown in Fig. 6. With the exception of the cube and trapezohedron the faces are in general fresh and lustrous.

A series of seven crystals was measured, and the following forms observed. Those marked (*) are new for pyrite. In addition, two forms (15.11.7) and (11.8.5) have been previously observed from but one locality,—Porkura.[1]

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[1] B. Mauritz in Groth's *Zeitschrift*, Vol. 39 (1903), p. 357.

	Diploids.	Trisoctahedrons.	Trapezohedrons.
110	321	221	211
210	*753	*774	*744
111	11.8.5	*552	
	15.11.7		
	14.11.8		
	*876		
	*12.11.10		

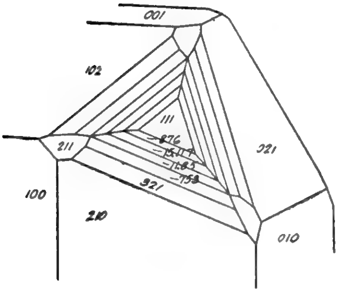

Fig. 7. Type II.

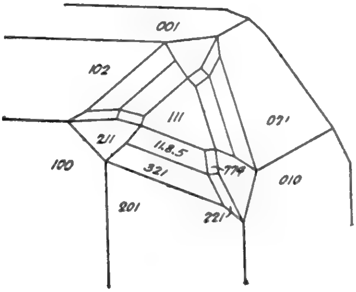

Fig. 8. Type II.

The observations determining these forms are as follows:

(321) was a face commonly observed. Some of the measurements are:

321 ∧ 111.

Measured	22° 14′	(Crystal 22)
Measured	22° 13′	(Crystal 22)
Measured	22° 10′	
	etc.	
Calculated	22° 12½′	

showing the degree of accuracy that may be expected in this type.

(753), a new form, was observed on crystals 23, 26 and 27,—

753 ∧ 111.

Measured	18° 05′	(Crystal 23)
Measured	17° 58′	(Crystal 26)
Calculated	18° 05½′	

753 ∧ 321.

Measured	4° 05′	(Crystal 27)
Calculated	4° 07′	

It is almost unnecessary to state that the above, as well as all other diploids observed on this type, were found to be sensibly in the zone of (111) and (210), so that one angle from the octahedron determines them.

(11.8.5) was observed as one narrow face on crystal 21, and two fairly good faces on crystal 23.

11.8.5 ∧ **111.**

Measured	17° 05′	(Crystal 21; signal poor and faint)
Measured	16° 59′	(Crystal 23; signal good)
Calculated	17° 01½′	

(15.11.7) was observed as one face on crystal 25. The face is quite good and is about 1 mm. wide.

15.11.7 ∧ **111.**

Measured	16° 33′	(signal good)
Calculated	16° 32′	

(14.11.8) was observed once only on this type, on crystal 26. The face is about 0.2 mm. wide.

14.11.8 ∧ **111.**

Measured	12° 37′
Calculated	12° 33′

(876) was observed as one good face about 0.7 mm. wide, on crystal 23.

876 ∧ **111.**

Measured	6° 44′
Calculated	6° 39′

(12.11.10), a new form, may almost be regarded as vicinal. On crystal 26 the following measurement was obtained:

12.11.10 ∧ **111.**

Measured	4° 19′	(signal poor)
Calculated	4° 15′	

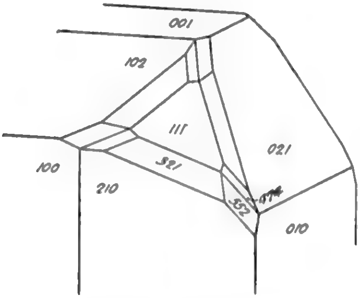

Fig. 9. Type II.

(221) and **(774)** were observed on crystal 23. They were extremely narrow faces, each lying in a zone with (211) and a diploid. Their narrowness prevented their giving any definite signal, but they were very perfect and gave fairly good results by direct observation, that is with the small auxiliary lens down in front of the telescope objective. There was no doubt as to the diploid determining (211). It gave,—

321 ∧ 111.

Measured	22° 13′
Calculated	22° 12½′

but the one determining (774), that is (11.8.5), was not definite. This problem was solved by measuring the angle between the zones (211)(774) and (211)(111) on a petrographical microscope reading angles to 5′.

121 ∧ 221.

Measured	11° 36′
Calculated	11° 30′

121 ∧ 774.

	ρ	φ
Measured	43° 39′	17° 01′
Calculated	44° 01′	17° 05′

The remaining trisoctahedron (552) will be discussed along with the trapezohedron (744) for a special reason.

(211) is of common occurrence and cannot be mistaken, on account of its striation. It usually gives a poor reflection. A measurement on crystal 22, however, gives,—

211 ∧ 111.

Measured	19° 28′
Calculated	19° 28′

(744) and **(552)** were observed on crystal 24. The pyritohedron (210) and the octahedron are present, and two faces of (321), one of which lies in a zone with (210) and (111). The two faces considered lie in a zone with (210) and the other diploid face (132). They are both extremely narrow, (744) not being wide enough to afford a satisfactory signal, and requiring measurement by direct observation.

	210 ∧ 474.	210 ∧ 552.
Measured	42° 23′	23° 48′
Calculated	41° 49′	24° 06′

There is considerable discrepancy here, (474) in particular being half a degree out. This is very probably due to the inaccuracy of the method by which it was measured. It is much more probable that the faces have the symbol given than that they are diploids with complex ratios approaching these symbols, for two other trisoctahedrons have been observed, (221) and (774), and one other trapezohedron (211), while no diploids were observed except in the zone of (111, 210).

It will be noticed that one form given by Blake, namely (432), was not observed,—a form with simple ratio, and one which if it occurred at all would be more common than forms of more complex ratios, as (15.11.7) and (11.8.5) There is some doubt in the writer's mind as to whether the (432) of Blake really is (432) or is a diploid with more complex ratio, as the above.

The following table sets forth the forms observed on each crystal.

Form.	Crystal No.						
	21	22	23	24	25	26	27
100	*	*	*	*	*	*	*
210	*	*	*	*	*	*	*
111	*	*	*	*	*	*	*
321	*	*	*	*	*	*	*
753			*			*	*
11.8.5	*		*				
15.11.7					*		
14.11.8						*	
876			*				
12.11.10						*	
221			*				
774			*				
552				*			
211	*	*	*		*	*	*
744				*			

Crystal 23 is perhaps the only one of this series worthy of a separate description. This crystal is about 1.8 cm. in diameter. It shows the cube (100) with its adjacent pyritohedron faces (210) and (2$\bar{1}$0), and (001) with (102). There are two octahedron faces, (111) and (1$\bar{1}$1). The former face, which is surrounded by the zones containing the new diploids, would have been 3 mm. on a side, but another smaller crystal has grown into it, leaving only two corners. Of the three zones of diploids that surround (111), one has two faces in it, (11.8.5) and (321); the second three, (876) (11.8.5); and the third two, (753) and (321). The (876) and (753) faces are about 0.7 mm. wide, and the (11.8.5) face in the second zone is 1 mm. wide. All are perfect faces. The (11.8.5) zone is good at one end, but at the other runs off into a broken vicinal face. At its good end is a face of (211), about 1 mm. long. Between this latter and the (11.8.5) and (321) faces lie the trisoctahedrons (221) and (774) described above.

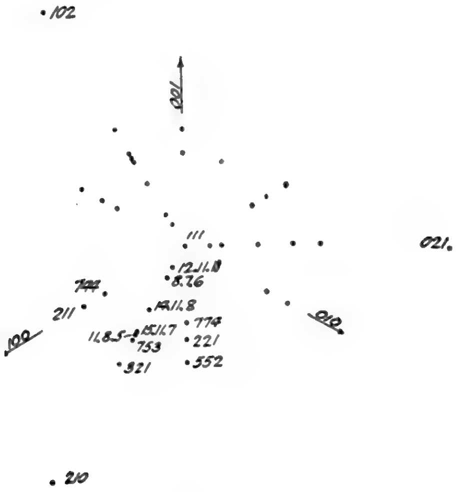

Fig. 10. Type II. Gnomonic projection on plane of octahedron. Illustrating zonal relations.

Zonal Relations of Type II.

The most important zone in this type, outside of the cube-pyritohedron zone, is that of the octahedron and pyritohedron, symbol (1$\bar{2}$1). This affords all the observed diploids, seven in number, one of which (321) is practically always present. The trisoctahedron zone was developed on one crystal, affording (221) and (774), each of which was in a zone with (211) and an observed diploid. Thus,—

211	211
212	747
213	8.5.11

This is not true of (552), which would agree with the diploid (741). No diploids were observed between (321) and (210), (321) being the last of the series.

The development of the zone between the pyritohedron and a non-adjacent (321) face, e. g., (210) (132) on crystal 24 is probably accidental, due to the irregular development of the crystal. This zone affords (552) and (474).

The writer wishes to call attention to the method described above, used in determining the small faces (221) and (774). He has not heard of its previous use as a practical method of measurement. The angle ρ can be measured to within 5′ or 10′ with a microscope, and this is accurate enough for all purposes if ϕ is not large. The easiest way of holding the crystal on the stage is to set it on a cone of wax on a glass slip; the face chosen as a reference face can be made approximately horizontal by hand. Having measured ρ, the crystal can be set up on an ordinary one-circle goniometer, and, using the Websky signal, ϕ may be measured by observing the face directly and finding the point of maximum brilliancy of reflection.

In concluding, the writer wishes to express his indebtedness to Professor Amos P. Brown, not only for the specimens which furnished the material for this paper, but also for valuable assistance in its preparation. Thanks are also due to Mr. Kenneth Williams for crystals of Type II used in the above series.

ELEMENTARY SPECIES IN AGRICULTURE.

By PROFESSOR HUGO DE VRIES,
AMSTERDAM.

(*Read April 18, 1906.*)

Franklin's name is honored all over the world, and the splendid services he has rendered to science and to humanity have had their influence in Europe as they have in America. Your president and secretary, in inviting me in your name to attend this celebration of the bicentennial of your great founder's birth, have offered me a welcome occasion of paying a tribute to his memory. It is not, however, without hesitation that I have accepted this honorable invitation. Philadelphia has been the center of botanical interest and research in this country for more than a century. The various contributions of your society to biological science are followed in Europe with intense interest. To speak before such a famous sphere of learning is not only a high honor, but also imposes a great obligation. In accepting the invitation I have trusted to your indulgence and to the interest shown by you in the broad questions of evolution, which of late have returned to the empirical methods and principles laid down by Darwin.

New facts and new conceptions are the result of half a century of industrious work. Darwin relied for a large part on the methods of selection which at his time were in use both in agriculture and in horticulture. He tried to show that the evolution of species at large has followed the same laws that underlie the evolution of races and varieties in culture. In broad lines he has succeeded in convincing his contemporaries of the validity of this analogy. Agricultural and horticultural experience, however, were at his time only imperfectly developed, and the amelioration of races, though successful in a large number of cases, had no really scientific basis. It did not afford all the evidence required for a thoroughly reliable theory. Complying with the prevailing belief of the most renowned agriculturists, which considered the breeding of races as a slow process of gradual amelioration, he proposed the same slow and almost imperceptible changes as the source of evolution in nature. Since his time experience and theory have made very manifest advances. Especially the principle of the unit-characters, which is the basis of the theory of origin of species by mutation, leads us to the acceptation of saltatory changes or so-called sports as the most probable way of nature to produce new forms. According to this theory species are not changed into one another, but new forms arise sideways from the old ones. The whole strain continues unchanged and only produces from time to time single aberrant individuals. These are the real sources of all progress, and experience has shown that in the main their new characters are hereditary, and that their progeny remains true to their new types even from its first appearance.

In agricultural breeding-practice the production of new races is an intricate problem. In many cases their relation to the theoretical conceptions is quite clear, in others it is still surrounded with doubt. In my book on the mutation theory I have explained how the obvious facts agree with that idea, but it was at that moment impossible to remove all doubts and so I purposed to return to these questions another time (Mut. Th., I, p. 82). Five years have since elapsed far more complete analysis of the agricultural breeding processes. and new discoveries have been published which enable us to give a Especially at the agricultural experiment station in southern Sweden quite unsuspected facts relating to the variability of agricultural crops have been brought to light. They are of a nature to throw over all the old ideas concerning race amelioration and give proof that the methods now generally in use in Europe are faulty as well from a practical as from a scientific point of view. The director of that station, Dr. Hjalmar Nilsson, has discovered that most of our ordinary agricultural crops are not only composed of elementary species, as was long known before him, but that each cultural variety contains hundreds of sharply definite types. These are widely distinct from one another as well in botanical characters as in those properties which decide on their utility from the breeder's point of view. Moreover, they differ so widely from one another as to respond to almost all the requirements of the agricultural practice. By simply searching among them, the proper type may forthwith be found for almost each gap in practice. In this way they are seen to afford almost unexhaustible material for selection.

For to-day's theme I have chosen an application of these discoveries of Nilsson to a criticism of the current views concerning the bearing of agricultural breeding processes on the theory of evolution. Formerly I gave the warning not to trust too much to these processes and to make use, in scientific discussions, only of the most simple and clear cases (Mut. Theory, I, p. 59). The new facts, now at hand, go to prove that even the apparently simple methods of selection have been far more complicated than their authors suspected. The slow and gradual working up of a cereal to a previously fixed ideal seemed to be a process of the simplest possible nature. In reality, however, it is composed of a series of factors, which the breeders themselves have not recognized, and which, therefore, it is now often impossible to discern in their descriptions. In broad lines such an analysis has been made practicable by Nilsson's discoveries. Unfortunately it conduces to a less high appreciation of the breeder's merits (Mut. Th., p. 82), but on the other hand it gives a stronger support to the theory of the saltatory origin of species.

The breeding of cereals results in varieties, which are as constant and independent as the best horticultural sorts. In some cases they are known to originate in the same way, by accidental sports, as in the instance of Beseler's oats losing their needles. Here their complying with the principle of mutation is obvious. In the large majority of cases, however, including the most renowned improvements of cereals and other crops, they are said to have been produced by the common slow and gradual process of selection. All such cases are surrounded with doubt, as well concerning their real origin, as in view of the degree of self-dependency which is reached at the end. Often practical reasons lead one to prefer the original seed to one's own harvest, especially when it is difficult to keep the cultures clean from vicinistic impurities. A race, which is really self-dependent, may in this way seem to be permanently related to the continuous selection of its pedigree. It is especially in Germany that this method of slow amelioration is much beloved and has given admirable results. One of the best known instances, and for which the historical records are the most complete, is the renowned rye of Schlanstedt, produced by Rimpau, which is now largely cultivated all over the central parts of Germany and the northern districts of France. In the year 1876 I had the privilege of visiting Mr. Rimpau on his farm at Schlanstedt and of studying his cultures. The choicest of his new rye occupied a small patch out on the fields, but surrounded by cultures of vegetables and other plants not belonging to the cereals. These minor cultures occupied a large square, which in its turn was surrounded by a complete range of shrubs. Thus the rye, standing in the midst of the square, was suffi- possible contamination by pollen of other varieties. On the other hand, it was given the same soil and exposure and almost the same cultural treatment as the average cultures.

This race had been started by Rimpau nine years before, in the year 1867. At the time of the harvest of that year he had inspected, as he told me, a large number of his rye fields and selected all the ears which seemed to him to surpass the others quite strikingly. He brought home a handful of them, repeated the trial and mixed their seeds. This mixed condition of his seed in the beginning of his race has now become the weak point, where the whole principle of his method is open to criticism.

The seeds were sown next year, and in the harvest the same selection of the best ears was repeated. Care was taken to exclude all those, which by some external condition could have profited from more space .or more manure than the remainder, and could have grown large by such accidental means. No care, however, was taken to isolate the individuals and to sow their seeds separately, the principle being that all the plants belonged to one race, and that this race had to be ameliorated. This principle of ameliorating a race without isolating its possible constituents seemed at that period to be the right one, though now it can hardly be considered as scientifically correct.

Each year in the same way the best ears were chosen from the continuance of the choicest strain, and after the exclusion of all ears of minor value the remainder were sown on a field and multiplied without further selection in order to produce all the seed required for the sowing of the whole farm. It took three or four years to reach this quantity. After twenty years of continued selection the choice strain was so much improved as to produce a race distinctly richer than the ordinary varieties of rye in middle Germany, and slowly but gradually it found its way first into the surrounding farms and afterwards over large parts of the country. During this period Rimpau was enabled to sell all his harvest as seed-grain, obtaining in this way a most satisfactory recompense for his labors. Shortly afterwards the rye of Schlanstedt was introduced into France, where it soon overthrew the local varieties, especially in the departments north of Paris. Even there it is ordinarily cultivated from original seed, produced directly by Rimpau or multiplied only during some few generations by seed merchants.

For our critical purpose, it is highly interesting to note how a French agriculturist, Professor Schribaux of the Institut Agronomique of Paris explains the conditions of keeping the Schlandstedt rye up to its original qualities. He says: "In order to do this, care must be taken to sow the seeds on a field which is as far removed as possible from all other cultures of rye. Moreover, the field should be large and protected all around by a hedge of trees and shrubs. Without this precaution the rye of Schlanstedt would soon degenerate through accidental crosses with the local varieties." Such crosses would, under any other conditions, be unavoidable and soon wholly deteriorate the race ("Almanach du Cultivateur," 1892, p. 69).

From this judgment, given by an authority who has so much contributed to the wealth of northern France by the introduction of this variety, we may deduce some conclusions as to the constancy of Rimpau's rye. It is clear that Schribaux takes the race to be substantially constant, and explains the necessity of continued selection only by the impending danger of crosses with varieties of minor value. Hence it follows that the main significance of the pedigree culture on the farm of Rimpau must be the same, and that at least in later years his pedigree must have gained a degree of uniformity, which is in no need of any further improvement. The real act of effective selection is thereby brought back to the first years, but how many generations of true selection it has taken to render the rye of Schlanstedt uniform and pure it will, of course, always remain impossible to tell. The explanation of Rimpau's success must, therefore, for a large part remain hypothetical. If now we try to give such an explication on the ground of the theory of mutation and of the already quoted discoveries of Nilsson we may suggest the following: At the period when Rimpau started his pedigree, his rye fields must have contained numerous elementary species, not observed or distinguished by him or by any other agriculturist of his time. Among the ears which he selected a good number of these aberrant types will, of course, have been represented, since he selected only those which caught his eye by some striking useful difference from the main type. Of course, he sought for ears of one and the same ideal type, having a large number of big kernels. But notwithstanding this, his handful of ears must have belonged to more than one elementary species. Among these units of his selection some will have been better yielders than others, and the subsequent selection of his twenty years of pedigree-culture will slowly but surely have eliminated the units of minor worth. This would result, at the end, in a complete isolation of the best one of all the types, which he originally, but unconsciously, selected and mixed.

Or, in other words, Rimpau's pedigree culture was started as a mixture of a number of excellent types, and his yearly selection gradually reduced this number, until he had isolated and purified the very best one among them. This point was, of course, only unconsciously reached, but then it must have made his rye independent of all further real selection, reducing the process to the care of excluding vicinism.

If this explication of Rimpau's process is true it, of course, holds good for all similar cases of slow and gradual amelioration of agricultural plants by selection. Thereby it would deprive the theory of the origin of species by small and continuous changes of its last support in the realm of the vegetable kingdom.

It remains to be shown that the new facts give sufficient proof of the exactness of this suggestion. They relate to the question of the part which fluctuating variability and mutability may have played in the selection culture of Rimpau. An exact notion of the first phenomenon, as stated by the works of Quetelet (1870) and Galton (1889) had only found its way into botanical investigations about the year 1894, or nearly twenty-five years after Rimpau started his pedigree of rye. At his time, therefore, no distinction of this kind could be made, and it is only natural that he took his selected specimens to be the extremes of ordinary fluctuating variability

This point of view and this lack of distinction between the now so clearly contrasting processes has prevailed for a long time among agriculturists. As an instance I may quote the work of Willet M. Hays, now in Washington (1899, Bull. No. 62, Agric. Exp. Station, Minnesota). He has ameliorated the wheat of Minnesota by breeding, from the local races Fife and Blue Stem, better and more yielding varieties, which now in large part have supplanted the old types. Besides his practical results he has given some theoretical considerations in which he compares his selected mother plants with the principle of fluctuating variability and explains them as extremes in the curves which constitute the law of Quetelet. "In each one thousand plants of wheat," he says, "there are a few phenomenal yielders, and the method of single-seed planting makes it practicable to secure these exceptional plants, and from these new varieties can be made" (p. 429). But according to our present knowledge, the isolation of such plants, if they were truly extremes of fluctuating variability, would lead to a regression to mediocrity, as it has been called by Galton, and not to constancy nor to an exact keeping up of the extreme type. Therefore the supposition is allowed that the phenomenal yielders of Hays, were in reality representatives of distinct elementary species, which had been hidden until his time. His method of selecting enabled him to single them out, and his new principle of single-seed planting, which conduced to his high achievements, at the same time indicated the way for an explication on the basis of our present views concerning the different types of variability.

It would take me too long to describe the methods and cultures of the Minnesota Experiment Station, and I may assume that their leading principles and practical results are well known. But I wish to point out that, exactly in the principle of sowing the seeds of individual selected plants separately, Hays gained a distinct advantage over the slow process of Rimpau and the other German breeders. He found, by his method, that the isolated strains are at once constant and pure. They had only to be multiplied in order to give a new race. Of course, the different mother plants had to be compared in their progeny, and among a large number of such new pedigree races only one or two were found to be the very best. The remainder had to be rejected, and only those few excelling ones could be introduced with advantage into the field-cultures of the state.

If now we compare this principle of Nilsson and Hays with the method of Rimpau we find that the Swedish and American breeders by one single choice isolated the very best strains and observed them to be constant and pure. The German breeders, on the other hand, by selecting a number of ears, must have got impure races, and wanted a long succession of years and a constantly repeated selection in order to reach the same result in the end.

Hence we may deduce the supposition that if Rimpau in starting his experiments, now forty years ago, had had at his disposition our present knowledge of variability, he would have sown the kernels of his selected ears separately and selected at once among the resulting strains the very one which now bears the name of his farm. No continuous culture and repeated selection would have been needed, and the false appearance of a slow and gradual improvement of a race by selection would simply have been avoided.

The German breeding process has always been one of the most valuable arguments for the theory of gradual selection and was of late considered as its last botanical support. By means of the discoveries of Nilsson and Hays this support has now been broken down, and agricultural selection is no longer an argument against the conception of an origin of species by saltatory changes.

DILUTE SULPHURIC ACID AS A FUNGICIDE.

By HENRY KRAEMER.

(*Read April 20, 1906.*)

Sulphur has been used as a disinfectant, or deodorant, since very ancient times. In The Odyssey reference is made to the burning of sulphur for this purpose. As a parasiticide, insecticide and fungicide it is extensively used at the present time.

The value of sulphur as a fungicide in destroying epiphytic fungi, particularly the powdery mildews of the rose, vine, hop, etc., is well known. Sulphur is also sometimes used in destroying the spores of endophytic fungi, or at least in preventing their growth. As a fungicide sulphur is used or applied in several ways. (1) In the sublimed form (flowers of sulphur) it is dusted on the plants. (2) Both sublimed sulphur and ground roll-sulphur, or "brimstone" (flour of sulphur), are mixed with water to form a paste, which is applied to the heating pipes in green-houses, the finely divided vaporized sulphur thus produced collecting on the plants; or small pieces of roll-sulphur may be carefully heated on a sand bath, the finely divided vaporized sulphur being distributed in the same way; or sulphur in the form of an impalpable powder known as "ventilated sulphur" may be used, as in Germany and Italy where it is used for fighting the vine blight. (3) Sulphur is also applied in combination with lime in the form of solution, as in the "lime-sulphur" washes, these being solutions of a number of sulphides and lower sulphates of calcium.

Sulphur is insoluble in water and is not affected by most reagents under ordinary conditions, and of itself could not be considered to have fungicidal properties. Nor is it likely that it exerts a mechanical action like certain of the insecticides, as pyrethrum flowers, or insect powder, which act by closing the breathing pores of the insects. The action of sulphur as a fungicide is probably due, then, to certain compounds of sulphur which result from the manner of using it. It is generally considered by plant pathologists that when sulphur is used in a powdered form as a remedy for plant diseases, the effects are due to sulphur dioxide. While sulphur dioxide undoubtedly does have a powerful fungicidal action, it is usually conceded that sulphur dioxide and sulphurous acid are exceedingly toxic to higher plants as well. Owing to the high temperature at which sulphur inflames (260° C.) and the rapid evolution of sulphur dioxide, as well as its unequal diffusion, sulphur is never burned in order to secure its fungicidal effects, but is employed either in the form of a powder, or by gently heating it, when a certain portion of it is sublimed, thus distributing it over the plants in a finely divided state. In Italy the grape-growers and horticulturists have found that the efficiency of powdered sulphur when dusted on plants is in direct ratio to the degree of fineness.

It is stated by Bloxam[1] that "finely divided sulphur, especially sublimed sulphur, is gradually oxidized and converted into sulphuric acid when exposed to moist air." It is also well known that sublimed sulphur contains a certain amount of sulphuric acid. Not only is this true, but it is claimed that if the sublimed sulphur be not dried after washing it to free it of acid, sulphuric acid is again formed. Furthermore, it has been shown by Pollacci[2] that, independent of the presence of organic matter, sulphur is changed directly into sulphuric acid when mixed with the soil.

The fact that sulphur is easily oxidized in a moist atmosphere to sulphuric acid taken together with the fact that sulphur when vaporized on steam pipes is more efficient as a fungicide than when the flowers, or flour, of sulphur are dusted on the plants, led to the supposition that this increased efficiency might be due to the increased proportion of sulphuric acid formed. Accordingly, experiments were undertaken to determine what compounds are formed when sulphur is slowly heated. An apparatus was constructed for heating sulphur and for collecting the gases formed. The apparatus for collecting the gases consists of a bell jar connected with two U-tubes. The bell jar is suspended over a plate in such a manner as to permit of the free entrance of air from below, or at a distance of 5 to 10 cm.

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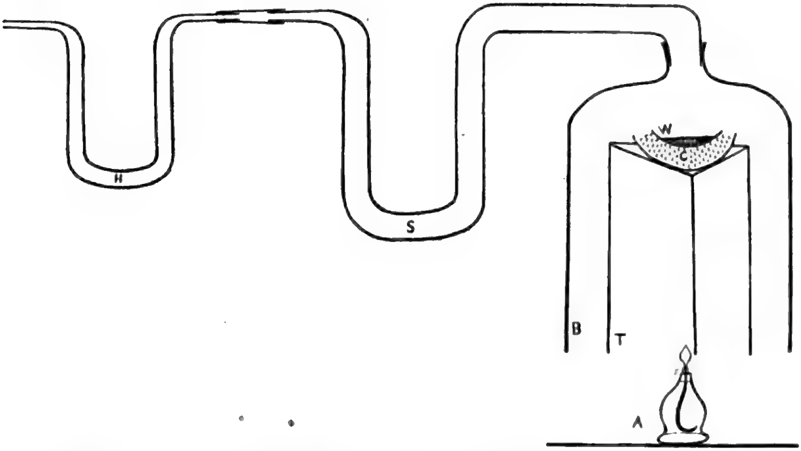
[1] Bloxam's Chemistry, seventh edition, 1890, p. 189.

[2] Pollacci, *Gaz. chim. ital.*, through *Jour. Pharm. Chim.*, 1874, p. 330.

above the plate. The sand bath used for heating the sulphur is placed on a glass tripod under the bell jar, and an alcohol lamp is used to heat the sand. A definite quantity of N/10 sodium carbonate solution is placed in the U-tubes and the free end connected with an aspirator, or suction pump.



Apparatus for heating sulphur and collecting acid gases formed. *A,* alcohol lamp; *T,* glass tripod; *C,* porcelain dish containing sand; *W,* watch crystal containing sulphur; *B,* bell jar supported by a clamp; *S,* U-tube with one end ground to fit bell jar and containing 15 c.c. of tenth normal sodium carbonate solution; *H,* U-tube containing distilled water, and having one end connected with U-tube, S, by means of rubber tubing, and the other with an aspirator.

The operation was conducted as follows: To the first U-tube, which was well ground to fit the opening in the bell jar, were added 15 c.c. of tenth normal sodium carbonate solution to combine with the sulphur acids formed. To the second U-tube 5 c.c. of water were added. Definite quantities of sulphur were weighed out in a watch crystal, and heated on the sand bath for several hours. The heat was regulated to a certain extent so as to minimize the loss of the vaporized products due to their escaping from below the bell jar, that is, the flame was removed at times and the space between the plate and bell jar was reduced to but a few centimeters. At the end of the operation the apparatus was allowed to cool, and the watch crystal containing the sulphur weighed to determine the total loss of sulphur by vaporization. The contents of the two U-tubes were then poured into a graduate and the U-tubes, the bell jar and the plate carefully rinsed with distilled water and the rinsings added to the graduate, the contents of which was made up to about 200 or 300 c.c. One-half the quantity of solution was used for determining the sulphuric acid formed, while the other half was used for determining the sulphurous acid present.

To the portion used for determining the sulphuric acid 5 additional c.c. of tenth normal solution of sodium carbonate were added, and the solution titrated with tenth normal sulphuric acid, using methyl orange as indicator. The difference between the number of c.c. of sodium carbonate solution used and the number of c.c. of sulphuric acid solution consumed gives the equivalent of sulphur acids formed. That the greater proportion of acids formed under the conditions described is sulphuric acid, is shown by adding barium chloride with an excess of hydrochloric acid to a hot solution, the precipitate formed being insoluble. The amount of sulphur converted into sulphuric acid is very easily calculated.

The other half of the original solution was titrated with decinormal iodine solution using starch test solution as indicator, but it was found that the proportion of sulphurous acid formed by heating or subliming the sulphur in the manner indicated was quite small.

In the following table the results of a number of experiments are given:

	Weight of Sulphur Heated.	Number of Hours Heated.	Loss of Sulphur by Vaporization.	Percentage of Vaporized Sulphur Converted Into Sulphuric Acid.	Percentage of Vaporized Sulphur Converted Into Sulphurous Acid.	Percentage of Vaporized Sulphur Driven Off as Sublimed Sulphur.	Remarks.
1	0.200	3	0.050	8.02	0.31	91.67	
2	0.500	3	0.120	7.08	0.53	92.39	
3	0.500	3	0.110	7.57	0.28	92.64	
4	0.500	3	0.120	9.53	0.10	91.37	
5	0.500	3	0.128	8.72	0.24	91.04	
6	0.500	3	0.150	13.64	0.31	86.05	
7	0.500	5	0.200	7.08	0.15	92.77	
8	1.000	7	0.030	25.34	0.26	73.40	Very slowly heated.
9	1.000	8	0.035	28.03	none	71.97	Very slowly heated.
10	2.000	3	0.880	6.90	0.25	92.85	Rapidly heated.
11	1.000	3	0.975	3.76	0.29	95.95	Rapidly heated.
12	3.000	3	1.085	1.68	1.38	96.94	Rapidly heated.

The table shows that when sulphur is heated very slowly with free access of air a large percentage (nearly one-third) of the vaporized sulphur is converted into sulphuric acid, very little or no sulphurous acid being formed. When the temperature is increased and the sulphur more rapidly vaporized, the percentage of sulphuric acid formed is very much smaller, the percentage of sulphurous acid formed remaining about the same. When the sulphur is heated rapidly with access of less air the proportion of sulphurous acid is increased while the percentage of sulphuric acid formed is very much lessened. It is of course difficult to carry on an experiment for collecting the gases formed which would at the same time simulate the conditions in a greenhouse where there is free access of air and the temperature is comparatively low. But the results of my experiments as given in the table would lead to the inference that a large percentage of sulphuric acid would be formed and very little or no sulphurous acid, as shown in experiments 8 and 9.

While it is known that sulphurous acid has marked germicidal properties and while a small percentage of sulphurous acid is produced when sulphur is heated under certain conditions, still owing to the large percentage of sulphuric acid produced when sulphur is heated under conditions which more nearly simulate those where sulphur is heated on steam-pipes in the green-house, the question arose as to whether sulphuric acid has not marked fungicidal action and as to whether it is not one of the active agents when sulphur is used as a fungicide. In order to determine whether sulphuric acid has fungicidal properties a number of experiments were carried out.

Spraying Experiments.

During the summer of 1904 some preliminary experiments were conducted to determine what strength of solution of sulphuric acid could be used for spraying plants without injurious effects. In this series of experiments the solutions were made with distilled water, and used in strengths varying from 1 part in 200 to 1 part in 10,000. The plants experimented with were the common field plants, such as wild cherry, elder, ailanthus, phytolacca, yellow dock, burdock, abutilon, blackberry, wild rose, milkweed, locust, etc. An ordinary hand atomizer was used for the spraying operation, and the plants were usually sprayed late in the afternoon so as to avoid concentration of the solutions before they were absorbed, as also the burning effects of the sun. It was found that a solution of 1 to 200 produced a marked burning effect on the leaves, and in the case of some plants, as blackberry, polygonatum, oxalis, pear, rose, mulberry, raspberry, etc., contiguous parts of the plant exhibited signs of injury.

It was soon found that there is considerable difference in the resisting power of plants when sprayed in this manner. The most sensitive plants were peach, raspberry and wild carrot, while the most resistant were poison ivy and plantain (*Plantago major*). As a result of these experiments it was found that most plants could be sprayed with a solution of sulphuric acid varying in strength from 1 part in 500 to 1 part in 1000, without serious injury to the plants, and the conclusion was reached that it would be perfectly safe to use sulphuric acid solution as a spray in the strength of 1 part of acid to 1000 parts of water.

Later in the summer of 1904 it was my good fortune to find a rose garden in which some of the plants were badly affected with mildew and to obtain permission of the rose-grower to treat the affected plants. In the series of experiments a solution having a strength of 1 part of acid to 1000 parts of water was freely applied by means of a garden syringe, so that the plants were thoroughly wetted. Sprayings were made as follows: August 11 at 4 P. M., August 12 at 8 A. M., August 13 at 8 A. M., August 14 at 8 A. M., August 15 at 8 A. M., August 16 at 8 A. M.

Within about a week afterward the mildew had about all disappeared and the young leaves which were putting forth showed no signs of injury. During 1905 the mildew did not re-appear, and it was noticed that the plants which had been sprayed with the sulphuric acid solution the year before had never done better.

It may be of interest to state that parallel experiments using copper sulphate, 1 to 1000, were conducted with the result that while the mildew was eradicated the tips of the young leaves were injured.

During the winter of 1904–05 I had an opportunity of trying the solution on roses growing in the green-house. The plants were sprayed with solution of sulphuric acid 1 to 1000, on December 7, 8, 9, 10, 12, 14, 16 and 19, with the result that the mildew was completely eradicated, the new growth was vigorous and the mildew did not reappear.

A few months later another lot of roses growing in the greenhouse were treated. These included pink rambler, crimson rambler, Victoria rambler, bridesmaid, golden gate, liberty, American beauty and Carnot. The plants were sprayed with sulphuric acid solution, 1 to 1000, on January 9, 10, 11, 12, 15 and 17, with results similar to those obtained in the previous experiments. I have also used the spray since with similar results.

In conclusion I may add that it appears that dilute solutions of sulphuric acid have a beneficial effect on the plants treated apart from their fungicidal action. In fact they seem to act as a tonic to the host plant while they kill the mildew.

Should subsequent experiments confirm the results here recorded, it will be found that sulphuric acid has certain advantages over sulphur, in that it does not discolor the foliage as sulphur does, its employment is more easily controlled, and it does not have the disagreeable odor of certain other compounds associated with sulphur.

Inasmuch as the antiseptic properties of sulphurous acid are well established, it may be possible that a combination of sulphuric and sulphurous acids in solution would be more effective than sulphuric acid alone. Taking the proportions in which these acids are formed when sulphur is heated, it may be that a solution containing 1 part of sulphuric acid and 0.1 to 0.01 part of sulphurous acid to 1000 parts of water, would be more effective than sulphuric acid alone in the proportion given.

Stated Meeting May 4, 1906.

President Smith in the Chair.

Dr. David L. Edsall, a newly elected member, was presented to the Chair and took his seat in the Society.

Letters accepting membership were read from:

Hon. Joseph Hodges Choate, New York.
John W. Harshberger, Ph.D., Philadelphia.
William Francis Hillebrand, Ph.D., Washington.
Charles Rockwell Lanman, LL.D., Cambridge, Mass.
Ernest Fox Nichols, D.Sc., New York City.
Hon. Elihu Root, LL.D., Washington.
Thomas Day Seymour, LL.D., New Haven, Conn.
Edward Bradford Titchener, M.A., Ithaca, New York.
Otto Hilgard Tittmann, Washington.
Dr. Hendrik Antoon Lorentz, Leyden.

The decease was announced of Mr. Edward Floyd deLancey, at Spining, N. Y., on April 7, 1905, æt. 83.

The following letter was read from Prof. George Davidson, of San Francisco, in relation to

The San Francisco Earthquake of April 18, 1906.

In the matter of the earthquake of April 18, while you were celebrating the two hundredth anniversary of the birth of Benjamin Franklin, I beg to give the following memorandum as a matter of record:

The shock occurred at 5^h 12^m 00^s A. M., Pacific standard time. The first movement was short and sharp, north and south, and of slight amplitude at my home 300 feet above the Bay; it changed to movements east and west and every direction, with very quick shocks (say four per second), of small amplitude and complex, for 60 seconds; when a slight but gradual weakening was apparent for 30 seconds more. After this the vibrations weakened gradually to quietness in another 60 seconds. The whole movements occupied 2^m 30^s. I began my count with the first shock, and soon afterwards timed the event with my chronometer watch.

The sensation suggested the sharp vicious action of a terrier seizing and shaking a rat.

There was a slight shock at 5^h 17^m (approx.) and another at 5^h 29^m (approx.) and some later ones of no consequence.

In other parts of the city and the adjoining districts the amplitude of the shocks has been much greater, if we judge by results; and yet very unequal.

The general indications point to this statement: *That the greatest ampli-*

tude of the movement of the immediate earth surface is to be found in low alluvial situations or in made-land areas.

That was the deduction in 1868; with indications that at the extremity of a rocky ridge the full amplitude was seen.

The report of the 1868 Committee (of which I was a member), was never made public.

The Governor of California, George C. Pardee, has appointed the following persons members of a committee to investigate the phenomena of this earthquake: Professors A. C. Lamson, University of California (geology), G. K. Gilbert, U. S. Geol. Survey (geology), Harry Fielding Reid, Johns Hopkins University (geology), J. C. Branner, Stanford University (geology), A. O. Leuschner, University of California (geodesy, astronomy), George Davidson, University of California (geodesy, geography), Charles Burchatter, Chabot Observatory. This board has already met to confer on the plan of investigation.

I may mention here, incidentally, that the number of deaths officially reported as occurring from the earthquake and fire, is 277.

There is no epidemic; there is capital order, with the military in charge; and those who remain are stout-hearted; 125,000 encamped in parks, etc., are being fed.

This is the sixty-third earthquake I have experienced.

San Francisco, April 25, 1906.

This communication was discussed by Professor Haupt, Dr. Goldsmith, Mr. Wharton and Mr. Richard Wood.

The following papers were read:

"Prof. de Vries' Theory of Evolution in Relation to Results," by Dr. Philip Calvert, which was discussed by Dr. Conklin and Dr. Calvert.

"The New Agriculture," by Mr. Burnet Landreth.

"The Present Condition of the Bar at Aransas Pass, due to the Operation of the Reaction Breakwater," by Prof. Louis M. Haupt.

"THE NEW AGRICULTURE."

By BURNET LANDRETH.

(*Read May 4, 1906.*)

The use of Peruvian guano and other concentrated fertilizers may be said to have been the initial steps leading to a new birth of agriculture.

Peruvian guano was introduced about one hundred years ago, but did not get into general use until about 1840, at about which time was introduced into frequent use ground bones and super-phosphate of lime, two manurial agents which have accomplished more than any other soil fertilizers, and at the same time into partial use nitrate of soda and nitrate of potash; all these objects of fertilizing force being greatly concentrated in comparison with the bulky fertilizers of earlier years, manures which still are highly valued, but such as cannot be profitably transported long distances. The use of these concentrated fertilizers in Europe increased the area of the sugar beet fields to an enormous extent as well as the area of everything else in the agricultural line.

In America, by these very rapid and cheap methods of adding plant foods to the soil, so-called fertilizer factories sprung up in all directions over the land and the crops of grain, cotton, tobacco, and other productions of the garden and field were doubled in product per acre as well as quadrupled in area. This subject of fertilizers, however, may be looked upon as a back number, or as hardly now to be classed in the process of "the new agriculture," but increased crops led up to other developments. Neither can steam plowing be looked upon as a new process of "the new agriculture," for it was practically pursued fifty years ago largely in England and Germany and to some extent in Egypt, but it broadened immensely the scope of agricultural practice. In America, however, steam plowing in early days never took a very prominent place, because until of late years there were few great American farming estates operated by wealthy and very progressive men, it not being profitable to steam plow at one operation less than a field of forty or fifty acres, but of late years consequent upon the rapid extension of agricultural operations, especially in wheat growing, fields of grain have become so immense as to satisfy the most progressive. In England the system of plowing is distinct from what Americans have always clung to, even although up to quite lately with disappointing results. In Europe the system is altogether what is called the cable or rope system, where upon a field preferably not less than four hundred yards long a traction engine with a large horizontal revolving drum beneath the boiler has placed opposite to it at the other end of the field a corresponding heavily anchored revolving horizontal drum. Around these two drums passes a wire cable of about three fourths of an inch thick, which drags backward and forward a gang of plows of six, or even ten or twelve mould boards, an automatic device, after each set of furrows are turned, moving forward the anchor at the far end; the steam engine on the headland also moving forward to the same distance as the anchor, so that they are always on parallel lines. One engine and opposite anchor windlass forms what is termed the "single system," while in the "double system" there are two opposite engines.

These plowing outfits cost from ten to twenty thousand dollars, an amount which only can be assumed by a large operator. Not more than four or five of these outfits have been imported, one in the neighborhood of Philadelphia being imported by Colonel Joseph Patterson, who thirty years ago endeavored to establish a beet sugar farm and factory at Egg Harbor, N. J., which operation on account of unfavorable climatic influence was a failure, the plowing apparatus being subsequently sold to Wade Hampton, thence to Cuba.

Americans have wanted something cheaper, something easier to manage, and have spent large amounts of money in endeavoring to plow by *direct* action, using self-moving engines on three or four wheels pulling after them gangs of plows, just as a locomotive pulls a train of cars. This process up to a few years past met with so many difficulties, that really there were few thoroughly satisfactory applications of the American idea, for at seasons when the land is muddy and slippery the tractive force of the engine is reduced greatly, sometimes entirely nullified, at other seasons when the soil is dry and dusty, the engine kicks up such a dust as to ruin its own working parts.

Some years ago I had considerable experience in endeavoring to promote the direct traction system and devoted much of the three successive summers in 1872, 1873 and 1874 in an endeavor to do practical work.

I worked first with a three-wheeled Scott-Thompson engine with solid rubber tires, six inches thick for the purpose of increasing the tractive force, which tires flattened out under the weight of the engine as does a cat's foot. The two driving wheels were of a sixteen-inch face having clamped upon them segmental lags or blocks of rubber; as the whoeels revolved these rubbers flattened under the five-ton weight of the engine and gave a tractive grasp of the soil of quite four hundred superficial inches under each wheel. I also worked with a four-wheeled rigid-tire engine, the periphery of the wheel fitted with angle irons to increase the tractive power.

Succeeding these trials, or in the year 1887, General Roy Stone in my farm wheel-wright shop at Bloomdale invented and erected a steam digger of ten spades, a system established on a horizontal shaft, the spades jabbing into the earth directly in the rear of the engine. This required such an immense amount of power that we turned to something better; this being a rapidly revolving shaft actuating fifty or more cutting or chopping knives slicing off slivers of earth by a downward centrifugal movement just as a wood planing machine by a horizontal centrifugal movement chips off slivers of wood. These fifty knives, each on a distinct arm eighteen inches long, were fastened to a horizontal shaft revolving rapidly, throwing forward the various arms in centrifugal movement, the knives at the ends of the arms going down into the earth in oblique and curved direction, throwing the earth six feet behind the machine as a hay tedder throws back the grass, the knives always keeping open a broad trench beneath and behind them.

The land, when thus worked, was left as a perfect garden bed, needing no further working because it formed a bed of chippings ten inches deep and perfectly level, every particle of soil cast back quite three or four feet from its original location; but the machinery was not perfect; no one at the time was ready to take it up; consequently, the engine was sold for another purpose and the chopping apparatus went to the scrap pile.

A photograph of the Bloomsdale steam chopper gives a partial idea of the best steam chopper ever used, and I take great credit that it was developed on my own farm. It was an ideal machine, but perhaps more theoretical than practical. This was in the years 1887 and 1888.

Since then there has been a great advance in methods of steam plowing by direct traction, and that by the western people, always leaders, if not in the invention, certainly in the adoption of all new machines and appliances, and they have not been laggards in perfecting the American idea as connected with steam plowing, for it has rested with a California establishment to most successfully promote the system of direct traction plowing.

This establishment makes a sixty-horse power traction engine which draws a gang of twenty-one earth-cutting disks, each cutting a foot wide and nearly a foot deep and doing in this style fifty acres a day, sometimes eighty acres.

In England no double engine cable system has ever done anything like this, and in addition the traction engine has sufficient power to drag at the same time a gang of grain sowers which seeds the same width of twenty-one feet. The engine makes its own electric light, so that operations can be conducted throughout the full twenty-four hours. One of these machines is in use in South Africa.

And there is another case of most remarkable development in agricultural mechanism, my reference being to harvesting machines, and this again by western people, who, by reason of their enormous breadths of ground and scarcity of labor, have always been very active in promoting the use of labor-saving machinery.

Many farmers in Minnesota and the Dakotas have for years been winning the admiration of the old east by using thirty and forty improved reapers at one time in the same field, all attended by traveling machine shops to effect repairs, but it has been left to a

Californian to devise a perfectly practical machine for cutting, threshing, cleaning and sacking all at one operation, the grain being made ready for market five minutes after the machine touches the field.

This machines requires the united power of thirty to forty horses, still better that of a traction engine, which by its electric light turns night into day. The machine will take care of five acres an hour or over one hundred acres in the twenty-four hours. These machines are used in California, Washington and Oregon, districts where the grain thoroughly matures on the stalk and where no rain occurs during the harvest season. The outfit costs $8,500, and one farmer in Spain has had nerve enough to purchase the full system.

And what other remarkable advances in farm implements, few of them as impressive as the steam plow or the combined harvester, but in their places equally as important. It would be too comprehensive a subject to endeavor to refer to them all, but as examples will name the corn stalk cutter and binder which handles a crop of corn much as a grain binder handles wheat or rye, or the corn husker which handles a standing crop of corn, assorting all stalks and husking the ears.

These mechaines are of special importance, for be it remembered, there is annually grown in the United States two acres of corn to every man, woman or child.

A later development of "the new agriculture," but not a novelty in practice, is the process of spraying or washing the stems and foliage of plants with liquids containing poisons for the destruction of insect life. This was introduced about 1870, and was at once found very practical in checking the ravages of many kinds of insects; but countrymen noted that there were certain other insects which were not killed; they observed some which seem to delight in a poison bath, some which put on the top surface of an open barrel of paris green seem to be more lively in the morning than they were the night before. This, brought to the attention of entomologists, elicited the information that there were two great classifications among insects which must be recognized by the farmer. Firstly, those which ate the tissues of plants, and, consequently, took into their stomachs any poisons which accidentally or artificially rested upon the outer surfaces of the plants; while secondly, there were other insects which entomologists had recognized for one hundred and fifty years, but which the farmer had never critically observed, which did not masticate their food, those after the order of the mosquito, sap suckers, having a little pumping apparatus which they inserted beneath the cuticle of stem or leaf and drew out the sap, ignoring entirely any application of poison.

The entomologists advised the adoption of a different policy, saying that these sapsuckers must be strangled, and the way to do that would be to spray them with some liquid of an oily, caustic or soapy basis, because insects do not have lungs, in the ordinary understanding of the term, but breath through orifies along the sides of their bodies or abdomens sometimes covered by scales as in the case of the fish-like lamina of the honey bee. The entomologists told the farmer that an oily spray put upon these or, indeed, any other insects, would gum up the orifices and the insects, consequently, die. Thus, between the surface application of poisons for the tissue-eating insects and the application of oily fluids for the sap suckers, most insects can be kept in check.

The insect now attracting most attention on the part of the fruit growers is the San Jose scale, a native of China, believed to have been brought to California on imported trees. These insects have by gradations of progress eastward covered the entire country, and if left alone would destroy every fruit tree in the land. But very practical steps are now being taken everywhere to eradicate this insect by the application of a liquid combination of lime and sulphur, which is caustic in its action, killing most of the half-grown hibernating scale insects in winter, and preventing the settling of any young that may come from the few parent insects escaping the wash.

As much as forty years ago scientific men indicated to seedsmen the use of several insecticides, principally carbon-bisulphid for the treatment of weevil-infected seeds, or for the treatment, in fact, of any seeds bearing on their surfaces or within them insects or mites. Seedsmen and grain merchants are especially annoyed in the conduct of their business by the depredations of the weevil family which burrow or ensconce themselves notably in the seeds of peas or beans, corn or wheat. These grubs may be killed in their holes by the fumes of carbon bisulphid, to effect which the seedsman erects a room say thirty or forty feet square, lined top, bottom and sides with tin and with a door which can be hermetically sealted. Such a room can be loaded up with a carload, say six hundred bushels of peas, beans or anything else.

A carload of peas or beans thus treated and subjected to the fumes from a gallon of the liquid for thirty hours may then be taken out with all the larva killed, the fumes penetrating not only to the center of the pile, but to the center of every sack. The odor of the fumes soon dissipates, leaving no resultant injury to seed vitality nor to edible properties of either seeds or food stuffs: A more efficient agent is hydrocyanic acid gas, but it is dangerous in the hands of ignorant people.

About 1860 the use of carbon bisulphid was introduced among the vineyardists of France as an agent to arrest the ravages of the phylloxera, a plant louse feeding on the roots of the grape-vine. Possibly at this date 300,000 acres of vineyards are being annually treated, a half ounce of a liquid being applied by injection to every square foot of soil surface, the vapor filling all the soil interstices, the application being two treatments of ten days apart.

But an entirely new practice based upon scientific observations and a rather amusing contribution of science to agriculture is the introduction of cannibal bugs, sorts which do not injure vegetation, but luxuriate on the meat of other species of bugs. Some of these precious insects have been imported from China and are doing a fair amount of work on the California coast, while certain imported Mexican ants are very active in Texas cotton fields, feeding upon the cotton boll worm and its eggs. Thus science, while going hand in hand with agriculture, encourages cannibalism.

The farmer again had to turn to scientists to learn how to arrest the injuries from fungus diseases on his growing plants of grain, vegetables and fruits, and there has grown up a large industry in the manufacture of fungicides, principally copper compounds, which are sprayed upon the plants the same as are the insecticides. In fact, as a matter of economy of application, both fungicides and insecticides often can profitably be mixed and the plants treated by one operation. All designs and sizes of apparatus for the application of insecticides and fungicides are now made ranging from the small air gun to cumbrous four-horse trucks carrying tanks and powerful pumps and requiring the attention of several men—some forms of dust spraying apparatus being made for hillside use, where it would not be practical to haul great weights of water.

The electric light as a forcing agent is used to advantage in stimulating the growth of vegetables and flowers. Some market gardeners in the vicinity of Boston, Mass., finding that its use pays them a profit by influencing a never-ceasing growth of lettuce, radish, cucumber and some other table vegetables, the plants not being allowed any rest day or night, but kept under a continuous activity which in the end amounts to about a ten per cent. shortening or hastening of the period of maturity, that is to say, plants which under ordinary circumstances would take thirty days to arrive at marketable condition, will under the electric light process be fit for market in twenty-seven days, which earlier maturity results in a profit, and allows space and time for an additional crop in the year's series.

These Boston men also apply continuous currents of electricity passing from end to end of plant beds in their glass houses, which is found to have a stimulating effect on the productivity of the soil. They have successfully accomplished what many others have failed to do. Even as far back as 1866 electric stimulation of the soil was tried most extensively and expensively. In that year I visited Tipptree Hall, the estate of Alderman Mechi of London, where were conducted most extensive electric experiments on field crops covering broad areas, but the so-called electric experts of that day failed entirely, although Mr. Mechi was lavish in his expenditures.

Nitroculture while several years pursued may be classed as one of the novelties in agriculture practice.

Ages ago the ancients were aware that certain plants increased the fertility of the soil, as for example clover, beans, vetches, and other legumes, but the most intelligent did not know why, simply knew of the favorable results. Later on, intelligent observers concluded that it was because of the deep rooting habit of plants of the legume family; that it was entirely the result of the collection out of the three, four or five feet depth of soil to which the roots reached, of all the potash there which was seized upon and drawn up by the roots and concentrated near the crown within an inch of the surface, and, no doubt, to an extent the theory was correct, but scientists now tell us a new story and a most interesting one. No intelligent farmer has failed to observe upon the roots of his clover or bean plants that occasionally there is presented to his view a something which looks like a diseased condition of the roots, a growth of warts or nodules, sometimes four or five to a plant and sometimes a hundred. Occasionally this will occur at one end of the field and not at all on another field.

The bacteriologists now tell us that these nodules are caused by the attachment to the roots of plants of the leguminosæ family of certain bacteria, that these bacteria within the resulting nodules or swellings absorb out of the air of the soil a portion of its introgen and store it for the support of the growing plant itself, as also for the succeeding year's crop and this stored nitrogen is now said to be the principal secret of the imparted fertility to soil by the cultivation of a crop of clover.

Some parties are now commercially developing these microorganisms, offering for sale in small sealed tubes portions of the living cultures sufficient to develop an indefinite quantity, just as a yeast cake which one may carry in his pocket may leaven sufficient dough to inoculate a large amount. The commercial preparations being mixed as directed in water, the solution may be used to inoculate as much soil as the solution will moisten, and this soil afterwards mixed with four or five times its bulk, and the whole thinly spread broadcast over that portion of the field to be inoculated; or the system now most generally advised is to inoculate the seed before planting it by subjecting it to a spraying or a bath. The seed may be partially dried before sowing, or sown in its wet condition.

The practice so far, to a large extent, is experimental, there being various degrees of success and failure, but the day is close at hand when better results will be obtained. Little benefit results to any particular crop if the bacteria of that crop is already present in the soil, and little benefit if the soil is already well charged with nitrogen, or very deficient in potash or phosphate. Inoculation is only one factor in securing a successful growth of legumes and unless the soil conditions are favorable to the multiplication and activity of the bacteria, inoculation will be ineffectual.

Now as to the use of chloroform in plant culture. This is a process, only a few years introduced, of forcing the blooming of plants away ahead of their natural period, as for example making lilacs, azaleas, lilies of the valley, hyacinths and violets burst into bloom at Christmas time instead of their natural period about two or three months later. This system is well established in Germany, Belgium and France, and will, no doubt, be practiced in all parts of the world.

It is not a new thought to say that all plants must undergo a season of repose, a repose induced by some internal and external force, the plants seemingly being dead, there being no perceptible movement whatever excepting a slow increase in the size of the buds. All country people have observed that after a fruit tree loses its leaves unusually early in the autumn that it sometimes bursts into bloom before winter; it has had its season of repose, a short season it is true, but it undertakes then to proceed with further development. Now to force or intensify an early repose or sleep, chloroform is used. The susceptibility of plants chloroformed is an interesting discovery, all plants being similarly effected much as in the case of animals; it is even claimed chloroform puts metals to sleep. A test can easily be made with a sensitive plant or mimosa, placed under a bell glass with a small sponge dipped in chloroform; after a few minutes the plant will be observed to have lost all its sensibility, but when exposed to the open air, the sensibility returns. The exposure of plants to chloroform produces on short notice a most intense and deep rest, similar to that after the autumn shedding of their foliage, but a rest far deeper than under the ordinary circumstances. It has been observed that plants which had been exposed to cold and dry winds are likely to bloom early, alpine plants for example, and it is observed that the action of chloroform produces a drying up of the tissues rendering the buds quickly susceptible to subsequent heat and moisture. This discovery opens up a broad field for the producing of blooms ahead of time as also a guarantee for the gathering of early crop of fruit. Of course, these processes can best be done in properly prepared chambers or glass houses, but, no doubt, means will be devised for covering outdoor plants with something similar to hugh bell glasses for treatment by chloroform.

One of the latest scientific discoveries in connection with agriculture is that of the influence of electric air currents. Travellers in far northern regions have noticed a wonderful rapidity of growth and brilliancy of coloring to the arctic flowers and a strength of perfume. They have noticed that succeeding a snow surface there will, forty or fifty days after its disappearance, be developed a growth of natural grass, as tall as would be produced in four times the period in temperate regions and this on poor soils. They have noticed that these growths are even still more astounding throughout spring months of unusual auroreal disturbances. Usual auroras occur about once every ten years, and in those the growths of all vegetation is far in excess of the ordinary years. This has been especially noticed in northern Sweden and Norway. Cutting down a tree there, it is observed that the annual rings about every ten years are much broader than those before and after. All this has led close observers to believe that these unusual growths are a consequence of electrical air currents, and with that theory in view most interesting experiments have been conducted proving that the theory is correct.

Professor Lemstrom, of the University of Helsingfors, was the first in these experiments, which experiments have been pursued at Avidaberg in Sweden and at Kryschanowitz in Germany and at Durham College in England, all proving conclusively that electric air currents greatly forward the growth of plants. In some cases not only ten and twenty per cent. increase in productiveness, but in other cases sixty and eighty per cent. increased development. Strawberry plants subjected to electric air currents are increased fifty to sixty per cent. in productiveness and sugary qualities and just so with other garden crops. It has been discovered that the electric current is ineffectual on a wet and cloudy day, and positively injurious on a very dry day.

The processes of application may be generally described as follows: A plot of ground, say forty by forty feet or larger, for example, a portion of a strawberry patch, is covered by a wire netting two feet above the level of the crop insulated on wooden posts; an electric battery upon wheels is moved up close to the edge of the wire netting and the wire of the positive pole attached to the iron netting, the negative pole plunged into the earth; the electric machine is started and alternating currents are passed backward and forward, jumping from the wire netting to the leaves and conversely from the leaves to the netting. This application of electric currents repeated at intervals, the wire netting being moved from part to part of the crop, until in the end the whole field has finally been treated. Of course, this system cannot be used over very broad surfaces, but it has been put into practical use on areas as much as of three acres. Professor Lemstrom estimated the cost of an efficient electrical machine at $500 and $150 as the annual cost of the material and extras.

A still newer, and probably the newest scientific application in the line of "the new agriculture," is the electrocution of insects, a system just patented by an electrical engineer at Odessa, and in this connection I will relate an interesting incident.

Twenty years ago I was a special correspondent of a European Ministry of Agriculture upon subjects concerning the development of agriculture in America, and on one occasion, when I was asked to report upon the phylloxera in the United States, I supplemented the report by a suggestion that it might be possible to kill the phylloxera by a current of electricity passing through the stems and roots of the grape vine. The Ministry of Agriculture in office at that date thanked me for the suggestion, but said it was impractical; yet, strange to say, this Russian electrical engineer has taken out patents for doing this very special thing. If this be so, then by electricity, capable, on the one hand, as a mighty force to rend a mountain, or of a movement soft as zephyr, it may be possible yet to destroy every bug on tree or plant, whether in the open field or in glass houses.

What I have said are generalizations very incomplete, yet they, nevertheless, indicate that the practice of agriculture and horticulture has taken a highly mechanical and scientific tone, a pursuit worthy of the most intelligent mind.

Stated Meeting May 18, 1906.

President Smith in the Chair.

Dr. J. W. Harshberger and Mr. Russell Duane, newly elected members, were presented to the Chair and took their seats in the Society.

A letter was read from Prof. Theodor Nöldeke accepting membership.

The decease was announced of the following named members:

Richard Garnett, C.B., LL.D., at London, on April 1, 1906, æt. 68.
Prof. E. Renevier, at Lausanne, Switzerland, on May 5, 1906.
Hon. Carl Schurz, at New York, on May 11, 1906, æt. 77.

The following letter was read by Prof. George Davidson, of San Francisco:

POINTS OF INTEREST INVOLVED IN THE SAN FRANCISCO EARTHQUAKE.

At the request of your Secretary I draw up this short account of some of the points of interest involved in the earthquake and conflagration of San Francisco.

I hardly know what I have already written, or where to begin to tell a coherent story, although a thousand facts have been seared upon my brain. I see the flames rising yet, 500 feet above the earth.

It really seems very long since the earthquake aroused us: it would have been forgotten by this time. It is the conflagration, its awful grandeur, and its ruin of tens of thousands of people, that staggers us with its horror, and its consequences. But San Francisco will arise from this supreme blow. We are to control the Pacific.

I cannot do better than to first give you a broad idea of the geography and orography of this part of the State; and of the ocean washing it. The general direction of the coast range of mountains is northwest and southeast from Cape Mendocino in latitude 40° 27′ to Point Conception in 34° 27′. North of latitude 35½° the range comes sharply to the ocean in two or three high, sharp, rocky plications, folds or ridges, with regular or irregular valleys between them; and *broken down* (1) at the Bay of Monterey in latitude 36° 36′; (2) at the Golden Gate in 37° 49′; and (3) at Bodega Bay in 38° 18′.

Inside of this coast range lies the Great Valley of California, having an average width of 65 miles, 450 miles in length, and bounded on the east by the Sierra Nevada.

The *ocean face* of the coast mountains is high, rocky, bold, and grand in certain parts, for example: one of the Twin Peaks of the Santa Lucia range in latitude 36° 03′, rises to 5100 feet only three miles from the sea; Mt. Santa Lucia about ten miles east, with deep intervening valley, rises to 6100 feet; Loma Prieta (formerly Mount Bache on Coast Survey charts) is in the San Francisco Peninsula range in latitude 37° 07′ and reaches 3793 feet above the sea; Tamalpais Mount in the range on the peninsula north of the Golden Gate in latitude 37° 55′, and only four miles from the sea, is 2604 feet high. To the westward of this mountain is the outstretching Point Reyer Head 597 feet high, stretching eight miles beyond the general direction of the coast and connected therewith with low-lying alluvial lands.

North of Bodega Head (38° 18′) the general height of the range is 2200 feet, until within twenty miles of Cape Mendocino, it reaches 4265 feet at Krag Peak latitude 40° 09′.

Beyond Cape Mendocino (40° 27′) the range is continued under the sea, as shown by a few deep sea soundings. It is here that ships have experienced the effects of submarine earthquakes.

The greatest *transverse line of rupture* in these ranges is the Golden Gate, one mile wide and sixty-three fathoms deep. A less but longer line of *low rupture* is from Bodega Bay to Petaluma Creek that empties into the northwest part of San Pablo Bay.

Ocean Depths.—Off this bold and rocky coast the profound depths of the Pacific lie close aboard.

In 1874 Captain George Belknap, U. S. Navy, ran several lines of deep sea soundings broad off certain points of the coast to the depths of 1000 and 2000 fathoms, so that we know the depth of 1000 fathoms is found at 40 to 60 geog. miles from shore and the deep plateau of 2000 fathoms, 2¼ miles, at 46 to 72 geog. miles. The shortest distance is off the Santa Lucia range: the middle off the Gulf of the Farallones, and the longest off Cape Mendocino. At the latter the 1000 geographical miles sounding was gotten at 25 miles; and the 2263 sounding at 85 miles.

And it should be noted that off the high bold coast just south of Cape Mendocino there are *four submarine, or submerged valleys* that head directly at the shore under the highest peaks, and at right angles to the coast. (See my paper on Submerged Valleys.) Those are the more general features of the coast line. The coast survey charts exhibit the line of coast, the heights, and the depths.

The Bay of San Francisco.—This bay lies in one of the valleys of the coast range, and therefore is mainly parallel therewith; and the great transverse break through this range helps to form the bay.

The line of Suisun Bay, Karquines Strait, San Pablo Bay, and the Golden Gate is the course of the drainage of the Great Valley of California, and the adjacent valleys. It is the natural highway of commercial activity between the interior regions and the Pacific.

The area of the Bay proper is 301 square miles, of which 83 square miles have four fathoms and more of water; San Pablo at the north has an area of 123 square miles, of which 17 square miles carry over four fathoms of water.

Both these bays are largely bordered by broad, low, salt marshes, cut by many sloughs. Inside of this marshland (which marks the shoreline of high water) the low, alluvial soil may be from one to six miles wide, to where it joins the foot of the rocky ridges. In places the rocky points reach into deep water.

Some of these features are well shown on the coast survey chart *"Entrance to San Francisco Bay";* and Professor J. D. Whitney's map of San Francisco Bay (1873) will give a general idea of the confining ridges; and the cross breakings.

There are two features that are particularly noticeable: (1) the subordinate valley running from Bodega through Tomales Bay and Valley to Bolinas Bay. The heights on the southwest side of this narrow valley reach 1356 feet in height; and on the northeast reach 1500 to 2604 feet.

(2) On the prolongation of this valley, across the Gulf of the Farallones, there commences a narrow valley about six miles below Point Lobos, and stretches southeastward, as a line of depression on the east side of the crest line of the Peninsula of San Francisco range, for thirty-six miles in a direct line. In this long stretch lie several small lagoons, and the Cañada de Raymundo where the Spring Valley Water Company has its main reservoirs. Of course the bottom of this valley is alluvial soil. The water company has two reservoir dams across it, and a third at the exit of drainage through San Mateo Creek or Cañon to the bay on the east. Neither of these dams is reported injured. Professor Branner, of Stanford University, has found some earthquake crevices in the line of depression. No rock rupture is reported there.

With regard to alluvial soil, it is well to bear in mind the relation of this whether in the valleys or around the bay and ocean shores, and the relation thereof to the rocky base upon which it rests or abuts.

I was one of the committee of investigation of the 1868 earthquake, and it demonstrated that the *course of greatest dislocation at the surface of the ground was on the line of contact between the "made" land or the alluvial soil with the rocky stratum.*

This is repeated in this earthquake, and therefore we make a few more remarks upon the bay shore line of San Francisco.

In the vicinity of the city proper, and within the area of the "City and County of San Francisco" (42.8 square miles), there are several indentations of the shore between rocky points. (1) Buena Cove, where the present "city front" or shipping point lies on "made" ground; (2) Mission Bay, Creek and Lagoon, being filled in; (3) Islais Creek, farther south.

In Buena Cove the mud has been found ninety feet deep; in Mission Creek ninety-six feet. The "filling in" has been sand, the waste of the city, and occasionally rock. To erect buildings thereon piling must be driven to "hard pan." In Mission Creek this has rarely been done; and therefore we must expect to find, and do find the greatest dislocation in such areas, and at the contact with fast land.

The towns of Redwood, Palo Alto, San José, etc., are built on the alluvial soil immediately inside the marshland, and are probably twenty feet above the bay.

These places and others about the bay show large destruction of property.

Up the Valleys of Petaluma and Napa similar results are presented.

On the rocky parts of San Francisco the horizontal resultant movement was three to four inches, on the "filled" in land near the new Post Office, the building, piled upon a clay foundation, suffered but little, while the street in front, made upon twenty-three feet of sand over a marsh, sank two feet and slid out from the building about one and one-half feet.

Area of Disturbance.—The Committee appointed by the Governor to gather data is doing what it can to define the locus of principal action, and the extent and direction of the lines of disturbance.

In my early inquiries I learn of injury to the Light House at Point Arena, latitude 39° 00′, 110 miles from San Francisco, and at "landing places" on the coast this way; damage fifty to sixty miles up the Petaluma and Napa Valleys; movement in the Yosemite Valley, 175 miles from San Francisco; and damage at Carmel Bay south of Monterey, ninety miles from San Francisco.

Up the Napa Valley material was thrown to the west-southwest; at Yosemite motion east and west; at Carmel Bay material to east; at Carson, Nevada, first movements nearly east and west, distance 180 miles to northeast, ended in several tangles.

At San Francisco movement from north to south, then east and west; and a final series of reverberations too puzzling to decipher. I judged these shocks to be about three or four per second.

There are reports of crevices in the west slope of the hills north of Berkeley; in the Cañada de Raymundo, with depression on one side; and on the outer coast at Half Moon Bay, twenty miles south of San Francisco; and at Bolinas Bay ten miles north of the Golden Gate. But all are in alluvial soil.

Vessels at Sea.—The action upon vessels in the Gulf of the Farallones, off the entrance to the Golden Gate, has been given me by Pilot Hayes. The Pilot boat Gracey S. was lying in eighteen fathoms off the light ship, ten miles from the Head of the entrance to the Golden Gate, and shivered as if the chain were running through the hawse. When the pilot boarded the German ship the captain told him he thought his vessel was on the rocks; and a second pilot boat thought that she was upon the rocks.

This striking and shivering of the ship has been felt many times off the northwest of Cape Mendocino, or the prolongation of that range.

That is a very unequal description of the region affected by the earthquake and its results. When we get a coherent report, a copy will be sent to the Society.

I wish in conclusion to say a few words of the conflagration that covered an area ten times greater than that of 1666 in London, with a vastly larger proportion of loss.

1. The electric power lines running to hundreds of industrial plants in the city are believed to have been broken, and that a fire was started at each break as if by magic. All the city fire engines were called out but *found no water.*

2. Where the 30-inch main crossed the "made" or "filled" in land at the head of the old Mission Lagoon, there was a sinking of the material, and the joints were drawn asunder at a drop of several feet. Therefore the main supply was unavailable; and the reservoirs ran to waste. Then the water was pumped from Lake Merced; and finally some repairs were made and the water was gotten to help the dynamiters when the fire had really crossed the wide Van Ness Avenue. The Claus Spreckels brownstone mansion on the west side stopped its progress to the northwest.

At our elevation of 340 feet we got *no water for* two weeks; no lights except candles yet; no fires in houses; and the people are cooking in the streets; rich and poor.

That is the gist of the matter so far as I can gather without having official authority. We expect much from the Light House Board; from Coast Survey tidal observations, and other sources.

Area of the burned district.

4 square miles; say 2560 acres
London 436.
490 blocks

and probably 25,000 houses

SAN FRANCISCO, May 10, 1906.

This letter was discussed by Prof. W. B. Scott.

The following papers were read:

"The Nutritive Requirements of the Human Body," by Prof. Francis G. Benedict, which was discussed by Professor Houston, Professor Haupt, Mr. Wilcox, Mr. Goodwin and Professor Scott.

"A History of the Major Classification of the Mammalia," by Prof. Henry F. Osborn.

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THE PARAGENESIS OF THE MINERALS IN THE GLAUCOPHANE-BEARING ROCKS OF CALIFORNIA.

BY JAMES PERRIN SMITH,
STANFORD UNIVERSITY, CALIFORNIA.

COMMUNICATED BY PROF. J. C. BRANNER.

(*Read October 5, 1906.*)

OCCURRENCE OF GLAUCOPHANE-BEARING ROCKS IN CALIFORNIA.

Glaucophane-bearing rocks are widely distributed in the Coast Ranges of California, in the Franciscan series, commonly associated with serpentine (altered peridotite). The rocks that contain glaucophane are classed together because of the occurrence of this mineral, but they are of widely different characters and origin. It is likely, however, that they were all affected by the same sort of metamorphism, although the writers that have discussed these rocks are by no means agreed as to whether this metamorphism was due to contact or to dynamic action.

No glaucophane is developed in the extensive series of mica schists along the granite contacts of the Coast Ranges, where micaceous and hornblendic gneissic and schistose rocks are abundant. And yet those schists are not essentially different in chemical nature from the glaucophane rocks. In the Franciscan series, on the other hand, glaucophane is abundant in altered rocks ranging in character from acid quartzites and diorites to very basic gabbros and diabases. It is, therefore, clear that the chemical nature of the altered rocks can have had little influence on the formation of the glaucophane, though of course it decided the petrographic character.

The glaucophanes in the Coast Ranges of California are always products of metamorphism, and never of crystallization out of a magma. And while they are found in altered igneous rocks just as often as in altered sedimentaries, they are always secondary in both. It is thus clear that the petrographic character can have had just as little influence as the chemical constitution on the formation of glaucophane. It is true that the sedimentary rocks altered to glaucophane schists are all acid, with the exception of tuffs, and that the igneous rocks that have had glaucophane developed in them are mostly basic. But then non-calcareous sediments are usually acid, on account of the predominance of quartz sand; and acid intrusive rocks are rare in the Franciscan series.

Ransome[1] has shown that glaucophane has been developed on a small scale in chert at the contact with serpentine, and with metabasalts or "fourchites." There are, however, in the Coast Ranges extensive glaucophane schist masses near which neither serpentine nor metabasalt has been found. And often where such schists are near serpentine they are in much greater masses than the igneous rocks to which the metamorphism might be ascribed. From this it would seem illogical to assume that the formation of glaucophane schists is due in every case to contact with basic intrusive rocks. There are also numerous contacts of such basic intrusives with sediments where no schists have been formed.

On the other hand, we might assume that dynamic metamorphism has been the cause of the formation of the glaucophane schists, for the Franciscan series is usually crushed, and zones of shearing are common in it. But while the glaucophane rocks are extensively developed for several hundred miles in the Coast Ranges, from Oregon to San Diego, there are no continuous masses of schists. Thus the cause of metamorphism can hardly have been regional. And it is hard to see how this agency can have been intermittent or local in its action, since the rocks of the Franciscan series are

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[1] "Geology of Angel Island," *Bull. Dept. Geol. Univ. Calif.*, Vol. I., p. 223.

crushed and sheared almost everywhere, even where no glaucophane has been developed in them. Yet, in a sense, the phenomenon is regional, that is, confined to a petrographic region, or, more properly, a region of one sort of geologic activity.

Neither in the Rocky Mountain, the Lake Superior, nor in the Appalachian region, are glaucophane schists developed; although amphibole and garnet schists are common. Nor yet in the Sierra Nevada of California, where extensive schist masses, both of dynamic and of contact origin, are common, is any glaucophane schist known. And there, too, extensive masses of peridotites occur, and altered sediments of the same chemical character as in the Coast Ranges, but of totally different petrographic character. Altered quartzites are abundant in the Sierra Nevada, also altered clay shales and altered diabase tuffs. But the quartzites have been changed to sericite schists, the clay shales to andalusite schists, and the diabase tuffs and the peridotites to amphibolites, all without glaucophane. And, what is more puzzling, the metamorphism in the Sierra Nevada and in the Coast Ranges seem to have been contemporaneous.

MINERALS OF THE GLAUCOPHANE-BEARING ROCKS.

Primary Minerals.

Under this head are treated those minerals that were present in the original rock, and have remained unaltered. Feldspars were important constituents of the original rocks, both igneous and sedimentary, but they have rarely resisted the readjustment consequent upon metamorphism. In a quartz diorite from Oak Ridge, about five miles east of Calaveras Valley, Santa Clara County, the original oligoclase is largely intact, though decomposed in spots where secondary glaucophane (crossite) and lawsonite have been formed.

The original labradorites of the pseudodiorites and the pseudodiabases (altered diabases) of Sulphur Bank and other localities are partly preserved as such, although usually they are entirely decomposed. In general, the feldspars have been the first minerals altered, as can be seen in the pseudodiabases, where often the hornblende or the augite is unaltered, and the feldspars are changed to a saussuritic mass.

Original hornblende has been observed in the altered quartz diorite of the Oak Ridge, where it shows all the characters of the titaniferous kataphorite. This has been decomposed around the borders, with the addition of soda to form glaucophane (crossite), and the setting free of titanium to form leucoxene, and titanite. Most of the hornblende of the pseudodiorites and pseudodiabases is not original in the strictest sense, for it is largely uralite after augite, but it was already present before the metamorphism which later formed epidote and glaucophane in these rocks.

Quartz is present as an undoubted original constituent only in the quartz diorite, but much of the quartz in the siliceous glaucophane schists may be original, since there is no criterion by which the original granular quartz may be distinguished from the same mineral when recrystallized.

Original augite is present only in the pseudodiabases and pseudodiorites, and a few greenstones, where the metamorphism has not gone very far.

Secondary Minerals.

Of all the constituents of the metamorphic rocks of the Coast Ranges of California glaucophane is the most characteristic. There are probably several varieties of it, but at present it is not practicable to separate them. In all the true glaucophanes in these rocks there are certain general characteristics that distinguish them. The vertical axis is the axis of least elasticity, the extinction angle of $c \wedge \mathfrak{c}$ is small, ranging from 5° to 14°. The character of the double refraction is always negative, and the plane of the optic axes lies in the ulane of symmetry. The angle of the optic axes in some varieties is so small that the mineral is almost uniaxial, in others the angle is moderately wide. The pleochroism is always strong, the light vibrating parallel to $\mathfrak{a}$ being pale yellowish, that parallel to $\mathfrak{b}$ deep violet, and that parallel to $\mathfrak{c}$ intense sky blue. The crystal faces are extremely simple, only three distinct forms being seen, the orthopinacoid (100), the prism (110), and the clinopinacoid (010). The crystals are usually in long prisms after (110) rarely showing the other faces.

This mineral is equally at home in the acid quartzites, altered diorites, diabases and gabbros, so the acidity or basicity of the original rock has had no influence on its formation. Naturally glaucophane is more abundant in the basic metamorphics, where there was more iron and soda necessary for its genesis.

A rare variety of glaucophane has been described by Palache[1] from the Coast Ranges under the name of crossite. This differs from common glaucophane chiefly in the orientation of the axes of elasticity, the plane of the optic axes being transverse. The pleochroism is the same as that of glaucophane, except that the colors are more intense. The polarization colors appear to be lower than those of glaucophane. There appears also to be a chemical difference, as will be seen in the analyses quoted below, the iron being higher and the alumina lower than in any published analyses of glaucophane, except three from Rhodus, described by H. B. Foullon[2] as rhodusite, a variety of glaucophane. Foullon, however, did not give the optical orientation of the mineral, but stated that it lacked the violet pleochroism so characteristic of true glaucophane. It is probable that the mineral from Rhodus is either crossite or crocidolite. But since we have only one analysis of crossite, it is hardly proper to base conclusions on that.

Crossite has been identified in the Coast Ranges only in the albite-crossite gneiss from North Berkeley, in a quartz-glaucophane schist from Tiburon peninsula about a mile and a half north of the ferry, and in a metamorphosed quartz diorite from Oak Ridge, Santa Clara County, five miles east of Calaveras Valley. The last two occurrences were identified solely by the optical orientation, the analysis published by Palache being of the mineral from North Berkeley.

In some of the more ferruginous schists there is an asbestiform glaucophane-like mineral that is probably crocidolite, and a probable identification of this mineral has also been made in a dynamically metamorphosed albite syenite from Spanish Peak, Plumas County, where a brownish titaniferous hornblende (kataphorite) has frayed out at the ends in fine blue needles of crocidolite, with the characteristic pleochroism of that mineral.

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[1] "On a Rock from the Vicinity of Berkeley Containing a New Soda-Amphibole," *Bull. Dept. Geol. Univ. Calif.*, Vol. I., pp. 181–192.

[2] Sitzb. Akad. Wien (1891), Bd. 100, pp. 172–174.

There can be no doubt that the blue aphibole that is so common in the metamorphic rocks of the Coast Ranges, from Oregon to San Diego, is mostly true glaucophane, for the writer has examined at least 300 slides from all over this great region, and has found the orientation to be that of the type from Syra, except in the cases mentioned above. It is likely, however, that a chemical study of this mineral from these different localities will disclose the presence of many varieties, some of which may eventually be designated by special names. It is associated with epidote, garnet, and lawsonite, and usually with mica, either muscovite, paragonite, biotite, or margarite. The rocks in which it is found are metamorphosed diorites, greenstones, pseudodiorites and pseudodiabases, eclogites and glaucophane schists.

It is probable that the variety gastaldite is present among the glaucophanes of California, for there are numerous occurrences of

TABLE OF ANALYSES OF GLAUCOPHANES.

	I. Glaucophane From Berkeley, Calif. Per Cent.	II. Glaucophane From San Pablo, Calif. Per Cent.	III. Glaucophane From Syra, Greece. Per Cent.	IV. Glaucophane From Syra, Greece. Per Cent.	V. Crossite From Berkeley, Calif. Per Cent.	VI. Rhodusite From Rhodus, Greece. Per Cent.	VII. Gastaldite From St. Marcel, Italy. Per Cent.
SiO_2	52.39	54.52	55.64	57.67	55.02	55.06	58.55
Al_2O_3	11.29	9.25	15.11	11.07	4.75	0.49	21.40
Fe_2O_3	3.74	4.44	3.08	3.20	10.91	15.48	
FeO	9.13	9.81	6.85	9.68	9.45	7.40	9.04
MgO	11.37	10.33	7.80	9.85	9.30	11.49	3.92
CaO	3.03	1.98	2.40	0.95	2.38	0.98	2.03
Na_2O	6.14	7.56	9.34	6.80	7.62	6.38	4.77
K_2O	trace	0.16		0.42	0.27	0.80	
MnO	trace	0.46	0 56	0.06	trace		
H_2O	2.57	1.78		0.48		1.98	
TiO_2	0.14	0.39					
Total.	99.80	100.68	100.78	100.18	99.70	100.06	99.71

I. Analyst, W. C. Blasdale, *Bull. Dept. Geol. Univ. Calif.*, Vol. II., 338.

II. Analyst, W. C. Blasdale, *Bull. Dept. Geol. Univ. Calif.*, Vol. II., 338.

III. Analyst, O. Luedecke, *Zeitschr. Deutsch. Geol. Gesell.*, Vol. 28, p. 249.

IV. Analyst, H. S. Washington, *Amer. Jour. Sci.*, 4th Ser., Vol. XI., p. 40.

V. Analyst, W. S. T. Smith, in C. Palache, *Bull. Dept. Geol. U. C.*, I., p. 188.

VI. Analyst, H. B. Foullon, *Sitzb. Akad. Wiss. Wien*, Vol. C, p. 174.

VII. Analyst, Cossa, *Accad. Linc. Rom.*, 1875, Vol. II., p. 33.

a pale blue mineral with the optical orientation and the pleochroism of glaucophane, but evidently poor in iron. In default of analyses of this variety it can not be assigned with certainty to gastaldite.

Pargasite.—In the basic eclogites of California the writer has found a black amphibole resembling pargasite from Pargas, but differing from that mineral in the orientation of its optical axes, and in its pleochroism. This mineral has been analyzed and its physical properties studied by Mr. W. O. Clark,[1] assistant in geology at Stanford University. It fuses readily to a black globule, and colors the flame yellow. It occurs abundantly in short, thick-set prisms with a cleavage angle of about 124°. It is monoclinic, with parallel extinction on planes in the zone of the symmetry axis, and on the clinopinacoid gives an extinction of $c \wedge \mathfrak{c}$ about 18°. The axis of least elasticity is nearest to crystallographic c. The plane of the optical axes lies in the symmetry plane, and the $\mathfrak{a}$ axis is the acute bisectrix, hence the double refraction is negative. The absorption is strong and the pleochroism decided, $\mathfrak{a}$ = yellowish brown, $\mathfrak{b}$ = deep olive green, and $\mathfrak{c}$ = greenish blue. The absorption formula is $\mathfrak{b} > \mathfrak{c} > \mathfrak{a}$. The angle of the optical axes is very wide, being nearly 90°.

This mineral was first mentioned by Mr. R. S. Holway,[2] and by him doubtfully referred to pargasite. It is abundant in the black eclogites of Calaveras Valley, Santa Clara County, and has also been identified by the writer in a similar rock near Reed's Station on the Tiburon Peninsula. It is associated with glaucophane, carinthine, actinolite, red garnet, epidote, margarite, albite, and lawsonite, and is known in only those rocks that are unusually rich in iron. In these rocks the glaucophane is a later product than the pargasite, and appears only as a replacement rim around the latter. Pargasite is most nearly related to an amphibole made artificially in the wet way by Chrustschoff.[3] The artificial amphibole was black, and showed strong pleochroism from yellowish green to bluish green; $c \wedge \mathfrak{c}$ 17° 56′, $2 \vee 82°$, and sp. gr. 3.245. $\mathfrak{a}$ and $\mathfrak{b}$ = yellowish green, and $\mathfrak{c}$ = bluish green. There is a re-

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[1] Unpublished paper.

[2] *Journal of Geology,* Vol. XII. (1904), "Eclogites in California," p. 352.

[3] *Bull. Acad. Sci. St. Petersburg,* 23 Oct., 1890; and *Neues Jahrb.,* 1891, Vol. II., p. 86; cited by Hintze, "Handb. d. Min.," Vol. II., pp. 1232 and 1242.

markably close agreement between pargasite from Pargas (Finland), that from Calaveras Valley, California, and the artificial amphibole made by Chrustschoff in chemical composition, orientation of the axes of elasticity and the pleochroism. Pargasite from Finland is optically positive, while that from California is negative, but that is a minor difference. Chemical analyses of pargasite from California, pargasite from Finland, and the artificial amphibole are given below on the table with the actinolite.

Carinthine.—This variety is rather common in the medium basic and acid rocks of the glaucophane series. It resembles pargasite, but is greenish rather than jet-black, and in thin sections the colors are not so dark. It occurs chiefly in long prisms, without terminal planes. The plane of the optical axes is the symmetry plane, the optical angle is wide and the character of the double refraction is negative. The extinction angle of $\mathfrak{c} \wedge c$ is slightly smaller than in pargasite, varying from 14° to 17°. The pleochroism is strong, $\mathfrak{a}$ = yellowish, $\mathfrak{b}$ = light olive green, $\mathfrak{c}$ = greenish blue. This mineral is common in the altered diorite near Searsville dam, Santa Clara County, in a similar rock three miles southwest of Redwood, in a quartz-lawsonite-glaucophane schist near the Schrader farm, two miles west of Redwood, in the eclogite of Oak Ridge, Santa Clara County, in the basic glaucophane schists at the Junction School House near Healdsburg, and in the glaucophane gneiss of Melitta, near Santa Rosa. No analyses were made of the mineral in these occurrences, but W. C. Blasdale[1] has described a similar variety in the glaucophane schists from near Berkeley. Blasdale's mineral has the same optical properties as those given above for carinthine, $\mathfrak{a}$ = light green, $\mathfrak{b}$ = yellow green, and $\mathfrak{c}$ = bluish green. The analysis published by Blasdale and quoted below shows this mineral to be like the typical carinthine of the Alps, in its having lower magnesia and higher soda than common actinolite has. It is safe to say that the mineral described by Blasdale under the name of actinolite is the variety carinthine, which should be distinguished because of its association with glaucophane, both in California and in the Alps. It is quite likely that carinthine grades over into pargasite, but the two are rather distinct in their occur-

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[1] *Bull. Dept. Geol. Univ. Calif.*, II., pp. 328–335.

rence and associations, the carinthine being more common in the rocks poorer in iron, and the pargasite in rocks where the iron is extremely abundant. Both appear to be older than the glaucophane which accompanies them, for that mineral is often found in replacement rims around the borders of the others, probably from later accession of sodium silicate due either to a further decomposition of plagioclase, or to the bringing in of soda by solutions.

Actinolite.—Common green actinolite of the variety smarag-

ANALYSES OF ACTINOLITE AND PARGASITE.

	I. Actinolite from Berkeley.	II. Actinolite from San Pablo.	III. Carinthine from Carinthia.	IV. Smaragdite from Lake Geneva.	V. Actinolite, Zillerthal, Alps.	VI. Pargasite from Finland.	VII. Pargasite from California.	VIII. Artificial Amphibole.
SiO_2	55.21	55.56	49.33	54.30	53.10	41.26	42.68	42.35
Al_2O_3.........	3.45	2.05	12.72	5.15	4.10	11.92	9.96	8.11
Fe_2O_3			1.72			4.83	6.12	7.91
FeO	7.49	5.97	4.63	3.87	21.80	9.92	12.25	10.11
MgO	18.77	19.45	17.44	19.01	10.40	13.49	9.58	14.33
CaO	10.50	12.13	9.91	13.72	10.60	11.95	11.83	13.31
Na_2O	2.45	1.94	2.25	2.80		1.44	3.30	2.18
K_2O..........		0.30	0.63			2.70	0.89	1.87
H_2O..........	1.75	2.58	0.29	0.30		0.52	3.28	0.91
TiO_2							0.68	
Fi.............			0.21			1.70		
MnO							trace.	
Total......	99.82	99.98	99.13	99.15	100.00	99.73	100.57	100.98

I. Actinolite from Berkeley, California; W. C. Blasdale, *Bull. Dept. Geol. Univ. Calif.*, Vol. II., No. 11, p. 333.

II. Actinolite from San Pablo, California; W. C. Blasdale, analyst, *loc. cit.*

III. Carinthine from Carinthia; Rammelsberg, analyst; cited in Hintze's Handbuch der Mineralogie, Vol. II., p. 1235.

IV. Smaragdite from Lake Geneva, Switzerland; Hunt, analyst; cited in Hintze, *op. cit.*, p. 1236.

V. Actinolite from Zillerthal, Alps; Beudant, analyst; cited in Hintze, *op. cit.*, p. 1235.

VI. Pargasite (black) from Finland; Rammelsberg analyst; cited in Hintze, *op. cit.*, p. 1239.

VII. Pargasite from Calaveras Valley, California; W. O. Clark, analyst (unpublished paper).

VIII. Artificial amphibole, made by Chrustschoff, cited by Hintze, Handbuch d. Mineralogie, Bd. II., p. 1242.

dite is very common in the glaucophane schists and eclogites, in long slender prisms and needles, with small extinction angles, and pleochroism from pale to bright grass green. This is often intergrown with glaucophane, and frequently shows replacement rims of that mineral. It sometimes results from the alteration of primary hornblende in the rocks, but more often from the uraliting of pyroxenes. Whether the smaragdite is soda-bearing has not been determined by analysis. Probably there is a complete intergradation between pargasite, carinthine and actinolite. The analyses quoted below show the composition of the bluish green actinolite from Berkeley and San Pablo, of the pargasite from Calaveras Valley, Santa Clara County, California, and for comparison the composition of carinthine from the Alps, of pargasite from Finland, of typical actinolite (smaragdite) from Lake Geneva, and of an artificial amphibole with the same chemical composition as pargasite.

Diopside.—The pale green variety of diopside without terminal planes, and with very high double refraction, known as omphacite, is rather common in the basic glaucophane-bearing rocks, especially in the eclogites. It is quite abundant in the eclogite from near San Martin, Santa Clara County, California, and also occurs in the glaucophane-lawsonite eclogite of Reed's Station near Tiburon, California. In appearance it resembles actinolite closely, but differs in its cleavage, and its high extinction angle in clinopinacoid sections.

The augite with fine orthopinacoid cleavage known as diallage, while essentially characteristic of igneous rocks, has been found in the eclogite of the Junction School-house near Healdsburg, as a secondary mineral associated with red garnet, glaucophane, margarite and titanite.

Epidote.—This mineral is very common in the basic metamorphics of the glaucophane series, but it is not the deeply colored mineral usually found in contact products. It is colorless, without pleochroism, and the interference colors in thin slides are not above the bottom of the second order. The crystals are elongated parallel to the symmetry axis, and consequently most sections give rectangular parallelograms with rather distinct cleavage parallel to the symmetry axis, and the optical figure across the plate. This variety of epidote is poor in iron, and is very likely the mineral described by Becker[1] as zoisite, with which it is probably identical chemically.

Zoisite.—This is rare in the glaucophane rocks, being known to occur as an abundant constituent only in the basic schists associated with the ecologite of Hilton Gulch on Oak Ridge, Santa Clara County, California. Its form and appearance are exactly like the white epidote, with which it is associated, but its double refraction is very weak in all sections, the colors in thin slides not rising above blue-gray of the first order. A section of the white epidote cut normal to an optical axis can not be distinguished from zoisite in parallel light, but in convergent light the optical figure distinguishes the epidote. Below there is quoted from Becker an analysis of a mineral described by him as zoisite, although his description agrees exactly with the white epidote described above. The mineral analyzed was found in a glaucophane schist, of which the constituent minerals were glaucophane, zoisite (epidote?), quartz, muscovite (paragonite?), albite and titanite.

Analysis of Zoisite (White Epidote?) from Glaucophane Schist.[2]

	Per Cent.
SiO_2	39.80
Al_2O_3	22.72
Fe_2O_3	4.85
FeO	1.49
MgO	3.89
CaO	17.55
Na_2O	4.09
K_2O	0.12
TiO_2	trace
MnO	0.26
H_2O (above 100° C.)	5.25
Total	100.02

This pale epidote was described by F. L. Ransome[2] from an eclogite near Reed's Station on the Tiburon Peninsula, but it is also abundant in most of the eclogites and glaucophane schists of California.

A complete analysis of the glaucophane schist (No. 98, Sulphur

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[1] Mon. XIII. U. S. G. S., p. 79.

[2] G. F. Becker, Mon. XIII., U. S. G. S., p. 79.

[3] *Bull. Dept. Geol. Univ. Calif.*, Vol. I., No. 10, p. 310.

Bank), from which the supposed zoisite was separated, is given by Becker in the work cited. This analysis is quoted below in the table of analyses of the metamorphic rocks, and agrees very well with those of the ordinary basic glaucophane schists.

Garnet.—This is one of the most abundant minerals in the glaucophane rocks of California, but only in those rocks that have been entirely recrystallized. It is usually in the form of rhombic dodekahedrons, the only form observed, but in the pargasite eclogites of Calaveras Valley the garnet is in anhedrons. It seems to be equally at home in the eclogites, glaucophane schists, and acid gneisses, so that the acidity and the basicity of the rocks have had no influence on its formation, which has been determined entirely by the intensity of the metamorphism. The crystals vary from extremely minute microscopic forms to those 15 mm. in diameter, the largest having been observed in the pargasite eclogite of Hilton Gulch on Oak Ridge, Calaveras Valley. It often occurs in large masses several inches long, but these are really aggregates of small crystals. The crystals in the eclogites are usually surrounded by a kelyphite ring, or reaction zone, of white mica, and chlorite is often formed in this same zone. Occasionally the garnet is entirely replaced by chlorite.

The garnet is, apparently, one of the last minerals to be formed, for, although it usually shows well-developed crystal faces, it contains inclusions of most of the other minerals. It is pale red, and appears to be of the same character in all of the glaucophane series, but as yet little chemical study has been made of the various occurrences. The only analysis of the garnet is that of the crystals in a diopside eclogite from Coyote Creek, about six miles north of San Martin, Santa Clara County, made by Mr. W. O. Clark, assistant in geology at Stanford University.

ANALYSIS OF GARNET FROM ECLOGITE ON COYOTE CREEK.

	PER CENT.
SiO_2	38.69
Al_2O_3	19.10
FeO	26.81
MgO	5.07
CaO	10.64
Total	100.31

This corresponds to a mixed molecule, of approximately one-half alumina-iron garnet (almandine), one-sixth alumina-magnesia garnet (pyrope), and one-third alumina-lime garnet (grossular).

Titanium Minerals.—Sphene or titanite is abundant in small patches, more rarely in crystals, in most of the glaucophane rocks, but is especially characteristic of the more basic members of the series. The titanium appears to have been derived chiefly from the titaniferous hornblendes and pyroxenes of the original rocks. In the only partly recrystallized diabases and gabbros, where the augites have been uralited, or changed to actinolite or carinthine, the titanium has not remained in the ferro-magnesian mineral, but has separated out as leucoxene and titanite. In the partly recrystallized quartz diorite of Oak Ridge the original titaniferous hornblende has been changed on the borders to crossite, and the titanium has in this case, too, crystallized out as leucoxene and titanite. The same thing has been observed in a syenite of Spanish Peak in Plumas County, where the hornblende has been partly changed to crocidolite. In an orthoclase-glaucophane gneiss of Melitta sphene is abundant in sharply defined rhombs in thin sections, probably derived in the same way.

Rutile is rather common in minute crystals in most of the basic schists and eclogites, but in an eclogite from Coyote Creek near San Martin it is abundant in large crystals of 10 mm. in length. Its genesis is probably the same as that of sphene.

Lawsonite.—This is one of the most characteristic minerals in the glaucophane bearing rocks of California. It was first described by F. L. Ransome,[1] from the eclogite near Reed's Station on the Tiburon Peninsula. It has since been found to be abundant in schists, gneisses and eclogites from many parts of the Coast Ranges, and has also been found in similar rocks in Europe. Since it is such a constant companion of glaucophane, its occurrence and origin throw much light on the genesis of that mineral.

Lawsonite is orthorhombic in form, and of simple habit, the prism (110) and the basal pinacoid (001) being the commonest

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[1] "On Lawsonite, a New Rock-forming Mineral, from the Tiburon Peninsula, Marin County, Cal.," *Bull. Dept. Geol. Univ. of California,* Vol. I., No. 10 (1895), pp. 301–312.

faces observed in the rocks, the prism faces meeting at an angle of 67° 16′; consequently longitudinal sections usually give rectangular outlines, and basal sections rhombs. There are three cleavages, that parallel to the basal pinacoid, and that parallel to the brachypinacoid being rather distinct, while the cleavage parallel to the prism is faint. In longitudinal sections the pinacoidal and basal cleavages are at right angles to each other, while in basal sections the distinct brachypinacoidal cleavage bisects the obtuse angle (112° 44′) of the faint prismatic cleavage. The plane of the optical axis lies in the brachypinacoid (010) and the acute bisectrix is the axis of least elasticity, and is the normal of the basal plane. The character of the double refraction is therefore positive. Sections parallel to the vertical axis show high double refraction, and in thin slides show second order interference colors, while basal sections give weak double refraction and in thin slides show no higher color than yellow or orange of the first order. The orientation of the optical figure with reference to the cleavages and the axes of elasticity affords a ready means of identifying this mineral in microscopic sections. The relief is high and in thin sections the mineral is colorless, though in thicker sections and in macroscopic crystals the color is light blue. These characters make the determination of the mineral easy, even in minute crystals.

Two analyses of lawsonite are quoted below, both made of the mineral from the original locality. No analyses of this mineral

ANALYSES OF LAWSONITE FROM THE TIBURON PENINSULA.

	I.	II.
SiO_2	38.10	38.45
Al_2O_3	28.88	31.35
Fe_2O_3	0.85	0.86
FeO		0.10
MgO	0.23	0.17
CaO	18.26	17.52
Na_2O	0.65	0.06
K_2O		0.23
TiO_2		0.38
H_2O (above 100° C.)	11.47	11.21
MnO		trace
Total	98.39	100.33

I. F. L. Ransome, *Bull. Dept. Geol. Univ. of California*, Vol. I., 307.
II. Schaller and Hillebrand, *Amer. Jour. Sci.* (1904), p. 197.

have been made from other localities, but the physical properties are so striking that the identification is certain.

These analyses show that lawsonite is essentially a silicate of alumina and lime with two molecules of water, $CaAl_2Si_2O_8 + 2H_2O$; the iron, magnesia, alkalies and titanium may be neglected as probably coming from inclusions, and the mineral may be regarded as a metamorphic feldspathoid of the chemical nature of anorthite with the addition of water.

Lawsonite is known only as a product of metamorphism, and it is doubtful if a silicate with such a high percentage of water, over 11 per cent., could originate as a product of magmatic crystallization. It occurs only in rocks that have been more or less recrystallized; these are invariably rich in lime, and hence usually basic, though not necessarily so. It was described first from an eclogite rich in garnet, actinolite, glaucophane and margarite, and in nearly all the other rocks where it has been found glaucophane, or some other of the soda-bearing amphiboles, is a common associate, and albite occurs very commonly. On the other hand, garnet and epidote, which are abundant in most of the glaucophane-bearing rocks, are usually either lacking or scarce in the rocks with lawsonite.

The source of the lime for the lawsonite must have been the plagioclases which were abundant in most of the igneous rocks, the arkoses, and tuffs out of which the lawsonite rocks were made. The plagioclases were the first minerals to be disintegrated in the chemical readjustment that accompanied metamorphism. The albite molecule usually joined itself to pyroxenes or hornblende, forming glaucophane or some other member of the glaucophane group, and if there was an excess of sodium-aluminum silicate, it crystallized out as albite. If iron, alumina, and magnesia were in excess, the anorthite portion of the plagioclase molecule joined itself to them, forming garnet and epidote; but if there was an excess of lime, the anorthite molecule simply took up water and formed lawsonite. These reactions explain clearly why glaucophane is an almost invariable companion of lawsonite, why albite is a common associate, and why garnet and epidote do not usually occur in considerable quantity in lawsonite rocks.

Lawsonite occurs in glaucophane-bearing eclogites, in greenstones, glaucophane schists, in lawsonite gneisses and schists, and in an altered quartz diorite, in which lawsonite and crossite are the only secondary minerals, the feldspars being only slightly altered in patches and the hornblende changed to crossite only around the borders.

Feldspars.—Orthoclase occurs rarely in the glaucophane rocks, and only in the acid schists and gneisses, for what little potash was in them has usually gone into the formation of muscovite.

Albite is very abundant in both acid and basic schists and gneisses, although it was almost never present in the original rocks. In the albite gneisses of Berkeley and Angel Island it makes up a large part of the rock, and in many others it occurs in veins and scattered through the mass. Its genesis is due to the decomposition of the plagioclase molecule, and to the crystallizing out of the excess of sodium aluminum silicate. It is not present in any cases where members of the glaucophane group have not been formed, which is sufficient proof that its presence indicates excess of soda.

Oligoclase has been observed in a quartz glaucophane gneiss near San Luis Obispo, but it was in small grains, and it was not possible to determine whether the grains were original fragments.

Labradorite, which is usually characteristic of igneous rocks, has been observed in considerable quantities in veins in a glaucophane eclogite from the Junction School-house near Healdsburg. It was determined by its extinction angles on the twinning planes, the orientation of its interference figure, and the character of its double refraction, all agreeing with labradorite. A qualitative test showed the presence of both lime and soda. Its occurrence in veins showed clearly that it is secondary.

Zircon, etc.—Zircon has been observed in minute crystals in the albite-crossite gneiss of Berkeley. Sillimanite occurs in the quartz-glaucophane schists of Catalina Island, and in similar rocks near San Luis Obispo. Cyanite, which is said to be a common companion of glaucophane in the glaucophane schists of Europe, was found in California only in a diopside-glaucophane eclogite on the San Francisco Peninsula. The rock was a small piece of float, and its original locality is unknown.

Micas.—No micas are known in the original rocks from which the glaucophane rocks were made, and yet muscovite, paragonite and margarite are abundant, and biotite occurs rarely in the metamorphics. Muscovite is confined to the acid quartz glaucophane schists, probably made from original orthoclase in the arkose. Paragonite was probably made from the soda of part of the albite molecule when the plagioclase was disintegrated. In the lime rich rocks margarite is abundant, made from the excess of lime of the anorthite molecule when basic plagioclases were disintegrated. Biotite has probably the same origin as muscovite, with the addition of iron.

Quartz.—Recrystallized quartz occurs in all the rocks of the glaucophane series, in some forming the greater part of the mass. It was made partly from original quartz in the forms of grains and crystals, but in the diorites it was made from the excess of silica when more basic silicates, such as glaucophane, lawsonite, garnet and epidote, were formed.

Chlorite.—This is found in many of the rocks as a decomposition product, but in the actinolite schists it seems to be an original product of the recrystallization of basic rocks of the nature of pyroxenites. Actinolite is more acid than the original ferromagnesian minerals, and the residue would naturally be more basic.

Talc.—This occurs in large quantities in the lawsonite gneisses and schists. These rocks are made up largely of glaucophane and lawsonite, with crystal plates of talc scattered through them. The glaucophane is perfectly fresh, and there is no other magnesia-bearing mineral that might have given rise to the talc. The original rocks were probably made up of hornblende or pyroxene and plagioclase, and the disintegration of the plagioclase molecule furnished calcium silicate for the formation of lawsonite, while the albite molecule entered into combination with the hornblende or pyroxene to form glaucophane. The latter mineral requires more silica and less magnesia than the original hornblende or pyroxene, and there is left over an excess of silicate of magnesia, which crystallized out as talc. It is noteworthy that no garnets have been found in the rocks with talc, which shows that the metamorphism has not been so intense in this case.

Other Minerals.—Apatite, magnetite, and pyrite are present in small quantities in most of the glaucophane series, but only the pyrite is of any importance in throwing light on the origin of the rocks. Its presence suggests that the metamorphism was aided by thermal solutions, which would naturally be alkaline, since only such solutions carry sulphur in any quantity.

Types of Glaucophane-bearing Rocks of California.

The glaucophane-bearing rocks of California are varied in character, ranging from massive little altered igneous rocks to thin-bedded schists; but certain well defined groups may be distinguished under them. The groups defined below are independent of the genesis, based entirely on the petrographic character, for under most of them are found both rocks of sedimentary origin and rocks that were originally fused magmas.

I. *Eclogites.*

Eclogites were originally defined as massive rocks made up of garnet and omphacite, but since similar rocks often contain actinolite, glaucophane or other hornblendes as the chief ferro-magnesian mineral, the group has been extended to include these. All the eclogites known in California contain some glaucophane, although some other amphibole is often the predominant mineral. These rocks have been described by R. S. Holway,[1] and the principal minerals in them discussed. The typical variety, those composed of omphacite and garnet, is rare in California, having been described only from two localities. On Coyote Creek, six miles north of San Martin, Santa Clara County, is a massive rock composed of large dodekahedrons of red garnet, long prisms of omphacite, and a little glaucophane and white mica, probably paragonite. Some actinolite occurs in the rock, and abundant rutile in patches. This rock has been analyzed, and the results given on the next page are quoted from Mr. Holway's paper.

The sample analyzed did not contain any of the segregations of rutile. The estimated mineral constitution is approximately two parts red garnet, two of omphacite, and one of actinolite, glaucophane, and white mica together. The rock has the composition of a diabase, but the alumina, lime, and magnesia are lower than usual,

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[1] "Eclogites in California," *Jour. Geol.*, Vol. XII., 1904, pp. 344–358.

and the iron and alkali higher. The published analyses that come nearest to that of this eclogite are those of the basalts of Kilauea, Hawaiian Islands, quoted by H. S. Washington,[1] in which the potash has a similar high percentage.

ANALYSIS OF ECLOGITE FROM COYOTE CREEK.

	PER CENT.
SiO_2	44.15
Al_2O_3	10.18
Fe_2O_3	11.92
FeO	13.04
MgO	6.18
CaO	4.51
Na_2O	5.11
K_2O	2.09
P_2O_5	0.20
TiO_2	trace
H_2O (above 100° C.)	0.95
Total	99.31

A very similar eclogite occurs in a large mass at Hadsell's farm, on the Arroyo Hondo, at the northern end of Calaveras Valley. No analysis was made of this rock, but it is composed of abundant dodekahedrons of red garnet often nearly a centimeter in diameter, long prisms of omphacite, a little titanite, lawsonite, colorless epidote, and white mica, probably paragonite. Glaucophane is scattered irregularly through the mass, in places making up the greater portion of it, so that the rock might be described as an eclogite in the restricted sense, or as a glaucophane eclogite, according to where the hand specimen was taken. A little albite was found in the slides, but always where the glaucophane was most abundant.

A very beautiful and characteristic eclogite occurs near Reed's Station on the Tiburon Peninsula, from which Ransome[2] first described the important rock-forming mineral lawsonite. The rock is massive but grades over into glaucophane schist. It is composed of red garnets from two to three mm. in diameter, with sharply defined dodekahedral faces, long prisms of smaragdite, some green omphacite, considerable glaucophane, and pale epidote, numerous large

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[1] Prof. Papers No. 14, U. S. Geol. Survey, 1903, p. 325, Sylvester, analyst.

[2] "On Lawsonite, a New Rock-forming Mineral from the Tiburon Peninsula, Marin County, Cal.," *Bull. Dept. Geol. Univ. of California,* Vol. I., No. 10 (1895), pp. 301–312.

crystals of lawsonite, and plates of margarite. Sphene is scattered through the mass. In places the glaucophane is so abundant that the rock might appropriately be called a glaucophane eclogite.

Some of the California eclogites are remarkably like those of Syra in the Grecian Archipelago, from which glaucophane was first described. This resemblance is especially strong in the glaucophane eclogites of Camp Meeker in Sonoma County, and of the Junction School-house near Healdsburg. These are massive rocks made up of dodekahedral red garnets, and long prisms of glaucophane, with a little colorless epidote, and patches of titanite. A part of the Healdsburg eclogite is peculiar in having secondary diallage and secondary labradorite along with red garnet and margarite, and in showing replacements of both diallage and margarite by glaucophane. In both the Healdsburg and the Camp Meeker eclogites the garnets almost invariably show a kelyphite ring, or reaction zone, where mica has developed apparently at the expense of the garnet.

A new type of eclogite was described from Calaveras Valley by R. S. Holway.[1] This rock is composed of anhedrons of garnet and short thick prisms of an amphibole that is black in reflected light, and greenish blue in transmitted light. It fuses readily to a black magnetic globule, coloring the flame yellow, showing the presence of considerable sodium and iron. The pleochroism is intense, the $\mathfrak{a}$ ray being pale greenish yellow; the $\mathfrak{b}$ ray, deep olive green; and the $\mathfrak{c}$ ray, greenish blue. The extinction angle of $\mathfrak{C}$ on c is about 18°. This mineral has been determined as pargasite, and the rock called pargasite eclogite. Along with the pargasite some glaucophane is found in rims around the pargasite, and small quantities of white mica, either paragonite or margarite, and a little sphene and rutile. Albite is found as veins in the rock, but not disseminated through it. Colorless epidote is present in considerable abundance in some of the slides. This rock grades over into a pargasite schist or gneiss, without garnet, but considerable albite with small crystals of lawsonite scattered through it.

The handsomest eclogite found in California is that of Hilton Gulch on Oak Ridge, about five miles east of Calaveras Valley. This

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[1] "Eclogites in California," *Jour. Geol.*, Vol. XII., 1904, p. 351.

is a massive rock made up of large dodekahedral red garnets more than a centimeter in diameter, thick prisms of black pargasite, thinner prisms of bluish carinthine, and plates of margarite, abundant blue prisms of glaucophane, a few small crystals of lawsonite, a little colorless epidote, and small crystals of rutile. This rock grades over into pargasite and margarite schists. Associated with it are ordinary glaucophane schists.

II. *Pseudodiabases, Pseudodiorites, and Greenstones.*

Becker[1] introduced the terms pseudodiorite and pseudodiabase for certain basic metamorphic rocks, which he thought were metamorphosed sediments, in which the minerals were all products of recrystallization. These rocks contain plagioclase, hornblende, glaucophane, epidote, and zoisite, and, rarely, white mica. They occur in the form of dykes and sills, and often show a distinct porphyritic texture, even the ophitic structure of diabase being often visible in thin slides. Chemically, too, they are identical with normal diabases or metabasalts. But Becker[2] says that they were originally ordinary sandstones made up of the residue of decomposed granites, and that the magnesia was introduced into them, and all the minerals formed by hydrothermal action. Becker thought that this same process even made the serpentines of the Coast Ranges, where the magnesia was in great excess. Palache,[3] however has demonstrated that the serpentines of the Coast Ranges are made from genuine peridotites, and the writer has examined slides of serpentine from many parts of California showing abundant olivines and orthorhombic pyroxenes as original constituents. It is now recognized by all petrographers in California that these serpentines were originally genuine igneous rocks, and that in the process of serpentinization nothing has been added to them but water. And Ransome[4] has shown that the pseudodiabases and pseudodiorites of Becker are merely basic igneous rocks, metabasalts or "fourchites,"

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[1] Mon. XIII., U. S. Geol. Survey, p. 94.

[2] Mon. XIII., U. S. Geol. Survey, p. 135.

[3] "The Lherzolite-serpentine and Associated Rocks of the Potrero, San Francisco," *Bull. Dept. Geol. Univ. of California,* Vol. I., pp. 161–179.

[4] "The Geology of Angel Island," *Bull. Dept. Geol. Univ. of California,* Vol. I., pp. 207 and 233.

in which some secondary mineralization has occurred. This fact too, is now so commonly recognized by petrographers familiar with the rocks of California that no further discussion is needed.

However, if the pseudodiabases do not show the remarkable phenomenon of the formation of a pseudo-igneous rock out of a sandstone, they do show something equally interesting,—the very beginning of the formation of a metamorphic out of an igneous rock. In some of the pseudodiabases and pseudodiorites all the original minerals and the original texture are still present, and the pyroxenes are only partly uralited to form hornblende. In others the readjustment has gone a little further, the feldspars have been partly changed to a saussuritic mass, and some secondary epidote and actinolite formed out of the pyroxene. In still others the feldspar has been entirely decomposed and a little soda taken from the albite molecule and added to the ferro-magnesian minerals, forming a little glaucophane or other soda-bearing amphiboles. In still others the original pyroxenes, even, have all been decomposed, and abundant epidote, diopside, glaucophane and white mica formed by the readjustment. There are all possible gradations from the little altered diabase to the entirely recrystallized glaucophane schists. The igneous rocks described as pseudodiorites, pseudodiabases, and fourchites are widely distributed in the Coast Ranges in the pre-Cretaceous, or Franciscan series, and have been the original materials out of which much of the basic glaucophane schists was made. Analyses of them are quoted below to show their perfect agreement with normal igneous rocks, and their unlikeness to any sediments except tuffs.

Widely distributed in the Coast Ranges occurs a group of rocks in which the metamorphism is more complete than in the pseudodiabases and pseudodiorites. The original feldspars are all decomposed, the pyroxenes either uralited or entirely recrystallized as other minerals, and secondary actinolite, epidote, zoisite, glaucophane and mica are formed. These rocks are massive, often occurring in the form of dykes, but they could hardly be called metadiabases, since some of them were certainly diorites, and others of them were probably made from the alteration of tuffs. They could hardly be called actinolite schists, since some of them contain little actinolite, and they are usually massive, although they grade over into glaucophane schists and eclogites. Since they are usually of a greenish color, from the actinolite, diopside, epidote and chlorite in them, they have commonly been called "greenstones," and although this term has been misused in many vague senses, the writer prefers to call them by that name, restricting them to holocrystalline metamorphic massive rocks, regardless of whether they were made out of diabases, diorites or tuffs. In them the metamorphism has gone far enough to destroy entirely the original feldspars and pyroxenes; and diopside, epidote, zoisite, actinolite, carinthine, glaucophane, white mica, sphene and even occasionally lawsonite have been formed by the redistribution of the chemical constituents, but no garnet has been formed in them. They grade over on the one side to the little altered pseudodiabases and pseudodiorites, and on the other into schists and eclogites. The formation of the schists has taken place under what Van Hise[1] calls mass-mechanical conditions, and the eclogites were formed under mass-static conditions. When the metamorphism was less intense than in either case, the greenstones were made, and when the alteration was only slight, affecting only those minerals that were unstable even at comparatively low pressure in the presence of water, the pseudodiorites and pseudodiabases were formed.

A fine exposure of typical greenstone is seen on the Hellman ranch about two miles west of Redwood. It is massive, but grades over into glaucophane schist. The minerals that compose it are short prisms of diopside, rather slender actinolites and glaucophanes, abundant pale epidote, and patches of titanite. Pyrite is disseminated through the mass. Where the rock grades over into schist, glaucophane is more abundant, and rectangular sections of lawsonite occur as porphyritic constituents. In places this rock might even be called a lawsonite gneiss, on account of the massive banded structure, and the abundance of lawsonite. No analysis was made of the Hellman ranch greenstone, but a similar rock was described by Becker[2] as a pseudodiorite composed of actinolite, white mica, rutile, zircon and titanite.

[1] Mon. XLVII., U. S. Geol. Survey, p. 698.

[2] Mon. XIII., U. S. Geol. Survey, p. 101.

ANALYSIS OF PSEUDODIORITE, "56 KNOXVILLE" (GREENSTONE).

	PER CENT.
SiO_2	50.44
Al_2O_3	8.18
Fe_2O_3	1.06
FeO	6.28
MgO	17.63
CaO	11.55
Na_2O	2.98
K_2O	0.50
TiO_2	
MnO	0.21
Cr_2O_3	0.48
H_2O (below 100° C.)	0.07
H_2O (above 100° C.)	0.92
Total	100.30

This rock is entirely recrystallized, but Melville's analysis of it shows that it is a normal diabase in composition.

Near the Hopkins reservoir, about three miles west of Redwood, is a massive greenstone, composed entirely of chlorite, pale epidote, and lawsonite, the latter occurring only in seams. It is associated with serpentine, and was probably originally a pyroxenite, for none of the minerals developed in it contain any quantity of soda. The original pyroxenes were probably rich in lime, and this has gone to the formation of lawsonite and epidote.

A quarter of a mile below the Searsville dam on San Francisquito Creek, Santa Clara County, is a very massive greenstone which seems to have been a dyke. It is somewhat banded, but not schistose. The groundmass is composed of feldspar which is mostly secondary albite, and through this are scattered long slender prisms of carinthine often with borders of glaucophane, rectangular sections of pale epidote and thin plates of white mica, probably paragonite. Chlorite occurs in irregular clusters, and quartz grains and patches of titanite are disseminated through the rock. The quartz may be original, but is more likely recrystallized either from original quartz, or from an excess of silica set free when the more basic epidote was formed. Very little glaucophane was formed, the albite portion of the feldspar molecule having crystallized out as albite, and the anorthite portion, instead of forming lawsonite, has taken up iron and formed epidote.

A rock similar to this in mineralogical constituents, but of a gneissic structure, occurs a half mile southeast of the Hopkins reservoir, three miles southwest of Redwood. In this the feldspar and quartz are still more abundant, and the glaucophane less so. Both rocks appear to be recrystallized diorites, although either may be a metamorphosed dioritic arkose, or an acid adesitic tuff. They would both come under Becker's division of pseudodiorites.

Ransome[1] has described from Angel Island, under the name of "fourchite," a basic dyke rock which is identical with Becker's pseudodiabase. It is only slightly altered, having a little zoisite, and films and needles of glaucophane, the latter developed at the expense of the augite. Ransome found no feldspar in the slides studied by him, but H. W. Turner[2] found in other slides made from this mass abundant fresh plagioclase, from which he concluded that the so-called fourchite was merely a phase of diabase. The analysis quoted below from Ransome's paper shows that the rock agrees with pseudodiabase and with normal diabases or metabasalts in chemical composition.

Analysis of "Fourchite" from Angel Island.

	Per Cent.
SiO_2	46.98
Al_2O_3	17.07
Fe_2O_3	1.85
FeO	7.02
MgO	8.29
CaO	12.15
Na_2O	2.54
K_2O	0.53
P_2O_5	0.09
H_2O (loss on ignition)	4.86
Total	101.38

III. *Diorite.*

While there are many rocks in the metamorphic series of the Coast Ranges that were probably originally diorites, the writer has observed only one case where the identification was absolutely certain. On Oak Ridge, about five miles east of Calaveras Valley, was found a massive quartz diorite, in which incipient metamorphism

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[1] *Bull. Dept. Geol. Univ. of California,* Vol. I., "Geology of Angel Island," pp. 200–207.

[2] "Notes on Some Igneous, Metamorphic and Sedimentary Rocks in the Coast Ranges of California," *Jour. Geol.,* Vol. VI. (1898), p. 483.

was observed. The original minerals were oligoclase, quartz, and a brown titaniferous hornblende, probably kataphorite. This rock has been slightly crushed, and the feldspars partly decomposed, setting free part of the albite and anorthite molecules. The albite did not crystallize out as such, but joined itself to the amphibole molecule, forming a glaucophane-like mineral, crossite, around the border of the undecomposed kataphorite. The crossite contains no titanium, and that constituent has taken up some of the lime to form titanite, and in other places has formed leucoxene. The anorthite molecule simply took up water and formed lawsonite, which occurs in and around the oligoclase in slender prisms. This rock is not a schist, nor even a gneiss, having probably been metamorphosed under mass-static conditions. Glaucophane schists were seen near the diorite, but they were apparently of more basic material. The agencies that made the schists also affected the diorite, but to a less extent.

On Spanish Peak, in Plumas County, at the northern end of the Sierra Nevada, the rocks have been much affected by dynamic action, quartzites having been changed to sericite schists, and conglomerates having been granulated until they have become fine-grained micaceous quartzites. At this place was observed a massive syenite that shows in thin sections the results of dynamic metamorphism. The rock was originally an albite, plagioclase hornblende mass, and most of the original minerals are intact. But the feldspars have become clouded through incipient decomposition, and the original brown hornblende has frayed out on the borders to a felt of asbestiform blue amphiboles that have the physical properties of crocidolite. Their pleochroism is intense, but lacks the violet shade that glaucophane and crossite always show. Their genesis is certainly the same as that of the crossite in the quartz diorite of Oak Ridge, from the addition of the albite molecule to the hornblende. This mineral was titaniferous, and the secondary crocidolite could not retain the titanium, which has then, as in the Oak Ridge diorite, separated out as titanite. This rock has not become a glaucophane schist, and none are known in the Sierra Nevada. Neither lawsonite nor epidote has been formed in the syenite, for the original feldspar was probably too acid to furnish the lime necessary for the formation of these minerals.

The chemical relations of the group of rocks and their transformation into glaucophane-bearing schists or gneisses are fully discussed below, in the section on "chemical readjustment in the metamorphism of the glaucophane-bearing rocks."

ANALYSIS OF QUARTZ DIORITE FROM OAK RIDGE, FIVE MILES EAST OF CALVERAS VALLEY, WITH SECONDARY CROSSITE AND LAWSONITE.
W. O. CLARK, ANALYST.

SiO_2	61.55
Al_2O_3	17.48
Fe_2O_3	1.49
FeO	3.50
MgO	3.00
CaO	3.12
Na_2O	8.47
K_2O	0.07
H_2O (— 110° C.)	0.12
H_2O (+ 110° C.)	1.21
TiO_2	0.28
Total	100.29

IV. *Glaucophane Schists.*

1. *Glaucophane garnet schists.*—The normal type of glaucophane schists, those with glaucophane, actinolite, pale epidote and garnet as the principal constituents, is widely distributed in the Coast Ranges. They are thin bedded, foliated and crumpled, showing intense crushing and shearing. All the minerals in them are the products of recrystallization, sometimes of igneous rocks, and sometimes of clay shales, possibly also of basic tuffs. The glaucophane is usually in the form of long blades and needles, the epidote in short thick-set prisms, and the garnet in dodekahedrons. Actinolite is an almost invariable companion of the glaucophane, and titanite is present in irregular patches, rarely in good crystals. This type of glaucophane schist is often associated with eclogite, and grades over into it, which shows that at least some of the holocrystalline basic metamorphics were made out of igneous rocks. Some of the garnet-glaucophane schists, however, are thought to have been made out of ferruginous clay shales, and Becker[1] says that a transition from the glaucophane schist to little altered shale was observed in

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[1] Mon. XIII., U. S. Geol. Survey, p. 102.

the rocks of Mt. Diablo; Nutter and Barber[1] observed the same thing in the metamorphic rocks near Healdsburg.

The garnetiferous glaucophane schists resemble very closely those of Syra in the Grecian Archipelago, described by Lüdecke.[2] The minerals that compose it, and the rocks with which they are associated are the same in both. But one analysis of the garnet-glaucophane schist of California has been published, that given by H. W. Turner[3] of a schist from Pine Cañon, Mt. Diablo. This analysis is quoted below, along with another of a similar schist from Tupper Rock, near Brandon, Oregon, published by H. S. Washington.[4]

ANALYSES OF GARNET GLAUCOPHANE SCHISTS.

	I.		II.
SiO_2	47.84		49.15
Al_2O_3	16.88		15.87
Fe_2O_3	4.99		4.10
FeO	5.56		7.58
MgO	7.89		7.53
CaO	11.15		9.06
Na_2O	3.20		3.59
K_2O	0.46		0.54
H_2O (above 105° C.)	1.81	(above 110° C.)	1.07
H_2O (below 105° C.)	0.17	(below 110° C.)	0.16
TiO_2			1.19
MnO	0.56		trace
P_2O_5	0.14		
Total	100.65		99.84

I. Garnet-glaucophane schist, Pine Cañon, Mt. Diablo, California, Melville, analyst; H. W. Turner, *Bull. Geol. Soc. Amer.*, Vol. II., 1891, p. 418.

II. Garnet-glaucophane schist, Tupper Rock, near Brandon, Oregon, H. S. Washington, analyst; A Chemical Study of the Glaucophane Schists, *Amer. Jour. Sci.*, IV. Ser., Vol. XI., 1901, p. 53.

A comparison of these analyses with those of Ransome's fourchite and Becker's pseudodiabase, shows the agreement to be perfect, and it can not be doubted that they were all originally the same thing.

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[1] "On Some Glaucophane and Associated Schists in the Coast Ranges of California," *Jour. Geol.*, Vol. X. (1902), p. 741.

[2] "Der Glaucophan und die Glaucophan führenden Gesteine der Insel Syra," *Zeit. Deutsch. Geol. Gesell.*, Bd. XXVIII. (1876).

[3] "The Geology of Mt. Diablo, etc.," *Bull. Geol. Soc. Amer.*, Vol. II., p. 418.

[4] "A Chemical Study of the Glaucophane Schists," *Amer. Jour. Sci.*, 4th Ser., Vol. XI. (1901), p. 53.

The localities where garnet-glaucophane schists have been found in California are too numerous to mention in detail, but the principal occurrences from which they have been studied are: Camp Meeker, Pine Flat, Junction School-house near Healdsburg, and Gurneville in Sonoma County; Tiburon Peninsula; San Pablo; North Berkeley; Mt. Diablo; Arroyo Hondo in Calaveras Valley; Oak Hill near San Jose; near San Luis Obispo; Santa Margarita ranch in San Diego County; Santa Catalina Island.

2. *Glaucophane-epidote Schists.*—This type of schist is even more common in the Coast Ranges than the garnet-glaucophane schist; it is almost invariably associated with the greenstones, and grades over into them. From the chemical nature and association of the rocks it would seem that the glaucophane-epidote schists are invariably the product of metamorphism of basic igneous rocks, or possibly from basic tuffs. All the minerals in them are the product of recrystallization, and if they were ever tuffaceous all evidence of the original fragmental nature is obliterated. Becker[1] described

ANALYSES OF EPIDOTE-GLAUCOPHANE SCHISTS.

	I.		II.		III.
SiO_2	49.68		46.07		46.39
Al_2O_3	13.60		15.35		17.34
Fe_2O_3	1.86		3.61		6.32
FeO	8.61		9.87		4.62
MgO	6.27		7.83		4.93
CaO	10.97		4.37		13.07
Na_2O	3.09		3.22		2.95
K_2O	0.12		2.68		0.25
H_2O (above 100° C.)	3.84	(above 110° C.)	4.25	(above 110° C.)	1.48
H_2O (100° C.)	0.00	(below 110° C.)	0.16	(below 110° C.)	0.08
TiO_2	1.31		1.63		0.85
MnO	0.04		trace		trace
P_2O_5	0.21				
C_2O					2.24
Total	99.60		100.09		100.52

I. Epidote-glaucophane schist (98 Sulphur Bank, California); Melville, analyst; Becker, Mon. XIII., U. S. Geol. Survey, p. 104.

II. Epidote-glaucophane schist, Winston's Bridge, Oregon; H. S. Washington, analyst, *Amer. Jour. Sci.,* IV. Ser., Vol. XI., p. 53.

III. Epidote-glaucophane schist, Kyperusa, Syra, Greece; H. S. Washington, analyst, *Amer. Jour. Sci.,* IV. Ser., Vol. XI., p. 39.

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[1] Mon. XIII., U. S. Geol. Survey, p. 104.

such a rock from Sulphur Bank, under the name of zoisite-glaucophane schist, but, as shown above, the so-called zoisite is probably a pale epidote. Besides this mineral Becker noted abundant glaucophane, and a little albite, white mica, quartz, and titanite. The analysis given of this rock by Becker is quoted below. A similar epidote-glaucophane schist has been described by Washington[1] from Winston's Bridge, near Roseburg, Oregon. The analysis given by Washington is quoted below, along with an analysis of an epidote-glaucophane schist from Syra in the Grecian Archipelago.

These rocks all agree in the abundance of epidote, the occasional presence of zoisite, and the absence of garnet. Chemically they agree exactly with the garnet-glaucophane schists, and with the pseudo-diabases and fourchite, and they were all doubtless formed from metabasalt.

The chief occurrences in California of epidote-glaucophane schist that have been studied in detail are: Sulphur Bank in Colusa County; Junction School-house near Healdsburg; North Berkeley; Arroyo Hondo in Calaveras Valley, San Juan Mine at Oak Hill near San Jose.

3. *Glaucophane-lawsonite schists.*—Associated with the ordinary types of schists at a few places in California are found rocks composed almost exclusively of lawsonite and glaucophane. The glaucophane in fine needles forms a compact groundmass in which are imbedded rectangular prisms of lawsonite. Sphene is scattered through the mass in irregular patches. Neither garnets nor epidotes occur in any quantity in this type of schists. Talc is a common constituent of the lawsonite-glaucophane schists, and is apparently not a product of weathering, but was made at the same time with the other minerals. The original rock was either a basic igneous rock rich in lime and magnesia, or a basic tuff. When glaucophane was formed out of primary hornblende or pyroxene the excess of silicate of magnesia crystallized out as talc. No analyses have been made of the lawsonite-glaucophane schists, but estimates, based on a study of numerous thin sections, show them to have the constitution of diabases or diabase tuffs. They have been studied in detail at Guerneville; on the Hellman ranch three miles west of Redwood;

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[1] *Amer. Jour. Sci.*, IV. Ser., Vol. XI., p. 53.

at the San Juan Mine, Oak Hill near San Jose; one mile south of the mouth of Coyote Canyon, Santa Clara County; and near Cayucas, in San Luis Obispo County. At all these places this rock is associated with epidote schists rather than with the garnetiferous type.

V. *Hornblende Schists.*

The hornblende schists do not make up independent rock-masses, but occur in subordinate quantities, associated with eclogites. They are of two principal types; those composed of the green prismatic actinolite, known as smaragdite, and chlorite, and those composed largely of pargasite. Both contain glaucophane in varying amounts, but the actinolite schists are characterized by the abundance of chlorite, while the pargasite schists contain abundant epidote and margarite. Carinthine occurs in both, especially where glaucophane is more abundant. As in all the basic schists of the glaucophane group, titanite is a common, and often an abundant constituent. The pargasite schists also often show secondary albite, but only in the rocks where little glaucophane is present. In both actinolite and pargasite schists glaucophane occurs as replacement rims around the more basic ferro-magnesian minerals. Both types grade over into eclogites and into basic mica schists.

The only analysis yet made of this group is that given by Becker[1] of a pseudodiorite. This analysis is quoted above, and shows the rock to have been originally a diabase. Some of the actinolite schists in the Coast Ranges are apparently still more basic, and were probably made out of the pyroxenites, or at any rate out of rocks very poor in feldspar. Slides of actinolite schist have been studied from Camp Meeker in Sonoma County; the Junction Schoolhouse near Healdsburg; Knoxville; Tiburon Peninsula near Reed's Station; near San Pablo; Arroyo Hondo at the north end of Calaveras Valley; Oak Hill near San Jose; a mile south of the mouth of Coyote Cañon, Santa Clara County; on Coyote Creek, six miles north of San Martin; near the Hopkins reservoir, about three miles southwest of Redwood; near Cayucas, in San Luis Obispo County. These schists also occur at many other places in the Coast Ranges where no detailed study of them has been made.

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[1] Mon. XIII., U. S. Geol. Survey, p. 101.

Pargasite schists have been studied only from the Arroyo Hondo near the north end of Calaveras Valley; and from Hilton Gulch on Oak Ridge, about four miles east of Calaveras Valley. At the latter place carinthine is even more abundant in the rocks than pargasite. At both localities the pargasite schist grades over into pargasite eclogite.

VI. *Mica Schists.*

There are two distinct types of mica schists in the glaucophane rocks of California, one associated with acid sediments, altered quartzites or cherts, and the other associated with basic schists, probably made out of diabases or diabase tuffs.

The acid schists are interbedded with layers rich in quartz, and probably represent clay-shale layers in original siliceous sediments. The mica in them is chiefly muscovite, although biotite sometimes occurs in considerable quantities. Garnet is almost invariably present in this type, and epidote almost never. No pyroxenes of any sort are found in them, and actinolite but rarely.

The basic mica schists are associated with eclogites and greenstones, and have no quartz. They are composed of paragonite or margarite, glaucophane, epidote, pargasite and carinthine. Where garnet is abundant in them they grade over into eclogite, and where garnet is lacking, and where mica is less abundant they grade over into greenstones. Neither the acid nor the basic mica schists make up independent rock-masses, but occur as bands in quartz glaucophane schists and in eclogites.

No chemical analyses have yet been made of either type, but slides have been studied of these rocks from many localities. The acid schists have been studied from Pine Flat, and the Junction School-house near Healdsburg in Sonoma County; from the Tiburon Peninsula, about one and a half miles north of Tiburon Ferry; from Belmont Hill, one mile southwest of Belmont; from Oak Hill near San Jose; from the Arroyo Hondo at the northern end of Calaveras Valley; from near Cayucas in San Luis Obispo County; and from a locality three miles west of Redwood, and a half mile southeast of the Hopkins reservoir. Biotite occurs in these as an abundant constituent only in the rock from near the Hopkins reservoir, and in that from the Tiburon Peninsula.

The basic mica schists have been studied from Camp Meeker, and from the Junction School-house in Sonoma County; from near Reed's Station on the Tiburon Peninsula; from Oak Hill near San Jose; from the Arroyo Hondo at the northern end of Calaveras Valley; and from Hilton Gulch on Oak Ridge, about five miles east of Calaveras Valley. At the two last localities the basic mica schists grade over into pargasite eclogites, and the micaceous mineral appears to be margarite.

VII. *Glaucophane Gneiss.*

1. *Feldspathic Gneisses.*—The feldspathic gneisses of the glaucophane series are of two different types, the one with abundant orthoclase, and the other with albite as the dominant mineral. Neither type forms independent rock-masses, but both occur as bands in the ordinary glaucophane schists.

Orthoclase gneiss is known from but two places in the glaucophane rocks. The best example is that from a locality near Melitta in Sonoma County, California. This rock, which was found only in float, near a glaucophane schist outcrop, is massive and banded, with long prisms of glaucophane and actinolite in a groundmass of orthoclase and quartz. Small dodekahedrons of pink garnet and thin plates of muscovite are visible in the hand specimen. Thin slides show also abundant titanite in perfect rhombic sections. The glaucophane appears to have been formed after the actinolite, for it replaces the latter around the edges of the crystals. Part of the actinolite has a bluish color, suggesting that it may be carinthine. The glaucophane in this rock has the normal orientation of the axes of elasticity, but has an extremely narrow angle of the optical axes. Some of the bands are composed entirely of quartz with innumerable minute crystals of pink garnet and a few radial tufts of glaucophane. This rock was probably made out of an arkose, for no normal sediments could have the chemical nature necessary for its genesis, and it is hardly likely that an igneous rock would be interbanded with siliceous layers. It may, however, have been made from thin dykes of a granitic nature that were intrusive in the siliceous sediments.

Becker[1] has described from Sulphur Bank in Colusa County (No. 31, Sulphur Bank), a rock similar to that from Melitta. The

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[1] Mon. XIII., U. S. Geol. Survey, p. 102.

Sulphur Bank gneiss consists of quartz and orthoclase as a groundmass, with large prisms of glaucophane, small plates of biotite and muscovite, small garnets, prisms of zoisite, and abundant titanite. Ilmenite and zircon were also observed in small quantities. Some plagioclase was also seen, probably albite, although it was not definitely determined.

An arkose slightly altered, which by further recrystallization would have made a feldspathic gneiss similar to those described above, has been described by Becker[1] from the Coast Ranges. This rock, No. 13, Sulphur Bank, Colusa County, is composed of quartz, orthoclase, plagioclase (albite?). No glaucophane has been developed in this rock, and Becker does not state what the ferro-magnesian mineral was, but it is presumably hornblende, since this would be present in either a granitic or a dioritic arkose. A chemical analysis of this rock by Melville is given by Becker along with his description, and this is quoted below.

ANALYSIS OF ARKOSE, NO. 13, SULPHUR BANK.

SiO_2	68.50
Al_2O_3	12.82
Fe_2O_3	1.29
FeO	3.37
MgO	2.27
CaO	1.82
Na_2O	6.03
K_2O	1.26
H_2O (above 100° C.)	2.11
H_2O (below 100° C.)	0.28
TiO_2	0.60
P_2O_5	0.16
MnO	0.02
Total	100.47

Albite gneisses are described from but two places in the glaucophane-bearing series. That described by Ransome[2] from Angel Island consists chiefly of albite with some needles of glaucophane, plates of biotite, numerous very small crystals of garnet, and irregular patches of titanite. Quartz is entirely lacking in this rock, which is noteworthy, since the silica is so high.

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[1] *Bull. Dept. Geol. Univ. of California*, Vol. I., p. 212.

[2] Mon. XIII., U. S. Geol. Survey, p. 92.

A similar rock has been described by Palache[1] from the Contra Costa Hills, three miles north of Berkeley. It is composed of a groundmass of albite, with numerous prisms of crossite, and some actinolite in parallel growth with the crossite. There are also a few grains of zircon and titanite.

Rosenbusch[2] has calculated the composition of the albite-crossite gneiss, and his estimate is quoted below.

	I.		II.
SiO_2	65.2		67.53
Al_2O_3	15.8		18.57
Fe_2O_3	2.7		1.13
FeO	2.4		0.08
MgO	2.4		0.24
CaO	0.6		0.55
Na_2O	10.8		11.50
K_2O	0.1		0.10
H_2O		(above 100° C.)	0.31
H_2O		(below 100° C.)	0.15
P_2O_5			0.11
TiO_2			0.07
Total	100.0		100.34

I. Estimated analysis of albite-crossite gneiss, Berkeley, Rosenbusch, *loc. cit.*, p. 712.

II. Analysis of soda syenite porphyry (No. 1521 S. N.), from the Sierra Nevada; Stokes, analyst; H. W. Turner, 17th An. Rept. U. S. Geol. Survey, pt. I.; p. 727.

Turner says[3] that the soda syenite porphyry dykes of the Sierra Nevada are very like the albite-crossite gneiss in composition, and that when somewhat altered they often show a blue hornblende as a secondary mineral. He suggests that the feldspathic glaucophane gneisses of the Coast Ranges may be altered dykes. Turner does not state that the particular specimen of soda syenite porphyry selected for analysis contained the blue amphibole mentioned above, but this rock is probably identical with the albite syenite of Spanish Peak, Plumas County, California, which shows considerable quantities of a secondary blue amphibole, probably crocidolite.

2. *Quartz-glaucophane Gneiss.*—Siliceous glaucophane gneiss is very abundant in the Coast Ranges, often showing large masses. The rock is usually compact and massive, with gneissic banding,

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[1] *Bull. Dept. Geol. Univ. of California,* Vol. I., p. 182.

[2] *Sitzungsberichte d. K. Preuss. Akad. Wiss.,* 1898, p. 712.

[3] 17th An. Rept. U. S. Geol. Survey, Part I., p. 727.

but grades over into thin bedded acid mica schists. The constituent minerals are quartz, glaucophane, pink garnet, muscovite, actinolite and titanite, sometimes with a little feldspar. These rocks are undoubtedly siliceous sediments, either quartzite or chert, but everything in them is recrystallized, so that it is usually impossible to distinguish the product of the alteration of the quartzite from that of a chert.

Washington[1] has described from Four Mile Creek in Oregon a quartz glaucophane schist or gneiss, consisting of quartz, glaucophane, pink garnet, muscovite, and a little chlorite. The analysis given by Washington is quoted below.

A similar rock, but more massive and gneissic, occurs near Pine Flat, Sonoma County, on the Foss road from Calistoga to the Geysers, and near the house of J. Mueller. This locality is about two miles from the Geysers, and four miles from the Eureka mine. The rock is a massive banded gneiss, with abundant quartz, and muscovite, many long prisms of glaucophane, very small pink garnets, and a little actinolite. The glaucophane and actinolite are badly shattered, showing that at least a part of the crushing took place after they were formed.

A similar rock to that from near Pine Flat was studied by Nutter and Barber from the Junction School-house near Healdsburg.

On the Tiburon Peninsula, a mile and a half northeast of the Tiburon ferry is found a massive quartz gneiss. The rock contains abundant quartz, muscovite and biotite, numerous very small pink garnets, and a little actinolite, and small crystals and aggregates of titanite. There are numerous prisms that resemble glaucophane in appearance, but the pleochroism is more intense and the plane of the optical axes transverse, hence the mineral is the variety crossite. All the crystals in the rock are shattered, displaced and healed with secondary silica. Some of the clear glassy minerals may be albite, but none were found giving a biaxial figure.

Ransome[2] has described from Angel Island a quartz glaucophane gneiss very similar to that from the Tiburon Peninsula, and the two localities are not over two miles apart. But the Angel Island

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[1] *Amer. Jour. Sci.*, IV. Ser., Vol. XI. (1901), p. 53.

[2] *Bull. Dept. Geol. Univ. of California*, Vol. I., p. 215.

gneiss has normal glaucophane instead of crossite. Ransome[1] has also published an analysis of a quartz glaucophane schist or gneiss from Angel Island, but it is impossible to tell from the context just which rock was analyzed. The analysis is quoted below.

A massive quartz glaucophane gneiss has been observed about a mile and a half southwest of Belmont. This rock is almost exactly like that described from near Pine Flat, except that no actinolite was found in the slides, and that the glaucophane is mostly in radial aggregates.

Similar massive quartz glaucophane gneisses have been studied from the Arroyo Hondo at the north end of Calaveras Valley; from Oak Hill near San Jose; and from near Cayucas in San Luis Obispo County. In the rock from the latter locality striated feldspar, probably albite, is rather common. Washington[2] has described a quartz glaucophane gneiss from Little Harbor, on Catalina Island. The rock is fissile, composed of a groundmass of granular quartz, with numerous crystals and needles of glaucophane, and a few zircons and epidotes. No garnets are mentioned as occurring in this rock, and their absence is unusual in quartz glaucophane gneiss.

ANALYSIS OF QUARTZ GLAUCOPHANE GNEISS.

	I.		II.		III.
SiO_2	82.53		80.21		74.48
Al_2O_3	6.88		7.99		9.15
Fe_2O_3	0.59		3.35		1.41
FeO	4.11				4.12
MgO	1.86		1.54		3.04
CaO	0.68		1.10		2.84
Na_2O	1.21		5.97		2.24
K_2O	1.24		0.22		0.43
H_2O (110° C.)	1.35	loss on ig.	0.74	(+110° C.)	2.06
H_2O (110° C.)	0.07			(−110° C.)	0.08
MnO	trace				
Total	100.52		101.12		99.85

I. Quartz glaucophane schist, Four Mile Creek, Oregon; Washington, analyst; *Amer. Jour. Sci.*, 4th Ser., Vol. XI., p. 53.

II. Quartz glaucophane schist, Angel Island, California; Ransome, analyst; *Bull. Dept. Geol. Univ. of California*, Vol. I., 231.

III. Quartz glaucophane schist, Little Harbor, Catalina Island, California; Washington, analyst; *Amer. Jour. Sci.*, 4th Ser., Vol. XI., p. 48.

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[1] *Op. cit.*, p. 231.

[2] *Amer. Jour. Sci.*, IV. Ser., Vol. XI. (1901), p. 48.

The slides from all the localities mentioned are so similar that the conclusion is unavoidable that all these quartz glaucophane gneisses were made from the same material, either a chert, or a very acid sandstone. The rock can hardly be called a quartzite, for while it is composed largely of quartz, there is no proof that it was made of rolled quartz grains.

3. *Lawsonite Gneiss.*—Lawsonite has long been known as an important mineral in the glaucophane schists of California, but has only recently been found to be a dominant mineral in some of these rocks. The rocks in which it is found as a dominant mineral are massive and gneissic, and although they contain no feldspars their structure is such that they must be called gneisses. They are known at present at only three localities. The first found is at Guerneville in Sonoma County, where bands of massive gneiss are apparently interbedded with lawsonite glaucophane schist. The rock is composed of crystals of glaucophane imbedded in a groundmass of lawsonite, the latter mineral showing few crystal outlines.

The most interesting lawsonite gneiss yet found occurs about three miles southwest of Redwood. It is massive and banded, showing a groundmass of lawsonite crystals, with a thick felt of compact prisms of glaucophane. Titanite is scattered through the rock in irregular patches, and the whole is seamed with quartz veins. Some layers show many small garnets. No trace of the original feldspars and hornblendes or pyroxenes is left, every mineral in it being secondary. The rock is composed of approximately one-fourth lawsonite, one-third glaucophane, one-third quartz, and one-twelfth garnet, titanite and mica. The total silica in the rock is 65.91 per cent., and originally there could have been very little free silica. The original minerals were probably oligoclase, hornblende and a little quartz, but in recrystallization more basic minerals were formed, setting free a large amount of silica which crystallized out in quartz veins.

An analysis of this rock was made by Mr. W. O. Clark, assistant in mineralogy at Stanford University, and this is quoted below.

The similarity of the analyses of the completely recrystallized lawsonite gneiss and of the slightly altered quartz diorite shows that the original material was the same in both cases.

	I.	II.
SiO_2	65.91	61.55
Al_2O_3	11.62	17.28
Fe_2O_3	2.21	1.49
FeO	5.30	3.50
MgO	1.92	3.00
CaO	5.89	3.12
Na_2O	1.95	8.47
K_2O	0.04	0.07
H_2O (100° C.)	0.28	0.12
H_2O (red heat)	4.38	1.21
MnO	trace	trace
TiO_2	0.17	0.28
Total	99.67	100.12

I. Lawsonite glaucophane gneiss, three miles west of Redwood; W. O. Clark, analyst.

II. Diorite with secondary crossite and lawsonite, Oak Ridge, five miles east of Calaveras Valley; W. O. Clark, analyst.

A lawsonite glaucophane gneiss very similar to that above described occurs a mile south of the mouth of Coyote Canyon, Santa Clara County, associated with lawsonite glaucophane schist. No chemical analysis was made, but the slides show the same minerals in about the same proportions. The original rock was probably a diorite, or at least a medium basic rock rich in lime.

GENETIC CLASSIFICATION OF THE GLAUCOPHANE ROCKS OF CALIFORNIA.

I. *Certainly Original Sediments.*

A. Quartz glaucophane schists, gneisses and altered cherts, at Pine Flat; Junction School-house near Healdsburg; Tiburon Peninsula; Angel Island; one mile west of Belmont; Arroyo Hondo at the northern end of Calaveras Valley; near Cayucas in San Luis Obispo County; Oak Hill near San Jose; and Catalina Island.

B. Altered tuffs and arkose; lawsonite and epidote glaucophane schists at the San Juan Mine, Oak Hill near San Jose, apparently derived from a tuff which occurs unaltered nearby; epidote glaucophane schists of North Berkeley; orthoclase glaucophane gneiss from Melitta near Santa Rosa, interbanded with quartzites.

C. Altered clay shales; basic glaucophane schists grading over into unaltered shales, Junction School-house near Healdsburg; basic bands of glaucophane schist interbedded with quartz glaucophane schists and gneisses, Tiburon Peninsula; Angel Island; Pine Flat; one mile west of Belmont; Arroyo Hondo at the northern end of Calaveras Valley.

II. *Certainly Original Igneous Rocks.*

A. *Basic Igneous Rocks.*—Pseudodiabase; pseudodiorite; some greenstones where they grade over into diabasic dykes; "fourchites," where they have been partly altered to glaucophane-bearing rocks; metabasalts of the Roseburg and Coos Bay Folios, Oregon. These rocks all occur as dykes or sills, possible sometimes as old surface flows. In some of the pseudodiorites and pseudodiabases the original minerals and the original structure are so well preserved that no one could doubt their igneous origin. And there is a complete gradation from these through rocks where the original structure is preserved, while the original minerals are recrystallized, into schists and greenstones where both original minerals and original structure are lost. This group grades over into basic epidote glaucophane schists and eclogites. The occasional association of these basic rocks with the acid schists can easily be explained by the hypothesis that the original clay shales and sandstones were invaded by dykes and sills of diabase, and that the whole complex, igneous as well as sedimentary rocks, was altered together.

B. *Diorite.*—The slightly altered quartz diorite of Oak Ridge five miles east of Calaveras Valley, shows the beginning of alteration in the formation of secondary crossite, lawsonite and titanite. The pseudodiorite on San Francisquito Creek, a quarter of a mile below the Searsville dam, Santa Clara County, shows a step further in metamorphism, in that the igneous nature of the rocks is still apparent, while nearly all the minerals in the rock are products of recrystallization. The lawsonite gneiss three miles southwest of Redwood shows the final product from such a rock, where the structure and the minerals are all secondary, and the chemical nature is unaltered.

A similar beginning of metamorphism has been observed in a syenite from Spanish Peak, Plumas County, where dynamic action has caused a partial recrystallization, the plagioclases having given up some soda, which has joined the amphibole molecule, forming secondary needles of a blue glaucophane-like mineral, probably crocidolite. Soda syenite porphyry dykes are said by Mr. H. W. Turner[1] to show a secondary blue amphibole where some metamorphism has taken place, and he thinks that the albite glaucophane schists were probably made out of such material.

III. *Doubtful Original Sediments.*

Under this head must be classed many of the basic glaucophane rocks that are interbanded with more acid rocks of sedimentary origin. They were probably originally tuffs, but in many cases they may have been flows or sills. These appear as schists and greenstones, but their field relations often give no indication as to their origin, and they are so thoroughly recrystallized that their original fragmental nature is lost.

IV. *Doubtful Original Igneous Rocks.*

In the Coast Ranges there are many masses of holocrystalline rocks that resemble igneous rocks in their structural relations, but are made up entirely of secondary minerals. Where they do not grade over into undoubted igneous rocks their origin must remain in doubt, for a chemical analysis can not show whether they were originally fused magmas or fragmental tuffs. These rocks are eclogites, greenstones and basic schists, grading over into each other, and with the chemical constitution of gabbros or diabases, more rarely of diorites. Some of the more basic eclogites may even have been made out of pyroxenite. The eclogites are massive or gneissic, composed of garnet, omphacite, some amphibole, and epidote. The greenstones are essentially omphacite actinolite epidote rocks, and the schists associated with them are usually epidote glaucophane schists with either paragonite or margarite. No eclogites have been found that grade over into dioritic rocks, or have the chemical composition of that group, but the lawsonite schists and gneisses with their high percentage of lime and comparatively low iron and magnesia suggest a derivation from diorites.

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[1] 17th An. Rept. U. S. Geol. Survey, Part I., p. 727.

The albite schists and gneisses, described above, at least have the composition of soda syenites, and must have been made from such rocks, or from tuffs or arkose derived from them.

CHEMICAL READJUSTMENT IN THE METAMORHISM OF THE GLAUCOPHANE-BEARING ROCKS.

Igneous Rocks.—The original igneous rocks and the metamorphic glaucophane-bearing rocks made out of them have the same chemical constitution, for there has been little addition. But the two groups invariably differ mineralogically, for even when the same mineral species occurs in the two groups, it is represented by different varieties, one a characteristic product of crystallization out of a fused magma, and the other the product of solutions under pressure. When the metamorphism has not been intense, some of the original minerals have persisted, but in most cases every mineral in the glaucophane rocks is a product of recrystallization.

The feldspars, hornblendes and pyroxenes have been the chief factors in the readjustment. The orthorhombic pyroxenes have invariably decomposed, none being found in any of the metamorphic rocks of the glaucophane series. The monoclinic pyroxenes have fared little better, having usually been uralited to form some variety of amphibole, with the probable addition of soda in most cases. More rarely they have formed diopside, and in one instance secondary diallage has been observed, in the altered glaucophane-bearing gabbro of the Junction School-house near Healdsburg.

The original hornblendes and pyroxenes of the igneous rocks of the Coast Ranges of California have often been titaniferous, but the secondary minerals resulting from them by metamorphism generally contain no titanium. Hence, when glaucophane, epidote zoisite, or garnet was formed there was a certain amount of titanic oxide set free, which either crystallized out as rutile, or took up lime and formed sphene or titanite. Fringes of titanium minerals are common around the original titaniferous hornblendes or pyroxenes where the readjustment has begun.

In the feldspars the readjustment has been still more fundamental, for the material of which they were composed has been distributed in the formation of entirely different minerals. The original feldspars were mostly orthoclase, andesine, oligoclase, and labradorite. Of these andesine is unknown in the glaucophane rocks of California, labradorite as a secondary mineral in only one case,—in veins in the altered glaucophane-bearing gabbro of the Junction School-house near Healdsburg. Orthoclase is very rare, although it occurs in considerable abundance in some gneissic rocks. On the other hand, albite is very rare as an original constituent of the igneous rocks of the Coast Ranges, but is a very common companion of glaucophane in the metamorphics. The explanation of this is simple. The lime-soda feldspars are formed readily in fusions, but not readily in the wet way. On the other hand, it has been demonstrated by experiment that the alkali-feldspars, orthoclase and albite, are formed readily in superheated solutions under pressure, and not readily in magmatic fusion. Since the plagioclase molecule is unstable in the presence of water in hydrothermal metamorphism, oligoclase, andesine and labradorite are the first minerals to be broken up. The anorthite portion of the lime-soda feldspar is set free to form lime-rich minerals, either by taking up water to form lawsonite, or by taking up iron and alumina to form garnet and epidote. The albite portion of the molecule either joins with the ferro-magnesian minerals to form glaucophane or other soda-bearing hornblendes, or, if it is present in excess, crystallizes out as albite. This accounts for the fact that albite is the commonest feldspar in metamorphic rocks, not only in California, but all over the world, and that lawsonite is a common companion of glaucophane, in Europe as well as in California.

In the Coast Ranges are found considerable quantities of very basic actinolite schists with a small amount of glaucophane. These rocks appear to have been made out of pyroxenites, in which soda was very scarce, for olivine, augite and orthorhombic pyroxenes contain no soda. In addition to actinolite and glaucophane, the rock contains large quantities of chlorite not made at the expense of the actinolite, which is perfectly fresh. A mixture of about two-thirds actinolite and one-third chlorite would give the composition of the original pyroxenes; for when the basic ferro-magnesian mineral is broken down to form actinolite there is left a more basic portion rich in magnesia to form chlorite by the addition of water. Since the original rock contained soda only in extremely small quantities the blue hornblendes are developed only rarely in the secondary rocks resulting from them. When soda was more abundant some of it went to the formation of paragonite, and when there was an excess of lime in the pyroxenes it went largely into margarite. Both of these minerals are quite common in the basic schists and eclogites.

In the medium-basic rocks, gabbros and diorites, which form the bulk of the older igneous rocks of the Coast Ranges the products of metamorphism are much more varied. The first step is simply the formation of paramorphs of hornblende after pyroxene, uralitization, without any fundamental chemical readjustment. But when the lime-soda feldspars were partly broken down some of the soda went into the amphibole molecule and actinolite was formed. Since this mineral contains no titanium, while the original hornblende and pyroxene were often titaniferous, titanic oxide was set free, and either crystallized out as rutile, or combined with lime to form titanite. These are both very abundant in the basic metamorphics of this group.

The silica, lime, and alumina set free from the feldspars often took up iron from the ferro-magnesian minerals to form more basic silicate, usually epidote, and sometimes zoisite when the iron was deficient. When the feldspars were more broken down and more soda set free, or more soda added from outside sources, bluish pargasite was formed, accompanied by large quantities of white epidote. In other cases, but usually in the more acid diorites, the bluish green soda-bearing hornblende, carinthine, has been formed in considerable quantities. In the pargasite-bearing rocks there was more of the albite molecule set free than was needed for the formation of pargasite, and this has crystallized out in veins of very pure albite.

When still more soda was released from the feldspars glaucophane was formed, while the excess of lime went into garnet, more rarely into lawsonite, accompanied as before by white epidote. In some of the basic eclogites diopside is common, and in this case little epidote was formed, the excess of lime having gone into garnet.

When the rocks are low in silica and rich in lime, lawsonite has often been formed in great abundance by the plagioclase giving up its soda to form glaucophane, and setting free the anorthite molecule, $CaAl_2Si_2O_8$ which by taking up water becomes lawsonite, $CaAl_2Si_2O_8 + 2H_2O$. When this has taken place little epidote was formed, and usually no garnets, although in the rock from which lawsonite was first described both garnet and epidote are abundant.

As evidence that the metamorphism that produced the uralite and pargasite rocks was not so fundamental as that which formed the glaucophane, it may be stated that the pargasite rocks are more massive, and not true schists, and that in these rocks the glaucophane appears as replacements or rims around the pargasite. Most of the schistosity has been produced after the glaucophane was formed, for even that mineral is usually bent and fractured, the cracks being healed with secondary silica, forming quartz veins which occur as innumerable small seams through these rocks.

No original micas of any sort are found in the gabbros and medium basic diorites of the Coast Ranges, and yet the eclogites, glaucophane schists and gneisses are full of white mica, chiefly the sodium-bearing paragonite and the lime-bearing margarite. These are formed at the expense of the plagioclases, either by their giving up silica and alkali-silicate, or by their taking up alumina from aluminous silicates. The reactions given below show how the micas may have been formed.

$3KAlSi_3O_8$ (orthoclase) $+ H_2O = K_2SiO_3 + 5SiO_2$
$+ H_2KAl_3(SiO_4)_3$ (muscovite).

$KAlSi_3O_8 + Al_2O_3 + H_2O = H_2KAl_3(SiO_4)_3$ (muscovite).

$3NaAlSi_3O_8$ (albite) $+ H_2O = Na_2SiO_3 + 5SiO_2$
$+ H_2NaAl_3(SiO_4)_3$ (paragonite).

$NaAlSi_3O_8 + Al_2O_3 + H_2O = H_2NaAl_3(SiO_4)_3$ (paragonite).

$2CaAl_2Si_2O_8$ (anorthite) $+ H_2O = CaSiO_3 + SiO_2$
$+ H_2Al_4Si_2O_{12}$ (margarite).

$CaAl_2Si_2O_8$ (anorthite) $+ Al_2O_3 + H_2O$
$= H_2CaAl_4Si_2O_{12}$ (margarite).

Neither albite nor anorthite occurs in the original igneous rocks from which these metamorphics were made, but the albite and the anorthite molecule are both abundant in the plagioclases which made up a large portion of those rocks. All three of these micas are known as incrustations or reaction zones on corundum, having been formed by the adjacent feldspars taking up alumina directly from the corundum. And muscovite, paragonite and margarite are all common as kelyphite rings or reaction zones around garnets, accompanied by chlorite. In this case the feldspar molecule has robbed the garnet of part of its alumina, forming mica, and the residue, richer in magnesia, has taken up water to form chlorite. This phenomenon is especially common in the eclogites.

The potash-rich igneous rocks have given rise to metamorphics with muscovite, the soda-rich original rocks have formed metamorphics with paragonite, and the lime-rich igneous rocks have gone over into schists or eclogites with margarite. Naturally the more acid rocks usually have more muscovite, and the more basic ones more paragonite or margarite.

Cyanite has been described as abundant in the European glaucophane schists and eclogites, and its possible mode of formation is shown by the reactions:

$$2NaAlSi_3O_8 \text{ (albite)} = 4SiO_2 + Na_2SiO_3 + Al_2SiO_5 \text{ (cyanite)}.$$

$$CaAl_2Si_2O_8 \text{ (anorthite)} = CaSiO_3 \text{ (wollastonite)} + Al_2SiO_5 \text{ (cyanite)}.$$

But wollastonite is not known to occur in the glaucophane rocks anywhere, and cyanite has been found in these rocks in California only in one eclogite, the wollastonite and cyanite molecules having joined themselves to others, forming more complex silicates. Sillimanite, which has the same composition as cyanite, is not uncommon in the siliceous rocks of the glaucophane series.

The medium basic plagioclases are common in the diabases, gabbros and basic diorites of the Coast Ranges, and from these have come the bulk of the glaucophane-bearing schists, greenstones and eclogites. When the readjustment is only partial, we have massive rocks composed of partly decomposed feldspar (saussurite), uralite, and carinthine, with a little sphene and epidote or zoisite. These rocks are the pseudodiabases and pseudodiorites of Becker. When the disintegration of the plagioclase molecule is more complete, part of the anorthite molecule takes up water and crystallizes out as lawsonite, while a portion of the albite molecule crystallizes out as albite in veins or patches, and the rest is taken up by the ferromagnesian minerals to form glaucophane, or when soda is deficient, carinthine or pargasite. In the aluminous calcium augite there was more lime than was needed for the formation of glaucophane, and epidote was produced in considerable quantities, but when the original minerals were olivine or orthorhombic pyroxene there was no excess of lime. Thus in the metamorphics made from rocks of the nature of peridotites epidote and garnet are rare, while they are plentiful in those made from diabases and diorites.

When the amount of iron was higher than was needed for the glaucophane, and the lime too low for lawsonite, the iron-alumina garnet was formed, as in the black pargasite, and green actinolite and omphacite eclogites. And when the iron, alumina, lime and soda were all abundant, then glaucophane, red garnet, lawsonite and epidote were all formed together, as in the Tiburon eclogite. All this, of course, refers only to the predominating minerals, since in any of the metamorphic rocks of the basic series small quantities of either glaucophane, lawsonite, garnet, or epidote may occur.

Near the Searsville dam in Santa Clara County is a diorite in which the metamorphism is fundamental, every mineral in the rock being secondary, and yet the rock is still recognizable as a diorite. It is composed of carinthine, epidote, white mica, probably paragonite, albite, and abundant quartz, with titanite and pyrite scattered through the mass. A little glaucophane appears as replacements of the carinthine. The original plagioclases have been almost entirely decomposed, and the hornblende or pyroxene remade into carinthine by losing titanium and taking up part of the albite molecule from the plagioclase. No lawsonite nor garnet was made out of the anorthite molecule, but abundant epidote instead. This formation of more basic minerals has set silica free, and this has crystallized out as quartz in veins and patches all through the rock. As there was too much albite in the original feldspar for the needs of the carinthine and glaucophane, albite has crystallized out in veins, and the residue of the molecule took up alumina and formed paragonite. The crystals of pyrite in the mass tell of the nature of the solutions that aided in the readjustment.

On Oak Ridge in Santa Clara County, about five miles east of

Calaveras Valley, is a massive quartz diorite, in which the metamorphism has only just begun, and all the original minerals may be recognized. The rock is composed of original quartz, oligoclase, and brown hornblende (probably kataphorite), with a little secondary lawsonite and glaucophane (crossite). The readjustment consists in the partial breaking up of some of the oligoclase, and the addition of some of the albite molecule to the borders of the brown hornblende to form a fringe of glaucophane (crossite). Since the glaucophane contains no titanium, this constituent crystallized out around the borders of the parental mineral as titanite and leucoxene. Most of the oligoclases are still perfectly fresh, and those partially decomposed still show their outlines and twinning bands, giving evidence of decomposition in cloudiness due to kaolinization. The little lime they contained went to formation of lawsonite, which is present in minute crystals, with the characteristic optical properties of that mineral.

The original oligoclase, from its extinction angles on the twinning bands, has the composition of Ab_3An_1, with 63 per cent. silica. The small quantity of the anorthite molecule set free simply took up water and formed lawsonite, $CaAl_2Si_2O_8 + 2H_2O$. The albite molecule was disintegrated, giving sodium silicate to the hornblende to form glaucophane, and setting free kaolin and silica; $2NaAlSi_3O_8 + 2H_2O = Na_2SiO_3 + 3SiO_2 + H_2Al_2Si_2O_8 + H_2O$ (kaolin). In this none of the sodium silicate and little of the silica was left free, but went into combination with the hornblende on the borders to raise the percentage of silica and soda to that necessary to form glaucophane. The rock is composed of approximately four parts oligoclase, two of hornblende and one of quartz. This has nearly the same composition as that of the albite-crossite gneiss of Berkeley, and the albite-glaucophane gneiss of Angel Island. The analysis of the quartz diorite with secondary crossite and lawsonite is given below.

About three miles west of Redwood is a massive gneissic rock composed of approximately one-fourth lawsonite, one-third glaucophane, one-third silica (quartz), and the remaining one-twelfth of garnet, sphene and white mica. Every mineral enumerated above is clearly secondary, and yet the analysis given below is that of a quartz diorite. Even the quartz, which makes up about one-third of the rock, is secondary, for there could not have been any such quantity of free quartz in the original rock with only 66 per cent. of silica. The original minerals were probably oligoclase, hornblende, and quartz, and the addition of 4 per cent. water has lowered the silica from 68 per cent. to 65.9 per cent. The original hornblende was probably titaniferous, for numerous small aggregates of titanite are visible in the slides. The original rock was probably a quartz diorite similar to that of Oak Ridge described above, but with the readjustment complete. A chemical analysis of this rock by Mr. W. O. Clark, assistant in geology at Stanford University, gave the following results:

	Lawsonite Gneiss. I.	Diorite with Crossite. II.
SiO_2	65.91	61.55
Al_2O_3	11.62	17.28
Fe_2O_3	2.21	1.49
FeO	5.30	3.50
MgO	1.92	3.00
CaO	5.89	3.12
Na_2O	1.95	8.47
K_2O	0.04	0.07
H_2O (100° C.)	0.28	0.12
H_2O (above 110° C.)	4.38	1.21
MnO	trace	trace
TiO_2	0.17	0.28
Total	99.67	100.12

I. Lawsonite gneiss, three miles west of Redwood, W. O. Clark, analyst.

II. Quartz diorite with some secondary crossite and lawsonite, Oak Ridge, five miles east of Calaveras Valley, W. O. Clark, analyst.

A comparison of these two analyses shows the lawsonite gneiss to have been more like an ordinary quartz diorite in composition; while the diorite with secondary crossite is somewhat allied chemically to the soda syenite porphyries.

About one mile south of the mouth of Coyote Cañon, Santa Clara County, is a mass composed of glaucophane schist, lawsonite-garnet-glaucophane gneiss, and lawsonite-glaucophane schist. In the latter phase both lawsonite and glaucophane are perfectly fresh, but there is a large amount of talc formed, which was clearly not made at the expense of any mineral now visible in the rock. The original rock must have been richer in magnesia than the needs of the glaucophane demanded, and as no garnets or epidotes were formed the excess of magnesium silicate crystallized out as talc, in this case a primary constituent of the rock, and not a weathering product. This could only happen where the metamorphism was not very intense, and the absence of garnets would show that this was the case. No analyses have been made of this rock, but an estimate made from a study of numerous slides shows that the original material must have been a basic igneous rock or a basic tuff.

Sedimentary Rocks.—The altered sediments containing glaucophane are: (1) Basic tuffs, probably of the character of diabase; (2) arkose, of the character of granites or diorites; (3) sandstones, with varying proportions of impure clay; (4) clay shales, with varying proportions of sand and other impurities.

The basic tuffs consist largely of augitic and feldspathic fragments, and these recrystallize very readily. From this material have been formed schists composed largely of glaucophane and epidote, with accessory titanite, garnet and zoisite. The reactions here are the same as in the recrystallization of diabases and gabbros, the high percentage of iron usually necessitating the formation of abundant epidote.

While feldspars are usually present in small quantities in the altered acid sediments, they are known in large quantities in but few places in California. The arkoses usually have the composition of granites or acid diorites, and the recrystallization products are of the same chemical nature. In the gneiss from Melitta near Santa Rosa the original rock was an arkose composed of fragmental hornblende, orthoclase, acid plagioclase, and quartz. The orthoclase has recrystallized as such, the original hornblende has formed actinolite, and some of it has taken up soda, forming mixed crystals of carinthine and glaucophane. Some of the original ferro-magnesian mineral was probably titaniferous, for titanite is abundant in the rock. The excess of iron, alumina and magnesia from the partial decomposition of the hornblende and plagioclase formed abundant red garnets. It is noteworthy that neither epidote nor lawsonite nor any pyroxene was formed.

The albite-crossite gneiss of North Berkeley may have been formed from a syenitic arkose, as also the albite glaucophane gneiss of Angel Island. The lawsonite gneiss from near Redwood may have been a dioritic arkose, for its chemical composition is almost exactly that of a quartz diorite. The albite gneiss and the lawsonite gneiss both resemble somewhat in composition the altered arkose described by Becker,[1] which is evidently made up of fragmental material from a diorite or a granodiorite.

Sandstones usually contain varying proportions of clay, partly decomposed feldspars, and ferro-magnesian minerals. The feldspars were in part orthoclase, for we sometimes find this mineral in siliceous glaucophane schists. But more often we find evidence of the former presence of a potash feldspar in the secondary muscovite, which is almost never absent in this group. The silica has simply recrystallized as quartz, whether it came from original sand grains, or from the organic silica of the chert. Plagioclases are rarely present in the recrystallized product, but evidence of their former presence is seen in the soda that went into the glaucophane, and the lime and alumina that went into the garnets. Titanite is always present in the recrystallized siliceous rocks, but its origin is somewhat doubtful. The titanium probably came from titaniferous hornblendes, which on recrystallization took up soda to form carinthine or glaucophane and set free titanium. No pyroxenes of any kind are found in the recrystallized acid rocks. No epidote has been found in the siliceous rocks of the Coast Ranges, the lime of the plagioclase having invariably gone into the formation of garnet.

The reactions of the clay shales are essentially the same as those of the sandstones, except that there is usually more impurity in the shales, and consequently a greater variety of minerals. Glaucophane, garnet and white mica are the chief products, with subordinate quartz, from sand grains. No cyanite nor andalusite has been observed in any of the aluminous shales, though they would be expected, from the preponderance of silicate of alumina, and sillimanite occurs very rarely.

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[1] Mon. XIII., U. S. Geol. Survey, p. 92.

TABLE OF ANALYSES.

	I. Quartz Glaucophane Schist.	II. Quartz Glaucophane Schist.	III. Quartz Glaucophane Schist.	IV. Altered Arkose.	V. Lawsonite Gneiss.
SiO_2	82.53	80.21	74.48	68.50	65.91
Al_2O_3	6.88	7.99	9.15	12.82	11.62
Fe_2O_3	0.59		1.41	1.29	2.21
FeO	4.11	3.35	4.12	3.37	5.30
MgO	1.86	1.54	3.04	2.21	1.92
CaO	0.68	1.10	2.84	1.82	5.89
Na_2O	1.21	5.97	2.24	6.03	1.95
K_2O	1.24	0.22	0.43	1.26	0.04
$H_2O + 110°$ C.	1.35	0.74	2.06	2.11	4.38
$H_2O - 110°$ C.	0.07		0.08	0.28	0.28
TiO_2				0.60	0.17
MnO	trace			0.02	trace
P_2O_5				0.16	
Cr_2O_3					
CO_2					
Total	100.52	101.12	99.85	100.47	99.67

I. Quartz glaucophane schist, Four Mile Creek, Oregon; Washington, analyst, *Amer. Jour. Sci.*, IV. Ser., Vol. XI. (1901), p. 53. Abundant quartz, some chlorite, pink garnets, considerable glaucophane, and muscovite.

II. Quartz glaucophane schist, Angel Island, San Francisco Bay; Ransome, analyst, *Bull. Dept. Geol. Univ. of California*, Vol. I., p. 231.

III. Quartz glaucophane schist, Little Harbor, Catalina Island; Washington, analyst, *Amer. Jour. Sci.*, IV. Ser., Vol. XI. (1901), p. 48.

IV. Slightly altered sandstone, Sulphur Bank, California; Melville, analyst; G. F. Becker, Mon. XIII., U. S. Geol. Survey, p. 92. This is an arkose, slightly altered but has no glaucophane developed in it.

TABLE OF ANALYSES.

	VI. Albite Crossite Gneiss.	VII. Altered Diorite with Crossite and Lawsonite.	VIII. Soda Syenite Porphyry.	IX. Mica Glaucophane Schist.	X. Pseudodiabase.
SiO_2	65.2	61.55	67.53	58.26	51.27
Al_2O_3	15.8	17.28	18.57	16.21	15.04
Fe_2O_3	2.7	1.49	1.13	3.44	2.41
FeO	2.4	3.50	0.08	4.63	8.01
MgO	2.4	3.00	0.24	4.99	6.06
CaO	0.6	3.12	0.55	3.82	7.07
Na_2O	10.8	8.47	11.50	5.36	4.43
K_2O	0.1	0.07	0.10	0.39	0.12
$H_2O + 110°$ C.		1.21	0.31	0.98	3.34
$H_2O - 110°$ C.		0.12	0.15	0.22	
TiO_2		0.28	0.07	1.37	1.33
MnO		trace		trace	0.25
P_2O_5			0.11		0.13
Cr_2O_3					
CO_2					
Total	100.00	100.12	100.34	99.67	99.62

V. Lawsonite glaucophane gneiss, three miles southwest of Redwood; W. O. Clark, analyst. Abundant glaucophane and lawsonite, with a little garnet and white mica.

VI. Albite crossite gneiss, North Berkeley; H. Rosenbusch, *Sitzungsberichte d. k. Preuss. Akad.*, 1898, p. 712; calculated from the specific gravity of the rock, and the known composition of the two minerals that compose it, albite 75 per cent., and crossite 25 per cent.

VII. Slightly altered quartz diorite, Oak Ridge, five miles east of Calaveras Valley; W. O. Clark, analyst. Primary quartz, oligoclase, titaniferous hornblende, secondary crossite, lawsonite and titanite.

VIII. Soda syenite porphyry, Sierra Nevada, No. 1521 SN; Stokes, analyst, H. W. Turner, 17th An. Rept., Pt. I. (1896), p. 727.

IX. Mica glaucophane schist, Café Skarbeli, Syra, Grecian Archipelago; H. S. Washington, analyst, *Amer. Jour. Sci.*, IV. Ser., Vol. XI. (1901), p. 39. This analysis shows a mica glaucophane schist of the composition of a diorite, and is introduced here for comparison with those from California.

X. Pseudodiabase, Sulphur Bank, California, No. 36, Sulphur Bank; Melville, analyst; G. F. Becker, Mon. XIII., U. S. Geol. Survey, p. 99. The rock analysed showed no glaucophane, but Becker mentions that mineral as being not uncommon in similar pseudodiabases, which were thought to be altered sedimentary rocks.

TABLE OF ANALYSES.

	XI. Pseudodiorite.	XII. Glaucophane Schist.	XIII. Pseudodiabase	XIV. Fourchite.	XV. Glaucophane Schist.
SiO_2	50.44	49.68	49.08	46.98	47.84
Al_2O_3	8.18	13.60	14.68	17.07	16.88
Fe_2O_3	1.06	1.86	1.95	1.85	4.99
FeO	6.28	8.61	9.63	7.02	5.56
MgO	17.63	6.27	6.69	8.29	7.89
CaO	11.55	10.97	10.09	12.15	11.15
Na_2O	2.98	3.09	4.60	2.54	3.20
K_2O	0.50	0.12	0.20	0.53	0.46
$H_2O+110°$ C.	0.92	3.84	1.18	4.86	1.81
$H_2O-110°$ C.	0.07		0.28		0.17
TiO_2		1.31	1.72		
MnO	0.21	0.04	0.15		0.56
P_2O_5		0.21	0.23	0.09	0.14
Cr_2O_3	0.48				
CO_2					
Total	100.30	99.60	100.98	101.38	100.65

XI. Pseudodiorite, Knoxville, California No. 56, Knoxville; Melville, analyst; G. F. Becker, Mon. XIII., U. S. Geol. Survey, p. 101. This rock is a greenstone, composed almost entirely of actinolite, white mica, with rutile, zircon, and titanite. Becker does not mention glaucophane as occurring in this particular rock.

XII. Glaucophane schist, Sulphur Bank, No. 98 Sulphur Bank; Melville, analyst; G. F. Becker, Mon. XIII., U. S. Geol. Survey, p. 104. A basic glaucophane schist, with glaucophane, white mica, zoisite? (pale epidote?), albite, a little quartz, and titanite.

XIII. Pseudodiabase, Mt. St. Helena, California; No. 21 Coast Range; Melville, analyst; G. F. Becker, Mon. XIII., U. S. Geol. Survey, p. 98. A little altered igneous rock, with fresh plagioclases, some uralite, zoisite? (pale epidote?), and a little actinolite.

XIV. Fourchite, Angel Island; Ransome, analyst, *Bull. Dept. Geol. Univ. of California,* Vol. I., p. 231. A diabase dyke, slightly altered, with some secondary glaucophane.

XV. Garnet glaucophane schist, Pine Canyon, Mt. Diablo, California; Melville, analyst; H. W. Turner, *Bull. Geol. Soc. Amer.,* Vol. II. (1891), p. 413.

TABLE OF ANALYSES.

	XVI. Garnet Glaucophane Schist.	XVII. Epidote Glaucophane Schist.	XVIII. Eclogite.	XIX. Epidote Glaucophane Schist.	XX. Albite.
SiO_2	49.15	46.07	44.15	42.59	67.09
Al_2O_3	15.87	15.35	10.18	31.00	20.47
Fe_2O_3	4.10	3.61	11.92		
FeO	7.75	9.87	13.04		
MgO	7.53	7.83	6.18	5.10	
CaO	9.06	4.37	4.51	10.80	0.24
Na_2O	3.59	3.22	5.11	4.16	10.96
K_2O	0.54	2.68	2.09	1.01	
$H_2O+110°$ C.	1.07	4.25	0.95	5.34	0.59
$H_2O-110°$ C.	0.16	0.16			0.27
TiO_2............	1.19	1.63	trace		
MnO	trace	trace			
P_2O_5					
Cr_2O_3...........					
CO_2		1.05			
Total	99.84	100.09	99.31	100.00	99.62

XVI. Garnet glaucophane schist, Tupper Rock, near Brandon, Oregon; Washington, analyst, *Amer. Jour. Sci.,* IV. Ser., Vol. XI. (1901), p. 53. Abundant glaucophane, and garnets, with some epidote, zoisite, and white mica.

XVII. Epidote glaucophane schist, Roseburg, Oregon; Washington, analyst, *Amer. Jour. Sci.,* IV. Ser., Vol. XI. (1901), p. 53. Abundant glaucophane, epidote, and muscovite, with a few quartz grains.

XVIII. Omphacite eclogite, Coyote Creek, six miles north of San Martin, California; C. B. Allen analyst; R. S. Holway, *Jour. Geol.,* Vol. XII., p. 356. A massive rock composed of omphacite, garnet, white mica (muscovite), actinolite, titanite, and rutile.

XIX. Epidote glaucophane schist, North Berkeley, California; H. Rosenbusch, *Sitzungsberichte, K. Preuss. Akad. Wiss.,* 1898, p. 716. A partial analysis, with an estimate of the other components.

XX. Albite, from glaucophane schist, San Pablo, California; W. C. Blasdale, analyst, *Bull. Dept. Geol. Univ. of California,* Vol. II., p. 343.

XXI. Pargasite, from the pargasite eclogite, Arroyo Hondo, at the northern end of Calaveras Valley, Santa Clara County, California; W. O. Clark, analyst.

XXII. Glaucophane, from glaucophane schist, San Pablo, California; W. C. Blasdale, analyst, *Bull. Dept. Geol. Univ. of California,* Vol. II., p. 338.

XXIII. Glaucophane, from glaucophane schist, San Pablo, California; W. C. Blasdale, analyst, *loc. cit.*

XXIV. Crossite, from albite crossite gneiss, North Berkeley; W. S. T. Smith, analyst, in C. Palache, *Bull. Dept. Geol. Univ. of California,* Vol. I., p. 188.

TABLE OF ANALYSES.

	XXI. Pagasite.	XXII. Glaucophane.	XXIII. Glaucophane.	XXIV. Crossite.	XXV. Glaucophane.
SiO_2	42.68	54.52	52.39	55.02	57.67
Al_2O_3	9.96	9.25	11.29	4.75	11.07
Fe_2O_3	6.12	4.44	3.74	10.91	3.20
FeO	12.25	9.81	9.13	9.46	9.68
MgO	9.58	10.33	11.37	9.30	9.85
CaO	11.83	1.98	3.03	2.38	0.95
Na_2O	3.30	7.56	6.14	7.62	6.80
K_2O	0.89	0.16	trace.	0.27	0.42
$H_2O + 110°$ C.	3.16	1.78	2.57		0.36
$H_2O — 110°$ C.	0.12				0.12
TiO_2	0.68	0.39	0.14		
MnO	trace	0.46	trace	trace	0.06
P_2O_5					
Cr_2O_3					
CO_2					
Total	100.57	100.68	99.80	99.70	100.18

XXV. Glaucophane from Syra, Greece; Washington, analyst, *Amer. Jour. Sci.,* IV. Ser., Vol. XI., p. 40.

XXVI. Actinolite, Berkeley, California; W. C. Blasdale, analyst, *Bull. Dept. Geol. Univ. of California,* Vol. II., p. 333.

XXVII. Actinolite, San Pablo, California; W. C. Blasdale, analyst, *loc. cit.*

TABLE OF ANALYSES.

	XXVI. Actinolite.	XXVII. Actinolite.	XXVIII. Chlorite.	XXIX. Talc.	XXX. Zoisite?
SiO_2	55.21	55.56	27.38	56.02	39.80
Al_2O_3	3.45	2.05	26.15	9.02	22.72
Fe_2O_3			0.78	1.10	4.85
FeO	7.49	5.97	12.70	5.14	1.49
MgO	18.97	19.45	18.92	24.10	3.89
CaO	10.50	12.13		0.60	17.55
Na_2O	2.45	1.94	1.15		4.09
K_2O		0.30			0.12
$H_2O + 110°$ C.	1.75	2.58	11.44	4.34	5.25
$H_2O — 110°$ C.			1.51	0.16	
TiO_2					trace
MnO					0.26
P_2O_5					
Cr_2O_3					
CO_2					
Total	99.82	99.98	100.03	100.48	100.02

XXVIII. Chlorite from glaucophane schist, San Pablo, California; W. C. Blasdale, analyst, *Bull. Dept. Geol. Univ. of California,* Vol. II., p. 340.

XXIX. Talc, from glaucophane schist, San Pablo, California; W. C. Blasdale, analyst, *loc. cit.*

XXX. Zoisite (or pale epidote?), from pseudodiabase; Melville, analyst; G. F. Becker, Mon. XIII., U. S. Geol. Survey, p. 79.

TABLE OF ANALYSES.

	XXXI. Garnet.	XXXII. Lawsonite.
SiO_2	38.69	38.45
Al_2O_3	19.10	31.35
Fe_2O_3		0.86
FeO	26.81	0.10
MgO	5.07	0.17
CaO	10.64	17.52
Na_2O		0.06
K_2O		0.23
$H_2O+110°$ C.		11.21
$H_2O-110°$ C.		
TiO_2		0.38
MnO		trace
P_2O_5		
Cr_2O_3		
CO_2		
Total	100.31	100.33

XXXI. Garnet, from omphacite eclogite, Coyote Creek, six miles north of San Martin, Santa Clara County, California; W. O. Clark, analyst.

XXXII. Lawsonite from glaucophane eclogite, Reed's Station, Tiburon Peninsula, California; Schaller and Hillebrand, analysts, *Amer. Jour. Sci.,* IV. Ser., Vol. XVII. (1904), p. 197.

SUMMARY.

Glaucophane-bearing rocks have been made out of very different original materials. Siliceous fragmental sediments, deposits of organic silica, acid arkoses, medium basic clay shales, and basic tuffs have all been altered to a somewhat similar product. And igneous rocks have contributed to a like result. Syenites, diorites, diabases and gabbros, and probably pyroxenites have been changed into glaucophane schists. At first sight it would seem a hopeless task to attempt to find out the origin of a glaucophane schist when it might have been made from any one of these rocks. But, fortunately, the intrinsic character of the original rock has not been altered, and the chemical readjustment consequent upon metamorphism has done nothing more than change the mineralogical association. Each sort of rock, whether igneous or sedimentary, has its own peculiar chemical composition, and this is disclosed by a chemical analysis, no matter what molecular readjustment has taken place. Metamorphism was once considered as a sort of magical process by which almost any sort of rock might be made out of any sort of material. But metamorphism such as we have to deal with in the glaucophane-bearing rocks of the Coast Ranges may be defined merely as recrystallization. A siliceous rock remains siliceous, a basic ferruginous or magnesian rock remains ferruginous or magnesian. Little is added and little is taken away, not enough in any case to obscure the relations of the original material and the recrystallized product. The only essential difference is the water of constitution, which is always present in some mineral in the recrystallized rocks.

Analyses I., II. and III. in the foregoing table are of altered siliceous sediments, and are clearly recognizable as such by their chemical composition. Analysis IV. is of an arkose which corresponds closely to a granodiorite. No. V. is a lawsonite gneiss, with every mineral in it a product of recrystallization, but its chemical constitution shows it to have been originally either a quartz diorite or an arkose from such a rock. Analysis VI. is an albite crossite gneiss, with every mineral of secondary origin, but chemically it is a soda syenite, and it was originally such an igneous rock or an arkose from a soda syenite. No. VIII. is a soda syenite porphyry, showing a composition almost identical with No. VI., and glaucophane-like minerals have been observed at several places in the Sierra Nevada in dynamically altered soda syenites.

No. VII. is a little altered quartz diorite in which secondary lawsonite and crossite have been developed by dynamic metamorphism.

No. IX. is a mica glaucophane schist, but the chemical composition shows it to have been a normal diorite.

Nos. X., XI., XIII. and XIV. are of metamorphosed medium basic rocks, either diabases or gabbros, and the chemical composition shows only the addition of water, otherwise they are still gabbros in composition.

Nos. XII., XV., XVI., XVII. and XIX. are basic glaucophane schists, but the chemical analyses show them to have been gabbros or diabases.

No. XVIII. is a massive eclogite, composed of garnet, omphacite actinolite, glaucophane, white mica and titanite, but the chemical analysis shows it to have been a basic gabbro or even possibly a pyroxenite. No rocks of the chemical nature of peridotite have been discovered by chemical analysis among the glaucophane schists, but some of the actinolite chlorite schists would probably show such a constitution.

These results all show that there is no need of supposing that magnesian or alkaline or siliceous solutions have permeated the altered rocks, adding one substance and taking away another. The small amounts of mechanically contained water in all the sediments, and the water disseminated through all the cracks in the igneous rocks have been sufficient, when heated under pressure, to produce all the phenomena of recrystallization seen in the Coast Ranges. This small amount of water would not have been sufficient to produce aqueo-igneous fusion, nor is it likely that this condition was ever reached, for the minerals were not all crystallized at the same time. But each molecule of water, with almost unlimited time, could accomplish a great deal of work, and this work was not finished until that molecule was fixed as water of constitution in some mineral.

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Stated Meeting October 5, 1906.

President SMITH in the Chair.

Dr. Herbert Donaldson, a newly elected member, was presented to the Chair and took his seat in the Society.

Acknowledgments of elections to membership were received from

Prof. Charles A. Hastings.
Prof. Arthur Gordon Webster.
Prof. Adolf Engler.
Prof. Dmitri Ivanovitch Mendeleff.

Invitations were received from

The University of Aberdeen, to be represented at the celebration of its Quatercentenary, in September, 1906.

From the University of Pennsylvania, to be represented at the dedication of the Engineering Building in October, 1906.

From the International Congress for the Study of Polar Regions, to participate in the Congress to be held at Brussels in September, 1906.

Communications were received

From the California Academy of Sciences, thanking the Society for its resolution of sympathy of April 17th last, and accepting the offer of a set of the Transactions and Proceedings of this Society as a contribution towards the rehabilitation of the Academy's library.

From The President making the following appointments:

General A. W. Greely, U. S. A., to represent the Society at the International Congress for the Study of Polar Regions.

Mr. Andrew Carnegie to represent the Society at the Quatercentenary of the University of Aberdeen.

Prof. Angelo Heilprin to represent the Society at the Tenth International Geological Congress.

The decease of the following members was announced:

George Tucker Bispham, Newport, R. I., on July 28, 1906, æt. 68.

Edward Coles, at Bar Harbor, Maine, on August 4, 1906, æt. 69.

Robert Coleman Hall Brock, at Philadelphia on August 8, 1906, æt. 45.

The following papers were read:

"Pyrite from Cornwall, Lebanon Co., Pa.," by CHARLES TRAVIS (communicated by PROF. AMOS P. BROWN), which was discussed by PRESIDENT SMITH and PROF. A. P. BROWN. (See page 131.)

"Studies in Avian Anatomy," by MARGARET E. MARSHALL (communicated by PROF. THOMAS H. MONTGOMERY, JR.).

"The Paragenesis of the Minerals in the Glaucophane-bearing Rocks of California," by JAMES PERRIN SMITH (communicated by PROF. J. C. BRANNER). (See page 183.)

"The Aranda and the Loritza Languages, Central Australia," by R. H. MATHEWS (communicated by the Secretaries).

Stated Meeting October 19, 1906.

The following papers were read:

"Problems of Double Star Astronomy," by ERIC DOOLITTLE, which was discussed by MR. JOSEPH WHARTON, PROF. SNYDER and PROF. DOOLITTLE.

"Inheritance in the Female Line of Size of Litter in Poland China Sows," by GEORGE M. ROMMELL, B.S.A., and E. F. PHILLIPS, Ph.D. (communicated by PROF. E. G. CONKLIN). (See page 245.)

"The Cause of Earthquakes, Mountain Formations and Kindred Phenomena connected with the Physics of the Earth," by T. J. J. SEE. (See page 274.)

INHERITANCE IN THE FEMALE LINE OF SIZE OF LITTER IN POLAND CHINA SOWS.

BY GEO. M. ROMMEL, B.S.A., AND E. F. PHILLIPS, PH.D.

(*Read October 19, 1906.*)

Among the many problems of heredity yet to be solved, one of the most important is the determination in numerical values of the amount of transmission of different characters, which actually occurs. Galton (1897) and Pearson (1900) have given us their determinations of the theoretical correlation between parents and offspring, and several investigators have made mathematical determinations of the exact amount of correlation of various characters occurring between parents and offspring to the second generation.

One of the recent problems of the Department of Agriculture has been the determination of the size of litter of Poland China and Duroc Jersey sows, these results being recorded in Circular No. 95, Bureau of Animal Industry, "The Fecundity of Poland China and Duroc Jersey Sows," by the senior author of this paper. The results of this work may be summarized briefly as follows: "An undoubted increase is evident and the conclusion is inevitable, that, contrary to popular opinion, the Poland China breed has increased in fertility during the past twenty years. . . . The increase shown . . . (is) .48 per litter" (*n.* litters 54,515). The average size of litter in Poland Chinas, as recorded in the "American Poland China Record" and the "Ohio Poland China Record" for the first five years examined (1882–6) is 7.04, and for the last five years (1898–1902) 7.52. The Duroc Jersey records were calculated from the "National Duroc Jersey Record" for the years 1888–1902 inclusive with an average size of litter for the years 1893–1902 of 9.26 (*n.* litters 21,652). These records show no measurable change in size of litter for the period covered.

With these data at hand, it seemed desirable to continue the work to determine how far this character, size of litter, is inherited. The determination of the inheritance of fertility, fecundity, or number of offspring born at one time, is a most valuable point, both from the standpoint of practical breeding and from that of theoretical consideration. Up to the present time, statistics on the inheritance of number of offspring in mammals is represented only by the work of Pearson, Lee, and Bramley-Moore (1899),[1] on man and thoroughbred race horses. In this work, it was found that fertility (total number of offspring) is inherited. By limiting the duration of marriage to fifteen years or more, the correlation of fertility in man was found to be larger than when no such limitation was made, a result which is in itself obvious. In the work on thoroughbred race horses the same method was employed making limitations according to number of coverings. In the work reported in this paper the total number of offspring born at one time is the only problem considered, and the length of the breeding period of parents does not effect the results except in the way shown, that the correlation decreases with the age of the sow.

In a recent paper by Castle and others (1906), this problem is taken up in *Drosophila ampelophila,* Löw. The results of this work show that there is "an unmistakable tendency for the larger parental brood to produce larger filial broods, but *not so much larger* as the difference in the parental brood would lead us to expect."

The method of work for the original problem is described by Rommel (1906) as follows:

> "The Poland China records generally publish the registration of hogs according to the following plan or modifications of it:
>
> MAGNET'S MODEL 2D, 135786.
>
> Farrowed March 28, 1902. Litter, 7; raised—boars, 0; sows, 4.
>
> Black with white points.
>
> Bred and owned by W. C. Williams and Gardner, Bryant, Jay County, Ind.
>
> Sire: Tecumseh Magnet, 46925; he by Nelson's Magnet, 35535, and out of Miss Rosa, 75542.
>
> Dam: Gardner's Model, 122578, by Invincible Chief, 43377, etc.
>
> "This is the registration of a sow on page 282 of volume 26 of the 'Ohio Poland China Record.'
>
> "For each hog registered a card (size about 2 by 4⅞ inches) was written according to the following plan:
>
> Name and number of dam.

---

[1] Mathematical Contributions, etc., VI, "Genetic (Reproductive) Selection: Inheritance of Fertility in Man, and of Fecundity in Thoroughbred Race Horses." *Phil. Trans.*, A, CXCII, 257–330.

Date progeny was farrowed. Number of pigs in litter; boars raised, sows raised; total raised.

Volume and page reference.

"The above litter would therefore appear on its card as follows:

Gardner's Model, 122578.
March 28, 1902. 7—0—4 4.
26/282

. . . .

"After all the cards for a volume were written, two clerks compared them with the originals and corrected errors in copying, one reading the original records while the other corrected the copies. After all the cards for a breed were written and verified they were sorted according to years; next the cards for each year were sorted according to size of litter farrowed; next the cards for each size of littler were arranged numerically according to the numbers of dams and duplicates thrown out, and finally the cards were counted and averages calculated. Cards were counted at least twice by different persons, and the calculations were made twice by different persons. In writing the cards for the 'American Poland China Record' from 1898 to 1902 the page reference was not included, which we now know was a mistake, as it is almost impossible to find a litter record without a page reference. The cards for the 'American Poland China Record' were arranged alphabetically at first, and it was necessary to rearrange them numerically, which eliminated a large number of duplicates."

. . . .

"The reader will recognize the fact that the probability of error in this work is large. Breeders are not always careful in reporting the number of pigs farrowed, often relying on the memory. In spite of the painstaking care of secretaries, errors in copying and typographical errors are sure to occur, and in our own work errors in copying, indexing, and counting were probable, although the cards were handled many times. At the same time, although the probability of error is large, the probability of these errors affecting the final results is very small on account of the large numbers used. A mistake of one hundred litters for one of the later years of the 'American Poland China Record' would affect the average for that year slightly, but would have no serious effect on the five-year average. The writer believes that the factor of error in these calculations has been reduced below the point of practical importance."

The litters for 1902 of the "American Poland China Record" were chosen for this additional work on inheritance. In addition to the information as described above, the mother of the dam was looked up and the size of litter in which she was farrowed was recorded. The date of farrowing of the dam was also recorded so that a card similar to the one illustrated above for the "American Poland China Record" for 1902 would now read:

U. S. Bell, 195,564.
May, 1, 1902. 7—3—3. 6.
7. '01.

The cards were then arranged according to the age of the mother at the time the dam named on the card was farrowed. This division is necessarily rough, but the cards were arranged according to the year of the birth of the dam, 1901, 1902, etc. Naturally the sows placed in any one year are not all exactly of the same age, but this rough segregation is enough to eliminate the factor of age as far as is practical and necessary in this work.

The method for calculating the correlation between mother and daughter is given by Davenport (1904), "Statistical Methods," pp. 44–47, using the modification adopted by Yule (1897), which is expressed in the formula

Coefficient of correlation

$$(r) = \frac{\Sigma f x' x''}{n} - v_1' v_1'' \frac{1}{\sigma' \sigma''}$$

in which x' and x'' are the deviations from an assumed integral mean of the subject and relative classes respectively, f is the frequency of the several combinations, n, total number of individuals, v_1' and v_1'', the differences between assumed and real means of the subject and relative classes, and σ' and σ'', the standard deviations of the subject and relative classes respectively. The standard deviation is obtained from the formula

$$\sigma = \sqrt{\frac{\Sigma (x^2 f)}{n}}$$

and is also equal to $\sqrt{v_2 - v_1}$. For a further discussion of these terms and the method used in the determination, the reader is referred to the above-mentioned book by Dr. Davenport.

For the general reader, who may not be familiar with the approved methods of such work, it may be stated that the standard deviation (σ) is a relative measure, expressed in terms of the mean, of the concentration about the mean or average, and therefore is an excellent measure of the amount of variation. If the standard deviation (σ) is divided by the average (A), the result is the coefficient of variation (C) which expresses in percents the amount of variation of any group of individuals for the character under consideration. A large coefficient of variation indicates therefore that the individuals are not closely grouped about the average in the character measured, and consequently that the character is highly variable one and vice versa. The use of an assumed integral mean and then the correction of that mean by the subtraction of the product of the two differences between the assumed and real means ($-v'v''$ in formula) is merely for the sake of avoiding long fractions and has no effect on the general result. The formula

$$r = \frac{\Sigma f x' x''}{n \sigma' \sigma''}$$

represents the true formula for calculating correlations, but by the use of the first formula given, we get an identical result with very much less labor. In this formula, of course, x' and x'' are the deviations from the true mean of the subject and relative classes respectively.

The probable error (E) describes the probable limits above and below the calculated value within which the true value lies,—the absolute value being capable of determination only by an examination of an infinite number of cases. Thus $r = .0601 \pm .0086$ indicates that the coefficient of correlation probably lies between .0687 and .0515. The probable error for r is found from the formula

$$Er = \frac{0.6745\ (1 - r^2)}{\sqrt{n}}$$

for σ from the formula

$$E\sigma = 0.6745 \frac{\sigma}{\sqrt{2n}}$$

and for

$$A \text{ is } 0.6745 \frac{\sigma}{\sqrt{n}}$$

TABLE I.—Correlation of Size of Litters of Yearling Sows to Size of Litters in which Dams were Farrowed.—"American Poland China Record."—Litters of 1902.

Size of Litters of Yearling Sows.

Size of Litters in which Dams were Farrowed.

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	
1																		0
2				2	2	1	1			1								7
3		4	2	1	2	3	4	2	2	1								21
4			3	5	22	20	10	4	6									70
5		1	4	15	26	28	24	15	5	6	1							125
6		1	3	21	34	65	81	43	18	7	2							275
7	1	4	4	27	47	89	89	62	31	12	2							368
8	1	1	10	31	65	67	81	61	34	12	3	1	1					368
9		1	5	21	53	76	87	67	35	14	10	1	1					371
10	1	1	4	14	16	38	48	36	23	6	6							193
11		2	2	8	20	21	22	22	13	7	4							121
12		1	1	4	7	15	12	8	4	1	2	1						56
13			3	2	4	6	1	5		1			1					23
14					1	2	3	2	1	1								10
15										1								1
16																		0
17				1														1
	3	16	41	152	299	431	463	327	172	70	30	3	3	0	0	0	0	2010

TABLE II.—Correlation of Size of Litters of Two-Year-Old Sows to Size of Litters in which Dams were Farrowed. "American Poland China Record."—Litters of 1902.

Size of Litters of Two-Year-Old Sows.

Size of Litters in which Dams were Farrowed.

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	
1																0
2				1			1	2		1						5
3				2	4	2	2	4	6	2			1			23
4			3	4	3	10	14	10	12	2	3	1	1	1		64
5			3	6	10	33	34	36	27	6	6	2			1	164
6	1	2	7	15	26	52	70	68	36	27	8	4	1			317
7		2	5	10	25	71	86	84	59	32	12	2	2	1		391
8		7	4	15	36	55	95	78	64	32	11	6	4	1		408
9	1	2	2	8	20	36	61	65	70	33	20	4	1	1		324
10			1	5	14	30	33	45	23	14	15	6	1			187
11		2	1	2	7	4	21	21	15	13	7	2			1	96
12			2	1	4	10	9	9	2	4	3	1				45
13					1	3	2	3	4	1	1					15
14							3	1	1	1	1		1			8
15																0
	2	15	28	69	150	306	431	426	319	168	87	28	12	4	2	2047

TABLE III.—CORRELATION OF SIZE OF LITTERS OF THREE-YEAR-OLD SOWS TO SIZE OF LITTERS IN WHICH DAMS WERE FARROWED. "AMERICAN POLAND CHINA RECORD."—LITTERS OF 1902.

Size of Litters of Three-Year-Old Sows.

Size of Litters in which Dams were Farrowed.

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	
1																		0
2						1	1	1	1									4
3		1		1	2	1	2	4	5			1					1	18
4		1			2	3	6	9	7	6	2		1					37
5		1	1	5	11	17	15	22	16	4	3	3	1					99
6	1	1	3	5	14	27	37	29	35	14	12	3						181
7	1		2	9	19	25	50	48	31	34	12	4	2					237
8	1		5	3	13	31	40	47	35	22	18	2	3		1	1		222
9				3	8	17	33	30	31	24	8	2	3	2				161
10	1		3		6	11	16	25	24	17	4		1	1	1			110
11				4	5	5	14	13	12	6	4	3	1					67
12			1			2	3	1	2	4	5							18
13						1												1
14									1									1
15																		0
16								1										1
17																		0
	4	4	15	30	80	141	217	230	200	131	68	18	12	3	2	1	1	1157

TABLE IV.—CORRELATION OF SIZE OF LITTERS OF FOUR-YEAR-OLD SOWS TO SIZE OF LITTERS IN WHICH DAMS WERE FARROWED. "AMERICAN POLAND CHINA RECORD."—LITTERS OF 1902.

Size of Litters of Four-Year-Old Sows.

Size of Litters in which Dams were Farrowed.

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	
1								1				1				2
2									1	1						2
3			1		1		1	2	1	3						9
4				1		2	5	5	4	3	2	2				24
5				2	3	5	6	11	8	7	2			1		45
6				2	6	7	13	18	8	17	3	2	1	1		78
7		1		2	6	13	22	24	32	16	4	2		1		123
8			1	3	5	10	15	23	26	24	6	6	1	1	1	122
9		1	1	3	8	9	18	18	15	20	10	1	2	3		109
10				1	4	5	5	10	8	11	3			1		48
11				2	2	5	1	5	6	1	5	2	1			30
12					1	2		1	2	3	1					10
13						1	1									2
14						1				1						2
15																0
	0	2	3	16	36	60	87	118	111	107	36	16	5	8	1	606

TABLE V.—CORRELATION OF SIZE OF LITTERS OF FIVE-YEAR-OLD SOWS TO SIZE OF LITTERS IN WHICH DAMS WERE FARROWED. "AMERICAN POLAND CHINA RECORD."—LITTERS OF 1902.

Size of Litters of Five-Year-Old Sows.

Size of Litters in which Dams were Farrowed.

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	
1																0
2												1				1
3					1			1	2							4
4						2	1	2	3	2					1	11
5					1	3	3	6	6	4	2	2				27
6		1	1	1	4	8	4	10	6	8	7	1	1	2		54
7				1	1	8	12	10	13	11	6	1	3			66
8				2	3	3	11	14	10	4	3	3				53
9		2		2	4	3	8	11	12	6	4	2				54
10				1		4	3	5	4	3	3	2	1	1		27
11						1	3	2	2	4	1					13
12							1	2	1				1			5
13					1		1	1	1	1	2		1			8
14							1		1							2
15																0
	0	3	1	7	15	32	48	64	61	43	28	12	7	3	1	325

TABLE VI.—CORRELATION OF SIZE OF LITTERS OF SOWS ONE TO FIVE YEARS OLD TO SIZE OF LITTERS IN WHICH DAMS WERE FARROWED. "AMERICAN POLAND CHINA RECORD."—LITTERS OF 1902.

Size of Litters of Sows One to Five Years Old.

Size of Litters in which Dams were Farrowed.

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	
1								1				1						2
2				3	2	2	3	3	2	3		1						19
3		5	3	4	10	6	9	13	16	6		1	1				1	75
4		1	6	10	27	37	36	30	32	13	7	3	2	1	1			206
5		2	8	28	51	86	82	90	62	27	14	7	1	1	1			460
6	2	5	14	44	84	159	205	168	103	73	32	10	3	3				905
7	2	7	11	49	98	206	259	228	166	105	36	9	7	2				1185
8	2	8	20	54	122	166	242	223	169	94	41	18	9	2	2	1		1173
9	1	6	8	37	93	141	207	191	163	97	52	10	7	6				1019
10	2	1	8	21	40	88	105	121	82	51	31	8	3	3	1			565
11		4	3	16	34	36	61	63	48	31	21	7	2		1			327
12		1	4	5	12	29	25	21	11	12	11	2	1					134
13			3	2	6	11	5	9	5	3	3		2					49
14					1	3	7	3	4	3	1		1					23
15										1								1
16								1										1
17				1														1
	9	40	88	274	580	970	1246	1165	863	519	249	77	39	18	6	1	1	6145

The calculation of the correlation between sizes of litters in two consecutive generations in the female line gives the following results:

TABLE VII.—Correlation in Size of Litter of Poland China Sows between Mother and Daughter.—"American Poland China Record."

Age of Daughters.	Number of Cases.	A. M.	A. D.	σ M.	σ D.	r.	Er.
1 year.	2010	7.908	6.6451	2.0764	1.7582	.1088	.0149
2 years.	2047	7.6927	7.5598	1.9818	1.9415	.0885	.0148
3 years.	1157	7.5809	7.8799	1.9615	2.0693	.0883	.0197
4 years.	606	7.6304	8.2821	1.9856	2.0661	.0379	.0274
5 years.	325	7.6738	8.4031	2.1001	2.1571	.0032	.0375
1–5 years.	6145	7.7349	7.4391	2.0202	2.0312	.0601	.0086

Conclusions.

From the first work in statistics of fecundity of sows (Rommel, 1906), it is evident that in Poland China hogs there has been an increase of .48 in the size of litter in the twenty years between 1882 and 1902. This result may be attributed to one of two factors. It is either the result of selection (probably more or less accidental) of sows (and boars) from large litters and a consequent inherited tendency toward the production of larger litters, or a gradual improvement in the environmental conditions, bringing about the same result by external factors. From the first work it is impossible to do more than theorize as to the true cause, the theories being based, of course, on a knowledge of the usages of breeders.

The tabulation of the sizes of litters from mothers and daughters and the determination of the coefficient of correlation (*r*) shows that there is an actual correlation between the size of litters of two successive generations, and we are consequently justified in concluding that size of litter is a character transmitted from mother to daughter. The coefficient of correlation for the five years is small (.06), but it is appreciable and consequently it would appear proved that, by judicious selection for breeding purposes of sows from large litters, the average for the breed may be increased. This, combined with the fact that the average has actually been increased, gives us evidence that may be considered very strong.

Unfortunately the cards used in this work for litters for 1902 of the "American Poland China Record" were not written to include the sire, and we are consequently unable at present to determine from these data the influence of the boar on the size of litter of a sow of his offspring. This work is being continued, however, and these additional data are being recorded.

The decrease from .1088 to practically zero (.0032) from the first to the fifth year does not necessarily mean that the inheritance of fecundity is lost as a sow grows older, but probably indicates that inheritance from the first dam gradually plays relatively less and less of a part in the determination. So many other factors, body strength, maturity, and functional habit, can influence this, that the inherited tendency seems to lose its influence. The average for five years (.06) is, however, large enough to be of value to the breeder, and with the large numbers here used (6145) the probable error is small (.0086), and our results more nearly a true statement of the conditions of inheritance.

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Washington, D. C., October 1, 1906.

THE EFFECT OF SULPHURIC ACID ON THE DEPOSITION OF METALS WHEN USING A MERCURY CATHODE AND ROTATING ANODE.

By LILY G. KOLLOCK AND EDGAR F. SMITH.

[CONTRIBUTION FROM THE JOHN HARRISON LABORATORY OF CHEMISTRY.]

(Read November 3, 1906.)

In the course of a study of the precipitation of metals with the help of a mercury cathode and rotating anode, observations were made which led to a review of the experiments to ascertain more especially what effect varying amounts of acid would have upon the electrolytic decomposition of certain metallic sulphates.

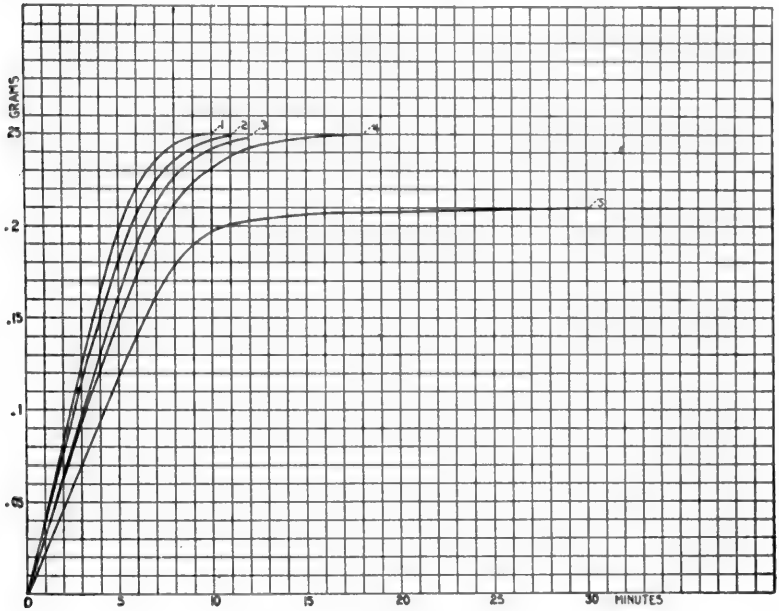

FIG. 1. Zinc.

The metals studied under these conditions were zinc, copper, cadmium, iron, cobalt and nickel. The volume of the solution of the salt was, in nearly all cases, ten cubic centimeters, containing about 0.25 gram of metal. The current was maintained at two amperes, while the pressure varied from 4 to 4.5 volts. The sulphuric acid was concentrated. Curves were plotted from the results obtained.

ZINC.

When one cubic centimeter of sulphuric acid was present in a solution of zinc sulphate, the zinc was completely deposited in ten minutes. The solution contained 0.25 gram of metal, the volume of the solution being eleven cubic centimeters. The following observations were made:

In 2.5	minutes,	0.1040	gram	of zinc	was deposited.
" 5	"	0.1974	"	"	"
" 7.5	"	0.2400	"	"	"
" 8	"	0.2428	"	"	"
" 10	"	0.2494	"	"	"

(Figure 1, Curve 1.)

When two cubic centimeters of sulphuric acid were present, the solution, which was siphoned from the cup, showed the presence of a trace of zinc after it had been electrolyzed for ten minutes. Eleven minutes were necessary for the complete removal of the zinc. The volume of the solution in this case was twelve cubic centimeters. The following rate of deposition was observed:

In 2.5	minutes,	0.0953	gram	of zinc	was deposited.
" 5	"	0.1805	"	"	"
" 7.5	"	0.2306	"	"	"
" 10	"	0.2467	"	"	"
" 11	"	0.2488	"	"	"

(Figure 1, Curve 2.)

In the presence of three cubic centimeters of acid, twelve minutes were required for the deposition of the zinc. The volume of the solution in these experiments was thirteen cubic centimeters. Under these conditions—

In 2.5	minutes,	0.0815	gram	of zinc	was deposited.
" 5	"	0.1633	"	"	"
" 7.5	"	0.2200	"	"	"
" 10	"	0.2428	"	"	"
" 12	"	0.2493	"	"	"

(Figure 1, Curve 3.)

When four cubic centimeters of acid were present, considerable retardation in the decomposition was observed; for at the end of the fifteen minute period, there was still some undeposited zinc. The test at the end of eighteen minutes showed that no zinc was present in the solution. The rate of precipitation was as follows:

In	2.5	minutes,	0.0668	gram	of zinc	was deposited.
“	5	“	0.1506	“	“	“
“	7.5	“	0.2050	“	“	“
“	10	“	0.2315	“	“	“
“	12	“	0.2423	“	“	“
“	15	“	0.2480	“	“	“
“	18	“	0.2495	“	“	“

(Figure 1, Curve 4.)

That the presence of five cubic centimeters of acid greatly retards the precipitation was shown when a solution, containing 0.2110 gram of zinc, was electrolyzed. It was thirty minutes before the last traces of metal were thrown out.

In	2.5	minutes,	0.0668	gram	of zinc	was deposited.
“	5	“	0.1306	“	“	“
“	10	“	0.1980	“	“	“
“	20	“	0.2077	“	“	“
“	30	“	0.2110	“	“	“

(Figure 1, Curve 5.)

In these experiments the volume of the solution was increased, in each case, by the addition of the acid. Thinking that, perhaps, the increased period of time might be due to the greater volume of solution, it was observed that the retardation was due principally to the acid present. From a solution of fourteen cubic centimeters volume, containing one cubic centimeter of sulphuric acid, all the zinc was deposited in twelve minutes. In the same volume of solution, containing four cubic centimeters of acid, eighteen minutes were necessary for complete precipitation.

COPPER.

The volume of the copper sulphate solution in these experiments, as well as in the solutions of the sulphates of the succeeding metals, was ten cubic centimeters, including the volume of the acid added.

The amount of copper present in the solution was 0.2485 gram.

In the presence of one cubic centimeter of sulphuric acid, all the copper was deposited in ten minutes. The current registered two amperes and showed a pressure of from 3 to 3.5 volts. The solution became colorless in seven minutes. Three minutes additional were necessary to deposit the remaining 0.007 gram of copper. The following rate of precipitation was observed:

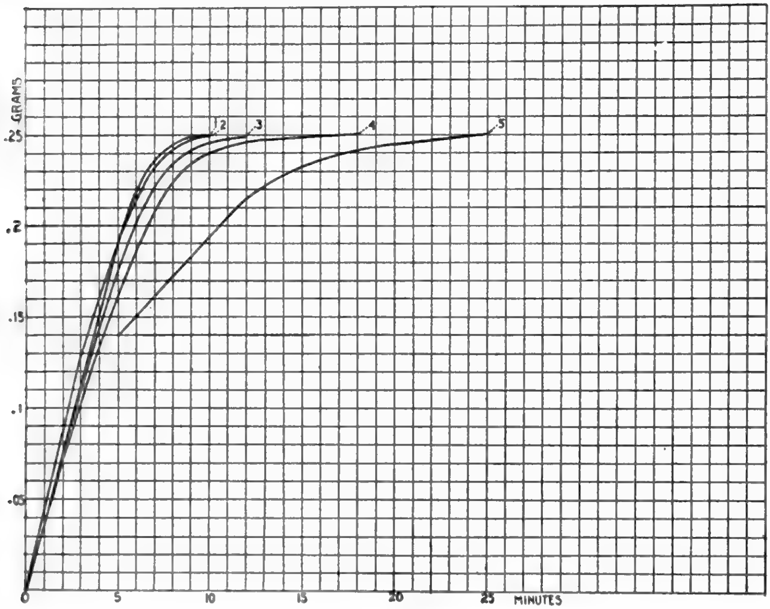

FIG. 2. Copper.

In 2.5	minutes,	0.0985	gram	of copper	was deposited.
" 5	"	0.1887	"	"	"
" 7.5	"	0.2418	"	"	"
" 10	"	0.2437	"	"	"

(Figure 2, Curve 1.)

When two cubic centimeters of acid were present and the current was maintained at two amperes, the pressure varying from 4 to 4.5 volts, it was found that the copper was completely deposited in ten minutes. It will be observed that the pressure in this case was half a volt to a volt greater than in the previous experiment. The two curves almost coincide.

In 2.5	minutes,	0.1157	gram	of copper was	deposited.
“ 5	“	0.1897	“	“	“
“ 7.5	“	0.2385	“	“	“
“ 10	“	0.2489	“	“	“

(Figure 2, Curve 2.)

The presence of three cubic centimeters of acid affects the curve but slightly. The departure is noticed in the deposition of the last traces of the metal. The copper was completely removed in twelve minutes.

In 2.5	minutes,	0.1079	gram	of copper was	deposited.
“ 5	“	0.1807	“	“	“
“ 7.5	“	0.2311	“	“	“
“ 10	“	0.2450	“	“	“
“ 12	“	0.2488	“	“	“

(Figure 2, Curve 3.)

The effect of a large amount of sulphuric acid was noticed again in the latter part of the decomposition, when four cubic centimeters of acid were added. It was then observed that it took six minutes longer than it did when but three cubic centimeters were present to remove all copper.

In 2.5	minutes,	0.1060	gram	of copper was	deposited.
“ 5	“	0.1684	“	“	“
“ 7.5	“	0.2078	“	“	“
“ 10	“	0.2402	“	“	“
“ 12	“	0.2458	“	“	“
“ 18	“	0.2487	“	“	“

(Figure 2, Curve 4)

In the presence of five cubic centimeters of acid, twenty-five minutes were necessary in order that the last traces of copper might be removed. The solution did not become colorless until after eighteen minutes.

In 5	minutes,	0.1422	gram	of copper was	deposited.
“ 10	“	0.1943	“	“	“
“ 12	“	0.2140	“	“	“
“ 15	“	0.2334	“	“	“
“ 20	“	0.2451	“	“	“
“ 23	“	0.2476	“	“	“
“ 25	“	0.2484	“	“	“

(Figure 2, Curve 5.)

From a neutral solution of the salt, under the same conditions, the copper was completely precipitated in eight minutes.

CADMIUM.

In the presence of one cubic centimeter of sulphuric acid, **0.25** gram of cadmium was deposited in ten minutes. The current equaled two amperes, and the pressure from 4 to 4.5 volts.,

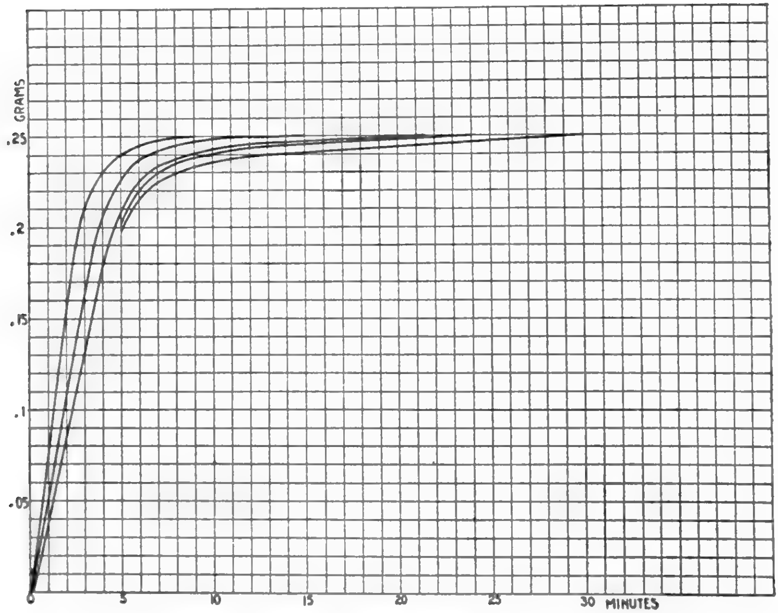

FIG. 3. Cadmium.

In 2.5 minutes, 0.1720 gram of cadmium was deposited.
" 5 " 0.2418 " " "
" 7.5 " 0.2480 " " "
" 10 " 0.2500 " " "

(Figure 3, Curve 1.)

When two cubic centimeters of acid were present, hydrogen sulphide, after fifteen minutes, gave a faint yellow color to the solution from the cell. In eighteen minutes there was no color on testing the liquid in the same way.

In 2.5 minutes, 0.1250 gram of cadmium was deposited.
“ 5 “ 0.2260 “ “ “
“ 7.5 “ 0.2398 “ “ “
“ 10 “ 0.2439 “ “ “
“ 15 “ 0.2480 “ “ “
“ 18 “ 0.2501 “ “ “
(Figure 3, Curve 2.)

Twenty-two minutes were required to precipitate all of the cadmium in the presence of three cubic centimeters of acid.

In 5 minutes, 0.2028 gram of cadmium was deposited.
“ 7.5 “ 0.2260 “ “ “
“ 10 “ 0.2433 “ “ “
“ 15 “ 0.2475 “ “ “
“ 20 “ 0.2486 “ “ “
“ 22 “ 0.2498 “ “ “
(Figure 3, Curve 3.)

When four cubic centimeters of acid were added to the electrolyte, twenty-seven minutes were necessary for the complete precipitation of the cadmium.

In 5 minutes, 0.2023 gram of cadmium was deposited.
“ 10 “ 0.2392 “ “ “
“ 15 “ 0.2491 “ “ “
“ 20 “ 0.2488 “ “ “
“ 25 “ 0.2495 “ “ “
“ 27 “ 0.2505 “ “ “
(Figure 3, Curve 3.)

When five cubic centimeters of acid were present in the solution, at the end of thirty-five minutes there was considerable cadmium undeposited (.003).

In 5 minutes, 0.1974 gram of cadmium was deposited.
“ 10 “ 0.2354 “ “ “
“ 20 “ 0.2419 “ “ “
“ 30 “ 0.2454 “ “ “
“ 35 “ 0.2475 “ “ “
(Figure 3, Curve 5.)

IRON.

Sulphuric acid retards the decomposition of iron sulphate to a far greater extent. Even after forty minutes, in the presence of one cubic centimeter of acid, a trace of iron was held in solution.

In 5	minutes,	0.0858	gram of	iron was	deposited.
" 10	"	0.1204	"	"	"
" 15	"	0.1639	"	"	"
" 20	"	0.2196	"	"	"
" 30	"	0.2439	"	"	"
" 35	"	0.2481	"	"	"

(Figure 4.)

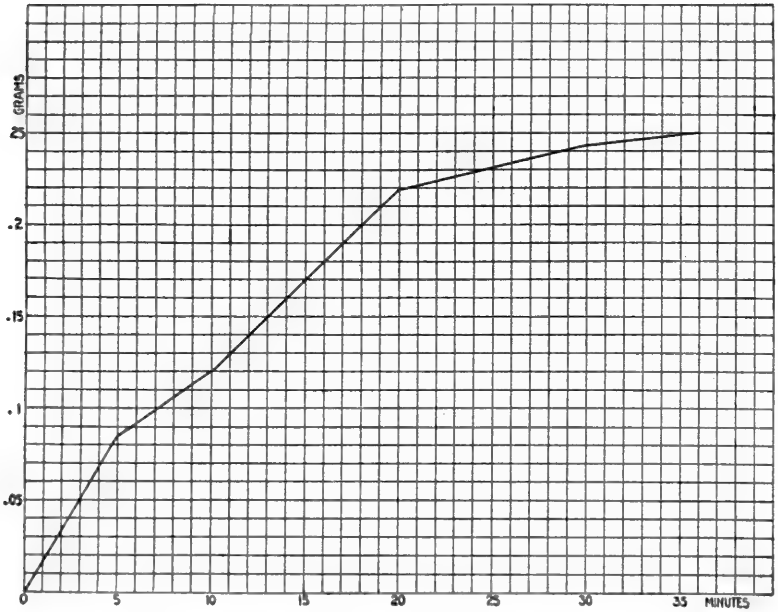

FIG. 4. Iron.

In the presence of three cubic centimeters of acid, in twenty-five minutes, .069 gram of iron was found in the mercury. Four cubic centimeters of acid were not enough to completely hold up the iron.

NICKEL.

In fifteen minutes the nickel solution, containing one cubic centimeter of sulphuric acid, became colorless. It required twenty minutes, however, to remove the last traces of metal.

In 5	minutes,	0.1404	gram of	nickel was	deposited.
" 10	"	0.2070	"	"	"
" 15	"	0.2370	"	"	"
" 20	"	0.2476	"	"	"
" 25	"	0.2511	"	"	"

(Figure 5, Curve 1.)

In the presence of two cubic centimeters of acid, after twenty minutes, the solution gave a brown color after adding ammonium hydroxide and ammonium sulphide. In thirty minutes the test showed that there was still a small amount of nickel unprecipitated.

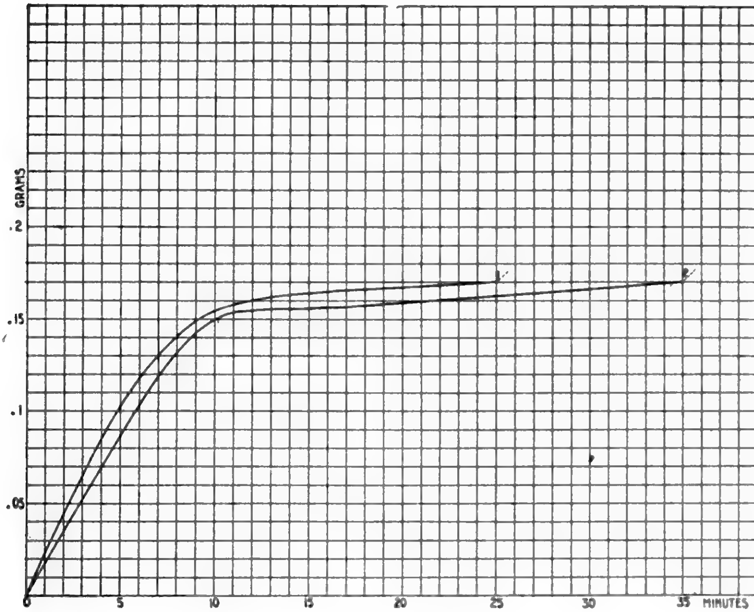

Fig. 5. Nickel.

In thirty-two minutes nickel was found in the solution siphoned from the cup.

In 5 minutes, 0.1265 gram of nickel was deposited.
" 10 " 0.2053 " " "
" 15 " 0.2290 " " "
" 20 " 0.2383 " " "
" 25 " 0.2445 " " "
" 30 " 0.2485 " " "
" 32 " 0.2500 " " "

(Figure 5, Curve 2.)

Cobalt.

The solution of cobalt sulphate contained 0.1700 gram of cobalt. Twenty-five minutes were required for its complete precipitation in the presence of one cubic centimeter of sulphuric acid.

In 5	minutes,	0.1010	gram	of cobalt	was deposited.
" 10	"	0.1539	"	"	"
" 15	"	0.1663	"	"	"
" 20	"	0.1681	"	"	"
" 25	"	0.1700	"	"	"

(Figure 6, Curve 1.)

When two cubic centimeters of sulphuric acid were present, ten minutes additional were necessary for the removal of the cobalt.

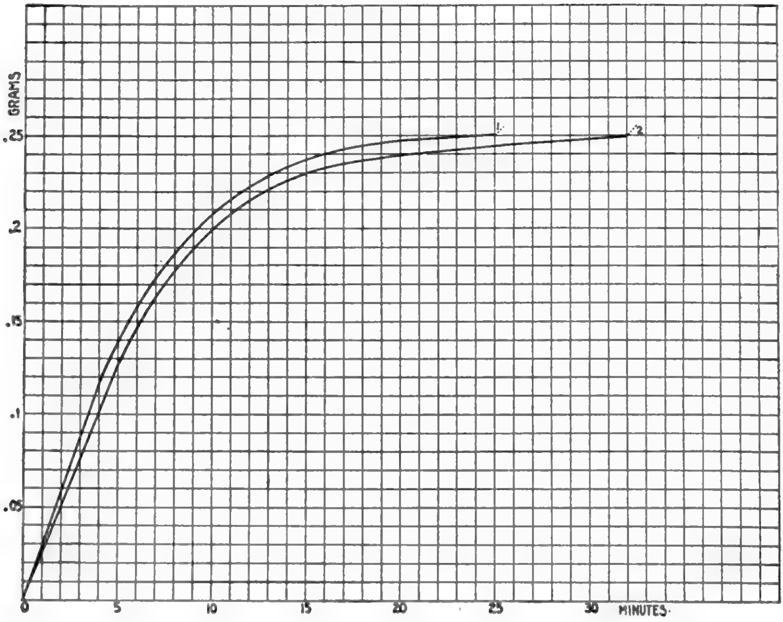

Fig. 6. Cobalt.

In 5	minutes,	0.0859	gram	of cobalt	was deposited.
" 10	"	0.1496	"	"	"
" 15	"	0.1555	"	"	"
" 25	"	0.1669	"	"	"
" 35	"	0.1709	"	"	"

(Figure 6, Curve 2.)

The ease and rapidity with which metals are precipitated upon the mercury cathode naturally suggested the inquiry as to the separations which were possible in this manner. The following lines communicate a few results obtained in this direction. The idea will, however, be pursued in detail as far as it can be carried out.

Cadmium from Aluminium.

The aluminium salt appears to retard the complete deposition of cadmium. In the presence of a few (4) drops of concentrated sulphuric acid with a current of two amperes and seven volts, considerable cadmium remained undeposited after the current had acted fifteen minutes. A higher current was, therefore, employed. The separation was complete in twenty minutes, using a current of three amperes and seven volts. The cadmium in solution was 0.25 gram, while the aluminium sulphate was equivalent to 0.1 gram of aluminium. The total volume of the solution was ten cubic centimeters. The increase in the weight of the mercury in the first experiment was 0.2502 gram and in the second 0.2495 gram.

Cadmium from Magnesium.

This separation was accomplished under the same conditions as the previous separation (cadmium from aluminium).

Conditions.

Volume of solution = 10 cubic centimeters.
Magnesium sulphate ≎ 0.1 gram magnesium.
Cadmium sulphate ≎ 0.25 gram cadmium.
Sulphuric acid = 4 drops (40 drops = 1cc.).
Current = 2–3 amperes.
Pressure = 11–9 volts.
Time = 25 minutes.
Cadmium found (first experiment) = *0.2505 gram.*
Cadmium found (second experiment) = *0.2497 gram.*

Cadmium from Chromium.

Inasmuch as chromium is deposited from its sulphate from slightly acidulated solution, the separation of chromium from cadmium was carried out in the presence of three cubic centimeters of concentrated acid. The conditions were as follows:

Volume = 10 cubic centimeters.
Chromium sulphate ≎ 0.1 gram chromium.
Cadmium sulphate ≎ 0.25 gram cadmium.
Sulphuric acid = 3 cubic centimeters.

Current = 2–3 amperes.
Pressure = 3.5–4 volts.
Time = 25 minutes.
Cadmium found (first experiment) = *0.2499 gram.*
Cadmium found (second experiment) = *0.2492 gram.*

Copper from Aluminium.

This separation was accomplished in the presence of 0.5 cubic centimeters of sulphuric acid (1.1). The current registered 1 ampere and 4 volts. In four minutes the solution was colorless. The current was allowed to act for ten minutes.

Volume of the solution = 10 cubic centimeters.
Copper sulphate = 0.1150 gram copper.
Aluminium sulphate = 0.1 gram aluminium.
Sulphuric acid (1.1) = 0.5 cubic centimeter.
Current = 1–1.6 ampere.
Pressure = 4–4.5 volts.
Time = 10 minutes.
Copper found = *0.1150* gram, *0.1153* gram, *0.1152* gram.

A low current (0.01 ampere and 2 volts) was passed through zinc sulphate in the presence of 0.25 cubic centimeter of sulphuric acid. In twenty minutes the mercury had increased 0.0006 gram in weight.

Iron sulphate containing 0.1110 gram of iron, acidulated with 0.5 cubic centimeter of acid, with a current of 0.01 ampere and a pressure of 2 volts gave in thirty minutes 0.0434 gram of iron. When one cubic centimeter of acid was present, a current of 0.1 ampere with a pressure of 2 volts gave 0.0991 gram of metal.

The separation of copper from zinc was attempted but it was not successful. A copper solution containing 0.1150 gram of copper, in the presence of 2.5 cubic centimeters of acid and a current of 0.6 ampere and three volts, caused the mercury to increase in weight 0.1360 gram, showing that 0.0212 gram of zinc had also been deposited.

The separation of copper from iron was tried, but this, too, failed. With a current (like that given in the preceding paragraph) iron was detected in the mercury, and in thirty minutes the solution still showed the presence of copper. 0.25 cubic centimeter of acid was present. The iron seemed to hold back the copper.

When three cubic centimeters of acid were present in a solution of iron sulphate, containing 0.1 gram of iron and it was electrolyzed with a current of two amperes and four volts, 0.069 gram of iron went into the mercury. The experiment was repeated after the addition of four cubic centimeters of acid. Even this quantity failed to hold up all the iron. The separation, therefore, of iron from copper and of iron from zinc was unsuccessful.

UNIVERSITY OF PENNSYLVANIA.

THE USE OF A ROTATING ANODE IN THE ELECTROLYTIC PRECIPITATION OF URANIUM AND MOLYBDENUM.

By EDGAR T. WHERRY AND EDGAR F. SMITH.

[Contribution from the John Harrison Laboratory of Chemistry.]

(*Read November 3, 1906.*)

I. Uranium.

The early suggestion of Smith (*Am. Ch. Jr.*, 1, 329) that uranium could be completely precipitated by the current from an acetate electrolyte has been amply verified, and the purpose of these lines is only to record the conditions under which the deposition takes place when using a rotating anode. The salt applied in the experiments was uranyl sulphate. The form of apparatus and mode of rotation have been fully discussed in the numerous communications from this laboratory relating to the rapid precipitation of metals. It will not be necessary either, to comment further upon the form in which the uranium is precipitated or upon the subsequent treatment of the deposit. These points have been sufficiently dwelt upon in earlier communications.[1]

The results and conditions are:

No.	U_3O_8 Present in Grams.	Acetic Acid cc.	Sodium Acetate in Grams.	Current in Amperes.	Volts.	Time in Minutes.	Temp.	U_3O_8 Found in Grams.
1	0.1527	0.2	2½	3	14	18	ord.	0.1513
2	0.1527	0.2	4¼	3	12	15	"	0.1525
3	0.2613	0.3	5½	7	15	8	60°	0.2611
4	0.2613	0.25	4½	4	12	3	50	0.0344
5	0.2613	0.25	4½	4	12	15	50	0.0530
6	0.2613	0.25	4½	4	12	10	50	0.1074
7	0.2613	0.25	4½	4	12	18	50	0.1935
8	0.2613	0.25	4½	4	12	25	50	0.2467
9	0.2613	0.25	4½	4	12	30	50	0.2611
			Am. Carbonate in Grams.					
10	0.2613		1	5	15	25		0.2600
11	0.2613		2	5	13	30		0.2613

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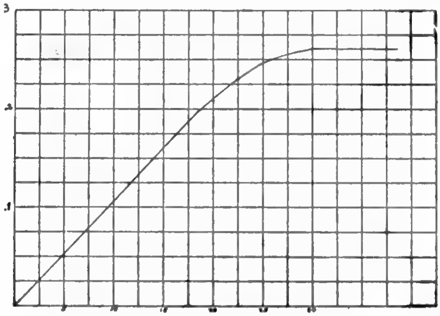
[1] Smith's "Electrochemical Analysis," p. 94.

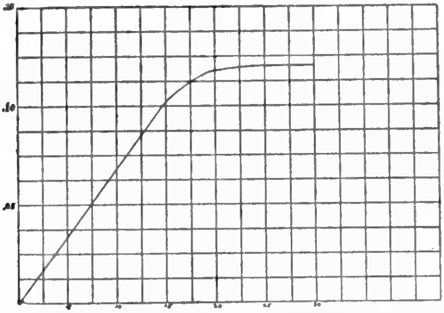
From both of these electrolytes the precipitation is not only complete but exceedingly rapid; hence, this method for the determination of uranium will recommend itself to the mineral analyst. The following curve was plotted from the preceding experiments.



CURVE I. Rate of deposition of Uranium Sesquioxide.

II. MOLYBDENUM.

From acidulated molybdate solutions the hydrated susquioxide is completely precipitated as an adherent deposit on the cathode. This procedure has given an excellent and satisfactory method for



CURVE II. Rate of deposition of Molybdenum Sesquioxide.

No.	MoO_3 Present in Grams.	Dilute Sulphuric Acid (1:10) in c.c.	Potassium Sulphate in Grams.	Current in Amperes.	Volts.	Time.	MoO_3 Found.
1	0.1200	2	1	5	16	30	0.1197
2	0.1200	2	1	5	16	5	0.0335
3	0.1200	2	1	5	16	9	0.0603
4	0.1200	2	1	5	16	15	0.1026
5	0.1200	2	1	5	16	20	0.1190
6	0.1200	2	1	5	16	25	0.1198

the estimation of molybdenum as well as a most gratifying means of separating it from the alkali metals. The conditions were as follows:

From these data the following curve was drawn.

The rapidity with which the oxide separates and the ease with which the determination of molybdenum may be carried out in this way bids fair to render the electrolytic procedure the preferable method with all who acquaint themselves with it.

UNIVERSITY OF PENNSYLVANIA.

A GRASS-KILLING SLIME MOULD.

By JOHN W. HARSHBERGER, Ph.D.

(*Read November 3, 1906.*)

It has been known for a number of years that the club root, or finger and toe disease of cabbage, radish, turnip and other cruciferous plants is due to a slime mould (*Plasmodiophora brassicæ* Wor.) which gains entrance to the root region of the host plant.[1] Each spore gives rise to an amœboid cell which with other similar cells lives in the roots of the host in a truly parasitic manner, finally destroying the cells which it inhabits. These amœboid cells cause great stimulation of the tissues of the host which enlarge to form large swellings, and the plant, as a result, fails to head out. The roots soon decay and the spores which have formed in the cells of the host are again set free. A somewhat similar disease described by Toumey[2] is called crown gall and is found at the base of young cherry, plum, peach and apricot trees throughout the United States. The amœboid cells that are formed from the spores enter the stem tissues of the host at the crown (a point where stem and root join) and produce there hypertrophied tissue known as crown gall. Minced galls, if used to inoculate healthy trees, will communicate the disease. This slime mould which produces true sporangia within the dead tissues of the host differs from the one on cruciferous plants and has been named *Dendrophagus globularis.* In both of these cases, the plasmodium exists in symbiosis with the protoplasm of the host plant as a mycoplasm, and this mycoplasm can be distinguished after treatment with osmic acid, which differentiates it.

On August 11, 1905, my attention was called to a lawn in Cynwyd, Montgomery County, Pa., the grass of which had been destroyed in spots by what the owner, Mr. H. P. Gardner thought

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[1] Frank, Dr. A. B., "Die Krankheiten der Pflanzen," II, 14–18; Sorauer, Dr. Paul, "Handbuch der Pflanzen-Krankheiten," II, 64–72.

[2] Toumey, J. W., "An Inquiry into the Cause and Nature of Crown Gall," Arizona Agricultural Experiment Station Bulletin 33, April 13, 1900.

to be a fungous disease. It seems that the trouble began eight or ten days before the above date after some showers at night, little patches of blackened grass appearing on the lawn in the morning. In a few days, these black patches, if disturbed with the foot or a stick, gave off little clouds of dark brown spores. The original patches were small and few in number, from six to twelve inches in diameter and of irregular shape. The rains and damp weather of early August, 1905, aggravated the injury to the lawn, for the patches spread over much larger areas and covered portions of lawn twenty-five feet in diameter, of irregular outline, with smaller patches scattered in the circumscribed space. The disease not only occurred in Cynwyd, but also on some lawns along the main line of the Pennsylvania Railroad. Subsequently Mr. Gardner informed me that only the blades of lawn grasses were destroyed, for after the disease had disappeared, leaving the aerial green portions of the grasses dead, the grass over the above mentioned patches regained its fresh, bright green color, proving that the plasmodium of the slime mould had not penetrated to the rootstocks (rhizomes) or to the roots.

Specimens were sent to me and an examination showed the presence of numerous sporangia of a slime mould, which I determined to be *Physarum cinereum* Pers. The blades of grass were killed by the plasmodium of this myxomycete spreading across the surface of the lawn. It is a well-known fact that in damp weather a plasmodium may grow with considerable rapidity, so as to cover areas of large superficial extent. Such a plasmodium, when active, constantly advances over the substratum by a slow amœboid movement, assuming as it goes a reticulate appearance with numerous pseudopodia extending in many directions out from the margin. As it moves it incorporates many organic substances as food, as I have previously shown,[1] and the dejectamenta is left behind in the form of a slimy detritus on the surface of the substratum. The plasmodium of *Physarum cinereum* Pers. which caused the destruction of the grass blades is a watery, white one found usually among dead leaves in the woods. In the above instance, it left its saprophytic habit, assuming a grass-killing one. The sporangia which are

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[1] Harshberger, John W., "Observations upon the Feeding Plasmodia of Fuligo septica," *Botanical Gazette*, XXXI, 198–203, March, 1901.

subsequently formed are sessile, subglobose, oblong, scattered or crowded and confluent, 0.3 to 0.5 mm. broad, white or cinereous, more or less warted, or veined. The sporangial wall is membranous with innate clusters of white lime granules. A columella is absent, or it is represented by confluent lime knots. The capillitium consists of branching hyaline threads with numerous white lime-knots varying in size and shape, sometimes confluent in the center, or forming a network with a few hyaline threads. The spores are bright violet-brown, almost smooth, or spinulose, 7 to 10 μ diameter. In conclusion, it may be stated that *Physarum cinereum* Pers. has been collected in England, France, Germany, Natal, Ceylon, Madras, Pennsylvania, Iowa, South Carolina, Cuba and Paraguay, usually on dead leaves. The occurrence of its plasmodium and sporangia on living grass leaves is, therefore, of interest and merits the attention that it has received in the above description of how it has changed its saprophytic habit into a grass-killing one.

Stated Meeting November 2, 1906.

President SMITH in the Chair.

The decease was announced of Mr. Cadwalader Biddle, at Philadelphia on October 28, 1906, æt. 69.

The following papers were read:

"The Decorative Art of British New Guinea," by DR. ALFRED C. HADDON, F.R.S., which was discussed by MR. LESLIE M. MILLER.

"The Effect of Sulphuric Acid on the Deposition of Metals when using a Mercury Cathode and Rotating Anode," by LILY G. KOLLOCK and EDGAR F. SMITH. (See page 255.)

"The Use of a Rotating Anode in the Electrolytic Precipitation of Uranium and Molybdenum," by EDGAR J. WHERRY and EDGAR F. SMITH. (See page 268.)

"A Grass-killing Slime Mould," by DR. JOHN W. HARSHBERGER. (See page 271.)

THE CAUSE OF EARTHQUAKES, MOUNTAIN FORMATION AND KINDRED PHENOMENA CONNECTED WITH THE PHYSICS OF THE EARTH.

By T. J. J. SEE, A.M., Lt.M., Sc.M. (Missou.), A.M., Ph.D. (Berol.),

Professor of Mathematics, U. S. Navy, in Charge of the Naval Observatory, Mare Island, California.

(*Read October 19, 1906.*)

I. General Considerations on the Cause of Earthquakes.

§ 1. *Introduction.*

The great San Francisco earthquake of April 18, 1906, presented certain remarkable characteristics which immediately became a subject of investigation on the part of men of science resident in this part of the United States. One very striking feature of this earthquake was the conspicuous rotatory motion of the earth particle; and another was the long duration of the disturbance. The rotatory motion appeared so remarkable and so difficult to reconcile with theories very generally held by geologists and seismologists that it seemed worth while to make a somewhat comprehensive survey of the general subject of earthquakes, in the hope of reaching a better understanding of the cause of these phenomena. And as the details of this particular earthquake will be fully treated by others,[1] the result of the present inquiry[2] into the physical cause of earthquakes

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[1] The Committee of Investigation appointed by the Governor of California: Professors A. C. Lawson, George Davidson, A. O. Leuschner, G. K. Gilbert, W. W. Campbell, H. F. Ried, J. C. Branner, Chas. Burkhalter. Investigations are being made also by Professor Omori of the Imperial University of Tokio, Messrs. Otto Von Geldern, Luther Wagoner, and Mr. Hoehl of the American Society of Civil Engineers, and perhaps by others.

[2] Rear Admiral H. H. Rousseau, U. S. Navy, Chief of the Bureau of Yards and Docks, has read this paper throughout, and made a number of suggestions which proved valuable. The independent judgment of an experienced engineer was felt to be no inconsiderable advantage in weighing some of the difficult questions here treated, and my most cordial acknowledgements are due to Rear Admiral Rousseau for his great kindness.

and other related phenomena may not be without interest to investigators of the physics of the earth.

Almost exactly four months after the earthquake of April 18, namely, August 16, 1906, another, much more terrible, laid waste Valparaiso and the surrounding cities of Chili, producing scenes of desolation which are rare—even in South America. The scientific need and the humanitarian demand for an investigation of the cause of these disturbances could, therefore, hardly be greater than it is at the present time. But if it be said that the researches of science are powerless to stay the hand of the destroyer, and only the laws of these terrible phenomena can be discovered, yet even the intelligent appreciation of natural laws may greatly mitigate the extent of the disaster and suffering which follow; and on both humane and scientific grounds, the prospects of extending the domain of useful knowledge furnish a high inspiration for earnest endeavor to penetrate the mystery of these hidden forces of nature, which so long have baffled the skill of philosophers.

Earthquakes and volcanoes were among the earliest physical phenomena to receive the attention of the ancients, and they have always occupied a prominent place in natural philosophy. Although the importance of the subject was derived originally from the terrible disasters which these mysterious agencies of unknown forces occasionally inflict upon large portions of mankind, in more recent times earthquakes have been studied also as about the only available means of throwing light upon the physics of the globe. No artificial forces at the command of the experimenter are great enough to produce vibrations of the earth's crust or to transmit them through the body of the planet when once established in the surface layers.

But notwithstanding all the labor and research which has been bestowed upon the subject, it can hardly be said that we yet have any satisfactory theory of the cause of these phenomena. This is the more regrettable, because, on the one hand, it places it beyond the power of science to predict earthquakes, or even to foretell the regions of their occurrence, which might afford some measure of security to life and property; while, on the other, it leaves many men of science without adequate hope that the true cause of these phenomena will ever be discovered, and at the same time so completely bewildered by a multitude of unsatisfactory theories that the progress of discovery itself is seriously embarrassed.

There will naturally be those who doubt the existence of one common and universal cause of earthquake and volcanic phenomena. Nevertheless, difficult as the subject is, we believe that such a cause exists, and that it is capable of demonstration, if not with mathematical rigor at least with such high degree of probability[1] as to render the resulting theory practically useful, and we ask nothing of the reader except a careful examination of the facts as interpreted in the light of the cause assigned in this paper. If such a view, associating the varied phenomena of earthquakes and volcanoes, with mountain formation and the development of great sea waves, under one common cause, renders them more intelligible, and enables us to see the relations of all the observed phenomena in a clearer and simpler light, there will be presumptive evidence of the truth of the proposed theory; and the probability of its correctness will increase with the harmony existing among all the known facts, and the effectiveness with which contradictions of other theories may be established. The final test of the theory will depend upon its usefulness in the advancement of discovery, so as to harmonize the whole body of earthquake and volcanic phenomena, including those associated with the origin and structure of mountains, the observations of geodesy, and of great sea waves, in their mutual relations, and in respect to the undisturbed parts of our globe. If the theory shall meet this test satisfactorily, we may feel confident that it assigns the true cause of the phenomena, and within certain limits the resulting laws of nature may be used to foretell events which will contribute to the repose and safety of mankind, and to the progress and usefulness of discovery in this interesting branch of natural philosophy.

§ 2. *The dynamical cause of earthquakes and volcanoes probably*

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[1] The unequivocal proof of the elevation of the coast at Yakutat Bay, Alaska, Sept. 10–15, 1899, seems to remove the last trace of uncertainty regarding the chief function of earthquakes, and makes the demonstration as rigorous as that of any theorem in geometry. See the important memoir of Tarr and Martin, Bulletin of the Geological Society of America, vol. 17, May, 1906. Professor George Davidson, President of the Seismological Society of America, kindly called my attention to this classic work after the present investigation was finished. Note added December 12, 1906.

depends upon the explosive power of steam formed within or just beneath the heated rocks of the earth's crust chiefly by the leakage of the ocean beds.

Some of the most complicated phenomena in nature depend upon the simplest and most obvious of causes, but there are several reasons why the true cause often proves very difficult to discover. On the one hand our mental operations are not infrequently thwarted by conflicting prejudices and contradictory theories, so that attention is diverted from the real questions; and, on the other, our clearness of vision and power of intuition are blinded by the very closeness and familiarity of the true cause, which is least suspected. Success in interpreting nature depends upon a combination of the proper elements of thought into one simple connected view which deals not with details but with the general tendencies. In the case of earthquakes and volcanoes this general view has been very difficult to obtain; and with the growth of elaborate scientific investigation and classification of earthquakes the difficulty has increased rather than diminished. For attention has been given to the attainment of high accuracy in the measurement of tremors by seismographs and other apparatus, and investigators have been occupied with the registration and discussion of the details of phenomena rather than with the general underlying causes.

We shall hereafter examine the porosity of matter and the problem of the penetration of water into the rocks of the earth's crust, both from the experimental and historical standpoints, but let us first consider the probable state of the internal heat of the earth. In *Astron. Nach.*, No. 4053, the writer has shown that when we consider the force of gravity alone, and suppose a body to be made up of gas reduced to the state of single atoms, over one-half of the primordial supply of heat is stored up within the condensing mass, while still in the gaseous stage; and in a later paper on the rigidity of the heavenly bodies (*A. N.*, 4104), it is shown that circulation and radiation become retarded and greatly restricted with increasing density, so that in the later stages of the development of a mass like the earth, much more than one-half of the heat generated is retained within the mass for raising the temperature. It is shown that all the heat of our earth depending on gravitation would raise the temperature of an equal mass of water 9954° C.; and as decidedly more than half of it is still retained in the globe, we may conclude that the internal heat of the earth is ample to raise the whole globe to a temperature of something like 20,000° or 25,000° C., according to the average specific heat of the earth's matter. If radium and other related elements exist within the earth in appreciable quantities, the amount of heat stored up, as Sir G. H. Darwin and others[1] have remarked, may be vastly greater yet. Now it is recognized that the crust or cooled layer on the outside of our planet is extremely thin, and we know that the temperature increases downward at an average rate of something like 1° C. for each 30 metres of descent. This accords also with Lord Kelvin's calculations on the cooling of a molten globe, carried out in conformity with Fourier's Analytical Theory of the propagation of heat in solid bodies.[2]

From this we may infer, as geologists have long since remarked, that, even without the penetration of steam, molten rock would be encountered at a depth of decidedly less than 30 kilometres. As the percolation of hot water and steam appreciably lowers the melting point of silicious and perhaps other rocks (the lavas are mainly silicates), and itself develops at the very low temperature of only 100° C. under atmospheric pressure, we may infer that it would form in the earth at a depth much smaller than 30 kms. At no more than 10 or 15 kms. under the ocean beds large quantities of it might be produced and give rise to imprisoned forces of tremendous power. Besides it would rapidly absorb and spread in the hotter layers of rock beneath, just as in the case of gases absorbed in hot steel, cited by Tait and quoted in § 5. That this absorption actually takes place is proved by the vast clouds of steam given off by melted lava after it pours from a volcano, such as Vesuvius.

We are thus confronted with the following situation:

The internal temperature of the earth is extremely high, with

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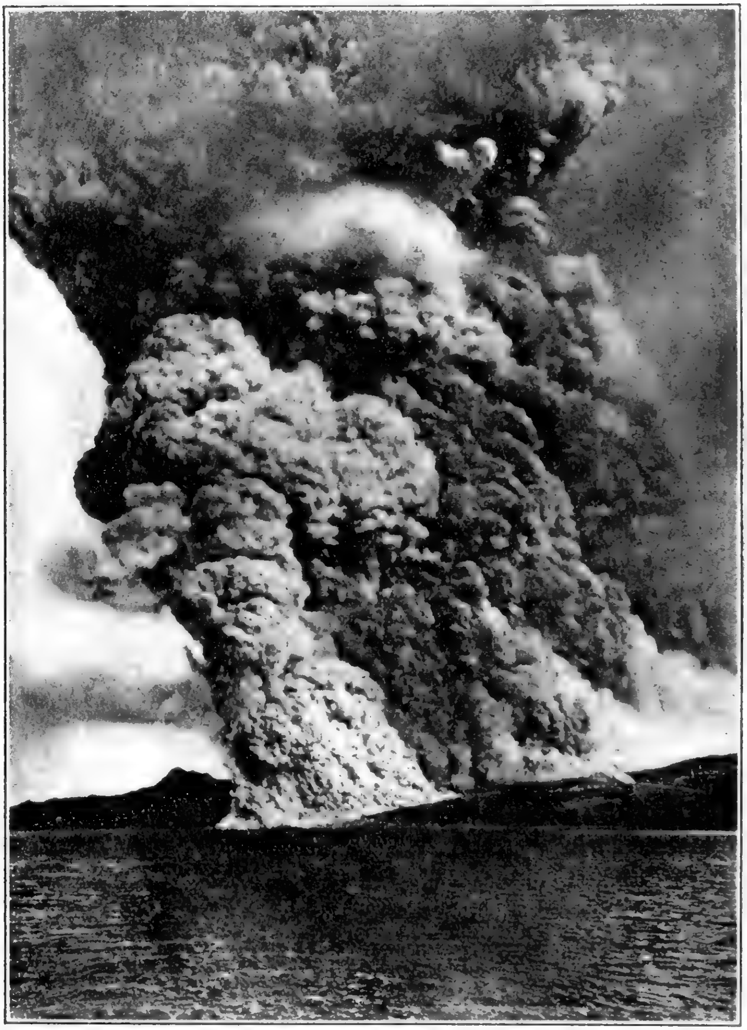
[1] Presidential address to the British Association for the Advancement of Science, Capetown, 1905; also a very recent paper presented to the Royal Society, April 5, 1906, by the Hon. R. J. Strutt, F.R.S., reported in *Nature* of May 17, 1906.

[2] "The Secular Cooling of the Earth," Appendix D, Thomson and Tait's "Nat. Phil."

FIG. 1.



Mt. Pelée. The burning cloud of December 16, 1902, seen from the sea. (From the Belgian Astronomical Society, tenth year. Plate V.)

heated rocks quite near the surface, while the crust is fractured and leaky everywhere, and especially where the depth of the sea is greatest. The sea covers three-fourths of the earth's surface, and earthquakes are found to be most violent where the sea is deepest.

and volcanoes most numerous on the adjacent shores. Could then anything be more probable than to suppose that both of these great natural phenomena depend simply and wholly upon the explosive power of steam which has developed in the heated rock of the earth's crust?

The mere statement of the facts seems almost enough to convince one of the truth of this theory. But in view of the wide differences of opinion heretofore prevailing we shall examine it in detail, and we believe it will be possible to show that no contradiction can be established, and that it probably is the correct explanation of the mysterious forces which have so long baffled investigators and wrought such havoc in numerous places throughout the world.

It would seem that the obvious fact of the leaky character of the sea bottom, covering three-fourths of the earth, with great internal heat everywhere so close beneath and volcanoes not only abundant on the shores adjacent to the deepest seas, but pouring forth vast quantities of steam when in eruption long ago suggested and apparently ought to have convinced investigators of the validity of this natural and simple explanation. But it appears to have been generally rejected, owing to several circumstances which did not enable investigators to obtain the proper point of view. On the one hand there were traditional theories of volcanoes and their relations to a supposed liquid or molten globe; and on the other little or no adequate knowledge of the enormous number and great violence of submarine earthquakes, which have recently been shown, mainly through the important researches of Professor Milne, to be the most powerful in the deepest oceans.

While volcanoes and earthquakes have been associated from the time of Aristotle and Pliny, and we think justly so, and some mutual connection could hardly be denied; yet even after this relation was especially affirmed by great original investigators like Humboldt and Charles Darwin, it has unfortunately become customary of late years to class earthquakes as volcanic and tectonic or structural. Instead of viewing volcanoes as outlets of pent-up-steam, which blows out if possible the molten rock in which it develops—a clear indication of every great eruption—an effort was made to explain earthquakes as volcanic, with only partial success, whereas both phenomena depend upon the common cause of steam pressure formed deep in the earth's crust, principally by the leakage of waters from the sea. This highly explosive agency is developed so abundantly in the infinitely thin crust between the underlying molten globe, and the overlying oceans, the outcome of a fire beneath and of water above, as in a boiler, that one should not wonder at terrible explosive or eruptive phenomena appearing upon our planet. Considering the vast extent of the oceans it would be strange indeed if something like volcanoes and earthquakes were not inseparably associated with the very nature of the terrestrial spheroid.

If we consider with attention the various causes which might be assigned to explain earthquakes and volcanoes, taking into account their recognized geographical distribution and relation, the relative situation of the inner globe of fire and the overlying layer of water separated from it only by the thin and leaky bottom of the sea, and remembering that both phenomena are augmented to the maximum in regions characterized by high mountains near the deepest oceans, as on the west coast of South and Central America, the Aleutian and Kurile Islands, Japan, Sumatra, Java and other islands of the East Indies, bordering on the deep waters of the Indian Ocean, New Zealand, and the Lesser Antilles in the West Indies, Iceland, Italy, Greece, etc., we shall find the probabilities that steam pressure developing in the earth's crust is the true and common cause both of earthquakes and volcanoes, are as infinity to one against any other conceivable cause, or all other causes combined. The widely extended relationship here pointed out is so intimate and everywhere so confirmatory of the theory that we cannot suppose it to be due to chance.

§ 3. *Views of Professor Milne and his methods of analysis.*

It has been justly remarked by many seismologists that the greatest belt of earthquakes surrounds the Pacific Ocean. Now each part of this great "fire girdle of volcanoes" with innumerable earthquake disturbances has been studied with care by one or more investigators. Without going into the detailed methods of recording and charting developed by Professor Milne, Professor Ewing, Dr. Davidson, Major De Montessus de Ballore, Dr. Agamennone, Dr. Cancani, Dr. Vicentini, Grablowitz, Omori, Koto, Nagaoka, and others, which are of great value for the close study of particular regions, we may call attention to the conclusions of Milne, and Montessus de Ballore regarding the slope of the seashores as important factors in the development of earthquakes.

As a result of careful study of Japanese earthquakes covering eight years, Professor Milne found that "the central portions of Japan where there are a considerable number of active volcanoes are singularly free from earthquakes. The greater number of disturbances originate along the eastern coast of the Empire and many of them have a submarine origin." "Lines 120 geographical miles in extent in running in an easterly or southeasterly direction from the highlands of Japan into the Pacific Ocean, like similar lines drawn from the Andes westwards into the same ocean, have a slope of 1 in 20, or 1 in 30, and in both of these districts earthquakes are frequent. On the contrary, along the faces of flexures which are comparatively gentle, being less than half of these amounts, which may be seen along the borders of most of the continents and islands of the world, earthquakes are comparatively rare. The inference is that where there is the greatest bending it is there that sudden yielding is most frequent."[1]

It seems advisable to quote more at length the full line of thought laid down by Professor Milne in his classic work on "Seismology" (London, 1898). On page 31 Professor Milne says:

> "A very much more serious objection to the volcanic origin of the majority of earthquakes is the fact that these disturbances are common in the Himalaya, Switzerland, and other non-volcanic regions. The destructive earthquake in 1891 in Mino and Owari occurred in a region of metamorphic and stratified rocks. Again, an analysis of some ten thousand earthquake observations of Japan shows that there have been but comparatively few which had their origin near to the volcanoes in the country. The greater number of this series originated beneath the ocean or along the seaboard, and as they radiated inland they became more and more feeble, until, on reaching the backbone of the country, which is drilled by numerous volcanic vents, they were almost imperceptible. Beyond this central range of mountains, earthquakes are only rarely experienced, and what is true of Japan seems to be generally true for the coasts of North and South America."
>
> "Throughout the world we find that seismic energy is most marked along the steeper flexures in the earth's crust, in localities where there is evidence of secular movement, and in mountains which are geologically new

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[1] *Cf. Seismological Journal of Japan*, 1895, p. xv; and Dutton, "Earthquakes in the Light of the New Seismology," chapter III.

and where we have no reason for supposing that brady-seismic movements have yet ceased.

"As examples of the flexures to which reference is here made, we may take sections running at right angles to the coast lines of the various continents. The unit of distance over which such slopes have been measured is taken at two degrees, or one hundred and twenty geographical miles.

"The following are a few of such slopes:

West Coast, South America, near Aconcagua	1 in 20.2	Seismic districts.
The Kurils from Urup	1 in 22.1	
Japan, east coast of Nipon	1 in 30.4	
Sandwich Islands, northwards	1 in 23.5	
Australia generally	1 in 91	Non-seismic districts.
Scotland from Ben Nevis	1 in 158	
South Norway	1 in 73	
South America, eastwards	1 in 243	

"The conclusion derived from this is that if we find slopes of considerable length extending downwards beneath the ocean steeper than 1 in 35, at such places submarine earthquakes, and their accompanying landslips may be expected. On the summit of these slopes, whether they terminate in a plateau or as a range of mountains, volcanic action is frequent, while the earthquakes originate on the lower portions of the face and base of these declivities. Districts where earthquakes, often followed by submarine disturbances, are most frequent are regions like the northeast portion of Japan and the South American coast between Valparaiso and Iquique. Here we have a double folding. The sea bed, as it approaches the shore line, instead of rising gradually, sinks downward to form a trough parallel to the coast, after which it rises to culminate in mountain ranges. The South American trough which lies within fifty or sixty miles off the coast, like the Tuscarora deep off Japan, attains depths of over four thousand fathoms, and the bottoms of these double folds are well known origins of earthquakes and sea waves."

Professor Milne then goes on to show that where secular movements are active, "the forces which have brought these mighty folds (mountains) into existence have not yet ceased to act." The most important question of all, however, is what are these forces? He says they appear where "mountain formation is geologically of recent origin," and adds:[1]

"The conclusion to which such observations lead is that wherever we find in progress those secular movements which result in the building up of countries or mountain ranges, there we should expect also to find a pronounced seismic activity. Thus, while admitting a few small earthquakes to be volcanic in their origin, we recognize the majority of these disturbances as 'the result of the sudden fracturing of the rocky crust under the

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[1] "Seismology," by John Milne, F.R.S., p. 33.

influence of bending. The after-shocks which so frequently follow large earthquakes announce that the disturbed strata are gradually accommodating themselves to their new position."

Professor Milne's statement that "the greater number of this series (10,000 Japanese earthquakes) originated beneath the ocean or along the seaboard, and as they radiated inward they became more and more feeble, until, on reaching the backbone of the country, which is drilled by numerous volcanic vents, they were almost imperceptible" seems to point directly to the cause set forth in this paper. If earthquakes depend upon the explosive power of steam, they ought not to be numerous near the volcanoes (unless these vents get stopped up), but they ought to be very numerous under the sea in the deep trough just east of Japan, which he says is found to be true by laborious and extensive observations covering a vast number of these phenomena. In order to leave no doubt as to the significance of these results we shall consider also the other lines of thought which he has worked out with so much care.

§ 4. *Inadequacy of the tectonic theory based on slipping and bending, and dislocational and fault movements.*

At present we shall not touch upon all the questions discussed by Professor Milne, but we may remark that slopes of 1 in 20 given above probably are not great enough to produce the least slipping, or fracturing or bending of rocks. The most effectual way to convince ourselves of the truth of this view is by an appeal to the cones of actual volcanoes. Take Mount Cotopaxi, for example. It is one of the tallest active volcanoes in the world, and the most regularly built of all the large volcanoes. The slope is 30°, the angle of the apex being 120°. A slope of 30° corresponds to 1 in 1.732; and thus Professor Milne's ratio of 1 in 20 is less than one-tenth that required to produce stability; and it has escaped his notice that slopes steeper than 1 in 35 are not such that the steepness could give rise to submarine earthquakes and their accompanying landslips. If the cones of volcanoes like Cotopaxi do not slip, when they are more than ten times steeper than the steepest sea slopes, and over twenty times that mentioned as unstable by Professor Milne, why should slips occur under the sea? Obviously the steepness, though no doubt considerable in certain places, is not the cause of earthquakes. Rains, snows and glaciers on Mount Cotopaxi ought to produce slipping of rocks, if anywhere, because the angle is steep and the material loose and unsettled. We are not aware that the slipping of any volcanic cone or other similar mountain has ever been observed to produce a real earthquake; and if slipping were the order of nature, we should expect some enormous slips with corresponding tremors due to this cause near Cotopaxi, Aconcagua, and other great volcanoes (especially when these mountains are shaken at the times of eruption), which are not observed.

We seem compelled therefore to abandon the theory of slipping and bending of rocks[1] except as producing all the time infinitesimal tremors called microseisms, which very likely depend to a considerable extent on this cause. Glaciers are known to be fluid masses, and they move accordingly, though very slowly. It has been shown by the writer (in *Nature*, 1902) that a rock such as marble undergoes secular bending, and is therefore fluid; and we take it that all large rock masses are very similar in their behavior, though their viscosity may be and generally is greater than that of marble; and hence if movement of mountain masses or other large rocks take place, it would seem that, wherever possible, it should be by a very gradual yielding. The cases in which very large masses of rock, like the sides of a mountain, acquire such unstable positions as to fall, do not seem to be very numerous.[2] Accordingly, it is difficult to believe that this cause is very effective in producing earthquakes; for such shocks as might result from it would be rare, small and unimportant. And moreover they could never occur where the average slope is anything like so small as 1 in 20. Besides the arguments here outlined there is another hardly less effective which we shall merely mention, namely: That the forces which may shake an entire continent and send waves of compression and

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[1] This hypothesis was originally proposed by Boussingault, from observations made on earthquakes noticed in the Andes remote from known volcanoes, and has at length developed into the tectonic theory now widely held by seismologists and geologists.

[2] The movement by sliding of one or two mountains in the Alps is recorded within the historical period. Among the Andes the most noted change is the collapse of the crater of Carihuairazo, adjacent to Chimborazo, during a violent earthquake on the night of 19–20 of June, 1698. Before this disaster Carihuairazo is said to have been taller than Chimborazo.

distortion through all the rocks between the two oceans, and disturb the whole earth, are not produced by so small a cause as the slipping and bending of ledges of rock.

Humboldt and Charles Darwin long ago associated earthquakes with secular elevations and depressions, and it is noticeable that Professor Milne likewise thinks these disturbances occur with increased frequency in regions where such changes are still in progress.

Montessus de Ballore concluded from his elaborate study of statistical data that in adjacent seismic regions, instability of the earth is increased by differences of topographic relief; and that the unstable regions are associated with the greatest lines of corrugation of the earth's crust. Like Professor Milne, he observes that rapidly deepening shores which slope gently, especially if they are the continuations of flat or moderately falling coast plains, are stable. His results are illustrated by steep regions of the seashore in South America, Japan, and other parts of the world, and by other regions where the slope into the sea is more gradual.

These views and others of similar tenor by several investigators have led some geologists and seismologists to conclude that many of the earthquakes noticed along shores which are steep are due to the sliding of unstable deposits of sediment settling on the rock slopes. But if we recall, as above, the smallness of these slopes, even where the descent is most rapid—it never exceeds that of our mountains upon the land, and is seldom as steep,—and observe that the surrounding sea water is quiescent and would both greatly buoy up and resist the motion of any supposed sliding deposit, so that it is doubtful if appreciable sliding really takes place, and certain that if it does occur the effect in disturbing the earth would be very slight, we shall find it difficult to believe that the theory is well founded. It appears that such a deposit, resisted by the surrounding water, would slide with extreme slowness, and settle gently without any appreciable jar, and consequently no earthquake of importance could be produced in this way.

II. ON THE POROSITY OF MATTER AND ON THE LEAKAGE OF THE OCEAN BOTTOMS.

§ 5. *On the porosity and penetrability of matter under the enormous fluid pressure operating in the deepest oceans, and the underlying crust of the earth.*

Somewhat extensive researches on the internal pressures, constitution, and rigidities of the sun and planets, carried out during the past two years and published in the *Astronomische Nachrichten,* have led the writer to the conviction that many of the laws of matter depending on molecular forces, such as impenetrability and solidity, are quite inapplicable to the conditions prevailing in the interior of the earth and other bodies of our solar system; that under the immense pressures there operating, whatever be the temperatures, but especially under the high temperatures known to prevail in the interior of these masses, the hardest natural bodies would yield like sponges, and admit of the most perfect interpenetrability of all the elements. The conclusion was reached from the study of forces of somewhat impressive magnitude that all matter is enormously porous, and quite leaky under forces much smaller even than those operating in the interior of the earth; so that solidity and impenetrability, long held to be among the most universal properties of matter, far from being absolute, appeared to be very relative properties, appropriate to very small, but wholly inappropriate to large, forces, and sometimes set aside by the direct evidence of our senses in common laboratory experiments.

There doubtless are many experiments which would enable us to appreciate the significance of these general principles in specific cases, but it will suffice to recall one close at hand, and directly connected with the question under discussion. In the series of soundings of the depths of the sea carried out some years ago by certain officers of the United States navy occupied with hydrographic and ocean surveys it was found that hollow glass balls with walls several centimetres thick, when subjected to increasing pressure at various depths, came up more and more completely filled with water, in proportion as the depth increased, though no fracture of the glass had occurred, and no holes in it could be discovered by examination of the surface under the highest microscopic power.

After a careful inquiry by many experienced physicists the conclusion was reached that the water had been forced slowly but bodily through the thick walls of the glass under a pressure of less than 1,000 atmospheres, in an interval of less than an hour's time.

In the year 1661 a well-known experiment was made by the Florentine academicians who forced water through the solid walls of a sealed hollow sphere of gold, and other metals, by changing the shape of the sphere under mechanical applications of pressure, so as to diminish the volume. The present case of the porosity of glass was thus verified from the opposite point of view, by the steady application of external fluid pressure, on the spherical surfaces of glass balls sent down in modern soundings of the ocean depths.

The great porosity of all matter has of course long been recognized by physicists, but we are so accustomed to dealing with small forces and the resulting doctrine of the impenetrability of matter that it is doubtful whether our appreciation of this fact has yet passed beyond the academic stage. In his well-known "Properties of Matter," fourth edition, p. 87, Tait says:

> "The porosity of wood, necessary for the circulation of sap, is beautifully shown by the fact that, from microscopic examination of a thin slice of fossil tree, a botanist can tell at once the species to which it belonged. The greater part of the material of the wood has disappeared for it may be millions of years, but its microscopic structure has been preserved by the infiltration of silicious or calcareous materials which, hardening in the pores, have thus preserved a perfect copy of the original. The rapid passage of gases through unglazed pottery, iron and (hot) steel, etc., shows the porosity of these bodies in a very remarkable manner. So does the strange absorption of hydrogen by a mass of palladium. The porosity of steel has recently been shown in a most remarkable manner by Amagat, who forced mercury through a thickness of more than three inches under a presure of at least four thousand atmospheres. The metal was quite impervious to glycerine under the same pressure."

At the time this passage was written, some twenty years ago, Tait remarked that decisive proof of the porosity of vitreous bodies, such as glass, had not yet been obtained, but added "that they form almost a solitary class of exceptions to an otherwise general rule seems highly improbable." He then proceeded to show that all bodies whatsoever must necessarily be porous and leaky when subjected to great fluid pressure, and he pointed out that the penetrability depended greatly on the character of the fluid, thus indicating the great influence of molecular and atomic forces.

To make a practical application of these principles, what shall we now say with respect to the ocean bottoms? In deep places the pressure of the sea water upon them is very great, sufficient to force the water through walls of solid glass several centimeters thick in a short time, and the bed itself in general no tighter than that of a pond in a common field. Obviously, most of these bottoms will leak, and leak at a rapid rate under the enormous pressure operating in the greatest depths of the sea. The bed of the ocean will not leak with equal rapidity in all places, but almost universal leakage will certainly develop; and the water will be driven down into the earth at various rates depending upon the fluid pressure and temperature and the physical character of the sea bottom. Where the rock is volcanic, and badly fractured, or sandy, the leakage will be most rapid, and where the bed is made of fine clay or unbroken granite, the leakage will be much more gradual. It will also depend directly on the depth of the sea, being a maximum where the ocean is deepest, and generally quite insignificant in shallow water. The amount of water leaking through any square meter of the sea bottom will be given by an expression of the form

$$w = P.\ p.\ f(t).\ \phi(T),$$

where P is the fluid pressure in the bed of the sea, and thus directly proportional to the depth; p the average porosity of the ocean bottom, and thus depending on the kind of ooze, dust, sediment and rocks underlying the sea and their state of compression; and $f(t)$ is some function of the time, depending on the average rate of leakage through the successive strata; and $\phi(T)$ is a function of the temperature, and thus increasing with the descent into the rocks of the earth's crust. As water is almost incompressible for small or moderate forces, its escape downward would depend upon the continued descent of that which first entered the bed of the ocean, the rate of which would be diminished under the increasing pressure and density encountered in the lower strata, but on the other hand increased by the rising temperature which makes the rocks more penetrable and also augments their power of absorption. Various values of these quantities, P, p, $f(t)$, $\phi(T)$, would give the several rates of leakage for the corresponding areas of the bottom of the sea. In general it is obvious that the leakage will be most rapid where the sea bottom is fractured or porous, the underlying temperature high, and the depth very great. A rapid rate of leakage would imply that large quantities of water quickly come in contact with the heated rock and develop correspondingly great steam pressure in the crust which underlies that part of the ocean. Tait's remark about the rapid passage of gases through hot steel obviously applies to the absorption and diffusion of steam in hot rock; for this is found by experiment to be quite general for many of the metals. And in the case of lava as it pours from a volcano, it is observed that the molten rock emits vast quantities of vapor, of which, according to Sir Archibald Geikie, 999 parts in 1,000 is steam. This fact in itself is extremely impressive; for it indicates that the remaining thousandth part of the gases emitted, including vapors of sulphur, hydrogen sulphide, hydrochloric and carbonic acid, are derived from the rocks of the earth's crust under the action of steam and the high temperature. We may therefore consider that steam is the only original vapor operating in the crust of the earth.

§ 6. *Daubrée's experiments on the effects of capillarity.*

After this paper was fully outlined and some references were being verified, the author had the good fortune to notice the following significant statement in Sir Archibald Geikie's admirable "Text Book of Geology," fourth edition, 1903, p. 354:

> "An obvious objection to this explanation is the difficulty of conceiving that water should descend at all against the expansive force within. But Daubrée's experiments have shown that, owing to capillarity, water may permeate rocks against a high counter pressure of steam on the further side, and that so long as the water is supplied, whether by minute fissures or through pores of the rocks, it may, under pressure of its own superincumbent column, make its way to highly heated regions. Experience in deep mines rather goes to show that the permeation of water through the pores of the rocks gets feebler as we descend."

In his "Physics of the Earth's Crust," second edition, p. 144, Rev. O. Fisher also makes some interesting remarks on Daubrée's experiments, which are included in his "Rapport sur les progrès de la Géologie expérimentale," Paris, 1867. After describing Daubrée's experiment, Rev. Fisher remarks:

M. Daubrée conceives that if the layer of rock were of great thickness, and a very high temperature maintained in the cavity, a correspondingly high steam pressure would result, which would be sufficient to raise lava in the vent of a volcano, and to produce earthquakes; while the force so obtained might after expenditure be again and again renewed.

"This theory requires the occurrence of cavities at great depth ('supposons une cavité separée des eaux de la surface') communicating with the volcanic vents. But the only argument in favor of cavities existing seems to be that the requisite mechanical force is obtainable by means of them; but it seems *à priori* impossible that there should be such cavities."

These passages are of interest in connection with Part VIII. of this paper, where it is shown that such cavities or partial cavities develop from the expulsion of lava from under the bed of the sea, and the resulting subsidence of the bottom causes the great sea waves which so frequently follow violent earthquakes.

§ 7. *Historical development of the theory of the penetration of sea water.*

Although these passages were found too late to have influenced the theory developed in this paper, they are cited here for convenience, and to show some of the historical aspects of the problem of the penetration of sea water. It was much discussed also in Humboldt's time, as we learn from his remarks in the *Cosmos:*

"The geographical distribution of the volcanoes which have been in a state of activity during historical time, the great number of insular and littoral volcanic mountains, and the occasional, although ephemeral, eruptions in the bottom of the sea, early led to the belief that volcanic activity was connected with the neighborhood of the sea, and was dependent upon it for its continuance."

"For many hundred years," says Justinian, or rather Trogus Pompeius, whom we follow, "Etna and the Eolian islands have been burning, and how could this have continued so long, if the fire had not been fed by the neighboring sea?" In order to explain the necessity of the vicinity of the sea, recourse has been had even in modern times, to the hypothesis of the penetration of sea-water into the foci of volcanic agency, that is to say, into deep-seated terrestrial strata. When I collect together all the facts that may be derived from my own observations and the laborious researches of others, it appears to me that everything in this involved investigation depends upon the questions whether the great quantity of aqueous vapours, which are unquestionably exhaled from volcanoes even when in a state of rest, be derived from sea-water impregnated with salt, or rather, perhaps, with fresh meteoric water; or whether the expansive vapours (which at a depth of nearly 94,000 feet is equal to 2,800 atmospheres) would be able at different depths to counterbalance the hydrostatic pressure of the sea, and thus afford them under certain conditions a free access to the focus."[1]

Again:[2]

"The great number of volcanoes on the islands and on the shores of continents must have early led to the investigation by geologists of the causes of this phenomenon. I have already, in another place (*Cosmos,* Vol. I, p. 242), mentioned the confused theory of Trogus Pompeius under Augustus, who supposed that the sea-water excited the volcanic fire. Chemical and mechanical reasons for this supposed effect of the sea have been adduced to the latest times. The old hypothesis of the sea-water penetrating into the volcanic focus seemed to acquire a firmer foundation at the time of the discovery of the metals of the earth by Davy, but the great discoverer himself soon abandoned the theory to which even Gay-Lussac inclined, in spite of the rare occurrence or total absence of hydrogen gas. Mechanical, or rather dynamical causes, whether sought for in the contraction of the upper crust of the earth and the rising of continents, or in the locally diminished thickness of the inflexible portion of the earth's crust, might, in my opinion, offer a greater appearance of probability. It is not difficult to imagine that at the margins of the up-heaving continents which now form the more or less precipitous littoral boundary visible over the surface of the sea, fissures have been produced by the simultaneous sinking of the adjoining bottom of the sea, through which the communication with the molten interior is promoted. On the ridge of the elevations, far from that area of depression in the oceanic basin, the same occasion for the existence of such vents does not exist. Volcanoes follow the present sea-shores in single, sometimes double, and sometimes even triple parallel rows. These are connected by short chains of mountains, raised on transverse fissures, and forming mountain-nodes. The range nearest to the shore is frequently (but by no means always) the most active, while the more distant, those more in the interior of the country, appear to be extinct or approaching extinction. It is sometimes thought that, in a particular direction in one and the same range of volcanoes, an increase or diminution in the frequency of the eruptions may be perceived, but the phenomena of renewed activity after long intervals of rest render this perception very uncertain."

§ 8. *Views of Lucretius on the penetration of sea water into Ætna.*

We have quote the above passage because of Humboldt's sagacious remarks, some of which deal with the theory of the penetration of sea water as held by the ancients. He mentions Trogus Pompeius under Augustus as the author of the theory, but it is remarkable that the same views were held by the poet Lucretius more than half a century before.

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[1] *Cosmos,* Vol. I, p. 242. Bohn's translation.

[2] *Cosmos,* Vol. V, pp. 431–2. All the citations of Humboldt's works are from the Bohn translations.

In "De Rerum Natura," Lib. VI, 680 et seq., we read, according to Munro's translation:

> "And now at last I will explain in what ways yon flame roused to fury in a moment blazes forth from the huge furnaces of Ætna. And first the nature of the whole mountain is hollow underneath, underpropped throughout with caverns of basaltic rocks. Furthermore, in all caves are wind and air; for wind is produced when the air has been stirred and put in motion. When this air has been thoroughly heated and raging about has imparted its heat to all the rocks round, wherever it comes in contact with them, and to the earth, and has struck out from them fire burning with swift flames, it rises up and then forces itself out on high, straight through the gorges; and so carries its heat far and scatters far its ashes and rolls on smoke of a thick pitchy blackness and flings out at the same time stones of prodigious weight; leaving no doubt that this is the stormy force of air. Again the sea to a great extent breaks its waves and sucks back its surf at the roots of that mountain. Caverns reach from this sea as far as the deep gorges of the mountain below. Through these you must admit (that air mixed up in water passes; and) the nature of the case compels (this air to enter in from that) open sea and pass right within and then go out in blasts and so lift up flame and throw out stones and raise clouds of sand; for on the summit are craters, as they name them in their own language; what we call gorges and mouths."

In one important part of this passage, the text is corrupt and the context, therefore, supplied; yet there is absolutely no doubt, from preceding passages stating that the sea penetrates the land, that Lucretius held that the mountain is hollow, the water filters through the crevices and cracks in the rocks, until it comes into contact with the subterranean fires which convert it into vapors that give rise to the explosive violence witnessed in the eruptions of Ætna.

We shall see hereafter that Aristotle describes a volcanic eruption as due to the urging blast of pent-up vapor, but it does not seem that he gave any satisfactory explanation of how the vapor developed within the earth's crust.

§ 9. *Lucretius' views on earthquakes.*

> "Now mark and learn what the law of earthquakes is. And first of all take for granted that the earth below us as well as above is filled in all parts with windy caverns and bears within its bosom many lakes and many chasms, cliffs and craggy rocks; and you must suppose that many rivers hidden beneath the crust of the earth roll on with violence waves and submerged stones; for the very nature of the case requires it to be throughout like to itself. With such things then attached and placed below, the earth quakes above from the shock of great falling masses, when under-

neath time has undermined vast caverns; whole mountains indeed fall in, and in an instant from the mighty shock tremblings spread themselves far and wide from that centre. And with good cause, since buildings beside a road tremble throughout when shaken by a waggon of not such very great weight; and they rock no less, where any sharp pebble on the road jolts up the iron tires of the wheels on both sides. Sometimes, too, when an enormous mass of soil through age rolls down from the land into great and extensive pools of water, the earth rocks and sways with the undulation of the water just as a vessel at times cannot rest, until the liquid within has ceased to sway about in unsteady undulations. . . .

"The same great quaking likewise arises from this cause, when on a sudden the wind and some enormous force of air gathering either from without or within the earth have flung themselves into the hollows of the earth, and there chafe at first with much uproar among the great caverns and are carried on with a whirling motion, and when their force afterwards stirred and lashed into fury bursts abroad and at the same moment cleaves the deep earth and opens up a great yawning chasm. This fell out in Syrian Sidon and took place at Ægium in the Peloponnese, two towns which an outbreak of wind of this sort and the ensuing earthquake threw down. And many walled places besides fell down by great commotions on land and many towns sank down engulphed in the sea together with their burghers. And if they do not break out, still the impetuous fury of the air and the fierce violence of the wind spread over the numerous passages of the earth like a shivering-fit and thereby cause a trembling" ("De Rerum Natura," Lib. VI, Munro's translation).

III. THE GEOGRAPHICAL DISTRIBUTION OF VOLCANOES AND THEIR RELATION TO EARTHQUAKE PHENOMENA.

§ 10. *Four fundamental facts to be explained by a theory of volcanoes.*

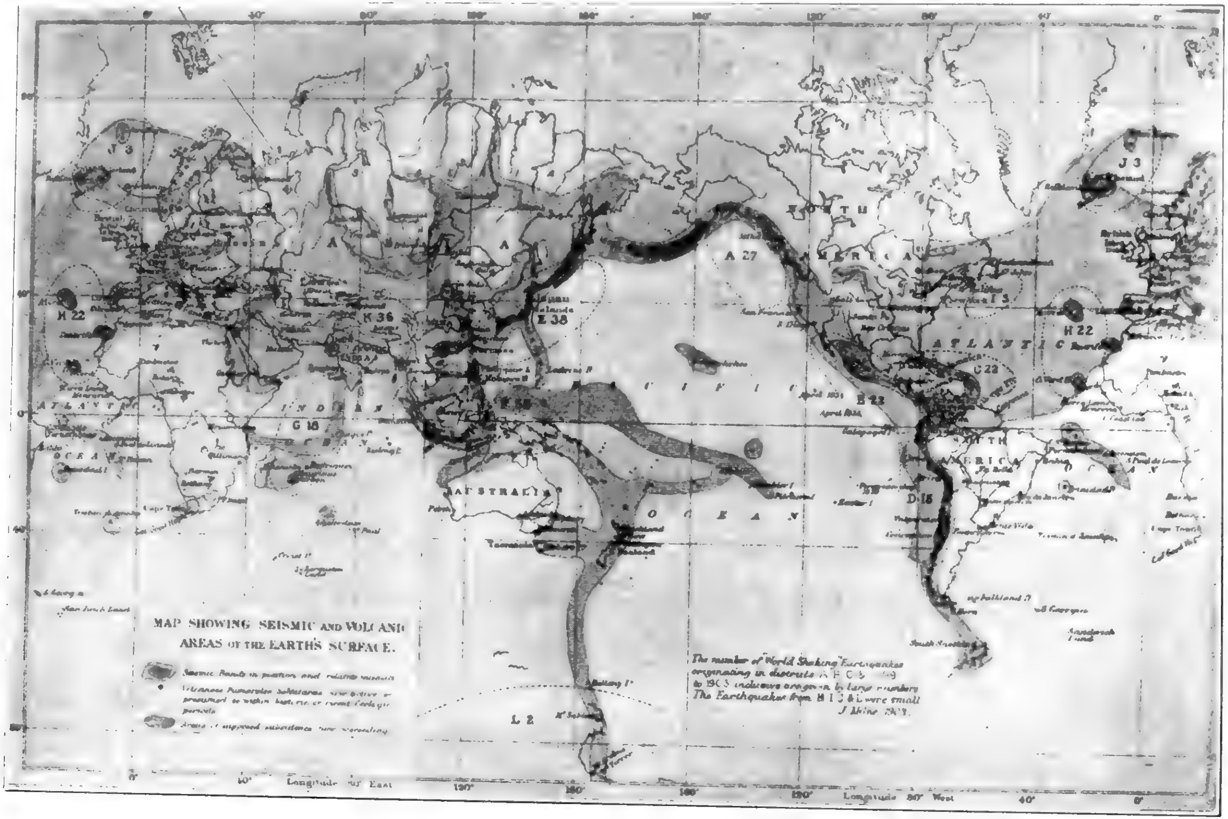
A satisfactory theory of the cause of volcanic action must account for the following phenomena:

1. The distribution of some 400 active volcanoes about the margins of the sea, and the numerous eruptions which take place in the sea or on islands, while none at all occur inland at distances exceeding about 100 miles from the ocean or equivalent large bodies of water.

2. The fact that 999 in 1,000 parts of the vapors emitted by volcanoes is steam, as if produced by the leakage of the oceans, near which the volcanic vents always are situated.

3. Volcanoes are particular mountains, and all mountains follow the seashore as if formed in some way by the action of the sea upon the adjacent land.

FIG. 2.


MAP SHOWING SEISMIC AND VOLCANIC
AREAS OF THE EARTH'S SURFACE.
The number of "World Shaking" Earthquakes
originating in districts A B C D
to 1903 inclusive are given by large numbers
The Earthquakes from H I J & L were small
J. Milne
J 3
H 22
K 36
E 38
A 27
E 22
C 22
I 3
G 18
D 18
L 2
A
ATLANTIC OCEAN
INDIAN OCEAN
PACIFIC OCEAN
AUSTRALIA
NORTH AMERICA
SOUTH AMERICA
ATLANTIC
Longitude 60° East
Longitude 80° West
0°
40°
80°
120°
160°

4. The close geographical relationship existing between volcanoes and earthquakes throughout the world, and the part played by earthquakes in mountain formation, and the eruption of volcanoes.

These four fundamental facts seem to admit of easy and natural explanation on the hypothesis that the penetration of sea water develops steam just under the crust of the earth, and the result is the upheaval of mountains and the eruption of volcanoes.

§ 11. *Professor Milne's researches on the distribution of earthquakes.*

One of the most remarkable results of recent research is the discovery of numerous regions greatly affected by submarine earthquakes, so that it is now known that these phenomena occur not only on land, but more especially under the sea. As we shall treat of this remarkable result hereafter, we shall at present confine our attention to the relations of earthquakes and volcanoes as observed upon the continents. It has long been recognized that both groups of phenomena occur in a series of belts, which follow the same general regions of the world, along certain so-called lines of weakness in the earth's crust.[1]

In a recent review of earthquakes published in the British Association Report for 1902, Professor John Milne has outlined twelve principal seismic regions, some of them of great extent. These several belts include the wide boundaries of the Pacific Ocean, the Antilles and Caribbean Sea region, and the great belt beginning at the Azores, and extending through the Mediterranean to the Himalayas and India. This last great belt is the only one in which the sea does not predominate over the land, and even here, the sea is paramount over a large part of the area included, while the rest includes or lies adjacent to the highest mountain range in the world. As Major Dutton has remarked, it may be doubted whether all of this last region should be included in one area, except, perhaps, as an outline to aid the memory; but at all events, the Azores and southern

[1] Cf. Professor Milne's work on "Earthquakes," edition 1903, which includes an excellent map of the world giving the distribution of both earthquakes and volcanoes. As earthquakes in the interior of the oceans until recently were seldom recorded, unless of great violence, the earthquakes charted on the map are chiefly those observed on the land, so that the centres of the oceans appear unduly vacant.

Europe, made up throughout of broken mountainous regions extending into the Mediterranean, with the Black Sea and Caspian on the east, are not essentially different from the earthquake regions surrounding the Pacific Ocean.

In his recent Bakerian Lecture at the Royal Society, March 12, 1906, Professor Milne explains his latest classification of seismic regions as follows:

> "Regions which lie on the western suboceanic frontier of the American and the eastern frontier of the Asiatic continents, and regions which lie on a band passing from the West Indies through the Mediterranean to the Himalayas.
>
> "In addition to these there are two minor regions, one following the eastern suboceanic frontier of the African continent, which I have called the Malagassy region, and an Antarctic region which lies to the southwest of New Zealand.
>
> "The following table gives the number of large earthquakes or mass displacements which have occurred in the subdivisions of these regions since 1899.

		1899	1900	1901	1902	1903	1904	Total.
Region of the Pacific Ocean.	1. East Indian Archipelago	11	17	13	14	11	9	75
	2. The coast of Japan	19	5	5	9	7	14	59
	3. Alaskan coast	14	11	1	1	3	0	30
	4. Central America	6	4	4	8	6	0	28
	5. West of South America	9	0	2	3	1	0	16
Western Atlantic and Eurasian regions.	6. Antillian region	6	7	3	6	3	0	25
	7. Azores	13	6	3	0	2	1	25
	8. Alpine, Balkan, Caucasian, Himalayan region	4	2	8	22	22	4	62
	9. Malagassy district	9	4	4	1	3	0	21
	10. Antarctic district	Between March, 1902, and November, 1903, 75 large and small disturbances were recorded.						
	Totals	91	56	43	64	58	29	341

> "Many of the disturbances included in this table are known to have been followed by hundreds and even thousands of after-shocks. The most active district is at present that of the East Indies, which might well be considered as an eastern prolongation of the Himalayan region. The scene of this activity it may be noticed, is at the junction of two lines of rock folding, which meet almost at right angles. Whether the Antillean and Central American region should be separated is open to question. If we unite their registers as belonging to two comparatively near and parallel earth ridges, the movements of one influencing those of the other, we have a region of hypogenic activity approximate to that of the Japan seas.

"Generally it would appear that these regions of instability are to be found along the margins of continents or tablelands, which rise suddenly to considerable heights above oceanic or other plains.

"At the present time we may, therefore, say that megaseismic disturbances do not occur anywhere, but only in districts with similar contours. Are we dealing with primitive troughs and ridges which are simply altering their dimensions under the continued influence of secular contraction, or do these reliefs of seismic strain represent isostatic adjustments which denudation and sedimentation demand?"

Professor Milne then discusses other possible causes, such as the effects of ocean currents and the seasons, including meteorological causes, such as accumulations of ice and snow at the poles, and finally the motion of the pole in the body of the earth; and he says that in about thirteen years between 1892 and 1904, he "finds records for at least 750 world-shaking earthquakes," which affords one an impressive idea of the extent of his researches, and of the importance of the subject.

In general the geographical distribution of volcanoes is closely similar to that of the earthquakes, but the latter are the more general and widely extended phenomena, while the former are more special. It is remarkable that the volcanoes break out in the centers of the earthquake belts. This relation can not be accidental, but points to a common cause underlying both phenomena.

Besides the active volcanoes near the seashore, and on islands, many of which were heaved up originally by submarine eruptions, nearly every country has a long list of extinct volcanoes. The islands in which volcanic eruptions have ceased, may also be viewed as extinct volcanoes in the sea. In this respect the southern and central Pacific Ocean is particularly rich in extinct volcanoes, and there also, a great many submarine earthquakes are supposed to occur. But the greatest breeding ground for world-shaking earthquakes, as Professor Milne says, are the deep troughs along the continents, near which many volcanoes usually are burning. As volcanic regions, we may mention, especially, the west coast of South and Central America, the Aleutian and Kurile Islands, Japan, the Philippines, Sumatra, Java, and adjacent islands of the East Indies, New Zealand, the region of Erebus and Terror in the Antarctic, and Iceland, the Caribbean Sea, with the Azores and Canaries, the region of the Mediterranean and Central Asia, west of the Himalayas. In such of these regions as fall far within the continents, the volcanoes have in all cases died out for lack of water, but the earthquakes still exist as a survival of former conditions. This is true, for example, in the regions of Central Asia, which no longer has any active volcanoes, though some were active there not very long ago geologically, and thus exhibit still a fresh and even sulphurous appearance.

§ 12. *The outbreak of new volcanoes within the historical period.*

It may be mentioned here that within historical times the following new volcanoes have broken forth:

1. Monte Nuovo, eight miles from Naples, September 28, 1538.
2. Jorullo, in Mexico, September 29, 1759.
3. Izalco, San Salvador, February 23, 1770, 5,000 feet high, thrown up on what was formerly a cattle farm.
4. Las Pilas, on the Plains of Leon, Nicaragua, April 11, 1850, a small volcano.
5. Ilopango, Nicaragua, January 20, 1880, a small volcano, thrown up in a lake 600 feet deep.
6. Fusiyama, Japan, 12,365 feet high, which tradition says was thrown up in a single night, about 300 B. C.
7. Tarewera, New Zealand, January 10, 1886, a mountain with a flat top, which previously had given no volcanic indications.

There are perhaps other volcanoes, some of them mentioned by Strabo, which have broken forth on land; and a good many more which have been upheaved in the sea.

Many old volcanes long extinct have burst forth into renewed activity, generally with terrible violence. Any volcano may become extinct or dormant, and then again break forth. In his work, on "Volcanoes," Professor Bonney often speaks of a mountain as having lost its crater; and mentions in this class Ixtaccihuatl and Chimborazo; but although it is probable, it is not certain that either of these has ever been active.[1] Yet according to the view developed

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[1] In his two ascents of Chimborazo, Whymper found lava and other volcanic indications, which had escaped the notice of Humboldt and Boussingault, who did not regard the mountain as volcanic. The adjacent mountain of Carihuairazo, said to have been higher than Chimborazo before the crater collapsed, June 19–20, 1698, might have ejected the volcanic products noticed by Whymper on Chimborazo, though it seems improbable.

in this paper any mountain may become a volcano, on short notice, if the internal violence is sufficient to break open an outlet for the vapors which always slumber beneath. We shall see, hereafter, that the mountains are all filled with volcanic materials, and an explosion is all that is required to set them going, and this is usually effected by the throes of an earthquake. All new volcanoes, and old ones when they burst forth into renewed activity, do so with violent earthquake shocks. The shocks of an earthquake almost always have some effect on a burning volcano, and in earthquakes remote from erupting centers, the breaking out of a volcano causes the shocks to cease, as was long ago noticed by Strabo.

Since this intimate connection has been observed again and again, and the volcanic and earthquake belts are generally similar, though not strictly identical, throughout the world, there is a very strong indication that both depend upon a common cause, and that cause is nothing else than ordinary steam. It is worth while to notice that as Central America is a narrow country, with fairly deep seas on both sides, it is exactly where we should expect volcanic forces to have great sway, and observation shows that this is true for earthquakes as well as volcanoes. The recurrence of frightful earthquakes in that region, and the upheaval of three new volcanoes within historical times speaks for itself, and shows that all the mountains are not yet finished; and that some of the land in Central America is being elevated by forces depending on the influence of the sea, whether volcanic or seismic. As the result of his observations Darwin held that volcanoes break out in rising areas, most likely because an outlet is easily established when the outer layers are cracked open to a great depth.

§ 13. *The relation of earthquakes to volcanoes.*

In his interesting work on "Earthquakes in the Light of the New Seismology," p. 43, Major Dutton follows Professor Milne in his classifications, and remarks:

> "Though it is possible to indicate regions which present both volcanoes and earthquakes, there is no proof of interdependence between seismicity and vulcanicity in general. While there are earthquakes which are certainly of volcanic origin, the one phenomenon does not necessarily imply the other."

Professor Milne, Omori, Dutton, and others have recently attempted to disprove the relationship of earthquakes and volcanoes exhibited by the shores of the Pacific Ocean, and their mode of attack has been to show that the volcanoes around the Pacific are not a continuous "girdle of fire," but are bunched here and there, with large spaces between; and that while the earthquakes are also distributed with some irregularity, there is no visible connection between them and the volcanoes.

But if steam forming in the earth's crust from sea water leaking down is the common cause of both earthquakes and volcanoes, should there really be any immediate connection between the two classes of phenomena? Would not volcanoes develop chiefly where the force of the steam was sufficiently powerful and suddenly exerted to break through the crust or mountains, and therefore chiefly in the mountains along the seashore, where the crust is greatly fractured, and enables the violent explosions of steam to blow open an outlet by raising a mountain which would burst into a volcano? It is along such shores also that the leakage would be greatest and most volcanoes should exist, provided the crust becomes badly fractured.

If, on the other hand, the crust is not much broken and explosions of steam cannot break through, would there not result a great many earthquakes of the class now called tectonic because not visibly connected with volcanoes and supposed to be due to slipping of rocks or faults? When the crust is wholly unbroken, it would naturally be very difficult, even for deep-seated forces of enormous magnitude, to raise up a mountain that would become a volcano, because all the overlying strata would have to be violently broken in such a way as to radiate from a point like a star, and ordinarily the strain of the imprisoned steam is much more easily released by an earthquake which merely shakes up the crust in such a way that a neighboring fault moves and the internal pressure is relieved and equalized by scattering, without breaking through all the overlying strata at one time.

This indeed appears to be the process of nature, and if we consider it in relation to volcanoes and earthquakes we shall perceive, in accordance with observation, that the former should be the more special, the latter the more general phenomena. Also both phenomena should occur under the sea, and along the shores of the deepest oceans; but volcanoes would develop chiefly in certain regions where the rocks are already broken in the uplift of mountains and therefore easily burst open, whereas earthquakes might occur in any locality where the leakage of the sea developed sufficient steam. It is undeniable that this is in accordance with observation on our actual earth; and it shows that while both volcanoes and earthquakes should surround the Pacific Ocean, earthquakes are much more widely and uniformly distributed than volcanoes. Also in those regions near actual volcanoes, where the imprisoned steam has a vent, violent earthquakes should not occur; but if the activity of the volcano ceases, the danger of earthquakes would be increased.

Humboldt remarks that this opinion was widely spread among the people of the Andes, and it would be difficult to deny that this result of their long experience was well founded, though confessedly they did not know upon what principle the dreaded explosions depended.

If the leakage from the sea has moderate uniformity with respect to the time, it is clear that the cessation of the smoke of a volcano is really one of nature's danger signals, since the pressure within the subterranean reservoirs of the mountain and adjacent regions may then increase to such a degree as to become extremely dangerous. Neither Krakatoa nor Pelée had been active for long periods before the fearful explosions of 1883 and 1902. Krakatoa had been practically dormant for two hundred years, and while Pelée had experienced an eruption in 1851, it was small, and no important explosion had occurred since 1762.[1] In the case of Vesuvius the general experience is the same—the longer the eruptions are delayed the more violent they become. For it appears that in 79 A. D. no eruption had occurred for about six centuries, and Pliny's description of that outbreak shows that it was more violent than any that has occurred since. The volcano of Conseguina in Central America illustrates the same principle by the long repose preceding the frightful eruption of 1835, which spread devastation far and wide, and in many ways resembled the terrible outbreak of

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[1] Cf. Heilprin, "Mont Pelée and the Tragedy of Martinique," pp. 61–187, 188.

Krakatoa.[1] It has so often been observed that the earthquakes ceased on the eruption of a neighboring volcano, that one cannot doubt that direct relief was afforded by the eruption.

Viewing the relation of earthquakes and volcanoes in this light, we can easily understand why many of the so-called tectonic earthquakes are doubly severe—much more violent than those closely connected with volcanoes—because where no volcanic outlet has been available, the explosive strain increases to an enormous extent before it can obtain any relief whatever; and when the yielding does occur the shock is one of appalling violence, and does great damage causing the slipping of rocks, faults and subsidences, and is felt over a very large area, because the explosive strain has become deep-seated and intense.

The great depth at which many of the so-called tectonic earthquakes have been proved to occur, is at once an argument against the dislocational or faulting theory, and a convincing proof that shocks of this type are due to the explosive power of superheated steam. These shocks are obviously too deep-seated to be accounted for by subsidences, and moreover the resulting vibrations are too complex to be due to mere slipping of a ledge of rock, as will be more fully explained hereafter. It may be shown that no possible subsidence of rock faults could produce a conspicuously rotatory earthquake like that which destroyed San Francisco.

IV. The General Cause of the Formation of Mountains and their Geographical Distribution.

§ 14. *On the formation of mountains and cordilleras, as illustrated by the Andes.*

If we consider the deep trough running for a great distance parallel to the coast line of the western shore of South America[2] and recall that other deep troughs of the same kind exist parallel

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[1] In chapter XVI of his valuable work on "Earthquakes," edition of 1903, Professor Milne cites several other eruptions of fearful violence accompanied in each case by terrible earthquakes.

[2] The trough is not of uniform depth throughout its course, but the depression is always conspicuous, so that everywhere the earth's crust is arched downward.

to the Aleutian and Kurile Islands, the east shore of Japan, the west shore of Sumatra and Java; also near New Zealand and various islands in the deepest oceans, as Guam, the Bahamas and other West Indian Islands, we shall perceive that this arrangement is not by chance. The South American trough always appears parallel to the great mountain ranges of the Andes, and the fact that it is of about the same volume as the matter included in the Cordilleras appeared to be a suspicious circumstance. The relation above cited for Japan, Java, and other islands is similar, but in the case of oval islands the adjacent depression may not be a trough, but rather a hole of somewhat oval or elliptical figure.

Fig. 3.

Outline Map of South America, showing the great Ocean Trough parallel to the Andes.

Some years ago the writer noticed these remarkable sinks while examining a relief cast of the Atlantic Ocean exhibited in the office of the United States Coast and Geodetic Survey at Washington, and remarked that it seemed as if the volumes of the upraised islands were not very much larger than those of the adjacent depressions. Why should depressions exist so near these elevations above the sea, and, in the case of mountain ranges, so nearly parallel to them for long distances? Is there not obviously a direct connection between the elevated land and the unusual depression in the adjacent sea bottom?

To understand just what this connection is, we may recall that after the great eruption of Mt. Pelée in 1902, it was found by actual measurement that a considerable portion of the adjacent sea bottom had sunk down hundreds of fathoms.[1] It is impossible to believe that this settling of the bottom of the sea could be due to the

FIG. 4.

Vertical Section perpendicular to the Andes and Andean trough, drawn to natural scale, and showing the mode of operation of the trough in the formation of mountain and cordilleras.

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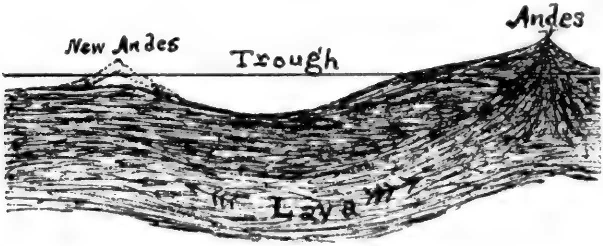
[1] This statement is perhaps somewhat too positive, for Dr. O .H. Tittman, superintendent of the U. S. Coast Survey, informs me that reliable determinations of depth before the eruption of Pelée seems to have been insufficient to decide the question satisfactorily. The cables were broken, and the subject investigated by the French Commission (Lacroix, Alf, La Montagne Pelée et ses éruptions, Paris, 1904), which includes M. Rollet de l'Isle's investigation of the reported changes of depth in the vicinity of Martinique. The French commission was inclined to ascribe the disturbances to submarine volcanic action, rather than to subsidences. This, however, is not a matter of great importance; for in his work on "Seismology," p. 35–36, Professor Milne mentions several well established cases of subsidences in the Mediterranean and in the Pacific Ocean off the Esmeralda River in Ecuador. He points out that "disturbances originating beneath the sea, which are much more numerous than those originating beneath the land, likewise emanate from a region of strain. Mr. W. G. Forster, who has paid so much attention to the earthquakes of the Mediterranean, tells us that they have been accompanied by great subsidences of the sea bottom."

mere shaking of the earthquakes accompanying that eruption; and we must, therefore, suppose that after matter had been expelled by the dreadful explosions which destroyed St. Pierre and devastated Martinique, or in earlier eruptions, a subsidence near the roots of the mountain actually took place. Again, in 1835, Captain Fitzroy and Charles Darwin observed that after the violent earthquake which destroyed Conception, the Chilian coast line in that region had been elevated from three to five feet for several hundred miles. Not only the coast but also the whole country back to the Andes was raised. This could only be explained by the injection or forcing in of a corresponding bulk of lava under the land; and this lava could come from nowhere except from under the bed of the great trough in the adjacent sea. The ultimate effect would be to cause the trough of the ocean to deepen correspondingly. And, moreover, such periodic injections from under the sea trough would not only push along the ejected stream of lava, step by step, until the end of the column reached the mountains, but the forces thus arising would supply the "lateral thrusts" which are said to be much needed for the explanation of the upheavals, the tipping of the strata, the inclinations and sometimes reversed positions of the rocks, and other geological phenomena observed in mountains like the Andes. Heretofore the abundant phenomena of this kind noticed in all high mountains have not been satisfactorily explained. A correct theory must account for the inclinations seen in the mountains as well as the rising of the coast, and such submarine earthquakes as Darwin observed to precede the uplift of the beach at Conception. The present theory seems to be capable of meeting this severe test, and it requires us to make no assumption except that molten lava may be forced from under the bed of the trough, and pushed along its course beneath the crust by the throes of successive earthquakes.

It is recorded that a great sea wave followed the Chilian earthquake of 1835, and such waves are very frequent along the Chilian and Peruvian coasts. They almost always follow an earthquake, and begin by a recession of the sea from the shore, which then returns as a great wave, carrying everything before it. Some have supposed the sea bottom to subside, thus withdrawing the water toward the sink, till it flows in on all sides to fill up the depression, and then piles up and returns as a great wave which continues to oscillate furiously, sometimes for days after the earthquake. We shall consider these waves more fully hereafter, and at present it is sufficient to remark that this explanation is satisfactory for the kind of waves usually observed along the west coast of South America.

We may then suppose that in such earthquakes a very large mass of lava is forced from under the sea, which then settles below its former level, and the great wave follows. If the lava is forced toward the land, the coast or mountains are upraised; if towards the ocean, a ridge may be upheaved there, or possibly a submarine volcano of large extent. In either case the trough of the sea bottom parallel to the coast eventually becomes less stable, and, at certain intervals, settles little by little, when the consistency of underlying lava has been thinned by successive ejections; and thus with the settling stability is again restored.

FIG. 5.



As the trough is arched downwards towards the exploding lava the steam pressure from beneath cannot force it upward; and the strain is necessarily relieved by motion of the lava towards the Andes or the ocean—usually towards the mountains till the trough gets broad and deep and the mountains very far away and so high that the movement of the column offers unprecedentedly great resistance, when the release will at length become easier towards the ocean by the forcing up of ridges or volcanoes along the other margin of the trough. Ridges with peaks in them will usually result, and this is the beginning of the new Andes or Cordilleras, which are destined to rise slowly from the sea, leaving a deep valley towards the ancient shore, to be drained and filled in by erosion.

Thus we explain some of the remarkable parallel ridges of the Cordilleras. And it is natural that in this upheaval of the crust, release of strain due to subterranean steam pressure should occasionally come by the throwing up of cross ridges, sometimes enclosing undrained areas, and thus lakes like Titicaca are formed. Proceeding upon this simple and natural principle, we may easily explain all the chief characteristics of the Andes. It is impossible to doubt that these mountains have been formed by the very forces which we see still at work there. The upheavals have been step by step, and earthquakes forcing up the mountains have at the same time caused the ejection, by the pushing along of a column or rather a layer, of the necessary matter from under the sea, thus sinking the bottom into a permanent trough, while the subsidences accompanying some of the earthquakes have produced enormous sea waves. Countless thousands and perhaps millions of these earthquakes and sea waves have occurred throughout past geological ages.

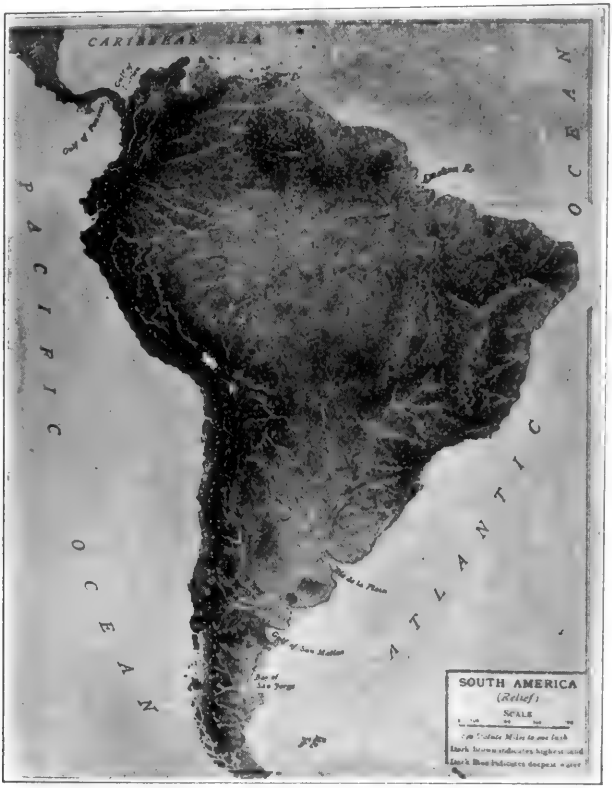
The trough parallel to the coast and the upheavals and sea waves now observed are a survival to show us just how the mountains have been formed, and where the next mountain range will form in the sea. We can predict the formation of the new Andes as confidently as we can an eclipse, though it will be a much longer time before the new mountains develop; for we recognize the cause to be a true one, and see just how it works by a kind of self-regulating automatic process. The working of the cause has been observed near Mt. Pelée, and the operation of the same process along the South American coast is proved by the observations of Charles Darwin and by the great sea waves frequently observed within historical times.

§ 15. *Investigation of the significance of the observed lay of mountain chains by means of the theory of probability.*

In their new work on "Geology," Vol. I (p. 543), Chamberlin and Salisbury remark that the relationship between the direction of folded ranges of mountains and the adjacent seacoast is "*a coincidence that is only in part causal.*" There are, doubtless, many ways in which this problem could be treated by the methods employed in theory of probability. Without claiming to exhaust the various lines along which the discussion might be developed, we believe the following method rests on equitable considerations.

Imagine a mountain chain like the Andes which runs near the shore cut up into pieces each long enough to reach the sea. If there is no physical cause why the chain should be parallel to the seashore, some of the pieces might be expected to lie at all angles with respect to the shore line of the coast from 0° to 90°, and thus the chain's most probable form is that of a zig-zag line made up of short pieces lying at all angles. Take the intervals of angular distribution of the pieces of the chain at 9°; then, excluding the existence of physical causes, any angle from the first interval, 0° to 9°, to the last of these subdivisions, 81° to 90°, must be held to be equally probable. At any place there are ten divisions of the quadrant, all equally available for the mountain chain to follow. The

Fig. 6.



probability that the direction of a piece of the chain will fall in any one of these divisions of the right angle is one-tenth; and the probability that all the n pieces throughout the whole chain will fall in the same angular division is $(1/10)^n$. When n is a large number, or the chain is long and close to the sea, this probability becomes practically zero. In such a range as the Andes it is observed that all the pieces of the chain do fall in the first division of the angle, between 0° and 9°; and, hence, in general, the probability is $(10)^n : 1$ that the observed lay of the chain so exactly parallel to the shore of the sea is not the result of mere chance, but depends directly on some physical cause which has made the chain essentially straight as well as laid out the general course parallel to the sea coast.

If the chain had the short bends in it here assumed to be possible, the total length would be greater than that of the existing chain, and n would be correspondingly increased. The data used, therefore, make n a minimum, and $P = 1/(10)^n$ a maximum for the given chain everywhere so closely following the seashore. Thus the calculated value of P is too large rather than too small, with the existing lay of the chain.

Another way of reaching analogous results is to consider what the deviation from strict parallelism is in so long a chain, and the probability that the coincidence would be so exact throughout, when all angles between 1″ and 324,000″ (the equivalent of 90°) are equally probable. If no physical cause is involved depending on the sea, there is no reason why the chain should not run at any angle across the shore line. Now, the Andes are made up on the average of at least two parallel chains, and, the probability of this double parallel trend throughout would be only

$$\frac{1}{324{,}000} \cdot \frac{1}{324{,}000} = \frac{1}{114{,}976{,}000{,}000}$$

In any case, we see that for the double chain, the chances are hundreds of billions to one against strict parallelism to the seashore.

In the first method of treatment each part of the chain is considered, without regard to the rest; in the second method, the chain is viewed more as a whole, and the individual parts neglected. In respect to the first method, it might be claimed that as mountains arise from foldings of the crust, and as the earth's crust is thick, a zig-zag form with many bends in it would be improbable, because a crack started in the rocks anywhere would be likely to run in a straight line. There may be some justice in this criticism, but it seems quite fully compensated for by the fact that the chain actually bends wherever the coast line alters its course. Thus in practice the chain is shown to be capable of flexure wherever the shore line changes its direction, and there does not seem anything improbable in many short bends, unless the trend has some connection with the coast. Whether the chain could fairly be conceived as bending at such short intervals is a question we need not enter into, for we may observe that short bends actually appear in certain chains, and thus the hypothesis is not contrary to nature under certain conditions. In the present case we have made no assumption as to causes, except that the lay of the chain is independent of the seacoast, and hence the hypothesis postulates nothing improbable.

To reduce the first method to numbers, we may observe that the length of the chain of the Andes is 4,400 miles and the average distance from the sea about 66 miles. Thus

$$n = \frac{4,400}{56} = 66\tfrac{2}{3} \text{ or } P = \frac{1}{(10)^{66}} = 1 : (\text{decillion})^2$$

If other parallel ranges be included, this divisor would be much increased, perhaps nearly squared. On the other hand, the method of viewing the chain as a whole makes the divisor a quantity of the order of one hundred billions. The truth must, I think, lie somewhere between these extremes. If we were to take account also of the mountains parallel to the Atlantic coast, it would certainly be moderate to conclude that the parallelism to the seashore noticed in the whole of South America, so far as it depends on chance, would be less than 1 : (decillion)^2.

Thus it is clear that the probability of a physical cause connecting the mountains with the parallel shore of the sea is probably more than a decillion decillions to unity, and certainly more than one hundred billions to unity.

The parallelism noticed in North America along the Pacific and Atlantic coasts, is not less pronounced, nor is there less extent of mountains involved. On the contrary, the ranges of North America exceed in length those of South America. The Rocky Mountains are indeed farther from the coast than the Andes, but the shore line has receded since they were formed; while the Sierra Nevada and Coast Ranges are nearer the present shore. When one considers all these circumstances, including the greater length of the chains, and the greater number of parallel ranges, notwithstanding their distance from the seacoast, it will be found that in North America P' is certainly not larger than P as found for South America. Thus we may put them approximately equal to each other, and write

FIG. 7.

$$P.\ P' = \frac{1}{(10)^{66}} \cdot \frac{1}{(10)^{66}}, = 1 : (\text{decillion})^4$$

In Africa the mountain ranges are not high, but they run quite parallel to the shore. We may, therefore, without appreciable error put

$$P' = \frac{1}{(10)^{66}},$$

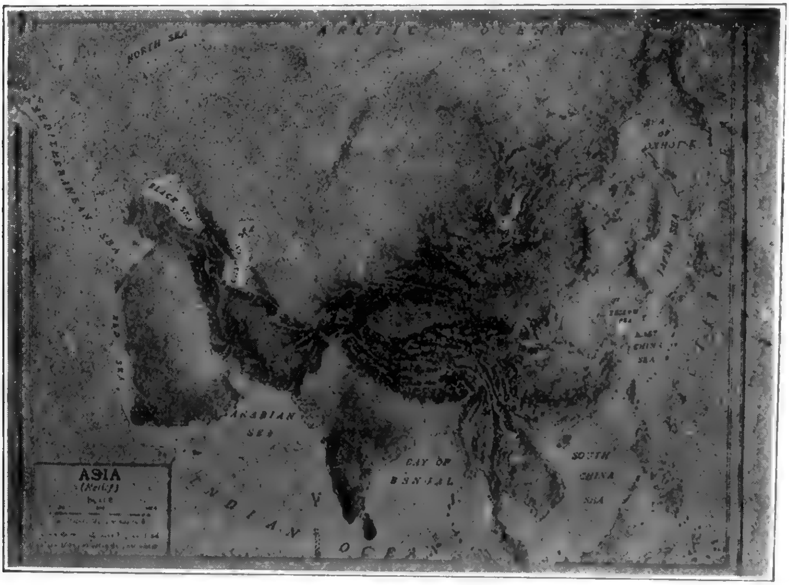
and

$$P.\,P'.\,P'' = 1 : (\text{decillion})^6$$

In the other three continents, Europe, Asia, Australia, the lay of the mountains is such as to justify us in taking $P'''.\,P^{iv}.\,P^{v} = P.\,P'.\,P''$. And, therefore, for the whole world, we may safely take

$$P.\,P'.\,P''.\,P'''.\,P^{iv}.\,P^{v} = 1 : (\text{decillion})^{12}$$

FIG. 8.



This number is so infinitesimally small, or the divisor is so fabulously large, that it becomes an absolute certainty that the parallelism of the mountains to the seashore depends on a true physical cause, and that cause can be nothing but the action of the sea itself. Since the mountains always *wall in* the land, it follows that they are erected by the sea, through injection of the coast by lava expelled from under the ocean bed. Accordingly, it follows that the crust is bent parallel to the seashore by a true physical cause.

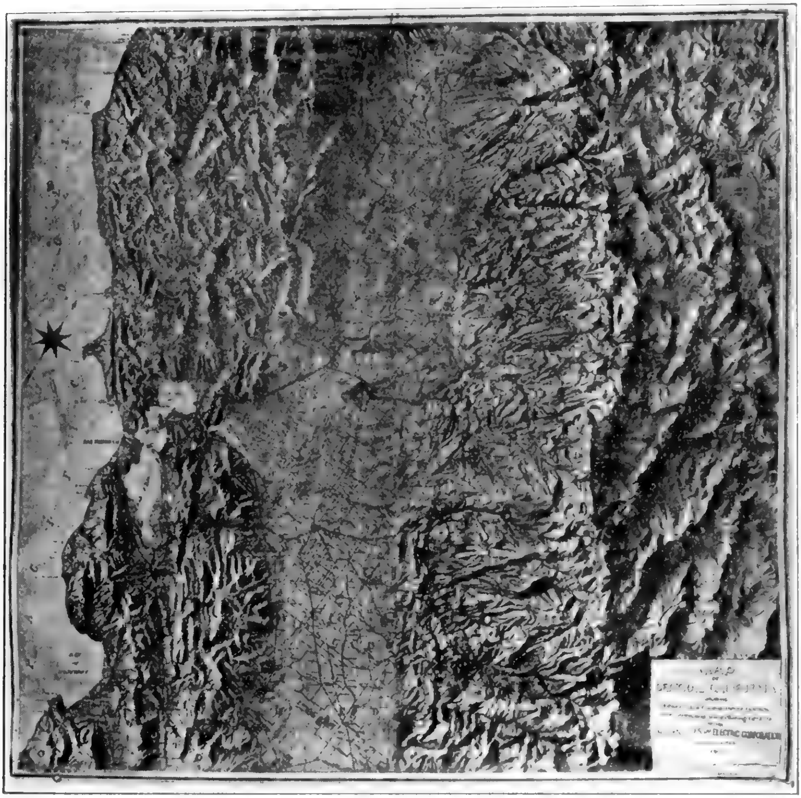
If instead of putting $P = 1:(\text{decillion})^2$ we had used $P = 1:(100 \text{ billion})$, the final result would have been $P. P'. P''. P'''. P^{iv}. P^{v}. = 1:(100 \text{ billion})^6$.

Even this divisor is so infinitely large that the fraction totally disappears, and it becomes an absolute certainty that the lay of the mountains depends on the action of the sea as a physical cause; and that action can be nothing else than the injection of the coast with lava. By this process the mountains were upheaved. The lay of the mountains parallel to the sea is, therefore, no "coincidence that is only in part causal," but the direct outcome of a general law of nature. How universal this law is may be inferred from the unerring precision with which it is exemplified in a region such as that about the Bay of San Francisco. Here the mountains line the shores on all sides, and change their course in such a way as to enclose the bay with walls so close by and well fitting as to leave no doubt about the origin of the surrounding mountains.

FIG. 9.

A deceptive way of viewing such an argument as the foregoing is to claim that the sea, because of its fluid nature, flows into the depressions in the earth's crust, which are due to subsidence or collapse; and when these depressions are filled up they are bound to be surrounded by higher regions of hills or mountains, due to wrinkles in the crust. This method of reasoning ignores the exact

FIG. 10.



parallelism to the seashore, which held for every mountain chain in the world at the time of its formation, and still holds for nearly all the principal ranges, though in a few cases the lapse of ages has modified the direction of the shore of the adjacent sea.

The result here established is, therefore, a fundamental law of nature, and it gives the key to the leading phenomena of the earth's surface.

§ 16. *How a sea valley develops and gives rise to parallel mountain chains, as illustrated in the San Joaquin, between the Sierra Nevada and Coast Range, in California.*

We have already seen that mountains are raised by the injection of the coast by steam-saturated lava exploding beneath the earth's crust. To understand the entire working of this process under good conditions, we might study the different sea valleys now existing in various parts of the world, or take one which shows the characteristic features of the process. If we could find one in which the process is complete, but of recent date geologically, its present form would, no doubt, enable us to make out the transformation which is undergone at different stages. The Sierra Nevada mountains, with the adjacent San Joaquin valley, appear to be an ideal case to illustrate the process in question.

A study of the Sierras in California shows that the western slopes of these gigantic mountains are very gradual—less than one in fifty—and braced by many spurs, with deep intervening cañons, of which Yosemite is the most famous. The eastern slopes of the Sierras are about ten times steeper than the western, and the jutting spurs are largely wanting. This shows that the injecting forces which raised these mountains came almost entirely from the west; and they continued so long that they finally gave the range an unsymmetrical form[1]—gently sloping and deeply corrugated with cañons on the west, and steep and precipitous on the east.

The form thus taken by the Sierras would indicate that at a late stage in their history the San Joaquin valley took the form shown in the accompanying figure, long and sloping on the east and

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[1] The more gradual slope of a mountain range toward the sea is due to two causes more or less distinct: (1) The vertical upheaval of the chain, with the successive horizontal thrusts which push it little by little from the sea, thus naturally making the farther slope the steeper; (2) the subsequent elevation of the shore, by injections under the crust, which tips the range still further over, by raising the base of an incline that is already gradual. These two causes combined will be found to explain the principal inequalities in the slopes of mountain ranges as observed in different parts of the world. When the range is being first upheaved, the injections give a nearly vertical uplift; as the range gets older the injections come more and more from the seaward side; when the range itself is finished, the injections only tip its base upward, and make the seaward slope more and more gradual.

steep and precipitous on the west. And when this form was once attained the upheaval of the Coast Range became inevitable. The present position of the San Joaquin river near the western side of the valley still shows this form of the valley, and enables us to see the precise process of transformation.

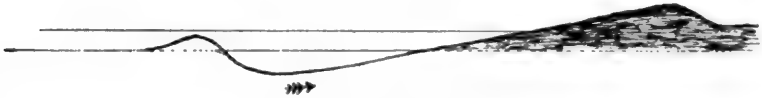
It is, perhaps, doubtful whether the elevation of these mountains has yet ceased; the earthquakes in California and the low level of the valleys would seem to show that the whole state is still rising, and I am told that this is also shown by beaches and shells at many places along the coast.

FIG. 11.

a. Mountain formation just beginning.

b. Mountain formation in the middle stages.

c. Mountain formation in the later stages.



d. New range rising from the sea.

The same principles which are here applied to the Californian mountains can be applied to other mountain ranges throughout the world.

§ 17. *Significance of the asymmetry of a mountain chain with respect to its principal axis.*

It is generally noticed that most mountains are unsymmetrical on the two sides; one side has a gradual slope, while the other is steep and precipitous. Moreover, the spurs jutting out from the range are unequally divided between the two sides, the larger number of spurs being on the side where the slope is gradual, which is always turned toward the sea. Let us now examine the meaning of this arrangement. In the new work on Geology by Chamberlin and Salisbury (Vol. I, p. 542–3) we read:

> "*Mountain-forming Movements.*—Along certain tracts, usually near the borders of the continents, and at certain times, usually separated by long intervals, the crust was folded into gigantic wrinkles, and these constitute the chief type of mountains, though not the only type. The characteristic force in this folding was lateral thrust. The strata were not only arched, but often closely folded, and sometimes intensely crumpled. In extreme cases, like the Alps, the folds flared out above, giving overturn dips and reverse strata, as illustrated in the chapter on "Structural Geology," pp. 501–511. In these cases there was an upward as well as a horizontal movement, for the folds themselves were lifted; but the more horizontal thrust so much preponderated, and was so much the more remarkable, that the upward movement was overshadowed. It is well to note, however, that these mountain ranges are crumpled *outward* and not *inward*, as might be expected if they resulted simply from the shrinkage of the under side of a thin shell. The folds are sometimes nearly upright and symmetrical, and sometimes inclined and asymmetrical, as illustrated in the chapter referred to. Where the folds lean, the inference has been drawn that the active thrust came, from the side of the gentler slope, the folds being pushed over toward the resisting side, and this seems to be commonly true."

Thus we have good authority for the statement that the lateral thrust came from the side of the gentler slope. It is well known that the gentler slope is turned towards the sea, and thus we realize that the forces which pushed the mountains horizontally was directed from the adjacent ocean.

In his great work on "Face of the Earth,"[1] Professor Suess records the following facts, all bearing on the above view:

1. In Vol. I, p. 452, Suess shows that the tangential movement in the Himalayas and Burmese Mountains on the opposite sides of the Bramaputra are in opposite directions, each being directed from the center of the river—exactly what the present theory requires, and not explainable on any other hypothesis.

2. In Vol. II, p. 34, Suess shows that the mountain folds in the eastern part of the United States "have been produced by a tangential movement directed from the existing Atlantic Ocean toward the mainland." On page 139 he shows that the same principle holds for South America.

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[1] Oxford Translation by Dr. Hertha Sollas.

3. In Vol. II, p. 121, Suess shows that the "prevailing tangential movement in the Alps and Pyrenees" is toward the north. Thus for the Alps, Lombardy is the most active ancient sea trough; and in the case of the Pyrenees, Suess shows that most of Spain was then in the bed of the sea, and has since arisen (pp. 123–128).

4. That the Sierra Nevada folds were produced by tangential movement from the side of the Pacific is generally recognized by geologists. Dana and others long ago have remarked the same thing about the Andes (Suess, Vol. I, p. 539), and recently, Chamberlin has written me that most of the mountains give evidence of having been upheaved by lateral thrusts from the direction of the sea.

In view of these facts is not the significance of the asymmetry of mountain chains perfectly plain? The unsymmetrical build of the chains shows that the forces by which they were formed were directed from the sea. And, as these forces could not have been subaerial, they must have been subterranean, working just under the earth's crust, and identical with those observed in earthquakes, when lava is expelled from the sea and pushed under the adjacent coast, along which the mountains always run so exactly parallel.

By no possibility could any supposed contraction of the earth have given rise to these features in mountain structure, since in that case it would be impossible for the shape of the ranges and tilting of the strata, pointing to lateral thrust, to be always directed from the sea. The arrangement and structure of mountains thus contradicts the contraction theory of the globe, and shows that any supposed effect of contraction was insensible.

§ 18. *Criticism of the contraction theory of the formation of a range such as the Alps.*

In addition to the considerations advanced by Fisher, as explained elsewhere in this paper, to show that mountains formed by wrinkles in the crust would in no case exceed a very small height, and should, moreover, be distributed with some uniformity over the globe, we may here consider some objections to the view that a chain such as the Alps has been produced by shrinkage.

To get Elie de Beaumont's theory clearly before us, we quote it as given by Lyell, "Principles of Geology," 12th ed., Vol. I, p. 119:

> "The origin of these chains depends not on partial volcanic action or a

reiteration of ordinary earthquakes, but on the secular refrigeration of the entire planet. For the whole globe, with the exception of a thin envelope, much thinner in proportion than the shell to an egg, is a fused mass, kept fluid by heat, but constantly cooling and contracting its dimensions. The external crust does not gradually collapse and accommodate itself century after century to the shrunken nucleus, subsiding as often as there is a slight failure of support, but it is sustained throughout whole geological periods, so as to become partially separated from the nucleus until at last it gives way suddenly, cracking and falling in along determinate lines of fracture. During such a crisis the rocks are subjected to great lateral pressure, the unyielding ones are crushed, and the pliant strata bent, and are forced to pack themselves more closely into a smaller space, having no longer the same room to spread themselves out horizontally. At the same time, a large portion of the mass is squeezed upwards, because it is in the upward direction only that the excess in size of the envelope, as compared to the contracted nucleus can find relief. This excess produces one or more of those folds or wrinkles in the earth's crust which we call mountain-chains."

It is unnecessary to dwell on the violence of the hypothesis that the nucleus has shrunk away from the crust, as here outlined. So far as one can see, no such result is possible, nor is the shrinkage ever appreciable.

But we may here remark that a radial shrinkage of one mile will give a shrinkage in the semi-circumference amounting to 3.14 miles. Whether correctly or not, it has been estimated by geologists that the amount of folding in the Alps exceeds one-third of the whole space now occupied by these mountains, the shortening of the original crust being placed by Heim at 74 miles. Analogous results have been reached by Claypole and others regarding the mountain ranges of America.

It should be observed that to afford a slack for folds amounting to 74 miles, a radial shrinkage of about 12 miles is required, even when all of the tangential movement is carried round to one point. To carry all the tangential movement round to one point would imply one of two things: (1) That the crust is loose from the globe it surrounds, and thus can be carried around to one point from a whole semi-circumference, which seems altogether improbable; (2) that if the crust is thus shrunk up without being carried around loose from the globe, the matter underlying it must be condensed to about three-halves its former density. The cone of matter underlying the Alps, and extending to the center of the earth would thus have a density 50 per cent. greater than that occurring under neighboring areas. This latter result is impossible, since observations show that the matter under mountains not only is not denser than the average, but actually lighter by an appreciable quantity. We know, therefore, that the wrinkling has not condensed the matter underlying the mountains.

On the other hand, it seems equally incredible that the shrinkage of the whole globe should be brought forward to one point, as if the crust were loose from the globe it covers. No part of the theory of mountains formed by wrinkles of the crust is to be seriously entertained. Besides the difficulties just mentioned, the postulated radial shrinkage of 12 miles is too great. It may well be doubted whether a shrinkage of one mile in the radius has taken place since the continents began to emerge from the oceans.

Professor Suess (Vol. II, p. 552) says:

> "As a result of *tangential thrusts,* the sediment of this (Mediterranean) Sea were folded together and driven upwards as a great mountain range, and the Alps have, therefore, been described as a compressed sea."

We must, therefore, seek the explanation of the formation of the Alps in some process by which this folding can have taken place in the sea, or along its borders, and thus we reach the theory outlined in this paper.

§ 19. *Why we abandon the contraction theory of mountain formation.*

While the considerations here adduced for the origin of mountains seem conclusive, it may not be wholly without interest to point out some difficulties which are not satisfactorily met by the contraction theory, which is the only one now in general use. It is usually stated that mountains result from a crumpling of the earth's crust, and that the crumpling takes place along the principal lines of weakness. This theory fails to explain the origin of isolated peaks or associated groups of peaks which sometimes rise like cones or groups of cones, often more or less intersecting, in the midst of comparatively regular plains. A theory with this serious defect is highly unsatisfactory.

If then the contraction theory fails to explain isolated peaks and groups, which are sometimes pushed up in comparatively level plains, and fails to explain the conspicuous parallelism to the seashores, while the account given of cross ranges and parallel ranges standing in isolation is unsatisfactory, it must be admitted that the theory itself is not well founded.

The assumption that since the continents began to rise from the oceans the earth has shrunk enough to produce great wrinkles in the crust comparable with our high mountains is undeniably a violent hypothesis. For in the writer's paper on the rigidity of the heavenly bodies (*A. N.*, 4104), it is shown that no circulation of currents within the earth has been possible since the globe became encrusted. If there are no currents within, the propagation of heat outward could take place only by conduction; and from Fourier's analytical theory of heat we know that the loss of heat would be extremely slow, and confined almost wholly to a shallow layer near the surface. Indeed it seems probable that the shrinkage of the entire globe is barely comparable to the secular contraction of the cooling crust alone. The approximate accuracy of this view is confirmed by the fact that the crust has not cracked open by pulling apart, as it would do if the crust shrank much more rapidly than the globe as a whole. The fact that the interior of the globe lost very little heat, while the crust cooled all the time, would lead one to think that so far from wrinkling by contraction, the crust ought to have cracked open by the shrinkage of the shell over a nearly unyielding nucleus; but no doubt the process was too slow and the rocks too plastic to give rise to actual rupture of the earth's crust.

Moreover, if the globe shrank, it is inconceivable that this shrinkage could fail to be fairly uniform in the different equal areas of the surface, and thus we should expect the resulting wrinkles to be distributed over the globe with moderate equality and uniformity. Instead of this, we find the mountains, heretofore assumed to be wrinkles, bunched into congested systems, and almost always parallel to the seashore, and larger in proportion to the depth of the adjacent ocean. Is it therefore at all credible that the mountains have really been formed by the shrinkage of the earth? Would it be going too far to say that the whole theory of secular contraction as applied to our encrusted planet is a misconception dating from a time when currents were supposed to circulate freely throughout a liquid globe?

After a careful consideration of the whole question, including all the forces at work, one may well doubt whether the radius of the earth has shrunk a mile since the continents began to emerge from the oceans. The crust could easily accommodate itself to such a small shrinkage as one part in 4,000, without producing any wrinkling whatever.

This subject of planetary wrinkles has been treated mathematically by Professor Sir G. H. Darwin, in his researches on the "Tides of a Viscous Spheroid" (*Phil. Trans. Roy. Soc.*, Part II, 1879, p. 588). And while his results are recognized to be correct, on the hypothesis, they seem to me to be inapplicable to the remote history of the earth, because I believe the shrinkage to have been nearly insensible, and certainly much less effective than has been generally supposed.

In his "Physics of the Earth's Crust," page 118, Rev. O. Fisher has discussed Darwin's theory thus:

> "It may be replied to this theory, that the formation of the existing continents cannot be looked at apart from their geological history, and that they are evidently dependent on, and as it were, gathered round, the great mountain ranges in which they culminate. Although these ranges primarily originated long ago in very early geological times, their present loftiness is due to quite late movements; and, if these had not subsequently occurred, they would before now have probably have been razed to the sea-level and have disappeared, so that, whatever cause it was which wrinkled the continents, seems to have continued active to times comparatively, if not quite, recent; and the moon is too far off now. The occurrence of great changes of level at no very distant geological period are manifest from such instances as that related by the elder Darwin."

It has always been extremely difficult to show how contraction could produce elevation of ranges, and the mechanical explanations which have been put forward are admittedly unsatisfactory.[1] The spurs which so often jut out from the main ranges do not look like wrinkles in the crust; for they are too numerous and terminate too suddenly at their extreme ends. The isolated parallel ranges so often met with in Nevada and Southern California also terminate too suddenly to be explained by shrinkage, or by lines of weakness.

In his work on the "Physics of the Earth's Crust," second edition, the Rev. O. Fisher has discussed with much care the inade-

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[1] Fisher's "Physics of the Earth's Crust," second edition, p. 123.

quacy of the contraction theory to account for the elevations of mountains actually observed upon the earth. In the case of a solid globe, he finds (p. 122) that the average height of the elevations would be only six and one-third feet. By the theory of probability we easily see that this would absolutely prevent individual elevations, even when exceptionally favored by circumstances, from attaining any considerable height. In the appendix to the second edition, page 58, he examines the mean elevations which will result from the hypothesis of a liquid substratum, and finds that when the radial contraction is 12 miles, "the mean height of all the elevations, due to the corresponding corrugation of the matter above the level owing to secular cooling, is about 44 feet." "It appears," he adds, "therefore as the result of the investigations in this chapter, that the hypothesis of a liquid substratum does not afford such an increased amount of compression as to render it possible to attribute the elevation of mountains to contraction through cooling in that case, any more than in the case of solidity."

When one recalls that our actual mountains are many hundreds and even thousands of times higher than the mean elevations resulting from the contraction theory, it is readily seen how utterly devoid of foundation that theory really is. Considerations adduced in the writer's paper on the rigidity of the heavenly bodies show that the radial contraction of twelve miles used by the Rev. O. Fisher is probably at least twelve times too large; so that the highest admissible mean elevations due to shrinkage would be only a very few feet.

It need scarcely be added that the Rev. O. Fisher must be given the chief credit for showing by long and patient research the inadequacy of this time-honored theory, which was originally suggested by Elie de Beaumont in 1829, and was no doubt a direct outgrowth of Laplace's nebular hypothesis.

We thus seem compelled to abandon the contraction theory entirely, and to explain both peaks and ranges with their striking parallelism to the coast by upheavals occurring near the sea, due to the explosive power of steam, which has heaved up the mountains from beneath. The mountains apparently show this mode of formation, and it explains with equal satisfaction cordilleras and ranges, whether of continued or isolated character, with their numerous jutting spurs and cross ranges, and isolated peaks, which are well-nigh unintelligible on any other hypothesis. And lastly it shows that all mountains are alike inside, whether they burst open and become volcanoes or remain intact.

A theory presenting so many desirable points should have a strong claim to acceptance.

In this connection one geological term in extensive use might perhaps be explained. We refer to the phrase *Line of Weakness* of the earth's crust, which was employed by Leopold von Buch to explain the arrangement of volcanoes along the seashore. It forms wherever the sea stands some time, especially if the sea is deep, because the explosive paroxysms of steam work under the edge of the sea, but not under the land, and, therefore, "lateral thrusts" from the sea begin, while they cease on the land; the result is an injection of the coast line from the direction of the sea, and mountains and volcanoes are upraised, according to the intensity and especially the difference of these forces, from the sea and land, and their duration.

It is not without significance that the height of the mountains are in general proportional to the depth of the adjacent sea, because the forces of injection depend upon the depth, and the elevations produced are proportional to the intensity of these forces. When the sea recedes, however, the extent of the land gained is proportional to the shallowness of the water, and hence arise the large flat plains in many countries. This explains the arrangement of mountains and volcanoes along the sea coast, which has, therefore, been called a line of weakness in the earth's crust. As a matter of fact, any line will prove to be weak where the sea stands for a long time, for mountains and volcanoes will be upheaved there. Thus I conceive that there is originally no such thing as a line of weakness in the crust, and we may with advantage dispense with that unfortunate term. This seems the more advisable, since the earth behaves as a solid, and local weakness developed in the formation of mountains has little effect at a distance, except in volcanic regions or ocean troughs, which act together sometimes throughout their whole extent.

It is shown in the paper on the rigidity of the heavenly bodies that the strength of a planet like the earth is not appreciably dependent upon the crust, but arises primarily from the great pressure acting throughout the body, which itself in turn depends upon the mass and density of the globe. The earth's crust, therefore, has little importance in the theory of the earth, except in our treatment of surface phenomena.

§ 20. *Is there a creeping movement of the fluid substratum beneath the crust?*

The existence of such a powerful seismic zone around the Pacific Ocean, which is surrounded by unfinished mountains and a 'fire girdle of volcanoes' leads one to inquire whether there may not be throughout this vast ocean, as well as in smaller seas, a tendency for the explosive stresses to find relief at the margins, by a slow creeping movement of the particles of the substratum towards the periphery, where the chief relief is afforded. For those stresses arising under the crust in the middle of such an ocean, some relief would be afforded by the rising and sinking of certain oceanic islands; but a greater relief would be afforded around the periphery of the sea, where the great mountain chains are in process of formation. As the crust under the sea is incessantly strained by the heaving of subterranean forces, some parts rising and others sinking, a slow creeping movement of the fluid substratum towards the periphery seems not only possible, but perhaps probable. Such a final movement would be the result of the countless earthquakes which disturb the sea bottom, and in any given earthquake the motion would be extremely slight. The creeping fluid would tend towards the avenues of escape in islands and on the margins of the sea, as well as towards areas still submerged but rising; and thus we recognize forces which under certain conditions may both elevate and depress islands in the sea; but in the long run the sinking tendency will predominate where there is water, and the rising tendency where there is land.

All along the west coast of South America Charles Darwin found conspicuous evidence of elevation within recent geological times; and at Valparaiso the amount was no less than 1,300 feet. The periodic subsidences indicated in certain places by beds of marine fossils of past geological ages, could, I think, be explained by tendencies developed under the crust, according to which the fluid substratum is alternately thickened and thinned, owing to the concurrence or non-concurrence of the subterranean forces. When they work towards a point the result is elevation, and when they tend to diverge from a point the result is depression, and the elevation is transferred to neighboring areas. This is a modern view of the periodic movement of the earth's crust so clearly foreseen by Strabo nearly 2,000 years ago.

According to this view the sea bottoms may oscillate, but on the whole tend to subside, not on account of the shrinkage of the globe, but by virtue of the gradual working out of the underlying fluid substratum, which in the long run pushes up the land.

In his "Principles of Geology," 12th edition, Vol. II, page 155, Lyell discusses with characteristic fairness the historical cases of elevation of coasts noticed in different parts of the world. We shall content ourselves with citing a very few cases of this type:

1. Islands in the sea innumerable, both volcanic and non-volcanic (apparently, though all are raised by volcanic forces).

2. The southwestern end of the Island of Crete, which even Professor Suess admits to have experienced undeniable secular elevation within the historical period.

3. The region about Pozzuoli and the Bay of Naples. This is shown by the famous temple of Jupiter Serapis, and by the elevation of the coast actually witnessed at the time of the eruption of Monte Nuovo in 1538. This raising of the land was confirmed on a larger scale for the whole Bay of Naples during the Vesuvian eruption of April, 1906 (cf. *Quarterly Journal of the Geological Society,* No. 247, 1906), by Professor Lorenzo, who found the elevation of the land at Pozzuoli to be six inches, and at Portici one foot.

4. The foundations of both Ætna and Vesuvius were laid in the sea.

5. Professor Suess cites the most ample evidence of raised beaches and other sea marks high above the present strand in almost all parts of the world. As these heights are very unequal, they cannot be explained by a simple sinking of the sea level, but there must

have been undeniable oscillations of the land, similar to, but on a larger scale, than those observed during the historical period.

6. Conclusive proofs of the upheavals of the Chilian coast during the earthquakes of 1822, 1835, etc., have been given by Lyell, and will not be repeated here. We content ourselves, therefore, with the following account of the Valparaiso earthquake of August 16, 1906, which speaks for itself:

THE GREAT EARTHQUAKE AT VALPARAISO, AUGUST 16, 1906.

A special copyrighted cablegram to the San Francisco *Examiner* of August 23, dated Valparaiso, Chili, August 22, says:

"The recent seismic disturbances in this region have thrown up several new islands in Valparaiso Bay. These islands are of various dimensions, some being very extensive while others appear to be mere cone-like rocks jutting above the waters. It is reported that islands have appeared at different points along the coast of Chili. . . .

"The wrenching given the earth's surface is still showing more and more day by day. In sections of the harbor and on the coast, the shore line has been materially changed. Promontories have slid bodily into the sea and in other places strips of coast line have been completely submerged. The theory is that there has been a great uplift of the Andes so as to change almost entirely the contour of the hilly region of the republic. Landslides are everywhere in evidence. Mountain sides have been stripped away and chasms in the hills filled up.

"Persons who have arrived here on horseback from points along the coast say that they witnessed nothing but devastation. Whole villages were wiped out."[1]

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[1] Since this paper was finished, Professor H. D. Curtis, in charge of the D. O. Mills Expedition of the Lick Observatory, at Santiago, Chili, has written an interesting letter to Professor Kroeck of the Pacific University at San Jose, which is published in the San Francisco *Argonaut* of November 3. Professor Curtis says:

"A Commission has been appointed to study the shock and its causes. I published a statement that the primary cause was doubtless the same as at San Francisco, the slipping or sliding of one stratum past another, due to the well-known geological fact that the Coast of Chili is very slowly rising. I learn since that the Bay of Valparaiso is now ten feet shallower. So I think the displacement in this shock will prove to be mainly vertical. It may be that the centre of disturbance was under the sea, as Valparaiso suffered much more than Santiago."

Professor George Davidson, President of the Seismological Society of America, informs me that during the great earthquake at Yakutat Bay, near Mt. St. Elias, Alaska, September 3–20, 1899, the land at the head of Yakutat Bay was raised 47⅓ feet. In the Bulletin of the Geological Society of America, vol. 17, May, 1906, will be found a careful investigation by Tarr

§ 21. *Avicenna's views on mountain formation.*

Lyell justly remarks that it is surprising to find among the extant fragments of Avicenna, Arabian physician and astronomer of the tenth century, a treatise on the "Formation and Classification of Minerals," characterized by considerable merit, the second chapter of which is "On the Cause of Mountains." Mountains, according to Avicenna, are formed, some by essential, others by accidental causes. And in illustration of the essential causes, he cites "a violent earthquake, by which land is elevated, and becomes a mountain." In regard to the accidental causes he mentions excavation by water or erosion, which produces cavities, such that adjoining land is made to stand out and form eminences.

The theory of mountain formation adopted in this paper was therefore foreshadowed by Avicenna in the tenth century of our era. It is extremely remarkable that so simple an explanation should have been allowed to slumber for so many centuries, while artificial and highly unsatisfactory hypotheses were in use.

V. EXPLANATION OF THE ELEVATION OF PARTICULAR MOUNTAIN RANGES AND PLATEAUS.

§ 22. *On the uplifting of the Andes.*

We have already seen that the Andes have been uplifted by the injection of lava beneath the crust in the earthquakes incident to the heaving of the Andean Valley in the adjacent sea. This has been the chief cause of the original uplift of these great mountains, and the resulting explanation suffices to account for all the principal phenomena. Thus we explain the gentle slope of the mountains

and Martin, who show that the coast was elevated for more than a hundred miles, though slight depressions also occurred in a few places. Elevations of 7 to 20 feet were common, and so little change had occurred in 1905 that Tarr and Martin were able to illustrate their memoir by photographs of the most convincing character. The barnacles and other marine animals were still adhering to the rocks, and there could be no possible doubt about the fact of the elevation. The depression of some areas was made equally clear by the encroachment of the salt water upon forests, which were thus killed. Two of the shocks at this great earthquake (September 10–15) were particularly terrible, the motions recorded in Tokio, 3,300 miles away, being ⅜ and ⅝ inch respectively. Note added December 3, 1906.

on the west and the extreme steepness of the descent on the east;[1] and we also account for the more numerous jutting spurs on the west. On the whole side spurs are much less conspicuous on the east, where descent is more rapid. These characteristics are probably the leading features of the Andes, but there are others deserving of attention, among which we may mention the following:

1. Very great volcanic violence throughout the whole range, and in the peaks of the eastern as well as of the western cordillera.

2. Enormous vertical uplifts, or fault movements, often amounting to thousands of feet, occurring throughout the cordillera, but becoming especially predominant on the eastern side.

3. The vertical uplifting of enormous plateaus such as those of Quito, Caxamarca, Cuzco and Titicaca, the latter being 12,500 feet above the sea.

The heaving of the Andean Valley in the sea seems to be the principal cause of the original elevation of the mountains, but it appears probable that after the mountains were raised to great height another secondary cause contributed to the forces operative in producing the present enormous elevation. This secondary force was nothing else than the soaking tropical rains constantly drenching the eastern slope of the mountains. As the earth's crust was already broken and faulted, the leakage of the water downward would be facilitated, while the ceaseless character of the rainfall would make the eastern slope of the Andes to all essential purposes an inland sea. Effectively, therefore, this great range of mountains is built upon a narrow strip of land with seas on both sides like the mountains in Central America, and hence, the violence of the volcanoes and earthquakes becomes more easily intelligible.

To make this theory more specific we may recall that all the principal peaks about Quito have been volcanic, and three or four volcanoes are still terribly active there now. Just east of Quito, at the head of the Amazon Valley, are the most terrible rainfalls on

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[1] Professor Solon I. Bailey, of Harvard Observatory, who crossed the Andes twice, once near Sorata, Bolivia, and again at the Aricoma Pass, and traversed the eastern range a third time through the river Urubamba, writes me that his impression is that the eastern slope is two or three times steeper than the western slope. Few observers have had better opportunities of judging of the general structure of the Central Andes than Professor Bailey.

the earth; and this region is so near to Cotopaxi, Sangay and other active vents as to leave little doubt that it contributes to the activity of the volcanoes. In Whymper's account of his "Travels among the Great Andes of the Equator," p. 240, he cites an interesting passage from the "Royal Commentaries of Peru," p. 632, which runs thus:

> "By reason of the continual Rains, and moisture of the Earth, their woolen Clothes and linen being always wet, became rotten, and dropped from their Bodies, so that from the highest to the lowest every Man was naked, and had no other covering than some few Leaves. . . . So great, and so insupportable were the Miseries which *Gonzalo Piçarro* and his Companions endured for want of Food, that the four thousand *Indians* which attended him in this Discovery, perished with Famine. . . . Likewise of the three hundred and forty *Spaniards* which entred on this Discovery, two hundred and ten dyed, besides the fifty which were carried away by *Orellana*. . . . Their Swords they carried without Scabbards, all covered with rust, and they walked barefoot, and their Visages were become so black, dry and withered, that they scarce knew one the other; in which condition they came at length to the Frontiers of *Quitu,* where they kissed the Ground, and returned Thanks to Almighty God, who had delivered them out of so many and so imminent dangers."

In his account of the ascent of Sara-urcu, Whymper says (p. 241):

> . . . The scouts came back with bad reports. The animals, they said, could go no farther; there was an end to paths and trails, except occasional wild-beast tracks; there was nothing whatever to eat, and everything must be carried; there was no place to camp upon, the whole country was a dismal swamp; and everlasting rain was falling; so much so that, although they supposed they had been near to Sara-urcu, they were quite unable to be sure. . . ."

Pages 241–242:

> ". . . This (food) arrived late, and delayed us so much that we could not reach the next camping-place by nightfall, and had to stop in a swamp, on a spot where, if you stood still, you sank up to the knees in slime. This place was just on the divide, nearly 13,000 feet above the sea, and during the greater part of the eleven hours' night sleet or rain fell, rendering it well-nigh impossible to keep up a fire out of the sodden materials. For me the men constructed a sort of floating bed, cutting down reeds, and crossing and recrossing them, piling them up until they no longer sank in the slime. For themselves they made smaller platforms of a similar description, and sat on their heels during the whole night, trying to keep up a fire. . . ."

Page 242:

> ". . . The land was entirely marshy, even where the slopes were considerable; and upon it there was growing a reedy grass to the height of

eight to ten feet, in such dense mass as to be nearly impenetrable. The machetas were found inadequate. It would have taken several weeks' labour of our whole party to have cleared a track over a single mile. The only way of getting through was by continually parting the reeds with the hands (as if swimming), and as they were exceedingly stiff, they sprang back directly we let go, and shut us out of each other's sight. The edges of the leaves cut like razors, and in a short time our hands were streaming with blood, for we were compelled to grasp the stems to prevent ourselves from sinking into the boggy soil. On this day we crossed the divide, and the streams now flowed toward the Atlantic. *The whole country was like a saturated sponge. . . .*"

Page 243:

". . . everything burnable was dripping with moisture, and the surrounding land was so wet that water oozed or even squirted out in jets when it was trodden upon. . . ."

Page 245:

". . . Rain continued without intermission. No one at Cayambe had spoken about these incessant rains. From the aspect of the country (so different from any other part of Ecuador), from the saturation of the hills, the innumerable small pools, streamlets and springs, I am convinced they are nearly perpetual. . . ."

In an excellent account of "A New Peruvian Route to the Plain of the Amazon," published in the *National Geographic Magazine* for August, 1906, Professor Solon I. Bailey, of Harvard College Observatory, describes the route through the Aricoma Pass at an altitude of 16,500 feet, and continues (p. 439):

". . . On reaching the eastern crest of these mountains, if the view is clear, one seems to be standing on the edge of the world. The eye, indeed, can reach but little of the vast panorama, but just at one's feet the earth drops away into apparently endless and almost bottomless valleys. We may call them valleys, but this does not express the idea; they are gorges, deep ravines in whose gloomy depths rage the torrents which fall from the snowy summits of the Andes down toward the plain. We might hunt the world over for a better example of the power of running water. The whole country is on edge. There all the moisture from the wet air, borne by the trade winds across Brazil from the distant Atlantic, is wrung by the mountain barrier and falls in almost continual rain.

"Near the summit of the pass only the lowest and scantiest forms of vegetable life are seen. In a single day, however, even by the slow march of weary mules, in many places literally stepping 'downstairs' from stone to stone, we drop 7,000 feet. Here the forest begins, first in stunted growths, and then, a little lower down, in all the wild luxuriance of the tropics, where moisture never fails. The lower eastern foot-hills of the Andes are more heavily watered and more densely overgrown than the great plain farther down. Here is a land drenched in rain and reeking with mists, where the bright sun is a surprise and a joy in spite of his heat. In these dense forests, with their twisting vines and hanging lianas, a man without a path can force his way with difficulty a mile a day. . . ."

From these accounts, and from the greater steepness of the eastern side of the Andes, I think it clear that the perpetual rain which falls there sinks down as if the country were overlaid by a deep sea, and thus in effect the Andes are on a narrow strip between two oceans. Hence the terrific effects of the volcanic forces, which have not only upraised the peaks and chains, but also plateaus like that of Titacaca. While the western ocean furnished the forces for the original uplift of the chain, the reeking tropical rains must have augmented these forces in the later stages of the Andean development, and this amply accounts for the activity of the volcanoes in the eastern range. These volcanoes might, it is true, be accounted for by the leakage of the ocean, yet it seems probable that the enormous surface rainfall can hardly fail to increase the volcanic violence where the range is already formed, and the rocks broken and tilted to permit of a maximum seepage of the ceaseless tropical rains which constantly soak the eastern side of the mountains.

§ 23. *On the process involved in the elevation of the Alps.*

In the "Face of the Earth," Vol. II, p. 121, Professor Suess shows that "the prevailing tangential movement in the Alps and Pyrenees" is towards the north. Thus we see that the plains of Lombardy and the Valley of the Po constituted the sea valley which was most active in exerting the northern tangential thrusts in folding the Swiss Alps. Professor Suess (Vol. I, p. 274) remarks on the similarity of the Swabian-Franconian sunken area north of the Alps, and the depression of the Adriatic to the south. An examination of almost any good map will convince anyone that the Adriatic once covered the whole of the valley of the Po. The maps given by Reclus in his large work "La Terre," p. 184, show the jutting spurs radiating from the plains of Lombardy into the surrounding mountains on all sides. This result, therefore, is a very happy confirmation of the theory. It is also satisfactory to find that the Pennine range of the Alps, nearest the valley of the Po, including Mt. Blanc, is the highest of these great mountains. The highest range of the Himalayas, including Mt. Everest, also stands nearest the valley of the Ganges; and the process of mountain-formation seems to be such that when several successive ranges are formed and completed that nearest the sea is the highest, probably because the forces there became most nearly vertical.

In Vol. II, p. 552, Professor Suess says: "As a result of *tangential thrusts* the sediment of this (Mediterranean) Sea was folded together and driven upwards as a great mountain range, and the Alps have, therefore, been described as a compressed sea."

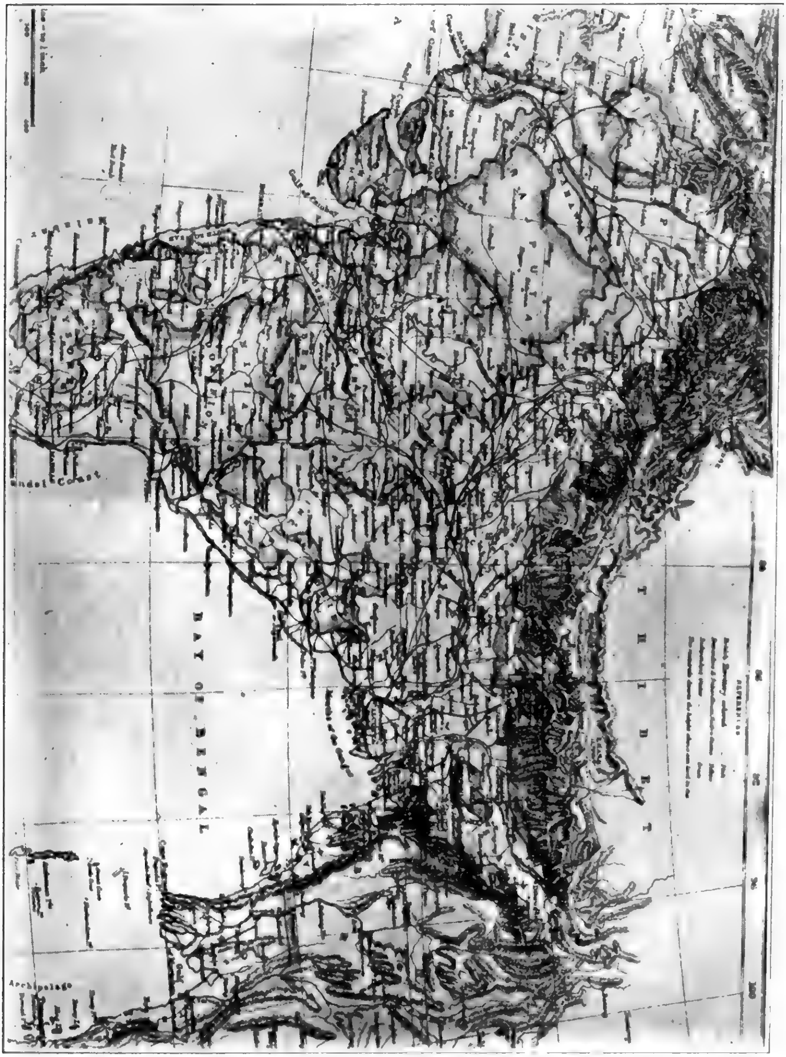
From these considerations, it is obvious that the Alps were injected from several sides, but especially from the side of the valley of the Po. In this way, the successive ranges of the Alps were formed, probably beginning near the north, and working southward; and thus we see that the valleys of Switzerland are the results of this successive wrinkling of the crust. Many lakes were formed in the Alps, and these contributed their part to the final shaping of the contours of the country. As the country was surrounded by seas and traversed by many valleys and lakes, all of which gave the water access to the bowels of the earth where breaks of the rocks were once started, the movements finally became very complex, and hence the great difficulty of unraveling the tangled skein of Alpine development. In no other way than this could such a system of mountains have arisen. The average height of the Alpine region is about 4,000 feet, but instead of a level tableland, it is a mass of broken chains and valleys, showing great horizontal crumpling, and also conspicuous and uneven vertical uplifts. The Alps, therefore, afford one of the best illustrations of the theory. For details of the various valleys of the Alps, and the great faults which mark these sunken areas, one may consult Suess, Vol. I, p. 200, et seq.

§24. *On the origin of the Himalayas and of the Plateau of Tibet.*

If we study the general character of the Himalayas by means of the excellent map given in the article, "India," *Encyclopedia Britannica,* ninth edition, we shall find that the most conspicuous feature of this great chain is the prominence of the jutting spurs on the south, facing the ocean. Perhaps a few remarks ought to be made about the process by which these spurs originate.

We have seen that it was by the injection of lava from the sea

FIG. 12.



Map of India.

valleys now occupied by the Ganges and Bramaputra that these mighty mountains were upheaved. In this process there would naturally be certain paths or outlets under the crust, along which the lava would escape most easily; and these outlets would depend upon the upheaval of the overlying strata. A break in the strata perpendicular to the direction of the chain would develop into a spur,[1] because the upheaval would then become easy, and more and more lava would be forced into the outlet thus afforded. By means of the earthquakes accompanying the expulsion of lava from the sea valley the crust is broken at various points, and thus the spurs are gradually developed. It is noticeable in all the great chains that these spurs are directed towards the sea. In the Himalayas, for example, they are on the convex side, while in the Sierra Nevadas they are on the concave side of the chain; which shows that the spurs are not due to any process of shrinkage of the earth. However the chains may curve, the spurs will be found towards the sea valleys from which the expulsions of lava have taken place. This arrangement of the spurs shows the process of mountain formation very clearly.

If we examine the map of Tibet given in the *Encyclopedia Britannica,* ninth edition, we shall find these spurs mainly on the outside of this great tableland. It was therefore injected from the seas on the north as well as on the south; for the Kuen Lun and Altin Tagh mountains on the north show the spurs almost as distinctly as the Himalayas, on the south; and on the north of Tibet the spurs point towards the Arctic ocean. Tibet was thus upheaved by forces injecting this great tableland on all sides. It is thus a very elevated plateau, enclosed by terribly high mountains. Before the upheaval had attained such great height, no doubt the enormous rainfall produced by these mountains, and hence sinking down in the faults thus opened to the bowels of the earth, contributed greatly to the uplifting forces due to the surrounding seas. In this way one may account easily and naturally for the gradual upheaval of the highest plateau in the world.

The forces depending on the valleys of the Ganges and Bramaputra are still active. And the bones of elephants and rhinoceroses now found 15,000 feet above the sea, an elevation at which these animals could not possibly have lived (cf. article "Himalayas," *Ency. Brit.*, by Gen. Strachey), show that the vertical uplift of Tibet took place in comparatively recent geological times. When this tableland had an elevation of a mile or less, it was no doubt inhabited

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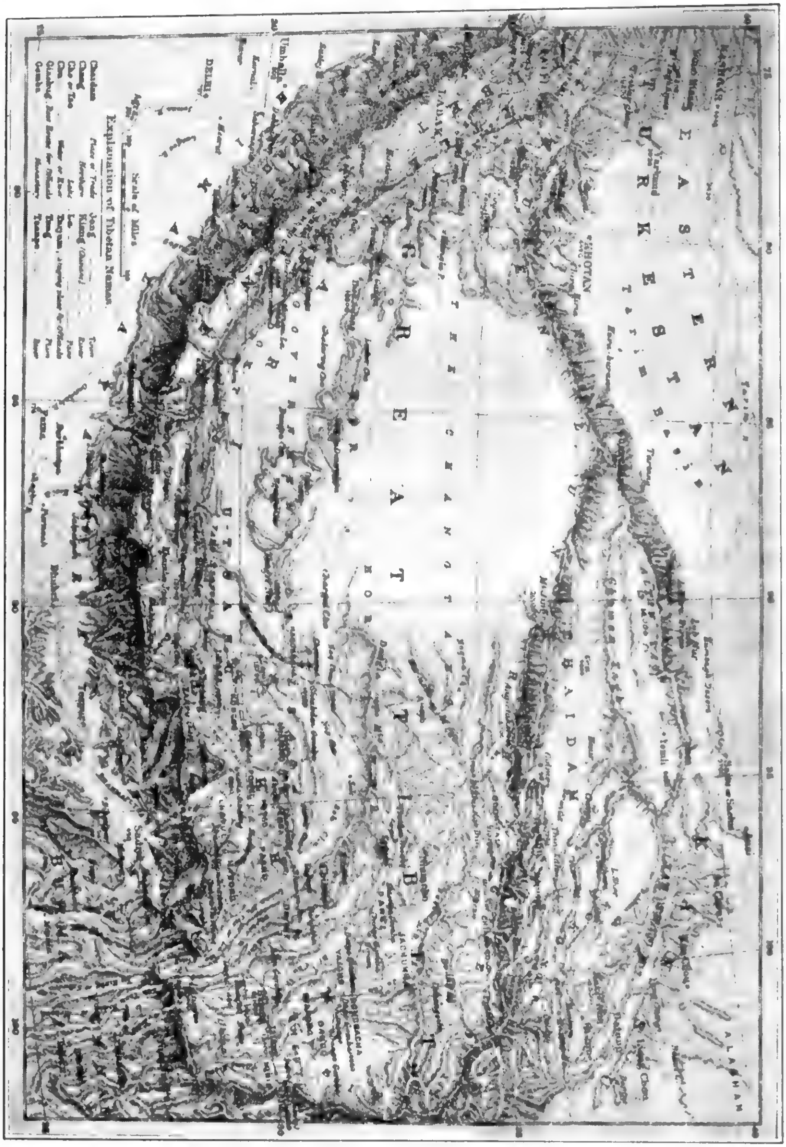
[1] In § 7 we have cited Humboldt's remarks about volcanoes in parallel ranges being connected by cross ranges, forming mountain-nodes. These are similar to spurs, and hence we see why eruptions occur at such points.

FIG. 13.



Map of Thibet.

by numerous large quadrupeds; but after it attained an altitude of two miles they had naturally deserted it; and now, at an altitude of nearly three miles we find only the bones to show that it was once habitable by such animals as now flourish in the low plains of India.

The terrific violence of the earthquakes which are still felt in northern India gives us an idea of the amazing power of the subterranean forces by which this great uplift was produced. In elevating the highest mountains of the world, it is not remarkable that they also raised the highest plateau.

In his work on the "Face of the Earth," Vol. I, p. 452, Professor Suess remarks with surprise that the tangential movement shown by the mountains has *opposite* direction on the two sides of the Bramaputra; this confirms the present theory in the most conclusive manner.

§ 25. *On the elevation of plateaus.*

It will be seen that in this paper we abandon contraction as the principal modifying cause and consider only the light matter proved to be injected under mountains and narrow plateaus by the power of steam, which gives all these masses a substratum of honeycombed material identical with the lighter lava and denser pumice. That this material now lies under these regions of greatest elevation seems certain, and in place of the contraction theory, we may substitute that of steam expansion and solidification, upon which mountain building depends. This is, of course, effected in accordance with Henry's law of gaseous absorption, and the still more general law that matter adapts itself to the pressure to which it is subjected.

It will be seen that all the mathematical reasoning is the same whether we suppose greater contraction under the oceans, or an actual heaving up of the land areas by the injection of light volcanic material more or less full of bubbles, which decreases the average specific gravity of the land and mountains.

Pendulum observations made at many points on land and sea also give the same indication as respects the earth's arrangement of density; and if the present view be sound the interpretation of these observations will now become more obvious.

If we apply the foregoing theory to a narrow plateau, like that of Mexico, which is a uniform tableland, with mountains on both sides, it will become evident that the mountains were formed when they were near the sea; and that when the shore line had remained fixed there for a long time it again receded, after the elevation of the mountains and the tableland, both of which probably were injected with light volcanic materials. Hence the substratum of such a plateau is composed of light materials similar to porous lava and pumice. Only a few mountains, as Orizaba, Popocatapetl, Colima, Jorullo, experienced such violent forces as to come to eruption, but the materials under all of them are the same. The detritus of ashes which the volcanoes eject is nothing but pumice ground to dust by friction and explosive violence. It is obvious that the pumice, ashes and other volcanic materials are not made at the time of ejection, but are always in store, and simply happen to be thrown out of any mountain which experiences eruption. We may in all probability conclude that the plateaus of Tibet, Quito, Caxamarca, Cuzco, Titicaca, and other elevated regions are underlaid by light materials like that thrown from volcanoes. This is certainly true of the great ridges of the Himalayas, Alps, Andes and other mountains which form cordilleras. In fact, the ridges of cordilleras always rest upon a substratum of light volcanic materials, and if they could be exploded from within they would dispense ashes, pumice and scoriæ as abundantly as any volcano. The quantity of this light material forced up under the ridges of mountains depends upon the elevation, and breadth, and is thus enormous in our highest ranges like the Andes and Himalayas. In this way we may explain many of the anomalies of geodesy, and pendulum observations, without any other hypothesis. Is not the accordance of observation with so simple a theory the best proof that the result represents a general law of nature?

§ 26. *On the theory that large segments of the lithosphere act as units and squeeze those segments which lie between.*

In the new work on "Geology" by Chamberlin and Salisbury, which represents the trend of current geological thought, the authors adopt a subdivision of the earth into large segments which are supposed to act as units. The contraction theory is the basis of the reasoning, and part of the discussion is as follows:

> "The downward movements are unquestionably the primary ones, and the horizontal ones are secondary and incidental. The fundamental feature is doubtless central condensation actuated by gravity, and the master movements are the sinking of the ocean-basins. The great periodic movements that made mountains and plateaus, and changed the capacity of the ocean-basins, probably started with the sinking of part or all of the ocean-bottoms.

In the greater periodic movements, probably all the basins participated more or less, but some seem to have been more active than others. For example, in the last great mountain-making period, the Pacific basin seems to have been more active than the Atlantic, while in the similar great event at the close of the Paleozoic, the opposite seems to have been true. The squeezing up of the continents doubtless took place simultaneously with the settling of the basins. The true conception is perhaps that the ocean-basins and continental platforms are but the surface forms of great segments of the lithosphere, all of which crowd towards the center, the stronger and heavier segments taking precedence and squeezing the weaker and lighter ones between them. The area of the more depressed or master segments is almost exactly twice that of the protruding or squeezed ones. This estimate includes in the latter about 10,000,000 square miles now covered with shallow water. The volume of the hydrosphere is a little too great for the true basins, and it runs over, covering the borders of the continents. The amount of the overflow fluctuates from time to time, and may be neglected in a study of the movements and deformation of the lithosphere."

Among the major group of squeezed segments we find: (1) Eurasia, (2) Africa, (3) North America, (4) South America; and the minor group includes Australia, Antarctica, the East Indian platform, and Greenland. The depressed or master segments are the oceans: (1) The Pacific, (2) the Indian, (3) the North Atlantic, (4) the South Atlantic; and a minor group of smaller seas, as the Arctic, the Mediterranean, the Caribbean, and the chain of deep pits between the Philippines and the platform of Borneo.

The authors discuss the crowding of these segments towards the center and the crumpling which follows along their edges, accompanied by fracture and slipping.

"If these segments be regarded as the great integers of body-movement, two-thirds of them taking precedence in sinking and the other third in suffering distortion, it is easy to pass to the conception of subsegments, moving somewhat differently from the main segments, so as to aid in their adjustment to one another, and thus to the conception of plateaus and deeps. It is easy also to pass to the conception of mutual crowding and crumpling at the edges of these segments, accompanied by fracture and slipping. These conceptions perhaps represent the true relations between the massive movements of the abysmal and continental segments, as well as the less massive plateau-forming movements and the mountain-forming distortions. The mountains and plateaus are probably the incidental results of the great abysmal and continental readjustments.

"The great movements are probably to be attributed to stresses that gradually accumulated until they overcame the rigidity of the thick massive segments involved, and forced a readjustment. In accumulating these stresses, some local yielding on weak lines and at special points was an inevitable incident in distributing more equably the accumulating stresses. So, also the first great readjustments probably left many local strains and unequal stresses which gradually eased themselves by warpings, minor faultings, etc., so that some minor movements were a natural sequence of the great movements."

From these citations one gets a good idea of the explanations heretofore considered most plausible.

Let us now examine this reasoning a little more closely. Is it mechanically conceivable that large segments of the lithosphere should thus act together as units? Would not such action imply that the the earth is cut deep down into the lithosphere and free to move along the boundaries of these severed segments? If the lithosphere were thus cut up into pieces, and all the segments could be regarded as solid, with abundant lubricating oil between them, they might by their mutual gravity crowd towards the center, and possibly the small ones would be squeezed between the larger. But when we recall that the earth is not cut up in this way, with oil between the pieces, but is one unbroken mass, it is clear that the greatest resistance would arise to relative motion of the parts. The friction of one part against the other would be so amazingly great that no motion would take place at the supposed joint. Even if motion occurred it is impossible to see how the crust could be crumpled without producing a corresponding condensation of the matter underlying the folded area, the density of which would thus be greatly increased, in some cases by 50 per cent. Now geodetic observations show that the density of the matter under the mountains not only is not greater than the average, but actually less; and thus we see that the folds of the crust could not be produced in this way. For in the first place, motion could not take place on account of friction; and in the second place, if it took place, the resulting condensation of the underlying matter would become sensible to geodetic measurement, which is contrary to observation.

Is it not therefore impossible to entertain the doctrine of large segments of the lithosphere moving together and squeezing others between them? Should not this whole theory, along with the hypothesis of contraction in general, be entirely given up?

VI. Confirmation of the Foregoing Theory of Mountain Building by Geodesy.

§ 27. General considerations on the attraction of mountains.

If the theory above outlined be admissible it will follow that all mountains are filled with light material like porous lava, which was originally injected as hot lava under great steam pressure, full of bubbles, and is thus similar to dense pumice. When the lava solidified, the bubbles dried up, and left a honeycombed structure of great strength, but comparatively small weight. The deep interior of all large mountains should be filled with material of this kind, which gives strength, but has low density, and the effect of its injection is to reduce the average density of the mountain below that of an equal mass of similar material taken from a plain.

It is remarkable that although some long tunnels through mountain chains have been bored by human effort, none of them has gone more than a mile or so deep, and thus all are too shallow to give us any experimental knowledge of the materials in the depths of the mountains. No large mountain has been eroded to great depth, and hence our only knowledge of the interior of mountains is derived from volcanic action, which blows out the inner portions of some of them. The mountains which happen to break out as volcanoes, either because the subterranean steam pressure is suddenly applied and abnormally great, or because they are weak in some point of their construction, are obviously not different from ordinary chains and peaks, until after they break forth.

Eruptions are always accompanied by violent earthquakes, and a mountain does not take fire and form ashes and cinders by burning, but blows out the volcanic materials already stored up in vast quantities. Thus we naturally infer that all mountains are essentially alike, but we are never able to see the inner contents except of the few of them which have become volcanoes. And if we are able to explore the interior of all mountains, it is only by studying the materials expelled from volcanoes by the explosive power of steam. It is doubtful if even the oldest mountains show erosion a mile deep, and hence we cannot penetrate the earth's crust to any depth except by an analysis of the materials which come out of the mountains after they are blown open in eruption. It is scarcely necessary to add that new ashes, cinders, scoriæ, pumice and other volcanic materials are not as a whole red hot, and therefore the bulk of them is not formed at the time of the first volcanic outbreak. They have been resting quietly under the mountain for immeasurable ages, and one cannot doubt that they exist in all mountains.

It is unfortunate that a custom has arisen of speaking of volcanic action as essentially superficial. While it cannot be said to have any fixed depth, and in many cases may not be extremely deep, it is safe to say that, as a general rule, it is by no means very shallow. On the average the depth is at least of the same order as the height of the mountains, plus the depth of the adjacent sea. There is every reason to suppose the forces which pushed up the Andes and keeps some of them in active eruption arises from about the same depth as the most violent earthquakes which visit that region. Only a few of the peaks of the Andes have become volcanoes, but some of them, as Aconcagua, in Chili, Gualateiri and Sahama, of the Sorata Range in Bolivia, and Cotopaxi and its associates near Quito in Ecuador are all very high; and the ejection of materials at that height requires deep-seated and most tremendous forces. Yet obviously only a small part of the energy originating under the base of a volcano has been spent in ejecting lava, rocks, ashes and cinders; a very large part of the energy exerted from below has been spent in raising the sides of the mountain before eruption broke out at the top.

We may then consider all mountains to be made up internally of light material like that blown out of volcanoes. Chimborazo, for example, may never have had an eruption, and yet it is impossible to believe that its constitution is essentially different from that of the neighboring mountains of Cotopaxi, Pinchinchi, Antisana, Tungurагua, Sangay, and Cayambe, all of which are volcanoes.

Now it is a very remarkable fact of observation that geodetic researches in many countries, and pretty much ever since the experiments of Bouguer and La Condamaine on Chimborazo in the year 1738, have tended to show that the matter under the roots of a great mountain is lighter than the average matter of the adjacent plain. In their experiments on the deviation of the plumb line near Chimborazo the existence of great cavities in this colossal trachytic mountain was suspected by Bouguer and La Condamaine, though, as Humboldt remarks, the evidence was unsatisfactory (*Cosmos,* Vol. V, p. 30). Yet the foregoing theory enables us to recognize that the similar widely extended indications of modern geodesy can hardly fail to be real; and according to the principle of continuity the law thus found for certain mountains must be held to be general for all the great peaks and chains of our globe. In fact the satisfactory explanation of this long-standing geodetic problem would seem to be one of the strongest arguments which could be adduced in favor of the truth of the present theory of mountain constitution and formation. For the facts of geodesy are based upon exact observation and experiment, and unbiased by any theory; and yet they become intelligible only when interpreted by means of a general theory like the present. The support thus furnished by a wide body of geodetical facts would seem to place the theory of mountain constitution on a basis where it may become a useful adjunct to the principles of geodetic measurement.

Probably no measurements involving long distances upon the earth have so high an order of accuracy as those of geodesy. Is it too much to hope that the principle of mountain constitution here outlined may become the means of still greater refinement, in the triangulation of the earth's surface?

In his well-known work on the "Figure of the Earth,"[1] Pratt announces a proposition:

> "*To deduce from the previous calculations some probable conclusions regarding the constitution of the earth's crust.* The first thing to be observed in the results given in the last paragraph is the very small amount of the resulting deflection at the two extremities of the Indian Arc—Punnæ close to Cape Comorin, and Kaliana the nearest station to the Himalayan Mountains; whereas the effect of the Ocean and Mountains has been shown to be very large. This shows that the effect of variations of density in the crust must be very great, in order to bring about this near compensation. In fact the density of the crust beneath the mountains must be less than that below the plains, and still less than that below the ocean-bed."

Again:[2]

> "The circumstance already noticed, that at seven coast stations out of thirteen the deflection is toward the sea, seems to bear testimony to the truth of the theory, that the crust below the ocean must have undergone greater

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[1] "Figure of the Earth," 3d edition, p. 134.

[2] *Ibid.*, p. 136.

contraction than other parts. The deflection toward the land at the other six coast-stations can of course easily be understood without at all calling in question the theory. The proximity of the land may easily be conceived sufficient to counteract any effect of the more distant parts of the ocean. It is the fact of even *some* of the deflections being toward the sea, that bears testimony to the theory, while the others offer no argument to the contrary."

"The least, then, that can be gathered from the deflections of these coast-stations is, that they present no obstacle to the theory so remakably suggested by the facts brought to light in India, viz., that mountain-regions and oceans on a large scale have been produced by the contraction of the materials, as the surface of the earth has passed from a fluid state to a condition of solidity—the amount of contraction beneath the mountain-region having been less than that beneath the ordinary surface, and still less than that beneath the ocean-bed, by which process the hollows have been produced into which the ocean has flowed. In fact the testimony of these coast-stations is rather in favour of the theory, as they seem to indicate, by *excess* of attraction towards the sea, that the contraction of the crust beneath the ocean has gone on increasing in some instances still further since the crust became too thick to be influenced by the principles of floatation, and that an additional flow of water into the increasing hollow has increased the amount of attraction upon stations on its shores."[1]

We have quoted this paragraph to recall how geodesists have heretofore attempted to explain the comparative lightness of the matter beneath the mountains, and the greater density of that beneath the plains and especially beneath the sea.

Pratt elsewhere says that the hidden cause of the deflection of the plumb line between the mountain masses on the north and the Indian Ocean on the south "may lie below, in the variations of the density of the earth's crust."[2] He then examines various arrangements of density, and finally calculates by three hypotheses that the sea level in Great Britain stands at a mean height of 1,567 feet higher than it would if the ocean hemisphere with pole in New Zealand became land, other things remaining equal.

§ 28. *Pratt's theorem on the equilibrium of the earth between the land and water hemispheres.*[3]

"There is no doubt that the solid parts of the earth's crust beneath the Pacific Ocean must be denser than in the corresponding parts on the opposite side, otherwise the ocean would flow away to the other parts of the earth. The following reasoning will explain this. Suppose the earth to be a sphere. Through any point on it suppose a surface drawn separating a

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[1] "Figure of the Earth," 3d edition, p. 137.

[2] *Ibid.*, p. 148.

[3] *Ibid.*, pp. 159–160.

thin portion on the right hand and through the same point a similar surface separating a like portion on the left. The sphere consists, then, of three parts, the middle portion being of a symmetrical form and attracting the point in the direction of the radius, and the two slender slices attracting it equally to the right and left of that radius. If one of these slices became fluid and of less density than the other, its attraction would be overcome by that of the other, and the fluid would be drawn away to the other parts of the sphere.

"It does not follow that the whole of the fluid would be drawn over. The above process would go in till the surface of the fluid at the circumference of the slice had become so inclined as to be at right angles to the direction of the resultant attraction of the whole mass, solid and fluid. If, however, a narrow channel were cut through this circumference (which would otherwise act as an embankment) the whole of the water would be drawn off.

"Now in the case of the earth there is a channel opening a passage from the New Zealand hemisphere into the opposite one, viz., the North and South Atlantic, and yet the ocean remains in that hemisphere. There must, therefore, be some excess of matter in the solid parts of the earth between the Pacific Ocean and the earth's centre which retains the water in its place. This effect may be produced in an infinite variety of ways; and therefore, without data, it is useless to speculate regarding the arrangement of matter which actually exists in the solid parts below."

Pratt then discusses the original fluidity of the earth, and says finally:

"The conclusion at which we have arrived in Art. 132, that the parts of the crust below the more elevated regions are of less density, and the parts beneath the depressed regions in the oceans are of greater density than the average portions of the surface, seems to bear additional testimony to the fluid theory. For it shows, that notwithstanding the varied surface, seen at present in mountains and oceans, the amount of matter in a vertical prism drawn down at various places to any given spheroidal stratum is the same although its length varies from place to place as the earth's contour varies. No better explanation of this phenomenon can be conceived than that which the fluid hypothesis furnishes; viz., that these prisms though now of different lengths, were, when the crust was fluid, of the same length; and as their lengths are now various simply from the fact that the surface in solidifying has contracted unequally, of course the amount of matter which they contain is the same in all of them."[1]

The views here expressed are very generally recognized by geodesists, and have been treated in different ways by Clarke, Tisserand, Helmert, and other leading authorities.

§ 29. *Results obtained by the United States Coast and Geodetic Survey from recent researches on the average depth of isostatic compensation.*

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[1] "Figure of the Earth," 3d edition, p. 162.

With the approval of Dr. O. H. Tittman, superintendent, Mr. J. F. Hayford has recently presented to the Washington Academy of Sciences and published in the *Proceedings* for May 18, a valuable summary of the results deduced from the geodetic operations in the United States. He has discussed with care the long series of deflections of the vertical as determined by Clarke's Standard Spheroid of 1866, which has been found to fit best for the area covered by the United States, and determined the depths at which isostatic compensation takes place. By five solutions of the problem he finds the residuals least for a depth of 71 miles. Some of the conclusions are stated thus:

> "The evidence shows clearly and decisively that the assumption of complete isostatic compensation within the depth of 71 miles is a comparatively close approximation to the truth, that the assumption of extreme rigidity is far from the truth—that the United States is not maintained in its position above the sea level by the rigidity of the earth, but is, in the main, buoyed up, floated, upon underlying material of deficient density.
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> "The conclusions just stated were based upon the 507 residuals considered as one group. The residuals have been examined in separate groups of 25, each group covering a small region. Not a single group of 25 contradicts the conclusion just stated.
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> "It is certain that for the United States and adjacent regions including oceans, the isostatic compensation is more than two-thirds complete—perhaps much more.
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> "The departure from perfect compensation may be, in some regions, in the direction of overcompensation rather than undercompensation but in either case the departure from perfect compensation is less than one-third.
>
> "In terms of stresses, it is safe to say that these geodetic observations prove that the actual stresses in and about the United States have been so reduced by isostatic adjustment that they are less than one-tenth as great as they would be if the continent were maintained in its elevated position, and the ocean floor maintained in its depressed position, by the rigidity of the earth."

Mr. Hayford assumed that the compensation is uniformly distributed with respect to the depth, but he remarks that this is only a convenient working hypothesis. Pendulum observations combined with deflection observations, he thinks, may furnish the means of detecting the distribution of compensation. Assuming that the compensation all occurs within a stratum 10 miles thick, the bottom of the stratum of isostatic compensation comes out 37 miles. Mr. Hayford, however, prefers the depth of 71 miles, though he adds that it rests upon an insecure foundation. He concurs with the view now generally held that compensation is always going on, through the changes of levels due to erosion and other causes. Mr. Hayford's paper is extremely important in showing that the depth of compensation is not great, and the principal inequalities of density are essentially limited to the earth's crust, complete average compensation for the whole country occurring within about three or four times that depth. The result, therefore, accords well with the views developed in the present investigation. In his discussion of Hayford's paper, Major Dutton, who was present, remarked that "the heavy masses of sediment which are formed upon the bottom of the sea can, I conceive, only be elevated by a positive uplifting force."

VII. The Origin of Islands and the Secular Movement of the Strand.

§ 30. *Submarine earthquakes and volcanoes.*

The eruption of volcanoes from the sea was noted by the Greeks, as we learn from Aristotle (*Meteor.*, ii, 8, 17–19), quoted by Humboldt in *Cosmos,* Vol. I, p. 240, Bohn's translation:

> "The heaving of the earth does not cease till the wind (ἄνεμος) which occasions the shocks has made its escape into the crust of the earth. It is not long ago since this actually happened at Heraclea in Pontus, and a similar event formerly occurred at Hiera, one of the Aeolian Islands. A portion of the earth swelled up, and with loud noise rose into the form of a hill, till the mighty urging blast (πνεῦμα) found an outlet, and ejected sparks and ashes which covered the neighborhood of Lapari, and even extended to several Italian cities."

Strabo (lib. i, p. 59) describes the flaming eruption observed at Methone in the year 282 B. C., near the town in the Bay of Hermione, saying a fiery mountain arose seven stadia in height, inaccessible by day on account of its heat and sulphurous flames, but emitting an agreeable odor at night. It was so hot that the sea boiled for a distance of five stadia, and was turbid and filled with detached masses of rock for full twenty stadia. This eruption is also mentioned by Ovid[1] (Metam., xv).

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[1] "Near Troezen is a tumulus, steep and devoid of trees, once a plain, now a mountain. The vapours enclosed in dark caverns in vain seek a passage by which they may escape. The heaving earth, inflated by the force of the compressed vapours, expands like a bladder filled with air, or like a goat skin. The ground has remained thus inflated and the high projecting

Another island eruption on Thera occurred forty-five years later, and the ancients were perhaps familiar with additional submarine outbreaks on the Italian coast; otherwise probably few if any more submarine volcanoes were noted till modern times.[1]

A few submarine volcanoes, either new or of recent origin, were noted by the early navigators, but the phenomenon of volcanic upheavals at sea did not attract much attention till the time of Humboldt, who made a careful study of all such outbursts. In the *Cosmos* Humboldt mentions five submarine volcanoes which appeared in the first half of the nineteenth century. Probably it would be difficult to give a complete list of all the submarine volcanoes which have ever appeared, as the records are widely scattered, and to be of value would have to be critically examined in each case. It is also clear that more such volcanoes would now be noticed than in former centuries, because the travel and exploration of the sea is more extensive than in former ages; yet even now great areas of the ocean are quite seldom visited by ships, so that the world knows little or nothing of what is going on in a large part of the globe. If a submarine volcano of short duration should be upheaved, it might again disappear and leave no record of its existence. Numerous islands are also raised in the sea without visible volcanic outlets. The average number of submarine volcanoes upheaved in a century, if careful watch could be kept on the oceans throughout the world, might prove to be something like one every two years, or 50 in a century. This, however, is merely the number of peaks which would raise their heads above the water. As most of the oceans are much too deep for them to show above the sea level, it is clear that the number which remain covered by the sea is very much greater than those which rise into view, in a ratio of perhaps something like 20 to 1. Thus in the whole earth there may be, and probably are, 1,000 submarine volcanoes erupted in a century, or an average of ten in a year. In this way we may explain some of the great sea waves so often encountered by navigators, and frequently noted by the tide gauges in civilized coun-

eminence has been solidified by time into a naked rock" (Humboldt's, *Cosmos,* Vol. I, p. 239).

[1] The list island chances in the Mediterranean recorded by Pliny in his Natural History is quite impressive (cf. Lib. ii.).

tries. Not all volcanoes thus upheaved would be sufficiently large and energetic in their action to be widely or distinctly felt, because some would be small and slow in their action and hence produce no sensible sea waves. Yet when we consider how much of the sea passes unobserved, and how many sea waves actually are reported, an average of ten submarine volcanoes per year does not seem too high an estimate. Those which are small and in deep water will escape notice entirely; and in midocean none except the powerful eruptions could make themselves felt, because there is no means of detecting a disturbance of sea level, unless it happens to be large enough to disturb a passing ship.

In his well-known article on "Geology" in the *Encyclopedia Britannica,* ninth edition, Sir Archibald Geikie says:

> "At the Hawaii Islands, on 25th February, 1877, masses of pumice, during a submarine volcanic explosion, were ejected to the surface, one of which struck the bottom of a boat with considerable violence and then floated. At the same time when we reflect to what a considerable extent the bottom of the great ocean basins is dotted over with volcanic cones, rising often solitary from profound depths, we can understand how large a proportion of the actual eruptions may take place under the sea. The foundations of these volcanic islands doubtless consist of submarine lavas and fragmentary materials, which, in each case, continued to accumulate to a height of two or three miles, until the pile reached the surface of the water and the phenomena became subaerial. The immense abundance and wide diffusion of volcanic detritus over the bottom of the Pacific and Atlantic Oceans, even at distances remote from land, as has been made known by the voyage of the 'Challenger,' may indicate the prevalence and persistence of submarine volcanic action, though at the same time, it must be admitted that an extensive diffusion of volcanic debris from the islands is effected by winds and ocean-currents."

The evidence therefore that the sea bottom is very leaky and in a constant state of eruption in many places seems conclusive. In other places very few eruptions occur, because the underlying rocks are less leaky and the sea is too shallow to exert much pressure. The number of earthquakes under the sea must be much greater than those on land, area for area; but here again few are observed, and still fewer traced to their centers, because the data are insufficient.

It is fully recognized, however, that many submarine earthquakes occur off the east coast of Japan and the Philippines, in the

Aleutian Islands, off the coast of Peru and Chili, near St. Paul's Island in the Atlantic, the Antilles and the East Indies. And while the investigation of these phenomena is still in its infancy, we may be sure they are of wide and universal distribution, especially in the deepest oceans, where the shores are steep and broken, as in volcanic regions. The southern Pacific Ocean, where so many volcanic islands exist, must be a prolific but unexplored center of such disturbances.

§ 31. *Views of Charles Darwin on the distribution of volcanic islands.*

> "During my investigations on coral reefs, I had occasion to consult the works of many voyagers, and I was invariably struck with the fact that with rare exceptions, the innumerable islands scattered throughout the Pacific, Indian, and Atlantic Oceans, were composed either of volcanic or of modern coral-rocks. It would be tedious to give a long catalogue of all the volcanic islands, but the exceptions I have found are easily enumerated: In the Atlantic we have St. Paul's Rock, described in this volume, and the Falkland Islands, composed of quartz and clay slate; but these latter islands are of considerable size, and lie not very far from the South American coast: in the Indian Ocean, the Seychelles (situated in a line prolonged from Madagascar) consists of granite and quartz: in the Pacific Ocean, New Caledonia, an island of large size, belongs (as far as is known) to the primary class. New Zealand, which contains much volcanic rock and some active volcanoes, from its size cannot be classed with the small islands, which we are now considering. The presence of a small quantity of non-volcanic rock as of clay slate on three of the Azores, or of the tertiary limestone of Maderia, or of clay-slate at Chatham Island in the Pacific, or of lignite at Kerguelen land, ought not to exclude such islands or achipelagoes, if formed chiefly of erupted matter, from the volcanic class.
>
> "The composition of the numerous islands scattered through the great oceans with such rare exceptions volcanic, is evidently an extension of that law, and the effect of the same causes, whether chemical or mechanical, from which it results, that a vast majority of the valcanoes now in action stand either as islands in the sea, or near its shores. This fact of the oceanic islands being so generally volcanic is also interesting in relation to the nature of the mountain-chains on our continents, which are comparatively seldom volcanic; and yet we are led to suppose that where our continents now stand an ocean once extended. Do volcanic eruptions, we may ask, reach the surface more readily through fissures formed during the first stages of the conversion of the bed of the ocean into a tract of land?"[1]

In connection with the above views of Darwin it should be noted that not all of the islands which are upheaved in the sea could be

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[1] "Geological Observations on Volcanic Islands," chapter VI.

expected to pour forth molten rock so as to give above the water a volcanic aspect. In some cases the islands would be raised without eruption breaking out at all, while in others the eruptive outbreaks would occur beneath the sea, and the surface appearance of such islands would be non-volcanic.

§ 32. *Dr. Rudolph's views on submarine earthquakes and eruptions.*

In the first volumes of " Beiträge zur Geophysik" (especially Vols. I and II) Dr. E. Rudolph has several elaborate papers dealing with the subject of submarine earthquakes and eruptions in an exhaustive manner. The large catalogues of such phenomena collected in this careful inquiry are certainly impressive, and the results of Dr. Rudolph's investigations may be said to confirm the view here taken that the sea bottom is in a constant state of volcanic disturbance. These disturbances are in fact so frequent that a similar view is held by many experienced navigators as the result of their own observations. What explanation can be given for such phenomena except the penetration of the sea water into the earth's crust? Volcanic outbreaks never occur except along the shores or under the sea. The significance of these disturbances of the sea bottom and their connection with the formation of islands seems therefore obvious. In the "Face of the Earth," Vol. I, p. 61, Professor Suess remarks on the calm attitude of the scholar in the midst of dangerous natural phenomena, and gives the following interesting account of Apollonius of Tyana:

> "In the year 62 or 65 A. D. Apollonius of Tyana was on the island of Crete. He was on that coast of the island which is washed by the Libyan Sea, on a promontory in the neighborhood of Phästus, and was engaged in conversation with a number of men who had come to do honor to the sanctuary on the promontory, when suddenly an earthquake took place. The roar of the thunder, says Philostratus, did not proceed from the clouds, but came from the depths of the sea, and the sea retired at least seven stadia, so that the crowd were afraid that in its retreat it would carry the temple with it, and wash them all away. Apollonius however said: '*Be comforted; the sea has brought forth new land.*' A few days later they heard that a new island had risen between Thera and Crete."

This account not only illustrates the character of a philosopher, but also shows that even Apollonius recognized that the sea brought forth new land.

§ 33. *On the upbuilding of the smaller areas by gradual upheaval.* We have already studied the process of injection from under the bed of the sea and have seen that the mountains have been upheaved in this way.

This same reasoning is immediately applicable to narrow islands like Japan and Java, with water on both sides. Not only have their mountains been thus upraised, and the volcanoes formed in the backbones of these islands, but the whole tablelands of the islands have been built up in the same way, by the gradual injection of porous lava from under the bed of the sea. If the sea was originally deeper on one side, it has given the backbone of the islands a somewhat unsymmetrical form, the stronger injection and development coming from the deeper sea, but the uplift sometimes makes a wider extent of land towards the shallower water.

This is clearly shown in the formation of numerous peninsulas, as Athos, Longos, Cassandra, Pelion, Attica, Corinth, Argolis, and the three peninsulas of the Peloponnesus in Greece; in Italy as a whole, and especially Calabria and Sicily, the latter being a triangular island with mountains facing the Tyrrhenian, Ionian and Mediterranean Seas; also in Scandinavia and Scotland; in Kamtchatka, Corea and the Malay Peninsula of Asia; and many other places. The same principle is beautifully shown in such islands as Cyprus, Crete, Eubœa and nearly all the islands of the Ægean Sea; in numerous islands along the coast of Dalmatia; in the Æolian Islands, Sardinia, Corsica, Elbe, Ischia and the island of Capri; Minorca, Majorca, Pine Islands, Isle of Wight, and numerous others around Scotland and Ireland. Good illustrations in America are found in the West Indies and the Catalina Islands off the California coast; and in Asia, it is shown beautifully by the form of Saghallien, the islands of Japan, Formosa, Sumatra, Java, and numerous islands in the East Indies; also in New Zealand; and in fact almost universally throughout the world.

In the case of Italy, the Apennines are nearly in the center of the country, but slightly nearer the Adriatic, which was the deeper sea, and did most to elevate the peninsula. The same arrangement is well illustrated in the island of Sicily, where the highest mountains face the deepest seas. All of these and many other obvious illustrations of the principle may be readily found, and must convince even the most sceptical of its general validity.

If we apply this principle to Guam, the Philippines, the Aleutian Islands, Puerto Rico and other islands of the Antilles, we shall be able to explain how the mountains of these countries have been raised, and the islands themselves developed by matter injected from under the troughs near them.[1] This seems to remove all doubt as to the process involved in the formation of islands, and the smaller land areas, and we may consider the plains and slopes of the larger land areas bordering on the sea.

If the injections are powerful, as in South America, the upheaval of matter by the power of steam brings the subsidence of the adjacent bottom and the accompanying sea waves. We need not assume, and we do not assume, any original weakness or line of weakness of the earth's crust, but simply that the ocean bed leaks, and the steam pressure tends to heave it up, and that the process works at various depths from ten to twenty miles, and is continued at irregular intervals over long periods.

This principle of ridges enables us to understand why islands rise from the sea in chains—in such cases they are really submerged and perhaps immature mountains. The Aleutian Islands are now in this state, and many others may be pointed out. When a ridge is once started, the chances greatly favor another parallel to the first, because of the way the sea bottom is depressed nearby, in forcing up the islands from beneath.

If we examine relief maps of the world,[2] we shall see by the mountains parallel to the shores exactly where the ocean used to be and how it gradually withdrew. Thus, the successive ridges of the

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[1] As throwing light upon some remarkable processes of stability in nature, it is interesting to notice that when a volcanic cone has been upheaved in the sea, and by successive uplifts raised above the water to become an island, the subsidence that eventually takes place, after much matter has been expelled from beneath the crust to make the island, does not appreciably endanger the island itself, which is powerfully braced by the conical form of its base. When a portion of the sea bottom sinks near by, to partially fill up the cavities in the crust, the settlement of the bed of the ocean scarcely disturbs the island, on account of the way in which the crust is upraised about its base, and braces it on all sides.

[2] Those of Rand, McNally & Co., given in the *Encyclopædia Americana*, have been found extremely useful.

Andes were formed, beginning on the east, and working westward; as the movement progressed the chains got higher and higher, requiring increased volcanic forces; and in the last range, therefore, occur the most and the highest volcanoes. This principle seems to hold not only in the Andes, but generally throughout the world.

§ 34. *On the formation of islands, and on the significance of the symmetrical disposition of their mountains.*

We have already explained the formation of mountains and have seen that they are not due to any shrinkage of the globe with resulting collapse of the crust, but to the injection of lava from the sea, which uplifts the crust into a parallel ridge. The subject of island formation, already alluded to in § 33, has not been much discussed, and it is impossible to say that there is any recognized theory on the subject. When the islands are small they are justly considered mountains in the sea. Some islands are volcanic and are thus volcanoes pushed up in the sea like those occasionally raised on the land. That volcanic islands are similar to ordinary land volcanoes seems clear enough; but how about the larger islands? How did they originate? To answer this question satisfactorily, we may remark that when we look at an ocean like the Pacific and notice how it is dotted all over with peaks projecting above the water, and remember that a still greater number do not reach the surface, we recognize that such peaks were no part of the original constitution of the globe, but have been developed in the sea in the course of immeasurable ages.

Now, as to the larger islands, many of them have conspicuous mountain chains, and the study of the lay of these chains is very instructive. If mountains are wrinkles in the earth's crust, due to the shrinkage of the globe, there is certainly no reason why they should be symmetrically placed on islands. On this theory, the mountains might cross the islands at any angle, or even miss the islands entirely. On actual examination, what do we find to be the fact? When we look at any good map we see the mountains running through the islands with the utmost symmetry, making in all cases a veritable backbone or central axis for the land on both sides. In no cases do the mountains run diagonally or crosswise. To appreciate the significance of this arrangement, look at the map of such islands as Cyprus, Crete, and others in the Mediterranean, Saghallien, Formosa, Sumatra, Java, and others in the East Indies. The regular symmetry of the mountains makes them in all cases the backbones of the islands, and this can only mean that they were formed in their present position by injections from the ocean on both sides; in no other way could such a symmetrical arrangement arise. The universality of this law shows that the mountains depend on the sea, and not at all on the shrinkage of the globe. The same symmetrical arrangement is shown in numerous peninsulas throughout the world.

§ 35. *Why all upheavals do not produce volcanoes.*

It is not necessary for the subterranean forces to break through and form volcanoes—the movement often becomes so deep-seated or feeble with the raising of the mountains that outbreaks do not occur. What takes place in this respect depends on the depth and recession of the sea and the suddenness and violence with which the upheaving pressure is exerted. Thus, along the east coast of South America no large volcanoes were formed, and such small ones as may have once existed have now lost all trace of a volcanic aspect, because the sea was shallow and kept retreating. On the northeast of South America, however, the Lesser Antilles are in deeper water, and when they rise to full growth may form a somewhat imposing chain of mountains, exhibiting volcanic violence depending on the depth of the sea.

In the case of the Alps the development was arrested by the rise of Italy from the Mediterranean, which stopped the sinking of the deep trough which has since become Lombardy. This was formerly the Alpine trough, and it is now so filled up by erosion that the Adriatic is the nearest sea, the recognized recession of which confirms the law.

In the case of the Himalayas also the development was arrested by the rise of the vast plain of India. And while the resulting mountain range became high, volcanic force was at length enfeebled by the shallowness of the troughs where the Indus and the Ganges now flow, and thus it is supposed that no active volcanoes broke forth on the tops of these mighty mountains. If the adjacent water had remained deep, as off the west coast of South America, the Himalayas would doubtless have been broken through by the resulting violence of the volcanic forces. As it was the shallow water, eventually supervening, gave the power for heaving the mountains little by little, and when they attained great height became so feeble or deep-seated that it left them unbroken by volcanic violence. Thus we see why the Alps and the Himalayas, in the main, failed to form volcanoes, and why Africa and Australia are also devoid of these vents (many small ones may have existed and have since lost all trace of this appearance), which chiefly develop near the deep sea, where the sudden exertion of these forces break through the mountain tops. This happens in some cases where the mountains are not very high, either because the seat of the explosion is shallow or the fractures such as to offer but little resistance from greater depth.

In order to break through high mountains, the force has to be extremely powerful, and this is not likely to be the case where the adjacent sea is shallow, as was true south of the Alps and the Himalayas.

Earthquakes in these regions, however, still continue, and have always been abundant, but they are deep-seated, owing largely to the filling in of the Alpine and Himalayan troughs, and lead to no eruptions, and hence have been called tectonic. They are clearly a survival, due to the same forces which upheaved the mountains, but the sea having so far receded they cannot blow open any cones at this late date. In fact the centers of disturbances usually are somewhat remote from the mountains at present and diffused over such an area in the ancient trough that their power for rupture is slight.

§ 36. *Gradual secular desiccation of the oceans indicated by the lowering of the strand lines throughout the world.*

The raising and lowering of the land by subterranean forces which have effected the withdrawal and encroachment of the sea over the land was first advocated by the Greek geographer Strabo, who adopted the theory of Archimedes that the figure of the ocean surface is that of a sphere (cf. Suess, " Face of the Earth," Vol. II, p. 2). This theory has been much developed in modern times and explains numerous movements of the strand line. But according to the elaborate study of this question made by Professor Suess, the elevation and subsidence theory even in the oscillatory form adopted by Lyell, is inadequate to account for the general lowering of the strand line noticed throughout the world. After his careful study of this question, Professor Suess is unable to adopt any satisfactory explanation of the observed phenomenon, and the whole problem remains an enigma to the investigator of the physics of the earth. An impartial examination of all the arguments leads me to believe that the conceptions of Strabo, as developed into the oscillatory theory advocated by Lyell, is adequate to account for many more of the phenomena of the earth's surface than Professor Suess concedes; yet it seems not only possible, but even probable, that an additional cause is at work which must contribute somewhat to the general lowering of the strand line throughout the world. This additional cause is nothing else than the secular desiccation of the oceans, brought about mainly by the sinking of the waters into the crust of the earth, where the resulting steam becomes the motive power in earthquakes.[1] A certain amount of water is also taken up as water of crystallization in the development of crystalline rocks, which extend somewhat deeper from age to age.

The view of earthquakes here adopted makes sea water the principal disturbing cause. Of the water which thus sinks below the crust, an appreciable part, but by no means all of it is expelled by volcanic action; and thus there is a steady accumulation of water deep down in the earth's crust. If this view be well founded, there is thus a secular desiccation of the waters of the globe, depending on the penetration of water to the deeper parts of the crust, and also on the absorption of water by rocks in crystallization and otherwise. How rapidly this process goes on cannot be accurately known at present; but on the basis of data observed within the historical period, we may reasonably hold that the fall of the strand line due to this cause is on the average less than a meter in 2,000 years, and most likely of the order of one-tenth of this amount.

The desiccation here postulated seems to be a *vera causa,* and this process, in connection with the oscillatory movement of the land recognized by Lyell and other geologists as necessary for the

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[1] The sinking of the sea bottom when lava is expelled from under it also increases the capacity of the ocean basins, and thus slightly lowers the strand line.

explanation of such phenomena as the coal measures, gives us a simple and natural way of explaining leading phenomena of the strand. We have elsewhere explained why the movements of faults must be attributed primarily to the formation of steam-saturated lava at the depths whence earthquakes originate.

The steam which induces this movement naturally remains hidden in the earth, and as the process goes on uninterruptedly from one geological age to another, there is a gradual secular desiccation of the waters of the sea, and a correspondingly slow lowering of the strand line.

To determine the average rate of this lowering of the strand we would have to take a figure somewhat smaller than that found in the different countries since a given geological epoch, and even then, the result would be partly vitiated by the effects of secular elevation of the land. In any case, the movement depending on secular desiccation of the oceans is extremely slow.

This view that there is a secular desiccation of the waters of the sea is not new, but was entertained in different forms by Benoist de Maillet (1692), Celsius (1743), Von Hoff (1822), Goethe and others (cf. Suess, "Face of the Earth," Vol. II, Ch. I).

Professor Suess' exhaustive discussion of the movements of the strand line will be found chiefly in chapters XII–XIV of Volume II of the "Face of the Earth." He considers the lowering of the sea level to the extent of hundreds of meters within recent geological time to be proved. The cause here suggested gives the only explanation of the phenomenon which seems at all probable or consistent with known facts.

§ 37. *On the gentle movements of the land.*

In many parts of the world, the rocks are comparatively unbroken, and leakage is very slow and gradual. This may correspond to the beds of shallow seas or to the land when level and unbroken by mountains. In all such regions the water which may seep down would give rise to a very evenly diffused subterranean steam pressure and the chances are that any movement which might take place would prove to be very slow and gradual. The strain being nearly equalized at all points, there would be no heaving required to adjust the nearly even balance of the forces, and consequently earthquakes of sensible strength would seldom or never occur. In this way we may explain the comparative immunity of many districts from earthquakes.

Yet, when an even pressure thus arises, it may produce a steady elevation of the land. Bending and warping of the strata may also occur where the stresses are steadily applied, and under the circumstances the rocks would probably change their figure slowly without snapping; when the earthquakes are more violent the rocks are broken into smaller pieces and fault movements increase.

§ 38. *Oscillations of the strand shown in such phenomena as the coal measures and fossil beds.*

No special effort is made in this paper to explain all the phenomena of the earth's crust which offer difficulty to the geologist; and hence we have been chiefly concerned with phenomena of elevation. But ever since the time of Aristotle and Strabo it has been justly remarked by sagacious observers that there has been not only elevation of the land, but under certain conditions also subsidence. Lyell and many other writers have discussed these oscillatory movements which are well exhibited by the successive layers seen in many of the coal measures. Some of these layers may be explained by the effects of damming up and drifting of vegetation to places where it did not grow; but even when allowance is made for these causes, there still seems ample evidence of an oscillatory movement of the land in many places. This is also well shown in fossil beds, where sea shells often alternate with brackish water species.

The expansion and contraction of the limits of the sea over large areas of the low-lying shore is a frequent phenomenon, and a slow oscillation of the strand seems the only rational explanation of it yet offered. Such an oscillation is most easily explained by a substratum of fluid beneath the earth's crust, such as we show to exist. Under just what conditions the land sinks is not clear. The instability may result from a number of causes of which the most probable would seem to be: upward movement of neighboring regions, thus weakening the support of the region in question and perhaps putting additional load upon it, while the underlying fluid layer slowly yields, thus causing subsidence. If during an earthquake a neighboring area should be started upward, the strain would naturally equalize itself in that direction, and the movement might continue for a long time until subterranean conditions changed. The earth's crust is complex, of unequal thickness in different parts, and broken into unequal blocks by various faults, which are continually adjusting themselves to the strains arising in the underlying substratum supporting them. That some areas should go up while others go down is therefore not at all remarkable; and most of the oscillations of the land will be found to depend upon causes of this kind. In my opinion the seat of the forces will be found to lie mainly in the fluid substratum beneath. Considering the great number of blocks into which the earth's crust has been shown to be broken by faults such as Professor Suess has so fully discussed for the regions of the Alps and the Tyrol, the oscillations of level with the changes of the strand in salt and fresh water regions seem easily accounted for.

In his work on "Meteorics" (lib. I, cap. 12) Aristotle justly remarks:

> "The distribution of land and sea in particular regions does not endure throughout all time, but it becomes sea in those parts where it was land, and again it becomes land where it was sea. . . .
>
> "And the sea also continually deserts some lands and invades others. The same tracts, therefore, of the Earth are not, some always sea, and others always continents, but everything changes in the course of time."[1]

§ 39. *Strabo's views on the elevation and depression of the land.*

In his "Principles of Geology" (pp. 24–25, 12th edition) Lyell quotes the views of Strabo regarding the elevation and depression of the land as follows:

> "It is not," says Strabo, "because the lands covered by seas were originally at different altitudes, that the waters have risen, or subsided, or receded from some parts and inundated others. But the reason is, that the same land is sometimes raised up and sometimes depressed, and the sea also is simultaneously raised and depressed, so that it either overflows or returns into its own place again. We must, therefore, ascribe the cause to the ground, either to that ground which is under the sea, or to that which becomes flooded by it, but rather to that which lies beneath the sea, for this is more movable and, on account of its humidity, can be altered with great celerity. It is proper to derive our explanations from things which are obvious, and in some measure of daily occurrence, such as deluges, earthquakes and volcanic eruptions, and sudden swellings of the land beneath the sea; for the last raise up the sea also; and when the land subsides again, they occasion the sea to be let down. And it is not merely the small, but the

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[1] Cf. Lyell's "Principles of Geology," 12th edition, Vol. I, pp. 21–22.

large islands also, and not merely the islands, but the continents which can be lifted up together with the sea; and both large and small tracts may subside, for habitations and cities, like Bure, Bizona, and many others, have been engulfed by earthquakes."

VIII. On the Great Sea Waves which frequently accompany Violent Earthquakes.

§ 40. *Great sea waves caused chiefly by the subsidence of the sea bottom after the lava has been expelled by the throes of earthquakes.*

We have already shown how the sea bottom may subside over a large area after a violent earthquake which has forced out from beneath the bed a large amount of steam-saturated lava. This lava is usually forced up under adjacent mountain ranges, mountain peaks and volcanoes, or under the intervening coast plains. If relief is afforded by forcing the column of molten matter along so that the remotest part raises in the mountains, which are at the top of the arched portion of the neighboring region of crust, we should have no means of discovering the resulting slight elevation of the peaks. For the heights of mountains are always uncertain by several feet, because extremely exact levels are difficult to establish so far above the sea. We are thus unable to say whether the various peaks and ranges are rising in height, or to tell what fluctuation of altitude they may undergo from time to time, by the heaving of earthquakes. In the case of plains near the sea it is easier to detect changes of level, but in many places characterized by violent earthquakes unfortunately no such observations are taken. Tidal observatories are best adapted to keeping record of any changes of level that may occur, because it is only by analyzing the tides carefully that we can detect changes of the sea level.

As already remarked, the sudden upheaval noticed in the seacoast of Chili after certain earthquakes was especially remarked by Charles Darwin and Capt. Fitzroy, who experienced the severe earthquake at Conception, February 20, 1835, and noticed the resulting elevations of the coast line.

Let us consider briefly the upheavals of the Chilian coast witnessed by Darwin and Fitzroy. The only reasonable explanation of this fact is that lava had been forced under the coast, and it is obvious that the matter must have been expelled from under the bed of the sea. We do not know whether the lava was injected as a thin layer or the whole body of the material under the coast slightly pushed back to afford relief of the strain under the sea, but a displacement of the latter kind seems the more probable. A sea wave of considerable magnitude[1] was noticed on that occasion, but it was not so large as sometimes develops, and the sea bottom may have subsided only very slightly. But on August 13, 1868, the whole South American coast from Valdivia in Chili to Guayaquil in Ecuador was violently shaken by a terrible earthquake, with its highest intensity near Arica. A few minutes after the earthquake the observers were surprised and alarmed to notice the sea slowly receding from the land, and very soon vessels which had been anchored in seven fathoms of water were left high and dry, with no means of escape. In a short time their surprise was converted into terror at the sight of a mighty ocean wave fifty or sixty feet high returning with terrible velocity and carrying everything before it. The vessels stranded on the beach at Arica, including the U. S. S. *Wateree,* were swept up by the gigantic wave and carried nearly a half mile inland and again left stranded higher than before.[2] The wave rolled back and after a short interval again swept the shore; and the furious oscillation of the water thus started continued for a day or two before the sea finally quieted down. This great sea wave was propagated over the Pacific and observed almost all over the world.

In 1877, May 9, another great earthquake visited the same region and was followed by a wave of even greater magnitude, of exactly the same type, the water first slowly receding from the land, and then returning as a gigantic wave carrying everything before it. This wave of 1877 is known as the Iquique wave. At Arica the hulk of the stranded U. S. S. *Wateree* was again picked up and carried still further inland, which would indicate that at Arica the height of this wave surpassed that of 1868. The sea continued to oscillate in periods of something like an hour and did not subside for a couple of days.

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[1] In his valuable work on "Earthquakes," chapter IX, Professor Milne gives a catalogue of sea waves. Different waves present different phenomena, and we here treat only of the best established types.

[2] Dutton, "Earthquakes in the Light of the New Seismology," p. 281.

These two are good types of those great sea waves in which the water recedes from the land in the first few minutes following the earthquake.

Major Dutton and others have suggested that the sea bottom sinks, and the explanation we have given of how this takes place seems satisfactory and free from objections. It is clear that if by the throes of the earthquake a large body of lava is forced from under the bed of the Andean trough, the bed might thereupon settle from twenty to fifty feet over a large area.[1] The great inrush of the water following this subsidence would withdraw it from the land, and as soon as the rushing currents met in the center of the trough, they would raise the water into a high ridge, and its subsidence would give the first great wave which rolled in upon the devastated shore. With the first depression of the water over the ridge, another inrush would take place, again withdrawing the sea from the shore, and another great wave would follow like the first, but of slightly feebler intensity. And so the oscillations of the sea would continue for a day or two, till they became reduced by friction to insensible magnitude. This explanation accords with all the known facts, and the recognized laws of fluid motion. Assuming it to be correct, the result is of interest as showing the effect of friction in destroying the motion of the sea, which has often been discussed in connection with the problem of the tides. In this case, the length of the wave of 1868 has been calculated to be about 100 geographical miles; and as the depth of the sea is between four and five miles, we see that the wave length involved is from twenty to twenty-five times the depth of the sea.

At the close of this section we shall give another possible explanation of waves of this kind, which begins with a recession of the water from the shore; but meanwhile we shall notice waves of a different class, sometimes encountered, which begin by a sudden rising of the water near shore.

The great wave which overwhelmed Simoda, Japan, December 29, 1854, may be taken as a type of those which are characterized by the sudden inrush of a great wave without any previous recession

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[1] The subsidence might be much greater if the area affected was proportionally diminished.

of the sea near the shore. At 9:15 a. m. Vice Admiral Putiatin on board the Russian frigate *Diana* noted very powerful shocks of an earthquake, and a little before ten o'clock a huge wave was seen coming which quickly overflowed the city. Major Dutton remarks that the time here involved, and the known speed of propagation of such a wave, indicates that it originated over 100 miles from the shore, in the Tuscarora trough, where the depth attained is over 4,000 fathoms.

If it originated so far away, it might have resulted from the sinking of the sea bottom, as in the Arica and Iquique waves, the withdrawal of the sea when the waters rush into the sink becoming so nearly insensible at the great distance of Simoda as to escape notice in the bay. But a more probable explanation is that the sea bottom just east of the Tuscarora deep was heaved up into a ridge with elevation of 20, 30, or 50 feet. This would produce the great wave which so suddenly appeared to overwhelm the city. In this case there would be no preliminary recession of the water whatever, and the wave would come without the least warning, as appears to have been the case.

In case the wave originated by a subsidence of the bottom of that trough, a slight withdrawal of the water from shore should have been noticed even at that great distance; but if the upheaval occurred beyond the trough, the greater inrush of water from that side may have obscured the slight recession which otherwise might be expected at Simoda. The upheaval is conceivable in the manner we have described, either with or without subsidence of the bed, and ordinarily the disturbance might be on either side of the trough; but in this case the time shows that the uplift probably was beyond the trough.

§ 41. *Another explanation of sea waves on the hypothesis of submarine eruptions.*

The only other rational way of explaining these great sea waves is by means of the uprush following the explosion of a submarine volcano. We consider his explanation much less satisfactory than that already given, but it is undeniable that in certain cases it might account for both classes of sea waves, especially where the water is deep and we can suppose the volcano to be upheaved near shore and of large size.

Let the accompanying figure illustrate a section perpendicular to the shore of the sea and land, on a coast like that of Chili or Peru, where this phenomenon has so often been observed in a typical way, and the water is deep.

Suppose a volcano to be formed at some distance, say 50 or 100 kilometers from the shore. If a great wave is produced we may assume the volcano to be raised to a height of 1,000 or 2,000 meters, or even higher; but as the sea is seven or eight kilometers deep, this will only reach one-seventh or, at most, one-fourth of the way to the surface. In the throes of the earthquake the volcano is raised, and the water forced up immediately over the eruptive center; and the steam, stones, lava, dust and ashes are driven upward towards the surface, as in the explosion of a land volcano. The explosion is resisted by the great depth of the water, which is hurled upward in a violent current from the orifice. The steam condenses to water by the low temperature of the ocean, and the other gases are absorbed, whether coming from the bed of the sea or formed in the water by the intense heat of the red hot lava. The current of steam and flying stones, lava, sand and ashes, by beating against the overlying stratum of water, forces such rapid upward movement that the level above is forced bodily upward, it may be several hundred meters. But the fluid medium is continuous and presses in on all sides, and is therefore drawn upward on all sides about the base of the cone to supply the uprush of water. The currents thus forced with enormous violence are shown in the figure. The drawing upward of the water about the base of the cone causes the inflow of water from the bed of the sea towards the base to maintain the upward movement, following the upheaval and explosion of the volcano. Thus the water near the shore some distance away is sucked down in the general lowering of the level and the sea is observed to slowly recede at the shore. All this is done in a short

FIG. 14.

Submarine volcano.

time, say 30 minutes, and then follows the sea wave which has taken form owing to the forcing up of the water over the volcano, thus forming the crest, and its withdrawal from the shore, forming the trough.

The water heaved up gradually settles and the wave approaches the shore often with a velocity of something like eight kms. per minute and thus sweeps everything before it. It then oscillates back and forth with period of some 60 minutes, and for a day or two the sea may continue to be agitated with appalling violence, and the wave propagated to the remotest parts of the earth.

If an observer were to witness such an earthquake in a region where the shore was steep and the sea of uniform depth, and should note the time of the sea wave and the direction of the normal to the wave front as it first returns, he would have a very approximate means of locating the situation of the new submarine volcano. It would lie on the normal to the circular wave front, and at a distance corresponding to the time of arrival in a sea of the given depth.

The interval τ required for the oscillation of the wave being known, the theory of the wave motion could be worked out by the general formula[1] for a wave of any length λ and any depth of the water k,

$$\tau^2 = \frac{2\pi\lambda}{g} \frac{\varepsilon^{\frac{4\pi k}{\lambda}} + 1}{\varepsilon^{\frac{4\pi k}{\lambda}} - 1} \qquad (1)$$

Perhaps it could be found with sufficient approximation by the more special forms, in which the velocity becomes, when the wave is long compared to depth of the water:

$$V = \sqrt{gk}, \text{ or } V = \sqrt{\frac{g}{2k}(k+E)(2k+E)}; \qquad (2)$$[2]

E being the height of the crest of the wave above the normal level of the water.

In this way we could find not only the distance of the eruption from the observer, but the direction, so as to fix its place with con-

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[1] Airy, "Tides and Waves," Art. 169.

[2] Report of Committee of the Royal Society on the Krakatoa Eruption, p. 94.

siderable accuracy. If the depth is not uniform, but the topography of the ocean basin known, the observer would still be able to locate the hypothetical submarine volcano with considerable precision.[1]

Within historical times several submarine volcanoes have been observed to rise above the sea about South America and elsewhere, in places where the depth was small. From the circumstances that the sea is generally very deep off the Chilian and Peruvian coast, where the most violent earthquakes occur, one would expect but very few of these volcanoes to reach the surface. Yet the large number of sea waves following violent earthquakes may afford us some idea of the activity of submarine earthquakes and perhaps volcanoes in that part of the world. It is probable that not less than one hundred such earthquakes with sea waves occur along the South American coast in a century, and of these not less than ten have done great damage.

If the supposed eruption is some distance out at sea, the effect on shore would be small, because the level is not so much changed, owing to the great body of the intervening water. Also, when a violent earthquake occurs and but slight recession of the water is noted, followed by a wave after considerable interval, the indications would point to a great eruption at considerable distance. On the other hand, if the recession of the water is quick and the wave returns after a short interval, the eruption should be comparatively near the shore. Thus by a study of the waves observed the place and character of the eruption may be approximately determined.

In some cases the sea is said to be bodily upheaved, and rises with the utmost suddenness. In such cases the volcano may be very near shore, or the sea bottom may be upheaved in the form of a ridge or cone without submarine eruption. Since the earthquakes under the sea are very numerous, there is a great probability that all these movements of the sea should be observed occasionally.

Major Dutton adopts the view that the sea bottom sinks when the sea withdraws from the shore, and I also consider this the most probable cause in the great majority of cases. It is the more logical to accept this view since we now see how a sinking of the

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[1] If such an eruption occurred the surface of the sea would be likely to show evidence of it, by ejected pumice, ashes, and other volcanic debris.

bottom can take place by natural process. So long as there was no means of explaining the subsidence of the bottom as a part of a general process in nature, the acceptance of such a violent hypothesis presented great difficulty.

To make entirely clear how collapse of the bottom may occur after the expulsion of the steam-saturated lava from under the bed of the Andean trough, we may observe that the release of the intense pent-up pressure must tend to produce a sudden and somewhat violent cooling in the stratum from which the lava is expelled. Its support of the overlying bed of the ocean trough is thus largely withdrawn, and sinking may easily follow. If this does not happen in every case (and we have no reason to think it so frequent an occurrence), it would probably follow at certain intervals, when the successive expulsions of material have reduced the underlying stratum of lava to a state of small density, in which the medium is filled very largely with bubbles of steam and therefore rapidly cooled when the pressure is released by an ejection of lava. This gives, I think, a simple conception of a self-adjusting system, such as is so often found in nature, by which the continuous process of expulsion of lava may go on, and the level of the sea bottom be adjusted automatically. But whether this is the exact process or some improvement may be suggested when our knowledge is more extended, it is clear that some such automatic mechanism is at work, and that it has operated in similar troughs all over the world throughout geological history.

As the water did not withdraw and later return as a great wave during the recent San Francisco and Valparaiso earthquakes, we know that the sea bottom did not sink in the case of either of these great disturbances. The expulsion of the lava, however, must leave the sea bottom less stable and increase the probability of its sinking when the next severe earthquakes occur at these places. As the San Francisco earthquake was much less severe than that at Valparaiso, the probability of the sea bottom sinking off the Californian coast is much less than off the coast of Chili; yet such a subsidence with the accompanying seismic sea wave is sure to come sooner or later in all places subject to heavy earthquakes. In 1812 the whole of southern California was severely shaken by earthquakes; on December 8, the water withdrew from the shore at Santa Barbara, and after a short interval returned as a great wave, which overflowed the coast and did much damage to life and property. All cities situated on such coasts should be prepared for the emergency of a seismic sea wave—that is, there should be a place of refuge for the people, and houses thus exposed to the inundations of the sea should not contain treasures and historical articles which cannot be replaced. A museum, for example, should never be built in such an exposed situation.

Perhaps a few words may be added in regard to the handling of ships, so many of which have been lost in the past, owing to the mystery surrounding these great waves. In South America the people have learned by bitter experience that when an earthquake occurs the first thing to do is to watch the sea. If it begins to withdraw from the shore they at once flee to high ground, for they know that the wave will follow. This same principle is eminently applicable to commanders of ships in the harbor. On the first indication of the retreat of the water from the shore the ships should be headed with all possible speed for the open sea. For if the ships remain in the harbor they may soon be stranded and unable to move, and sure to be carried inland when the wave returns; whereas if an effort is made to get out to sea, the ships may ride over the wave without difficulty and suffer no damage whatever. This rule is easily applied to all steam ships, whether belonging to the navy or merchant marine. Sailing vessels, being less under control than steamships, might be unable to escape in some cases; yet, if the state of the wind gave them the requisite motive power, even they might make the open sea. A wave does not come immediately after an earthquake, but something like half an hour or an hour afterwards, and this usually gives time for escape. After the sea bottom subsides, the water must flow from the shore into the depression, and then when the water piles up, it must again flow back to land to produce the wave; and if the ships are properly handled in this interval, most of them will escape undamaged.

IX. CONCLUDED THEORY OF VOLCANOES.

§ 42. *Other theories of volcanic action.*

The four fundamental facts mentioned in § 10 have been fully considered, and we have found that the hypothesis of the penetration of sea water into the crust of the earth affords a natural and satisfactory explanation of all volcanic phenomena. Such hypotheses as the following: (1) Lava flowing out of a molten interior, is contradicted by the rise and fall of the columns of lava in volcanoes, as if forced up by the elastic pressure of steam, which also escapes in eruptions; (2) molten reservoirs, contradicted by the same phenomena; and moreover neither (1) nor (2) enables us to account for the observed distribution of volcanoes; (3) melting by relief of pressure, and (4) melting by crushing, encounter the same difficulties, and others besides. None of these four hypotheses can be seriously considered.

There remains Major Dutton's recent suggestion that radium is the exciting cause. But the researches of the Hon. R. J. Strutt have shown that all the principal rocks of the earth's crust, especially granite, contain large quantities of radium, and since these rocks underlie all the continents, we should expect abundant active volcanoes everywhere inland if radium were the exciting cause, whereas in fact they appear in the depths of the sea or along the shores of the oceans. The cause of volcanic action is thus narrowed down to the penetration of water into the heated rocks of the earth's crust, and all other hypotheses may be unhesitatingly rejected.

We shall now adduce some further considerations bearing on the aqueo-igneous theory, with a view of throwing additional light upon particular phenomena.

§ 43. *Certain objections to the theory of the penetration of sea water.*

The beginning of this theory may be traced back to Lucretius, and perhaps to Aristotle,[1] and hence we shall first answer two objections which have been urged against it.

First, it is held that the temperatures of the lavas are too high, 2,000° to 3,000°, whereas one would expect the temperature to be

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[1] In more recent times it has been treated by Sir J. Prestwich, in a paper "On the agency of water in volcanic eruptions," Proc. Roy Soc., April 16, 1885.

no higher than from 700° to 1,000°. A sufficient answer to this objection is the pressure within the earth's crust, which removes the depth of fusion to a lower layer where the temperature is higher; and, moreover, in all eruptions the temperature is no doubt greatly raised by the violent churning the lava receives before reaching the orifice of the volcano, and by superheated steam escaping through it. The lava would naturally flow from that depth at which motion under the earth's crust is easiest, and the temperature observed is therefore about what should be expected.

Second, it is held that the fractured portion of the earth's crust which would permit a ready penetration of water is confined to a layer not more than five or six miles deep, the great pressure lower down operating to close all crevices; and it is therefore claimed that water going down fifteen or twenty miles would penetrate the remaining ten to fourteen miles of unbroken rock with great difficulty. In answer to this objection it may be said that the depth to which rocks are fractured is not certainly known; but whatever it may be, Daubrée's experiments show that the force of capillarity may cause the water and steam to keep on descending till the vapor reaches a temperature where it is rapidly absorbed and diffused among the rock, just as gases are in hot steel; and when the vapor becomes superheated its explosive violence is greatly increased, and hence this also would tend to raise the observed temperature of the lava, because it is chiefly the hotter lava, still further heated in ejection, which would be forced out of volcanoes.

These objections, therefore, present no serious difficulty to the theory that volcanic action depends on nothing but the penetration of sea water.

It is sometimes said that earthquakes accompanying volcanic eruptions are shallow, and it has therefore been inferred that the lava comes from no great depth. Perhaps a more correct view would be to hold that the throat of the volcano does not become closed to a great depth, by partial cooling of rock since the last eruption, and the shocks naturally proceed from this point of resistance rather than from the source of the steam and lava rising beneath the volcano, which may be much deeper, and yet give no sensible indication of their movement till the resistance becomes considerable. Besides the honeycombed pumice which underlies volcanoes, it may be supposed that they frequently contain a certain number of passages out of which the pumice has been blown, some of which may become real caverns when the lava subsides after an eruption. When a new eruption begins these old passages might offer little resistance till the lava column came within a short distance of the surface, and hence the shallowness of the shocks witnessed in eruptions. The shallowness of these shocks does not prove the superficial character of the lava erupted; on the contrary the earthquake shocks felt over the whole region around every active volcano shows that the subterranean disturbances arise in a layer which acts as fluid just beneath the crust. The forces developing in this layer find their relief in the eruption of steam and lava from the volcano.

§ 44. *The origin of volcanic ashes due principally to the breaking and grinding up of pumice.*

In his useful work on the "Volcanoes of North America," the late Professor Russell, of the University of Michigan, makes the following explanation of the origin of volcanic ashes (pp. 75–76):

> "*Sheets of Volcanic Sand and Dust.*—In the case of volcanic eruptions of the explosive type, the steam occluded in the lava expands as external pressure is relieved; this expansion is frequently so violent that the rock is disintegrated and the fragments projected high in the air. Besides this primary mode of reducing the lava to fragments, and much of it to the condition of dust, the larger fragments as they are shot upwards with a velocity in some instances even greater than the initial velocity of shells fired from modern rifle-cannon, strike against one another and against falling fragments, and are shattered, thus tending to increase the quantity of fine dust-like particles produced. While much fine material originates thus, and is carried away by the wind, many of the fragments that escape comminution fall into the the crater from which they were thrown and are again violently ejected, thus multiplying the chances of their being reduced to powder. An eruption of the explosive type thus tends to form much fine dust, which is carried high into the air by the upward rushing steam and falls most abundantly near the place of discharge. Should a strong wind be blowing, the dust is carried to leeward of the volcano, and on reaching the earth forms a sheet, which, owing to the winnowing action of the wind, is composed of finer and finer fragments, the greater the distance from the volcano."

Professor Russell appreciated more fully than many geologists the necessity of explaining the enormous clouds of dust which arise from volcanoes, but it is difficult to escape the impression that his explanation is somewhat labored. It would be more natural to say that a volcanic mountain is underlaid and filled with pumice and when the explosions become violent some of this porous material is ground up and blown out as dust. Much of it may already be in the form of powder from former earthquake shocks when the mountain was packed with pumice, and simply requires to be blown out; and hence the vast clouds which obscure the sun and darken the earth for hundreds of miles! If the rock broken up by the explosion were solid, as supposed by Russell, it would be less easy to account for the enormous outpourings of ashes, observed in such volcanoes as Hecla, Ætna, Vesuvius and Conseguina. Solid rocks would produce lapillæ and sand rather than fine ashes, which result from the breaking up of pumice with very thin bubbles.

When lava is forced up in the throat of a volcano some of it may run out, relieving the pressure which raised it, and the rest then sinks back into some of the passages which lead to the throat of the volcano. There may be, and in general probably are, several of these passages, unequally opened at different times, and the lava is forced up from some of them. After the lava is poured out and subsides, other passages formerly closed may be opened and eject vast quantities of volcanic ashes without encountering any molten rock whatever. It would be a great mistake to suppose that all ashes which pour from a volcano are forced through a layer of liquid lava before ejection. If the ashes were forced through a layer of liquid they would be red hot when cast out, and such heat would give the particles a ruddy glow. As a general rule, such a glow is not observed, and hence the theory that the ashes are ejected through a layer of liquid is untenable. The outpouring of lava is only a part of the operations of a volcano; the ejection of vapor, ashes and pumice being, perhaps, even more important. Steam is the one force which has to be relieved, and the other substances ejected are incidental thereto.

In his well-known on "Geology" (fourth edition, 1903), p. 173, Sir Archibald Geikie says:

> ". . . The finest dust is in a state of extremely minute subdivision. When examined under the microscope, it is sometimes found to consist not only of minute crystals and microlites, but of volcanic glass, which may be observed adhering to the microlites or crystals round which it flowed when

still part of the fluid lava. The presence of minutely cellular fragments is characteristic of most volcanic fragmental rocks, and this structure may commonly be observed in the microscopic fragments and filaments of glass. A characteristic feature of these minute fragments is the frequent occurrence among them of semi-circular or elliptical ('hour-glass') shapes, which evidently represent the sides of vesicles or pores that enclosed vapour or gas in the molten rock, and were disrupted and blown out during volcanic explosions."

§ 45. *On the supposed absence of volcanoes in the Alps and Himalayas, and on the former existence of these vents in the interior of continents.*

It is frequently remarked that volcanoes do not appear in the Alps and Himalayas, and the inference has been drawn that no volcanoes originated in the formation of these great mountain ranges. But it is well known that at some time in the past geological ages volcanoes existed in almost every part of every country, and mountain chains like the Alps and Himalayas are no exceptions to the general rule. Professor Suess ("Face of the Earth," Vol. I, pp. 201–274) mentions some volcanoes formerly active in the Alps, and undoubtedly similar vents once existed in the Himalayas. In the course of time nearly all surface trace of eruptions is lost where the glaciation, denudation and sedimentation are active, as in the Himalayas. When we consider how imperfect our knowledge of those mountains is, not only because they are high, but also inaccessible to exploration, the failure up to this time to find extinct volcanoes or their products is not remarkable. Craters are soon covered by ice and worn down by the grinding action of glaciers, while their ashes and lavas are equally covered and lost from view.

Major Dutton justly remarks that the regions which have been exempt from volcanoes are small in comparison with those which have had them; and he observes that going back to early Tertiary times we find them occurring where they have long been extinct.

"The grandest volcanic field in the world was central and southern India in Cretaceous times, when there was not a volcano in all Europe, and extremely few in North and South America. In the Jura-Trias, the Appalachian region, from Labrador to the Gulf of Mexico, bristled with them, and vast plateaux of lava were outpoured. In Paleozoic, they abounded in the region of the Great Lakes, in Missouri, in Arkansas, and in eastern Texas. There is hardly a county or bailiwick on the whole mundane sphere which has not had its volcanic cycle at some time or other, and there are many which have had two, three, four or five cycles. Volcanoes are not local phenomena, nor yet are they strictly universal. But they come very near being universal. I think that Charles Darwin's observation that they are associated with regions of elevation is very generally sustained, even in the depths of the ocean."

These remarks of Major Dutton, communicated to the writer in a private letter, are of unusual interest. At the present time we have no active volcano in the world more than 100 miles from the sea or equivalent large body of water. It has been remarked that Mt. Demavend is about 320 miles from the Mediterranean, but only about 50 miles from the Caspian Sea, which is a deep body of salt water. In the same way Jorullo in Mexico is about 80 miles from the Pacific coast, but it has never been active since the first outbreak in 1759. Some of the volcanoes in the eastern range of the Andes of Bolivia may be over 100 miles from the sea, but they are much nearer Lake Titicaca and the terrible tropical rains which constantly drench the eastern slopes of the Andes.

Thus all active volcanoes, to the number of about 400, are very near the sea or equivalent large lakes. In the interior of continents they die out for lack of adequate water supply. Unfortunately, we do not know the contours of the sea in past geological ages very accurately, but from Major Dutton's remarks, quoted above, it seems probable that volcanoes have always developed in the neighborhood of the sea and died out when the water receded to a considerable distance from them. This is well illustrated by the extinct volcanoes now found in the western part of the United States. Thus volcanoes of former ages seem to follow the same law as those now active.

The present distribution of volcanoes proves conclusively that they depend upon the sea. The erupting force is shown to be steam by the great preponderance (999 parts in 1,000) which that vapor has over all others.

The progressive extinction of volcanoes in the interior of continents is, therefore, clearly intelligible. Not only do the volcanoes die out for lack of motive power to keep open the orifices, but the earthquakes also famish in the same way, though to a lesser degree, because their explosive force is distributed over a wider area and does not require to be so concentrated. Unless a volcano keeps moderately active it becomes permanently closed by lava hardening in its throat; yet earthquakes which have no surface outlet may successfully maintain a languid existence on a small supply of water, and hence they may continue to be felt in a region long after all volcanoes have been extinguished.

§ 46. *Explanation of immense outflows of lava such as are seen in the plateau of Deccan and in Oregon and Utah.*

For a number of years it has been a subject of remark among geologists that the largest lava flows are not the output of volcanoes, but of immense fissures which opened in the earth's crust and permitted the welling forth of vast quantities of molten rock. Sir Archibald Geikie (cf. Suess, Vol. I, p. 145) emphasized this view as long ago as 1880. In recent years this theory of the origin of the immense deposits of sheet lava seen in the region of the Columbia River in Oregon and in Utah, as well as in the great tableland of Deccan in India, has been very generally adopted. In all such cases fissures no doubt opened and poured forth the molten rock throughout their length. The subsidence of considerable areas of the earth's crust may have contributed to this outflow, and different degrees of liquidity are invoked to explain the observed phenomena. Reyer suggests (cf. Geikie's "Geology," Vol. I, p. 301) that the degree of saturation with gases and vapors may have influenced the form of eruption, volcanic discharges resulting when the impregnation was strong enough to cause eruption, and tranquil outpourings when the rock is but feebly saturated with explosive gases.

Major Dutton thinks differences of temperature as well as chemical differences may have been more important in giving the great lava flows their peculiar aspects. In regard to these outflows in general, I believe that the crust cracked open on account of the relative movement of neighboring portions. There are many ways in which this could occur. If there were any appreciable tangential pressure between two portions of the crust, the outpouring of lava would be less easy; but since we abandon the contraction theory and deny that the mountains are due to the shrinkage of the crust, there is on this hypothesis no pressure between the two portions of the crust except that due to their weight when resting side by side. If, therefore, the subterranean movements under two neighboring parts should be such as to force a fault apart, there would be nothing to prevent the lava from rising and pouring forth.

In the valley of the Ganges the earthquake thrusts were towards the Himalayas, and at some period in the upheaval of these mountains the crust may have been cracked and also so pulled apart as to allow the great lava flows observed in the plateau of Deccan. Much of this may have occurred when the peninsula of India was still under the sea. No doubt the great lava flows of Oregon and Utah were similar in character, but the details of the process must be left to future investigation.

§ 47. *On the vapors exhaled by volcanoes.*

We have seen that, according to Geikie, 999 parts in 1,000 of the vapors emitted by volcanoes is composed of steam, and have concluded that this is the only original vapor operating under the earth's crust. For the one-thousandth part of other gases such as sulphurous oxide, carbon dioxide, hydrochloric acid, et cetera, may well be derived from the heated rocks when saturated with steam, without any other original gas existing in the earth. If any other gas except steam were really active in the earth, either under the volvanoes or in remote regions which are disturbed by earthquakes, we may be very sure that at some of the volcanoes some of this gas would escape and we should be able to recognize it. As such escape does not occur we may be quite sure that steam is the only original vapor operating within the earth.

The question of the dissociation of water vapor by the intense heat of subterranean lava is also worthy of remark. It has been found by analyses of the vapors escaping from Thera, where the outlets are generally submarine, that the water gases are dissociated. Immediately over the focus of eruption free hydrogen formed 30 per cent. of the gases emitted. In general the free hydrogen is fully twice as abundant as the free oxygen, so that the mixture on coming in contact with a burning body at once ignites with a sharp explosion. A considerable quantity of free nitrogen is also present, and traces of other gases. Fouqué, who has given most attention to this subject, infers that the water-vapor of volcanic vents may exist in a state of dissociation in the magma just beneath the earth's crust. The free nitrogen is supposed to be derived from the air absorbed in the water which percolates downward.

The question of the existence of true volcanic flames was first settled by the observations of Fouqué, who showed that true flames may arise when the free gaseous mixtures are ignited by red hot stones from the volcano or by the strokes of lightning which play so actively about the orifice of a volcano in eruption. No doubt the dreadful tongues of fire so often seen to radiate from erupting volcanoes are to be explained very largely by the ignition of free gases by thunderbolts produced by the condensation of clouds of aqueous vapor.

§ 48. *Strabo speaks of the volcanoes as safety valves.*

In his "Principles of Geology," 12 edition, Vol. 1, p. 25, Lyell remarks that the gifted Amasean geographer, Strabo, alluding to the tradition that Sicily had been separated by a convulsion from Italy, adds (Lib. vl, p. 396, edit. Almelov. Amst. 1707) that in his time the land near the sea in those parts was rarely shaken by earthquakes, since open orifices exist whereby fire and burning matter and water escape; but formerly, when the volcanoes of Ætna, the Lapari Islands, Ischia and others were closed up, the imprisoned fire and wind might have produced far more vehement movements. "The doctrine, therefore," continues Lyell, "that volcanoes are safety valves, and that the subterranean convulsions are probably most violent when first the volcanic energy shifts itself to a new quarter, is not modern."

X. CONCLUDED THEORY OF EARTHQUAKES.

§ 49. *All important earthquakes due to the action of explosive forces within or just under the earth's crust.*

Major Dutton seems to have had an inkling of the process here involved when he wrote the following ("Earthquakes," p. 49–50):

> "It remains now to refer to the possibility that many quakes whose origin is unknown, or extremely doubtful, may, after all, be volcanic. This must be fully admitted, and indeed, it is in many cases highly probable. Evidences that volcanic action has taken place in the depths of the earth without visible, permanent results on the surface abound in ancient rock exposures. Formations of great geological age, once deeply buried and brought to daylight by secular denudation, show that lavas have penetrated surrounding rock-masses in many astonishing ways. Sometimes they have intruded between strata, lifting or floating up the overlying beds without any indication of escaping to the surface. Sometimes the lava breaks across a series of strata and finds its way into the partings between higher beds. Or

it forces its way into a fissure to form a dyke which may never reach the surface. In one place a long arm or sheet of lava has in a most surprising and inexplicable manner thrust itself into the enveloping rock-mass, and in the older of metamorphic rocks these offshoots or apophyses cross each other in great numbers and form a tangled network of intrusive dykes. In other places the intruded lava formed immense lenticular masses (laccolites), which have domed up the overlying strata into mountain masses. These intrusions, almost infinitely varied in form and condition, are often, in fact usually, inexplicable as mechanical problems, but their reality is vouched for by the evidence of our senses. What concerns us here is the great energy which they suggest and their adequacy to generate in the rocks those sudden, elastic displacements which are the real initiatory impulses of an earthquake. They assure us that a great deal of volcanic action has transpired in past ages far underground, which makes no other sign at the surface than those vibrations which we call an earthquake."

The blowing out of a huge obelisk of granite some 300 meters long and 120 meters in diameter but too large to get through the orifice of Mount Pelée, and which therefore hung in the mouth of that volcano, while the cone itself was split on all sides by the fearful force of the explosion which had ejected the plug of granite from the roots of the volcano, affords an excellent and familiar example of what may be done by volcanic forces. This obelisk is illustrated in the *National Geographic Magazine* for August, 1906, by photographs made by Professor A. Heilprin, who gives an excellent account of the history of the obelisk and its gradual disintegration. If these forces are suddenly arrested, as they are in all earthquakes which do not produce immediate eruptions, the shock is taken up by the surrounding earth and we have a violent earthquake which may be felt all over the world.

If the explosions are no larger than those involved in the eruptions of geysers the results are mere microseisms or earth tremors, interesting enough to be sure, but of an unimportant character. A really serious earthquake, to be felt all over the world, implies the exertion of the most tremendous forces, and the way in which these forces set the earth particles vibrating shows that they must depend primarily upon the explosive power of steam-saturated lava at great temperature. The result is a violent shaking of the whole overlying layers of rock and the occasional upheaval of volcanoes where the strata are fractured and weak. The very way in which the earth twists, heaves, labors and vibrates shows the awfulness of the pent-up

FIG. 15.

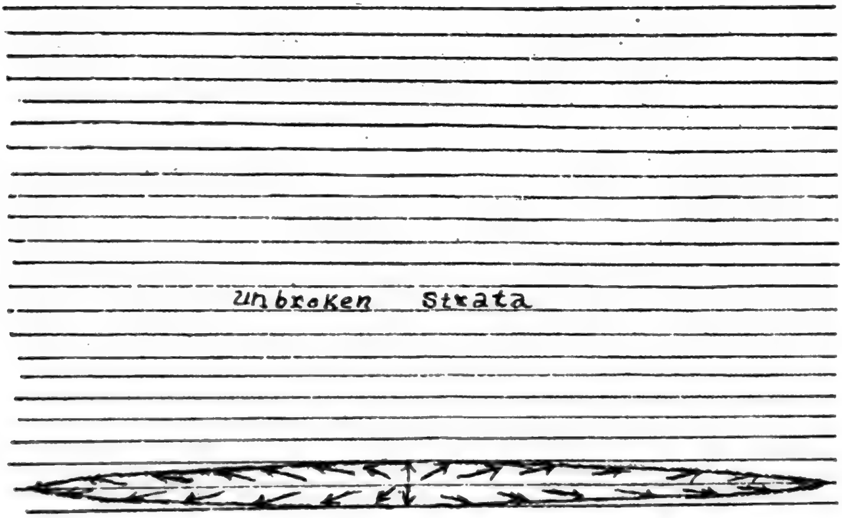
The shattered obelisk of Mt. Pelée, photograph taken by Professor Angelo Heilprin.

forces; and it is no wonder that such shaking should throw down buildings, settle soft "made" ground, leaving fissures where it is shaken down, and even cause faults in the overlying mountains to slip or open.

§ 50. *But the movements of faults are the effects of the earthquake, not the cause.*

Such small slips as are usually observed would not account for the enormous forces shown in the accompanying earthquakes. Besides the horizontal form of the vibrations, so frequently shown, as more fully pointed out in § 51, could not be explained by simple subsidence. Yet it is evident that these faults may easily result from the mighty steam pressure which has shaken the earth sufficiently to break the layers of rock, and give relief to the pent-up forces. In so-called tectonic earthquakes the forces usually are greater than those involved in volcanic action, because the whole body of the overlying strata must be shaken to afford the smallest relief, but are seldom so concentrated as would be required for the upheaval of a new volcano through the entire depth of the unbroken crust. If the crust is already badly fractured or breaks more easily than it gives relief when shaken, sometimes new volcanoes actually break forth, especially under the sea, where the explosive forces are greatest.

Fig. 16.



When we consider the terrific shock required to break all the horizontal strata in a situation where the imprisoned forces are deep-seated we can readily imagine that yielding will often occur through condensation of soft rocks, through tightening up of crevices, joints and faults, and sometimes by uplifts of all the strata, affording room for the injection of a layer of lava of large extent, or its diffusion by spreading into surrounding areas. The movement of the fluid is shown by the arrows in the figure. In that case the eruptions may be either subterranean or submarine, as in the elevation of the Chilean coast noticed by Darwin and Fitzroy in 1835.

On the theory of faults, as now held, it is difficult if not impossible to account for the observed elevations. It is perhaps true that elevations apparently are rarer than subsidences, yet they are much more significant and furnish a better criterion of the forces at work, since many subsidences on land are due to settling of soft ground which has never been consolidated under pressure. But we must remember that only very few absolute levels are accurately known and still fewer remeasured after an earthquake; and therefore while subsidences appear to be the more general phenomena, especially in regions of soft earth, one may well be very doubtful whether, in the case of hard ground, elevations, though mostly unnoticed because there is no easy means of measurement, do not really predominate. Reasons connected with the mode of formation of mountains and the elevation of coast lines, given heretofore, point strongly to elevation as the more general movement in nature. For it is this movement which has uplifted both mountains and continents, and we cannot suppose that it has ceased to be the dominant influence, though it generally escapes notice, because we have no means of detecting it, while local subsidences frequently are easily recognized, and we naturally look for it because of the frequent shaking down of "made" ground.

In most earthquakes the heaving force is distributed over a considerable area, and when the stress becomes great enough a movement takes place along the nearest fault line—the path of least resistance—and the observer who sees the slip says the movement of the fault caused the earthquake.

The fact that most earthquakes are found to originate at a depth of from ten to twenty miles shows that the epicentrum is below the depth at which the strata have any opportunity of moving; and the proof that the shock usually comes from an *area* and not from a point or from a line, shows that the shock depends on an explosive stress spread over a considerable region, and in no way depends on dislocational or fault movements, which are always quite superficial.

§ 51. *Explanation of the rotatory motion observed during an earthquake.*

If we consider with attention how simple the motion of subsidence or elevation, in a supposed fault, really is, we shall perceive that such displacement cannot give the earth particle an appreciable rotatory motion. For the two sides of a fault are conceived as continuous and unbroken parts of the earth's crust, and thus securely fixed at their backs, and moving only at their faces, where the fracture exists. The motion is essentially like that of a double cellar door opening or closing very slightly in the middle, with rigid hinges at their backs. They can only go up or down together, or one up and one down, with little or no horizontal motion; and thus cannot produce a revolving tremor when opened or shut.

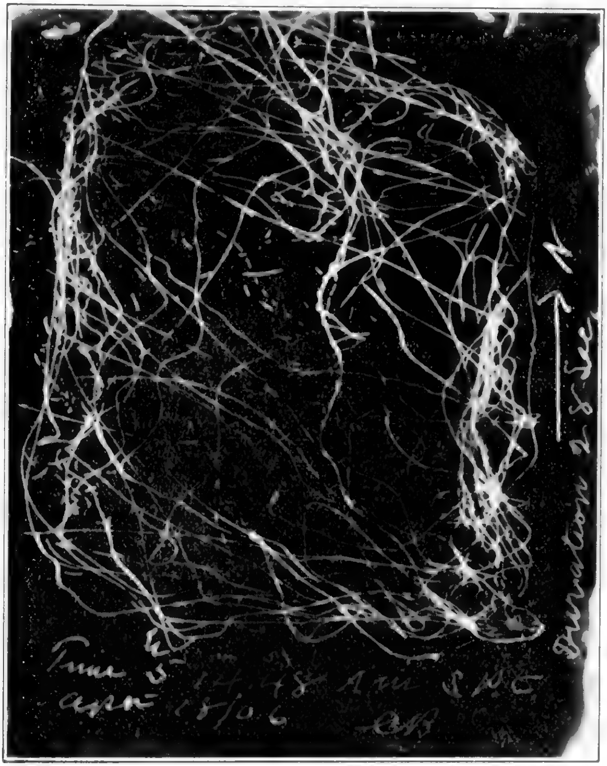
It is exactly so with the slipping of a supposed fault in the earth's crust. It is mechanically inconceivable how the vertical subsidence of a fault could give the earth particle a revolving motion in the plane of the horizon. At most, such a slip could produce an up-and-down oscillation, with the path of the ellipse described by the particle very nearly in the vertical plane.

Now the earthquake which destroyed San Francisco gave the earth particle a large and conspicuous rotation almost parallel to the horizon; it was "a twister," and this rotatory character was so marked as to attract instant attention. The conspicuous rotation and the difficulty of explaining such a motion by the theory of faults, as ordinarily stated, led the writer to question the validity of that theory. As most of the violent earthquakes have both rotation and vertical movement, it is evident that the difficulty felt in explaining the San Francisco earthquake is very generally encountered in earthquake phenomena.

It seems to have escaped the attention of seismologists that rotatory earthquakes require explanation, and the theory of subsidences of rocks and faults is incapable of furnishing it. How then can motion of rotation be accounted for?

To answer this question in the simplest way we may recall that there are many impulses which can give a motion of rotation, either by direct impact or by the "kick back," or recoil of reaction. If, for example, we suppose an orifice to be forced through an underlying or overlying layer of rock so that lava escapes under great pressure, but the resistance is unequal in the different directions, it is evident that as the imprisoned matter expands or explodes, the reactions in the different directions will be inversely as the corresponding resistances to the escaping fluid. In general the explosion will give a rotation to the surrounding particles, since the moment of the resistances will not pass exactly through the center of the exploding mass, and the rotation is as likely or more likely to be in the horizontal plane than in any other. If the explosion or rapid diffusion of steam pressure takes place in stratified rock, the strata are fairly sure to be approximately horizontal; but no doubt most all

FIG. 17.



Seismographic record of the San Francisco Earthquake, April 18, 1906, taken at the Chabot Observatory, Oakland, California, Professor Charles Burkhalter, Director.

violent earthquakes occur at greater depths, and no one can predict the inequalities of the encountered resistance. It would all depend on the shape and relative situation of the expanding matter in relation to the surrounding rock, which would also be at high temperature.

As the reservoir of steam-saturated lava would rarely be spherical and would usually be a layer, as we have seen in part IV, and several separate and distinct reservoirs might develop near one another, a streaming of the released matter, when the walls yielded and the strain is released and the fluid quickly adjusted itself to the new surroundings, would almost always produce some kind of rotatory motion, and it would always lie in a plane between the horizon and the zenith. If the layers were complex or of irregular figure this movement might be made up of several parts and the adjustment occupy several seconds, and possibly minutes, of time, even when the forces are enormous and the motion correspondingly rapid.

The successive powerful impulses or blows imparted to the surrounding earth might be of unequal intensity and not all in the same plane, and moreover the vibration would continue for a short time after the internal movement had ceased, on account of the elasticity of the rocks of the earth's crust. Lava saturated with superheated steam would behave essentially like steam in an exploding boiler, because it would give body and momentum to the spreading steam and be capable of transmitting shocks of appalling violence.

This gives us a conception not only of the process involved in an earthquake, but also of how the irregularities noted by seismographs might be accounted for; and when we recall that the subterranean boiler might surpass the largest mountain in size, or be flattened into an immense disc[1] of slight thickness, with vent chiefly or wholly at the sides, we can easily understand the terrific forces which shake the whole earth when once the surrounding walls give way or a fault moves, so that an explosion and diffusion of the lava is effected.

It must be assumed, for reasons already fully developed, that sensible readjustment and motion of large masses of steam-saturated

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[1] It is found by investigation that many of the tectonic earthquakes originate in an area of considerable extent.

lava or molten rock take place in every important earthquake; confining walls and caverns of unequal pressure are reduced to one common strain, and the resulting motion involves rapidly acting and enormously powerful forces, which may shake the whole earth and sometimes crack or derange the overlying strata of rocks many kilometers deep.

§ 52. *Explanation of the New Madrid earthquake and some other earthquakes often classed as tectonic.*

In his excellent work on "Earthquakes" Major Dutton remarks that the New Madrid earthquake could not be said to have any real depth, although he says it was strongly felt as far east as Boston, and in fact nearly all over the United States. His reasoning is based upon the supposition that it was due to the sinking of some bottom land, which seems to be quite unjustifiable. The subsidences happened, indeed, but we shall find the best reasons for holding that they were produced by explosions within the earth, which must therefore have been at least ten miles deep. Unless it had been of about this depth this earthquake could not have been so strongly felt throughout so wide an area of country.

It seems probable that the water seeped down from the Mississippi River, which always overflowed badly here, and worse in prehistoric times than now, because the country was then much more heavily timbered; or the seepage was a survival effect of the fractures of the Ozark Mountains, from the time when the Gulf of Mexico extended far up the river, and thus was beginning to form a sea valley about parallel to the general trend of the Ozark Mountains which were left unfinished.

Thus the cause producing this earthquake is apparently not different from those at Charleston, San Francisco and many other places which have experienced so-called tectonic earthquakes.

The most trustworthy accounts of the New Madrid earthquake show that it was the most violent and destructive earthquake ever felt in the United States. Lyell has given an excellent account of this earthquake, based upon observations gathered during a personal visit in 1846, but the records here followed are of still earlier date.

Over a region of 300 miles in length, but especially from the mouth of the Ohio to that of the St. Francis River, the ground rose and sank in great undulations; trees had to be felled across the fissures for the preservation of the inhabitants, and lakes were formed and drained again, and many large streams changed their channels and even their courses. Physical evidences of this terrible convulsion are still seen near New Madrid, where the loose land had settled so much as to form swamps, leaving great cypress and other trees so deep in the water that they died (some were entirely submerged), and in many places still remain to show where the water rose in 1811. Little Prairie, now called Caruthersville, 20 miles below New Madrid, was considered the center of greatest violence. The first severe earthquake occurred on the night of December 16, 2:15 a. m., and shook severely the whole region of the Ohio, Mississippi and Missouri rivers, as far north as the Lakes, as far east as the Alleghanies, south to the gulf, and west to the Rocky Mountains.

One of the best accounts of this great earthquake, drawn from contemporary sources of information, is that given by Professor G. C. Broadhead, of the University of Missouri, in the *American Geologist* for August, 1902. The leading points may be summarized thus:

1. After the first severe shock at 2:15 in the night, smaller shocks followed, and at 7 a. m., December 16, 1811, came a much more severe shock; then came lighter shocks daily or oftener, until January 23, 1812, when an extremely severe shock was felt; continual agitations were felt till February 4, which brought another severe shock, and four more followed next day; and on February 7, at 4 p. m., came one so much more violent than the preceding that it was called "the hard shock." Hundreds and even thousands of smaller after-shocks have continued at irregular intervals to the present time. "Two series of the (original) concussions were particularly terrible."

2. Thus there were at least eight very severe shocks, each of which did great damage and spread devastation far and wide.

3. The eye-witnesses generally agree that these severe shocks were preceded by heavy subterranean thunder; "a loud roaring sound like steam escaping from a boiler"; "distant rumbling sounds succeeded by discharges as if a thousand pieces of artillery were

suddenly exploded"; "a rumbling like distant thunder"; "an awful noise resembling loud and distant thunder, but more hoarse and vibrating," are some of the descriptions.

4. All the severe shocks were accompanied by the escape of sulphurous vapors from the great fissures which opened in the earth; and on December 16, after 7 a. m., the severe shock was accompanied by total darkness till sunrise; in one of the shocks witnessed by J. J. Audubon near Henderson, Kentucky, he took the cloud of vapor on the western horizon for a rising storm; and in general the eye-witnesses in Missouri agree that flashes were frequently observed as if these vapors were generating electric discharges in the air.

5. The severe earthquake shock of February 7, 1812, lasted four minutes, according to Jarred Brooks of Louisville, Kentucky, who seems to have given much attention to these phenomena, and classified them carefully. He said: "It seemed as if the surface of the earth was afloat and set in motion by a slight application of immense power, then a boiling action succeeded, houses oscillate, gables and chimneys of many houses are thrown down."

6. Great fissures running nearly north and south (Lyell's measurement in 1846 made the direction from 10° to 45° W. of N.) were formed five miles long, ten feet wide, and four feet deep, by the great undulations which came from the west; hills were sunk, forests inundated, lakes drained, swamps formed, the bed of the Mississippi River upheaved so that its waters backed up for a time, then came booming on, broke over and swept everything before them, and nearly all the shipping was destroyed; hills rose where lakes and swamps formerly stood; the waters of the Mississippi receded from its banks and then returned as a wall fifteen or twenty feet high, tearing the boats from their moorings and carrying them up a creek a quarter of a mile.

7. Water and sand, and some coal or lignite, and sulphur was ejected from the fissures, the materials being thrown forty feet high, which aided in filling the air with noxious vapors.

The ejection of this material from the ground would explain some of the noises which accompanied these earthquakes, but not all. The deep subterranean thunder preceding the shocks cannot be explained except by explosions in the earth. Some of the land was no doubt simply settled by shaking, but this will not account for the upheaval of the bed of the Mississippi River so as to make its waters flow upstream; and we seem obliged to admit that in addition to subsidence there was also elevation of ground.

The New Madrid earthquake is extremely remarkable for its numerous severe shocks and the long intervals at which they occurred, the last great shock of February 7, 1812, being the worst of all. The order of events does not harmonize so well in the tectonic theory of rock slipping as it does with Strabo's account of a volcanic outbreak near Chalcis, in Eubœa, in which the shocks ceased only after a fissure opened on the Lelantine Plain and ejected a fiery river of mud. To be sure, no lava was ejected in the New Madrid earthquake, but the phenomena resemble those described by Strabo, in which elastic vapors within the earth were seeking release. If no single outburst of lava and steam could break forth above ground the agitation might on that account continue all the longer, and an impartial study of this remarkable earthquake strongly suggests this explanation. The whole course of events is singularly inconsistent with any suggestion that mere slipping of rock or subsidence was involved.

We have examined this earthquake in detail, because it is little understood and so complex and extraordinary in respect to location and duration that it constitutes a severe test of any theory; and yet a comprehensive theory must be able to account for such phenomena without prejudice to the historical facts.

§ 53. *Other important earthquakes.*

If we examine attentively the available details of various other earthquakes which are classed as tectonic we shall find that most if not all of them give evidence of high explosive power within the earth. Under ordinary conditions it may be supposed that these forces are exerted long enough to afford partial or complete release from the strain, and then the agitation ceases or continues in the form of slight after-shocks. It is only when unlifts oçcur that we have direct proof of an uplifting force, the visible effect of subterranean explosive power; but it would seem that the heaving of the earth accompanying the rending of strata and breaking of rocks is a fairly obvious indication of imprisoned vapors seeking release from contact with molten rock.

In fact nearly all the recent important earthquakes of great violence, and inappropriately classed as tectonic, present strong indications of being due to steam power. It probably is not too much to say that it is doubtful if one of them could be fairly explained by mere subsidence due to the slipping of rock faults.

In almost every case it will be found that the rock slipping noted at the surface is small, while it is conceded that the chief effect must depend upon great forces exerted from deep down. This seems to be substantially admitted by most writers, who have a suspicion that the forces are singularly deep-seated and otherwise act in a strange way. The Charleston earthquake originated twelve miles below the surface, while the San Francisco earthquake will not prove to be of less depth. The Bengal-Assam earthquake of June 12, 1897, investigated by Mr. R. D. Oldham, must have had great depth, owing to the wide extent of country over which is was felt and the great intensity of the shocks. The same is true of the Mino-Owari earthquake of October 18, 1891, and all other earthquakes of high intensity which were widely felt. The Valparaiso earthquake of August 16, 1906, was among the most terrible of modern times, and it was so widely felt that the depth could hardly be less than fifteen or twenty miles.

It seems desirable to direct attention to the fact that probably not one of these great earthquakes has occurred in a region where seeping of water and formation of steam may not have been the dynamic cause. The center of the violent Bengal-Assam earthquake was under the great Bramaputra River, where it spreads to great width and drains an immense volume of water from the Himalayas. This region was originally a deep trough in the sea and has since been filled in; and owing to the great surface drainage is still essentially an inland sea not far from the Bay of Bengal, which receives also the Ganges as well as the Bramaputra. The situation is very similar to that of New Madrid, but is nearer the sea, and has the Himalayas on the north, from which the drainage is enormous. Besides there is in that region a natural survival of the forces which uplifted the Himalayas, and this ancient sea valley did most of the work involved in the raising of these mighty mountains.

If we examine the violent Japanese earthquakes we shall find that most of them, as Professor Milne says, arise under the sea near the seashore or at the heads of bays, where the influence of the sea would predominate. It seems impossible to doubt that they are due to the influence of steam formed in the earth beneath; for the island of Nipon is essentially narrow, mountainous and broken by great irregularity of topography, so that the seepage of water is easily accounted for, and more especially since the worst earthquakes are on the east coast and partly under the sea on the edge of the Tuscarora Deep, where the shore is steepest and the sea pressure greatest. Moreover, it is shown in § 33 that the whole island of Nipon has been uplifted by injections of matter expelled largely from under the bed of the Tuscarora Deep; and what is more natural than to suppose that this process is still going on? This inference in, fact is confirmed by the secular elevation of the east coast observed within the historical period.

The regions of Iberian Peninsula visited by violent earthquakes are those similarly exposed to the sea—Lisbon and the southern provinces, such as Andalusia, which are also broken and mountainous, and have been recently rising from the sea. In Italy the region of greatest and most violent disturbance is Calabria, the "toe of the boot," which is a long peninsula nearly surrounded by the sea and of remarkably fractured and broken topography. The islands of Sicily, Ischia, Lapari, Stromboli and the coast near Vesuvius is similar, and all these regions are still rising from the sea. Nearly all Greece is very broken and mountainous and it has always suffered severely from earthquakes. In Hungary, where the severe earthquake of Agram, Croatia, occurred November 9, 1880, the country is full of hot springs, indicating an abundance of underground water, and while the volcanoes have died out the earthquakes still survive. The earthquake in Silesia, June 11, 1895, occurred in a region of the same kind.

If now we turn to Central America we find it a narrow broken country with sea on both sides; and on the other side of the Pacific, New Zealand is a narrow island like Japan, and the presence of violent earthquakes there is not strange. The same may be said of the Aleutian Islands and the whole East Indian Archipelago, including the Philippines.

The same reasoning applies to all the South American earthquakes, including those which have destroyed Caracas, Cumana, and other places in Venezuela. This coast lies between the eastern spur of the Andes and the waters of the Caribbean Sea, which is one of the deepest parts of the Atlantic, and the intensity of the subterranean forces is shown by the violence of the volcanic outbreaks in the Lesser Antilles.

§ 54. *Are any earthquakes really tectonic?*

It is held that many earthquakes are tectonic because there is externally only a movement of the strata, as if they were seeking release from strain, and obvious volcanic forces do not appear. Boussingault long ago concluded that many of the earthquakes in the Andes depended on the settling of the strata in these mountains, and this was the beginning of the tectonic theory that most earthquakes are due to collapse or movement for release of strain. To test the validity of this theory, it is advisable to apply similar reasoning to the mountains of a country remote from the seacoast. We choose for this purpose the Rocky Mountains in Colorado, which were formerly near but are now remote from the sea. If what Boussingault witnessed in the Andes was really the settling of the mountains, the same effects ought to be going on in the Rocky Mountains of Colorado. So far as the records of history go, it may be safely said that not a single serious earthquake has ever visited Colorado, and yet many should have been felt if Boussingault's theory of the settling of the mountains is correct. As change is usually ascribed to secular cooling, why should the Andes settle and not the Rocky Mountains? From the absence of earthquakes in Colorado, it is evident that tectonic movements have ceased in that region, though secular cooling has not; and thus we see that these movements after all do not depend on settlement of the mountains due to the shrinkage of the earth. For, if so, it is incredible that the Rocky Mountains can have already attained a perfectly stable position. They ought to be still collapsing like the Andes, since secular cooling is always going on. Thus we see that all earthquakes must depend on underlying explosive forces, and not on mere adjustments of strata to secure release of strain or stability of position, required by the progress of secular cooling.

Another way to reach this result is to recall that the investigation of earthquakes in Hungary, Croatia, Bohemia and other countries shows that most frequently the impulse proceeds from an underlying area of considerable extent, and not from a point nor line, though the surface movement may be chiefly along the nearest fault. If the underlying area from which the shock proceeds is elliptical, as usually happens, we may be quite sure that this diffusion of the impulse over an area indicates an underlying outspread sheet of lava saturated with steam, which finally acquires such tension that it is enabled to shake the overlying crust and cause a movement along the fault lines where the crust is broken and movement is easiest. The lava sheet seeks readjustment, and in the process of equalizing the strain, movement of the molten rock takes place, and the faults not only move vertically, but often also horizontally. This is a natural and simple explanation of fault movement, and it accounts for the rotatory motion so frequently noticed in earthquakes. The great earthquakes of Lisbon, Arica and Iquique have usually been classed as tectonic, but in view of the sinking of the bed of the sea shown by the accompanying seismic sea waves, it is clear that all these terrible disturbances were due to the expulsion of lava from under the ocean.

§ 55. *The geological significance of earthquakes.*

In their new work on "Geology" (Vol. I, p. 534) Chamberlin and Salisbury remark that "Earthquakes are of much less importance, geologically, than many gentler movements and activities. Disastrous as they sometimes are to human affairs, they leave few distinct and readily identifiable marks which are more than temporary." Mention is made of the effects of earthquakes in fracturing the rocks of the earth's crust, but the fractures, it is pointed out, do not show at the surface when the soil is deep. These authors also remark (p. 527) that "the most prevalent (source of earthquakes) is probably the fracture of rocks and the slipping of strata on each other in the process of faulting."

Let us now examine these remarks a little more carefully. If earthquakes are due to fracture of rocks and the slipping of strata, it follows that the forces involved here have played no part in the original formation of the globe, but are the effects of collapse after the mountains were formed. On this view, earthquakes would be of very small consequence geologically, because they are associated only with destructive and not with constructive processes. But is such a view tenable? If earthquakes are not due to the same forces which upheaved the mountains, what other forces besides these have been active in the development of the globe?. The only forces of construction now felt upon the earth are those exerted in earthquakes. So far as we can see, no other constructive forces are at work. Therefore, the forces felt in earthquakes are identical with those which formed the mountains, and this is sometimes admitted, though mountain formation itself is assigned to the wrong cause.

Destructive forces such as erosion are wearing down the structure of the globe, while earthquakes are the only known forces which are building it up. We take it, therefore, that so far from being of little importance geologically the forces felt in earthquakes are of the greatest importance, and most of the constructive forces in the development of the earth are due to this cause. The destructive effects of earthquakes are only incidental to the more fundamental constructive purpose which underlies the operation of these forces. When an earthquake occurs rocks in unstable positions fall, loose sediment is shaken down, and other settlements occur, but the real constructive work consists in upheavals, little by little it may be, of mountains, islands, coasts, plateaus and larger areas. These elevations are actually witnessed in certain earthquakes, and could not possibly arise from any processes of collapse. Sometimes these constructive forces work slowly and quietly, but usually with more or less violence; and the usual method of elevation is by the injection of lava saturated with steam.

What has been taken to be the cause of earthquakes, namely, the slipping of rocks, is really the effects of more deep-seated explosive forces. Earthquakes, therefore, are not due to the effects of secular cooling, but to the vapor of steam arising from the penetration of water into the heated layers just beneath the crust. If earthquakes were due to cooling they ought to be as frequent in desert regions as in deep seas along the shores of continents, where they really are abundant.

XI. EARTH TREMORS AND TELESEISMIC DISTURBANCES.

§ 56. *Slight movements of the earth.*

A great many questions recently have been discussed relating to slight slow oscillations shown by horizontal pendulums and other instruments designed to detect delicate changes of level. No doubt very many of these changes are correctly ascribed to the yielding of the solid earth, due to variable loading of the soil by tidal, seasonal and meteorological influences. But may not others be due to the slow but steadily varying influence of subterranean forces as discussed in this paper? Some of the strains thus constantly arising would be released by microseismic disturbances which show no periodicity or regularity; while others would be cumulative and have at length a small secular effect. The fact that the ground in most places is comparatively so stable under the test of astronomical observations seems to show that these effects usually are slight, except in the neighborhood of the sea; but many small irregular disturbances occur, and it is not improbable that a considerable number of them may have their origin hidden deeper in the earth than heretofore has been suspected. If great earthquakes originate at depths of from eight to twenty miles, we may be sure that the forces there at work produce some surface changes of level even if no violent outbreaks occur.

§ 57. *Humboldt's views on earthquakes.*

In the fifth volume of the *Cosmos* (p. 288) Humboldt justly remarks how Charles Darwin, "with his peculiar generalizing view, has grasped the connection of the phenomena of earthquakes and eruptions of volcanoes under one point of view."

In his "Views of Nature," Vol. I, p. 361, Humboldt alludes to the nearly simultaneous occurrence of volcanic and seismic phenomena in places widely separated, and says:

> "All these phenomena prove that subterranean forces are manifested either dynamically, explosively, and attended by commotion, in earthquakes; or possess the property of producing, or of chemically modifying a substance in volcanoes; and they further show, that these forces are not seated near the surface in the thin crust of the earth, but deep in the interior of our planet, whence through fissures and unfilled veins they act simultaneously at widely distant points of the earth's surface."

And in his "Travels," Vol. I, p. 172–3, he adds:

"Everything in earthquakes seems to indicate the action of elastic fluids seeking an outlet to diffuse themselves in the atmosphere. Often on the coasts of the Pacific, the action is almost simultaneously communicated from Chili to the Gulf of Guayaquil, a distance of six hundred leagues; and, what is very remarkable, the shocks appear to be the stronger in proportion as the country is more distant from burning volcanoes. The granitic mountains of Calabria, covered with very recent breccias, the calcareous chain of the Apennines, the country of Pignerol, the coasts of Portugal and Greece, those of Peru and Terra Firma, afford striking proofs of this fact. The globe, it may be said, is agitated with the greater force, in proportion as the surface has a smaller number of channels communicating with the caverns of the interior. At Naples and at Messina, at the foot of Cotopaxi and of Tunguragua, earthquakes are dreaded only when vapor and flames do not issue from the craters. In the kingdom of Quito, the great catastrophe of Riobamba led several well-informed persons to think that that country would be less frequently disturbed, if the subterranean fire should break the porphyritic dome of Chimborazo; and if that colossal mountain should become a burning volcano. At all times analogous facts have led to the same hypotheses. The Greeks, who like ourselves, attributed the oscillations of the ground to the tension of elastic fluids, cited in favour of their opinion, the total cessation of the shocks at the island of Eubœa, by the opening of a crevice in the Lelantine plain."[1]

§ 58. *Views of Charles Darwin.*

In the "Voyage of a Naturalist," Chapter xiv, Darwin says:

"The forces which slowly and by little starts uplift continents, and those which at successive periods pour forth volcanic matter from open orifices, are identical. For many reasons, I believe that the frequent quakings of the earth on this line of coast (Chili) are caused by the rending of the strata, necessarily consequent on the tension of the land when upraised, and their injection by fluidified rock. I believe that the solid axis of a mountain differs in its manner of formation from a volcanic hill, only in the molten stone having been repeatedly injected, instead of having been repeatedly ejected.

"Moreover, I believe that it is impossible to explain the structure of great mountain-chains, such as that of the Cordillera, where the strata, capping the injected axis of plutonic rock, have been thrown on their edges along several parallel and neighboring lines of elevation, except on this view of the rock of the axis having been repeatedly injected, after intervals sufficiently long to allow the upper parts or wedges to cool and become solid; —for if the strata had been thrown into their present highly inclined, vertical, and even inverted positions, by a single blow, the very bowels of the earth would have gushed out; and instead of beholding abrupt mountain axes of rock solidified under great pressure, deluges of lava would have flowed out at innumerable points of every line."

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[1] "The shocks ceased only when a crevice, which ejected a river of fiery mud, opened in the plain of Lelantum, near Chalcis."—Strabo.

This view of mountain formation is essentially identical with that here adopted, except that I conceive the interior part of a mountain, whether a peak or a chain, to be filled underneath with porous lava, which explains the feebleness of the attraction of mountains and the readiness with which they are converted into volcanoes when once their tops are burst open during the paroxysms of an earthquake. The present views therefore confirm and somewhat extend those held by the elder Darwin seventy years ago. In Chapter VI of his "Geological Observations on Volcanic Islands" the great naturalist says:

> "Some authors have remarked that volcanic islands occur scattered, though at very unequal distances, along the shores of the great continents, as if in some measure connected with them. In the case of Juan Fernandez, situated 330 miles from the coast of Chile, there was undoubtedly a connection between the volcanic forces acting under this island and under the continent, as was shown during the earthquake of 1835. The islands, moreover, of some of the small volcanic groups which thus border continents, are placed in lines, related to those along which the adjoining shores of the continents trend; I may instance the lines of intersection at the Galapagos, and at the Cape de Verde Archipelagoes, and the best marked line of the Canary Islands. If these facts are not merely accidental, we see that many scattered volcanic islands and small groups are related not only by proximity, but in the direction of the fissures of eruption to the neighboring continents—a relation which Von Buch considers characteristic of his great volcanic chains.
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> "We ought not, however, to suppose, in hardly any instance, that the whole body of matter, forming a volcanic island, has been erupted at the level on which it now stands; the number of dikes, which seem invariably to intersect the interior parts of every volcano, show, on the principles of M. Elie de Beaumont, that the whole mass has been uplifted and fissured. A connection, moreover, between volcanic eruptions and contemporaneous elevations in mass[1] has, I think, been shown to exist in my work on Coral Reefs, both from the frequent presence of upraised organic remains and from the structure of the accompanying coral reefs. Finally, I may remark, that in the same Archipelago (Galagapos), eruptions have taken place within the historical period on more than one of the parallel lines of fissure: thus, at the Galapagos Archipelago eruptions have taken place from a vent on Narborough Island, and from one on Albemarle Island, which vents do not fall on the same line; at the Canary Islands, eruptions have taken place in Teneriffe and Lanzarote; and at the Azores, on the three parallel lines of Pico, St. Jorge, and Terciera. Believing that a mountain axis differs essen-

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[1] A similar conclusion is forced on us by the phenomena which accompanied the earthquake of 1835, at Conception, and which are detailed in my paper (Vol. V, p. 601) in the *Geological Transactions*.

tially from a volcano, only in pultonic rocks having been injected, instead of volcanic matter having been ejected, this appears to me an interesting circumstance; for we may infer from it as provable, that in the elevation of a mountain chain, two or more of the parallel lines forming it may be upraised and injected within the same geological period."

§ 59. *Views of Professor Milne.*

Professor Milne has recently expressed views of somewhat similar character, many of which agree closely with those reached in this paper. In the recent Bakerian Lecture before the Royal Society, he says:

> "But if, instead of confining our attention to a relationship between earthquakes, we consider the question of the relief of volcanic strain, many illustrations may be adduced which indicate a close connection between such activities. For example, all the known volcanic eruptions which have occurred in the Antilles, from the first which took place in 1692, have been heralded or closely accompanied by large earthquakes in that region, but more frequently by like disturbances in neighboring rock-folds, particularly that of the Cordilleras. This was notably the case in 1902. On April 19 of that year an unusually large earthquake devastated cities in Guatemala. Small local shocks were felt in the West Indies, and on April 25 it was noticed that steam was escaping from the crater on Mont Pelée, in Martinique. These activities continued to increase until May 8, when they terminated with terrific explosions, submarine disturbances, and the devastation of great portions of the islands of Martinique and St. Vincent.
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> "The last illustration of hypogene relationship between these regions occurred on January 31 of the present year. On that date a heavy earthquake originated off the mouth of the Esmeralda River, in Columbia. Sea-waves inundated the coast, islands sank, and a volcano erupted. The newspapers of February 2 announced that cables between Jamaica and Puerto Rico had been interrupted, and on later dates it was reported that severe shocks had been felt among the West Indian islands, that six or seven submarine cables had been broken, and that Mont Pelée and La Soufrière, in St. Vincent, were again active."[1]

We have quoted these views of Humboldt, Darwin and Milne in order to exhibit fairly the beliefs of all these great investigators in the development or the possibility of the development of seismic action at a distance when a disturbance is once started.

§ 60. *Teleseismic disturbances.*

From the theory developed in this paper we take is that when a severe earthquake is started at one place the tremors may cause disturbances to spread into neighboring regions or to break out at a distant point if the conditions of the steam pressure underlying

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[1] *Proc. Roy. Soc.*, Vol. 77, 1906, p. 374.

the crust are already highly unstable; but there is probably no communication through the depths of the planet, except a wave motion which spreads in every direction, the earth acting as an æolotropic elastic solid, because of the great pressure to which the matter is subjected. In adopting the view that, because sympathetic effects may be aroused at great distances, the disturbances are therefore very deep-seated, Humboldt appears to have been somewhat misled by the ordinary effects of tremors on unstable conditions even at great distances.

Yet there can scarcely be any doubt that the connection of the Andean trough from Valdivia to Guayaquil is so intimate that a disturbance once started under parts of it may easily be propagated over the whole length of this great trough, or even to another part of the globe; and thus we conclude that seismic activity easily extends, and has a widespread effect which was formerly supposed to be transmitted through the deep interior of the earth. It is impossible to doubt that Charles Darwin was entirely correct in concluding that a subterranean connection generally exists between a continent and its outlying islands, for both are often on the borders of the same continental trough. Occasionally the extent of this connection may be even wider, and sometimes cover a whole region or run from one region into another, as in the events mentioned above by Professor Milne; but the disturbances are transmitted principally by waves and by strains through the crust, and not by means of any currents through the deep interior of the earth. As regards the general question of slight disturbances bringing on greater catastrophes when the conditions of subterranean steam pressure are already highly unstable we may go even further. We occasionally read in History of the West Indies, especially the group including St. Vincent, Martinique, St. Lucia, Guadaloupe, Barbadoes and Trinidad, being visited by a terrible hurricane, followed by an earthquake and a "tidal wave"; so that it seems as if all the worst elements in nature were suddenly let loose to devastate these islands. In view of the cause assigned in this paper, it is evident that unstable conditions of subterranean steam pressure may not require anything more violent than the raging of a hurricane to bring on an earthquake, which in turn may be followed by a "tidal wave," for the reasons and in the manner above explained; and thus to the afflicted inhabitants all nature seems to be convulsed at once. From the observed course of events, we cannot doubt that this connection actually exists; and unfortunately it seems to be abundantly illustrated in the annals of the East as well as of the West Indies.

In the same way we explain Alexis Perrey's well-known laws of earthquakes. The forces assigned to account for these disturbances, however, are not the cause of the convulsions of nature, but only the occasions for outbreaks of highly unstable conditions depending on subterranean forces easily set off.

In like manner the violent outbreak of a volcano or the occurrence of a great earthquake in one part of the world may tend to bring on similar phenomena in another remote region.

The order of events often observed in the development of the volcanic and seismic phenomena following great outbreaks seems to support this view. While we do not regard it as proved that an eruption like the recent great outbreak of Vesuvius, for example, could indirectly bring on the earthquake in California and other similar disturbances, yet we do not regard such an influence as at all impossible. Conditions of instability once existing are contagious and tend to spread like a conflagration. As there are on the average over 60 world-shaking earthquakes every year, or more than one a week, it is evident that if one should break out in a region where it might accelerate the outbreak of others, several might be grouped into a small space of time, and these in turn might exercise wide influence on unstable conditions throughout the world, as often seems to be the case.[1] Our knowledge of these teleseismic effects, however, is still far from complete, and the settlement of the question must be left to the future.

§ 61. *Internal state of the earth.*

The investigations of Lord Kelvin and Professor Sir G. H.

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[1] In his "Journal of Researches during the Voyage of the Beagle," 1845, p. 291, Charles Darwin mentions this remarkable coincidence of phenomena: After a long slumber, Conseguina in Central America, and Aconcagua and Corcovado (S. lat. 32¾° and 43½°) in Chile, broke out the same day! His suspicion that this coincidence was not accidental would be confirmed by our knowledge of the Andean trough, in the line of which all these volcanoes are situated.

Darwin have shown that the earth is highly rigid, and in a recent paper on the rigidity of the heavenly bodies (cf. *Astron. Nachr.*, No. 4104) the writer has shown that no motion of currents deep down in our planet is really possible, because of the enormous friction due to the pressure at great depths. Thus no currents of fluids or gases exist in the earth, except just under the crust where the explosive strain is terrific. And even near the surface, where the lava is forced to move under the thin crust, in the building of mountain chains like the Andes, the motion is usually accomplished only by the dreadful paroxysm of an earthquake, which expels the molten rock from under the bed of the sea. The suspicion of Capt. Fitzroy and Charles Darwin that in the three months following the earthquake of 1835 the Chilean coast partially subsided to its former level seems not only possible, but extremely probable. Under great strain the viscous mass may have yielded somewhat, and thus there may have been a slow creeping tendency towards the former level.

Judging by the thickness of the sides of Aconcagua, Cotopaxi and other typical volcanoes of the Andes, one would probably be justified in concluding that the thickness of the crust under which the lava moves when expelled is not less than five miles, and it may be as much as fifteen or twenty, but ordinarily it could not well lie outside of these limits. For if we suppose it to be thicker, the leakage of the water would present greater difficulty if the temperature is low; and such thickness would not be required if adequate steam developed nearer the surface. On the other hand, the thickness could hardly be much less than ten miles without enfeebling the layers which must support great strain in the expulsion of matter from the broad trough of the ocean bed. The most probable thickness is from ten to twenty, and for most purposes we shall be safe in adopting the simple mean of fifteen miles.

There are other considerations which lead to substantially the same conclusion. It is found for example by the critical investigation of great earthquakes that most of these disturbances proceed from an average depth of something like ten to fifteen miles. Now, if the theory here developed be admissible, it will follow that these disturbances usually are in or near the lower stratum of the earth's crust, which thus fixes the thickness of the layer at about the same figure. It will be evident from this consideration and others that the thickness of the crust is by no means uniform throughout the globe. Determinations of the depths of large earthquake disturbances are probably the best means of approximating the thickness of the earth's crust, since data of this kind depend wholly upon observation and are indepenedent of any hypothesis.

XII. CONCLUSIONS.

§ 62. *Summary of results.*

1. We have seen that deposits of sediment on the continental shelves could not possibly produce anything but the most gradual increase of weight on these portions of the earth's crust; and since such rocks as marble are proved to be fluids of great viscosity and therefore capable of slow secular bending without rupture, we may feel sure that any stresses thus arising in the earth's crust would be relieved by gradual yielding, and that no violent earthquake shock could ever arise from such a cause.

2. The theory that earthquakes are due to fracture and slipping of rocks is disproved by the great depth (ten to twenty miles) at which world-shaking earthquakes are found to originate, and by virtue of the fact that they come not from a point nor from a line, but from an area; and moreover earthquakes follow the seashore, seldom occurring far inland, and never in desert countries, though abundant in the bed of the ocean.

3. It therefore follows that earthquakes must depend upon explosive forces within or just under the earth's crust, and frequently spread over a considerable area, and the preponderance of disturbances in the sea and along the shores of continents shows that the forces depend in some way upon the sea water. These explosive forces can be best studied in connection with the eruption of volcanoes, since volcanic outbreaks are also accompanied by earthquakes often felt over large areas.

4. Not all earthquakes lead to eruptions, but if the shocks in a given region cease on the eruption of a neighboring volcano, we may feel sure that the forces producing the eruption also produced the antecedent earthquake shocks felt by the surrounding country.

5. That steam is the cause of volcanic eruptions is proved by the

distribution of active volcanoes about the seashores and by the innumerable eruptions which occur in the depths of the ocean, whereas such vents always die out inland; and moreover by the fact that of the vapors emitted from volcanoes 999 parts in 1,000 is estimated to be steam, the remaining one-thousandth part being by-products incidental to the moisture and high temperature.

6. The *vera causa* of volcanic action and of certain earthquakes thus established for some particular cases must be held to be the universal cause in all cases whatsoever, according to Newton's rule of philosophy.

7. The heaving of steam accumulating within or just beneath the earth's crust is therefore the true cause of all world-shaking earthquakes, and volcanic outbreaks occur only when an outlet is forced through to the surface, which usually happens in mountains, where the earth's crust is already badly fractured and upheaved.

8. When the subterranean steam pressure becomes great enough to shake the earth's crust, it naturally moves at the nearest fault line, where the rocks are broken, *but the movement observed is the result, not the cause of the earthquake.*

9. Volcanoes are particular mountains blown open by steam pressure under the throes of earthquakes (cracks in the rocks appear to be the beginning of some few volcanoes), and since all volcanoes blow out pumice and ashes, these materials must be held to exist in all mountains, and are made by the inflation of molten rock with steam and other vapors.

10. Any mountain peak, therefore, is capable of becoming a volcano if the subterranean steam pressure be sufficiently powerful to break open an orifice. But orifices close up and volcanoes die out inland and elsewhere if the supply of steam is inadequate to keep open the vents upon which the activity depends. Even if stopped up for a time later heaving of the earth may give the volcano renewed activity, and when the mountain has been dormant for a long time it is found that the violence of the eruption is greatly increased. The violence of the subterranean pressure in such a case approaches that of a region which has no vent at all, and hence we see why earthquakes in non-volcanic regions frequently become so terrible, because the forces accumulate to frightful fury before any

relief whatever is afforded, and the result is a most terrible earthquake.

11. The mountains are formed by the injection of steam-saturated lava under the coast, which breaks the overlying surface rocks and gives rise to a ridge parallel to the sea. This is why all mountains are formed parallel to the seashores.

12. By continually injecting the land with lava from under the bed of the sea the coast is raised and the mountains upheaved, and some of them usually break out into volcanoes; while at the same time the support of the sea bottom is undermined by the thinning out of the fluid substratum, and at intervals the bottom sinks down to restore stability.

13. The sinking of the sea bottom in this natural process of earthquake injection of the land is the cause of that class of sea waves found to follow violent earthquakes, in which the water first withdraws from the shore and then returns as a huge wave. Those waves noticed to rise suddenly without previous recession of the water usually are due to submarine upheavals and eruptions in the bed of the sea.

14. Islands are built up by injection from the sea, and hence have their mountains as veritable backbones, because the injection is symmetrical from both sides. In many cases the sea bottom is thus undermined and finally sinks down, making a hole beside the island, or a trench. The fact that all islands are not accompanied by such sinks is no argument against the theory, because the subsidence has not always taken place; it is the occurrence of even a considerable number of such sinks beside islands which proves the validity of the theory. Such intimate associations between elevation and depression could not be the result of chance.

15. In the repair of ocean cables broken by earthquakes, subsidence of the sea bottom is frequently found to follow these disturbances. This is a direct observation of the above effects in certain cases which are established by actual measurement, the subsidences frequently amounting to hundreds of fathoms.

16. The sea bottom does not subside without the lava under the crust being forced out into some other place, as into islands, submarine ridges, or shores; none of this movement is due to the

secular cooling of the earth, but is all to be explained by the undermining effect of steam accumulating under the earth's crust.

17. Mountains in the interior of a dry country, as the Rocky Mountains in Colorado, exhibit no important movements, while those on the coast, like the Andes, are always heaving. This shows that the sea is the cause, and not the secular cooling of the globe, which is wholly insensible.

18. The only countries which are free from earthquakes are the deserts, and therefore practically uninhabitable; there is accordingly no escape from earthquakes, and buildings designed for permanency should be framed to withstand them without material injury.

19. While in the long run the elevation of the land predominates, there is also subsidence, due to the non-concurrence of the forces in certain regions beneath the crust. It is idle to deny these oscillatory movements of the crust, and many good illustrations of both are clearly established. Every island which is thrown up in the sea is a witness to one of the most general laws of nature.

20. As water is taken up in the crust both in the crystallization of rocks and in the processes of earthquake movements, and only a part of this vapor is restored to the surface through volcanic action, there is a secular desiccation of the oceans, but the process is excessively slow and not certainly recognizable during the historical period, though a part of the lowering of the strand line in later geological ages is no doubt traceable to this cause.

21. The elevation of the plateaus depends on the same cause which upheaved the mountains; and all plateaus, like the mountains, are underlaid with various forms of pumice, which accounts for their feeble attraction as shown by geodetic observations.

22. No doubt various chemical changes go on under the earth's crust where the water has penetrated the lava and the steam becomes superheated, but the predominance of water vapor in volcanoes shows that the other gases are only by-products, incidental to the moisture and great heat. Dissociation of water vapor is one of these effects.

23. The details of mountain structure admit of explanation on the present hypothesis, while heretofore no such explanation was forthcoming. A theory which accounts for the position of the

ranges relatively to the sea, the slopes of the ranges, and the side spurs, and the relation of mountains to earthquake and volcanic phenomena, should have a strong claim to acceptance. This theory was partially foreshadowed by the Arabian astronomer Avicenna, in the tenth century of our era.

24. The theory of the penetration of sea water into the crust of the earth and its connection with volcanoes and earthquakes dates back to Lucretius and Aristotle, while the upheaval of the land is distinctly announced by Strabo. We have, therefore, been simply verifying and extending the impressions of the ancients formed from the general aspects of nature long before the sciences had become exact.[1]

§ 63. *General considerations.*

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[1] Since finishing this paper the writer has been much impressed with the following passage in the article *Poseidon, Encyclopedia Britannica,* ninth edition:

"Poseidon, the ancient Greek god of the sea and of water generally. . . . He was the god of navigation, adored by all who sailed the sea. His temples stood especially on headlands and isthmuses. As god of the sea he disputed with other deities for the possession of the land—with Athene for Athens and Troezen, with Helios for Corinth, with Hera for Argos, with Zeus for Aegina, etc. Earthquakes were thought to be produced by Poseidon shaking the earth,—hence his epithet of 'Earth-shaker,' and hence he was worshipped even in inland places, like Apamea in Phrygia, which had suffered from earthquakes. Hence also may have arisen the custom in some places of sacrificing moles to him. The great sea-wave which often accompanies an earthquake was also his work; the destruction of Helice in Achaia by such a wave (373 B. C.) was attributed to his wrath. Once when an earthquake shook the ground where a Spartan army was encamped, the whole army sung a hymn to Poseidon. The island of Delos was thought to have been raised by him from the bottom of the sea, and in 237 B. C., when a new island appeared between Thera and Therasia, the Rhodians founded a temple of Poseidon on it. Thessaly was said to have been a lake until this god opened a way for the waters through the Vale of Tempe. Poseidon was also the god of springs, which he produced by striking the rock with his trident, as he did on the acropolis of Athens when he was disputing with Athene for the sovereignty of Athens. This dispute was represented on the western pediment of the Parthenon. . . . There were collossal statues of him at Helice in Achaia, on the Isthmus of Corinth (set up by the Greeks after the Persian wars) and at Tenos."

It is very remarkable to find that at an early age the Greeks had so directly connected earthquakes with the sea, probably through the seismic sea waves, which they often observed in this part of the Mediterranean. (Note added December 17, 1906.)

The way in which several different classes of phenomena find explanation by the most simple of causes may be considered not the least remarkable result of the present investigation. In the minds of those who follow Newton's first rule of philosophy, "to admit no more causes of natural things than are both true and sufficient to explain their appearances," this will probably tell strongly in favor of the truth of an hypothesis which explains easily and naturally such diversified phenomena as earthquakes and volcanoes, the formation of mountains and the deficiency in their attractions, the origin of cordilleras from the ocean trenches near continents, and of the great sea waves which frequently accompany violent earthquakes. But there will doubtless be others who will prefer a variety of causes, and will be slow to believe that the laws and order of nature are so simple.

In an investigation of such considerable extent it would not be surprising if many difficulties should require further elucidation than they have received in this imperfect outline; for the writer has not the geological learning required for the full treatment of many of the great problems of the earth's crust. But if, on the one hand, some defects or omissions should be found, and no doubt many of them will appear in special branches of the extensive lines of thought here traversed, may it not be thought, on the other, that the harmony established with geodetical measurements on the attractions of mountains and the deviations of the plumb line, combined with the explanation of the equilibrium of the terrestrial spheroid between the land and water hemispheres, is not wholly without a certain degree of compensation to those interested in the numerous and related problems of the physics of the earth?

That the existing theory of earthquakes and volcanoes and mountain formation is embarrassed by many difficulties has often been frankly admitted. While the theory here suggested may require extension or modification, one may confidently submit the question whether it is really possible to doubt its essential truth.[1]

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[1] The sinking of the sea bottom after great earthquakes have heaved up the coast, as along the west shore of South America, furnishes a truly remarkable criterion for unraveling the unseen processes of nature, hidden beneath the earth's crust. The sea bottom could not sink unless it was in some way undermined, and the adjacent coast could not be upraised unless

If this inference should prove to be well founded, may it not be possible to conclude the contest long waged between astronomy and geology with credit and advantage to both of these great sciences? For it seems to be proved that owing to the great pressure acting throughout the earth's interior, and the solidity of the rocks which cover its surface, the terrestrial spheroid, when subjected to great strain, behaves and vibrates as an æolotropic elastic solid; yet the fluid medium long contended for by geologists to explain crumplings and foldings of the crust is really found to exist or develop in a thin layer just beneath the outer crust, and we have explained its simple and somewhat automatic mode of operation. On several grounds it appears that this layer is extremely thin, certainly not much thicker than the crust itself; and, moreover, it too remains essentially quiescent and rigid, except when set in motion by the recurring paroxysms of steam pressure seeking release, which gives rise to an earthquake and the movements of molten matter essential to geological processes. The original conceptions of astronomy are thus apparently verified, while at the same time the long-standing needs of geology are amply met. And although no effort has been made in this paper to harmonize the conflicting views long prevailing in the two sciences, it would seem that this unexpected result may prove to be not the least interesting among several conclusions respecting the constitution of the globe.

something was pushed under it: both these movements are observed to occur, and always in the same order. Along the shore, just parallel to the sea coast, the crust is pushed upward to form mountains, while the adjacent sea bottom is correspondingly sunk down. Can anyone doubt that we have here a true cycle of events? One of these events necessitates the other, and all are so connected that one can begin at either end of the chain of phenomena and reach the other. Thus, if we begin with volcanoes we are led downward through these vents to the earthquake and mountain forming forces at work under the surrounding land; and finally we come out beneath the ocean where world-shaking earthquakes and seismic sea waves originate. On the other hand, if we begin with seismic sea waves, we are led downward through the observed subsidence of the sea bottom and pass under the crust to the mountains raised along the coast, by earthquakes occurring mainly under the sea, and thus finally reach the volcanoes at the top of the range, erupted by the pressure of subterranean steam, and blowing out lava, pumice, ashes, and vapors. All the grandest phenomena relating to the earth's surface are thus connected in an endless chain or circuit. Could such perfect order and harmony be established among all these varied phenomena without the discovery of the true physical cause? (Note added December 3, 1906.)

The details of the processes involved in the elevation of continents, and just how these great land areas originated, are yet to be worked out, and appear to be involved in considerable obscurity, most probably connected with the terrestrial origin of the moon. Perhaps our knowledge is still too incomplete for a satisfactory attempt at this inquiry; but it seems not improbable that the lines of thought here struck out may eventually prove fruitful of further discovery.

§ 64. *Origin of the present investigation.*

It will be seen from the foregoing account that the present investigation grew out of the difficulty of explaining satisfactorily the rotatory motion of the San Francisco earthquake. When the writer very soon became convinced that the accepted theory, which explained these phenomena by dislocations, subsidences and faults, was not well founded, the investigation was extended little by little till it covered a number of important natural phenomena not usually correlated. The order in which this extension took place is perhaps of no importance, but to enable anyone who might be curious to understand the original line of thought, it may not be wholly out of place to mention that the next step was to assign the explosive activity of volcanoes to steam; and then the traditional reasoning about these vents being safety valves was intelligible, and it became clear why violent earthquakes do not predominate in volcanic districts and why large tectonic earthquakes are always so violent.

While considering the mode of release of steam pressure in tectonic earthquakes, where the strata are unbroken, I was led to inquire into the origin of mountains. The only explanation I could find was by the theory of contraction producing folding along lines of weakness in the earth's crust. It seemed to me remarkable that the lines of weakness should nearly always follow the coast lines of continents. Such an arrangement should not occur by chance. It was easy to grasp the connection of earthquakes with volcanic activity when the volcano is once formed; and the question naturally arose, how did the volcanoes and mountain chains originate? I recalled that volcanic islands are always being thrown up, and numerous islands had been formed in this way on the bed of the sea, which are mountains under water. On the other hand most geological theories regarded the mountains as formed in the past, while it seemed to me that some of the mountains are near enough to the seashore to be still in the process of upheaval. In reflecting over these questions, on May 20th, I recalled, from impressions obtained at the Coast Survey in 1899, that some of the islands in the sea had depressions on the side of them as if the material had been thrown up from the adjacent bed, and that deep trenches in the oceans also ran parallel to the great ranges such as the Andes. I noticed also that the volume of the Andes and of the islands were severally not very far from equal to those of the adjacent trough and sinks. Could it be possible that the Andes also had been raised by matter pushed up under the crust from the bed of the adjacent oceanic trough? On recalling that the sea bottom near Pelée has been found by observations to have actually sunk after the great ejection of ashes and vapors in 1902, and that the hollow form of the Andean trough would prevent vertical eruptions from beneath, and cause any explosions which might originate under it to seek release at the sides, thus pushing up the mountains through columns or rather layers of lava advanced little by little, by "lateral thrusts," as the geologists say, from under the sea, I did not hesitate to conclude that the Andes had originated in that way. The existence of the long trough, so deep and so exactly parallel to the mountains for so great a distance, proved it; and it was easy to see also that later the resistance toward the land side would become so great, owing to the great distance and height of the mountains, that release would eventually come more easily by the other side of the trough folding up into a parallel range, which would thus emerge from the sea as the New Andes, and eventually cause a further recession of the west coast line of South America.

The provisional entertainment and private acceptance of these views seemed daring enough, but it appeared to me there was no escape from them. On the following day, May 21, I recalled that geodetic observations indicated a feebler attraction for mountains than if they were solid and of the same mean density as the crust; and it was evident that my earlier conception of pumice-filled cones and chains had not been altogether too rash. The rest of the work consisted in nothing but verification and elaboration of these intuitions.

Moreover, the cause of the great sea waves was now perfectly clear. It was impossible to doubt that these phenomena, of the class in which the water recedes from the shore in the first few minutes following an earthquake, generally arise from a subsidence of the sea bottom after the expulsion of porous lava in the natural process of elevating the mountains and coasts.

After these ideas had been worked out in such a way as to establish harmony among the widely diversified phenomena it was gratifying to find that such views were not inconsistent with the thoughtful remarks of Humboldt, and that Charles Darwin seventy years before had reached conclusions in most respects remarkably similar. It seemed, therefore, justifiable to suppose that the process of mountain formation thus outlined and verified was in all probability the correct one. Such was the order of the writer's thought, which he records with reluctance and hesitation, but chiefly in the hope that it may not be wholly without value to others.

In the final arrangement of the paper it was deemed best to introduce the citations from Humboldt and Darwin under the discussion of the several topics rather than as extremely long foot-notes. And this arrangement was the more agreeable on account of the great esteem in which the writer has always held these illustrious investigators. As Professor Milne is justly recognized to be the most eminent living authority on earthquakes, to whom we owe extensive international coöperation and the present widespread interest in the subject, which has contributed so greatly to the advancement of our knowledge, the same principle has naturally been employed in the exposition of the results of his researches.

While it is impossible to estimate too highly the great strides recently made in seismology under the leadership of Professor Milne, Dr. Davidson, Montessus de Ballore, Major Dutton, Von Rebeur-Paschwitz, Dr. Agamennone, Dr. Rudolph, Professor Omori, and others, who have reduced seismology to a science of precise observation and measurement, it is perhaps unfortunate that the views of Humboldt and Charles Darwin as to the causes involved in earthquakes were ever abandoned by modern investigators, though this probably has not retarded the progress of observation and experimentation on the constitution of the globe.

In certain respects the theory here outlined may be capable of observational tests. The importance of the subject would appear to be such that it may be worthy of consideration whether geodetic investigators of the different nations might not advantageously arrange to establish earthquake and tidal observatories and more precise levels on coasts such as those of Chili, Peru, California, Japan, Italy, Greece and other countries, for the more exact study of progressive secular movements. Has not the time come to test geological and seismological theories by the accumulation of exact empirical data, and is not this a debt which we of this generation owe to the future?

§ 65. *Seismic activity a maximum along the coasts of deep seas and a minimum in the great inland deserts.*

If the cause assigned for the explanation of earthquakes be confirmed by time and experience, it is evident that no place on the earth can be said to be wholly removed from the danger of these disturbances; yet the dangers will be a maximum on the coasts of deep seas where the shores are of leaky character and the troughs are at work,[1] and grow less and less along the coasts of the shallower waters where the troughs are absent and the stratification of the rocks is more secure. Thus northern and central Europe and the eastern coast of the United States are comparatively safe. Yet sooner or later, but fortunately to be reckoned in many cases by intervals of thousands of years, every place (except the great inland deserts which are nearly uninhabitable) is likely to be shaken by an earthquake of considerable severity; and those works of man which are to be preserved and to stand through the centuries should be built accordingly.[2] The great layer of water covering the earth,

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[1] In his thoughtful article on "Geology," *Encyc. Brit.*, p. 255, Sir Archibald Geikie justly remarks: "Some of the most terrible earthquakes within human experience have been those which have affected the western seaboard of South America." The cause of this is now plain, viz., the Andean Trough is probably the largest and most powerful in the world. And in general the seimicity of a region in the production of world-shaking earthquakes depends on the extent and power of its ocean troughs.

[2] Humboldt laments the destruction wrought by earthquakes upon works of art, architecture, monuments and inscriptions of the classic period, which were developed in a region of such high seismic activity as to render their preservation difficult.

which gives life to animals and plants, and in the form of steam is the greatest mechanical agent of man, when sunk into the crust becomes also one of his worst destroyers, on account of the explosive vapor generated beneath by the internal heat of the globe.

Finally, it may not be without interest to note that, so far as our knowledge extends, the earth alone among the encrusted planets of the solar system has an abundance of water and mountains, with volcanoes and earthquakes. Mars seems to have only a trace of water and no sensible mountains or volcanoes. And while Venus is largely obscured from our view, the chances are that its veil is due to clouds, indicating an abundance of water, and hence that its conditions of evolution may approach those of the earth.

BLUE RIDGE ON LOUTRE, MONTGOMERY CITY, MISSOURI,
September 23, 1906.

NOTE.—For kind permission to use Figs. 6, 7, 8, and 9 we are indebted to the courtesy of Rand, McNally and Co., of Chicago.

Stated Meeting November 16, 1906.

President Smith in the Chair.

A letter was received from the Kongelige Danske Videnskabernes Selskab, Copenhagen, thanking the Society for the gift of the Franklin Medal.

The decease was announced of the following members:

M. Louis Vossion, Cape Town, Africa, on October 6, 1906, æt. 59.

Prof. J. M. Hoppin, at New Haven, Connecticut, on October 15, 1906, æt. 85.

The following paper was read:

"Alcoholic Fermentation in the Light of Chemical Investigation," by Prof. Harry F. Keller.

Stated Meeting December 7, 1906.

President Smith in the Chair.

Letters were received from the Royal Geographical Society of London and the Imperial Academy of Sciences of St. Petersburg, thanking the Society for the gift of the Franklin Medal.

An address was received from the International Society of Electricians of Paris congratulating the Society on the Bicentenary of the birth of Benjamin Franklin.

The decease was announced of Prof. Henry M. Baird, D.D., at Yonkers, New York, on November 11, 1906, æt. 75.

Dr. Morris Jastrow, Jr., read a paper on "Divination in Ancient Babylonia," which was discussed by Profs. Lamberton, Conklin, Clay and Jastrow.

Stated Meeting December 21, 1906.

President Smith in the Chair.

The list of donations to the Library was laid on the table and thanks was ordered for them.

Dr. Edgar F. Smith delivered the President's Annual Address.

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NOTE.

By an error in imposing one form in No. 182, page 45 became page 50 and page 50 became page 45. In this issue these pages are reprinted with their correct pagination, so that they may be substituted by the binder for the incorrect pages in No. 182.

A BIOGRAPHICAL NOTICE OF J. PETER LESLEY.

BY HENRY M. CHANCE.

(*Read April 6, 1906.*)

In the year 1856 upon the thirteenth day of July, J. Peter Lesley was elected to membership in the American Philosophical Society. Although he had then lived not quite thirty-seven years, he was already widely known as a geologist of marked ability, as a man of parts, and as one likely to become a valuable member of this organization. This hope was almost immediately realized, for the new member at once became one of its active workers, and soon, by reason of his ability and vigorous personality, recognized as one of its leading spirits. That in according him leadership his fellow-members judged wisely and well, and that the confidence reposed in him was abundantly justified the records of this society bear ample testimony; and we who now do homage to his memory do know that so long as the American Philosophical Society shall have being, the memory of the life and work of J. Peter Lesley will be cherished as one who contributed largely to its welfare and added to the dignity and strength of its purposes and influence.

He was by divine right a leader of men, a nobleman, intellectually a giant. As a man among men he was of charming personality, generous to a fault, a tireless worker, energetic, practical, resourceful, yet withal a philosopher, student and profound thinker, one to whom no personal exertion was too great, who considered other men's mountains as mere molehills, too small to cause the hesitation of a moment, but to be surmounted quickly that the pleasant valley of scientific truth lying beyond might surely be reached; yet a man considerate and forbearing to others. One we must place among the great and grand historic characters.

In 1859 he became librarian and secretary to this society, the duties of which office he performed until 1885, a period of twenty-six years. In 1887 he became one of its vice-presidents continuing in this office for ten years, when failing health compelled him to retire from active participation in the affairs of the society.

Those to whom he was personally known, will always remember how earnestly, efficiently and tirelessly he labored for this society "for promoting useful knowledge," how dear to him were its traditions and purposes, and how faithfully he performed his official duties to it.

The promotion of useful knowledge was with him an absorbing passion. He loved to impart to others what he knew, seeking knowledge that he might give it to others. He had the inborn love of the teacher. It was his life to him to learn that others might learn and thus profit through him. He was no miser of his great wealth of knowledge, but spent it prodigally that there might be some for all, yet was he not denuded of it but enriched in the giving. Yet was he neither haughty nor austere nor proud. He was ever an attentive listener, always eager to grasp some new fact, willing to ignore or disregard mere authority, if thereby he might seize upon something new, something valuable, something useful to his fellowmen. And even when after years of successful work he became one of the foremost living geologists, he was still ready to listen thoughtfully and appreciatively to the laborer, miner or artisan, hoping thereby to learn something that might have escaped his own observation.

One may perhaps best comprehend the passion for science, for research and for scientific truth that seemed to pervade his every thought and to govern his every act by reviewing what he was to this society, what he did for it, and the underlying cause of his great love for it, doubtless to be found in the title of the society "for promoting useful knowledge," for his whole professional life was consecrated to this purpose and to a society pledged to this end, his allegiance naturally was given.

For several years he was professor of geology and mining at the University of Pennsylvania, where as a student in the year 1872 the writer first came to know, in common with all those who studied under him, his gentle and kindly personality and his conception of the value of knowledge. And doubtless many of his former students can recall how urgently he begged them to acquire knowledge, not from books only, but to go into the highways and byways, into the workshops and factories, and glean that which was useful and valuable; how repeatedly he taught that from the most ignorant something of value might always be learned, and how eager he was to impress upon all their duty to publish to the world every new or valuable fact which they might possess, that others might benefit thereby. Thus, in other channels than those of this Society, he was striving not only to "promote useful knowledge" but to imbue others with like impulses, and one cannot doubt that the influence of his life and precepts upon hundreds of students has given force and impetus to the dissemination of knowledge. He seemed to delight in teaching because it afforded him this opportunity of inspiring the young with a thirst for knowledge, of directing them how to find it and of imbuing them with a sense of their obligations to give it freely to their fellowmen. Hence, it is not surprising that after this society, which always held the first place in his heart, he seemed to love next best his work at the University of Pennsylvania where for nearly twenty-five years he was a member of the faculty.

While it was as a student at the University of Pennsylvania, that the writer first met and learned to admire and respect him, the acquaintance thus begun continued through a closer association of ten years (1874–1884) as one of his assistants, during a portion of the period in which Professor Lesley was state geologist of Pennsylvania.

As state geologist, his attention was absorbed to the exclusion of almost everything else, by a great task to be performed, by a great duty to the state to be faithfully discharged, and to this end he bent every effort, working prodigiously, doing the work of others, never sparing himself, always laboring, always earnest, never satisfied to take the rest or recreation most men deem indispensable. Yet he was ever considerate of others, never expecting them to strive as he did, but on the contrary continually taking from all of his assistants some of their burdens and carrying them himself.

Only an overwhelming sense of duty, of responsibility assumed to be conscientiously discharged, and great love for the work, could have enabled him to labor as he did during these last ten years.

At all other periods of his life he was a prolific writer and industrious worker, but prior to 1874 his activity was greatly diversified, his record for these years comprising many expert geological reports, editorials for the *United States Railroad and Mining Register* discussing current topics of general interest, and many scientific articles, philological, ethnological, palæontological, mineralogical, together with the discharge of his duties as editor and proof-reader of this society's publications. To few is given the ability to work so indefatigably, and few indeed so completely surrender the pleasures of life to labor. To the concentration of all his energies upon this last great task for the State of Pennsylvania, can be attributed the failure of his health and strength some ten years prior to his decease.

His relations to his fellowmen were marked by a wonderful kindliness, a childlike and sympathetic interest in others, an unrestricted generosity, and thoughtlessness or even carelessness regarding matters affecting his own welfare and comfort. Rarely in one man are found so many lovable traits and no written word of mine adequately can portray his character as man, as scientist and as engineer.

As a man he embodied and radiated those emulable qualities found in the courteous, kindly, genial Christian gentlemen; as a scientist he was infused with a devotion to science, an overpowering love of truth, a passion for knowledge, and a delight in research; as an engineer his ability was early demonstrated in that inventive faculty which enabled him to devise new and improved methods, and by that unbiased, even judicial quality which permitted him to draw his materials from all sources and utilize them to the practical benefit of his colleagues.

In temperament he was strongly poetic and artistic, often thinking, reasoning and talking as a poet and seeing as an artist; embellishing with poetic imagery the vigorous originality of his writings and accentuating with artistic touches the individuality which marks the maps and sketches with which he profusely illustrated his reports and many of his scientific papers.

Intensely interested in abstract science, loving it for itself alone, always alert to the discovery of every new fact, to the evolution of a new law or theory, speculating as a poet and artist upon the possibilities opened up to human advancement by such discoveries, dreamily looking back through the ages, reconstructing mentally the conditions and forces at work, which have given us the earth as we now have it, and perhaps looking forward striving to foretell the future,—yet withal he was eminently practical, a man of affairs, an engineer.

In a moment, divorcing these poetic dreams, he became a utilitarian, a conservative mining engineer, accepting and weighing only those facts and agencies having direct bearing upon the extent, quality and value of the minerals with which as a master of the art he continually had to deal.

As his abilities became known and recognized by men of affairs as fitting to render them valuable advisory services, he was retained to investigate and report upon mineral lands in many localities. Some of these private reports are classic productions, now used as models by professional geologists and engineers.

This society was indirectly interested in this phase of his life's work, for it was mainly through income derived from his private practice, that he was enabled year after year to give the time necessary to its service. It is practically impossible to enumerate in detail the work which stands to his credit here in our books. The files of the society's publications contain hundreds of communications from his pen, and many articles which he assisted the authors to prepare but to which his name is not appended. As librarian and secretary to this society he was its " editor-in-chief," revising, proofreading and editing most of its publications during the twenty-six years he held this position.

During this period he was rarely absent from the meetings of the society, frequently joining in the discussion of papers read at the meetings, and when the attendance was small or interest flagged, he would often supply something from his apparently inexhaustible fund of knowledge to entertain and benefit those present, invariably captivating their attention and making his discourse both delightful and instructive. His presence at these meetings was also valued for the many wise suggestions which he made relating to the conduct of its business.

In the practical affairs of life he was keenly alive to the value of modern methods. He was among the first to use the card index for general indexing purposes, he was the originator of stadia measurement surveying in governmental work in this country, he invented a method of shading topographical contour-line maps to get relief effects, and devised several methods of constructing geologic and topographic models which he regarded as the only satisfactory means of showing the relation between geologic structure and topography. He strenuously urged the construction of such models on equal horizontal and vertical scales, deprecating the then common practice of exaggerating the vertical scale.

Possibly his success in life was in a measure due to the courageous, fearless expression of his opinions and convictions that marked his advocacy of any cause, and to that equipoise which enabled him to listen patiently and considerately to those who might differ with him, although he seemed at the outset intuitively to master any subject, be it never so complex, and at a glance to grasp the whole range of possible combinations, deductions and interpretations, just as a great general will with a single sweep of the eye, pick out the strategic points on a field of battle.

He was naturally a scholar in the broadest sense of the word, for aside from his passion for science, and pleasure in work, he loved learning, taking delight in academic studies and spending all of the time he could take from his work in the material sciences, in these pursuits. He had marked ability as a linguist and aptitude for the study of languages and would have attained eminence as a philologist or egyptologist, had not his bent been more toward the natural sciences and his time so occupied with geologic work that he was unable to pursue these studies except, as he said, "as a recreation" and "to rest an overworked brain."

We cannot hope to record here anything approaching a biography such as his achievements justly merit and must rest content to briefly summarize the major works of his life with such dates and facts as the writer has been able to verify.

Professor Lesley was born in Philadelphia on the seventeenth day of September in the year 1819. His father was Peter Lesley and his grandfather also was Peter Lesley and both were cabinet makers. The son was also named Peter Lesley, but disliking the form Peter Lesley, Jr., at an early age he began writing it J. P. Lesley (Junior Peter Lesley) and this form he retained throughout life, although at times in his official capacities he was designated as Peter Lesley (which the writer understood him personally to say he regarded as his legal name) and in earlier years he occasionally wrote it J. Peter Lesley and also Peter Lesley, Jr.

He graduated at the University of Pennsylvania in 1838—he was then not quite nineteen years old—and the next year obtained an appointment as assistant on the Geological Survey of Pennsylvania under Professor Henry D. Rogers, who assigned him to duty at Pottsville with James S. Whelpley, a young assistant who had great ability as a topographical geologist. Whelpley shortly left the survey and Lesley continued the work that and the succeeding year in that region, and later in the bituminous coal-fields of Somerset and Fayette counties and in 1841 did additional work in the region of the Allegheny River and its tributaries.

In 1841 he entered Princeton Theological Seminary where he completed the course three years later and was licensed by the Philadelphia Presbytery. During these years, his vacations were spent in constructing a geological map of Pennsylvania and in drawing many illustrations for Rogers' final report.

In 1844 he went to England and from there tramped through France and Switzerland, returning the following year to Philadelphia.

After serving for two seasons as a distributer of Presbyterian tracts for the American Tract Society, he went to Boston to assist Professor Rogers in redrawing the Pennsylvania maps and illustrations for the final report.

In 1848 he became the pastor of the Congregationalist Church at Milton, Massachusetts, and soon after married Miss Susan Inches Lyman (February 13, 1849) at Northampton, Massachusetts.[1]

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[1] While Mrs. Lesley was still living at the time of his death, she survived him but six months, passing away January 16th, 1904. She was of charming

He was fortunate in securing as his partner for life one whom nature had richly endowed with intellectual gifts of an order similar yet complementary to his own. Her ability as a writer of delightful domestic sketches and reminiscences enabled her to appreciate and enter into the spirit of his aspirations, while the unruffled serenity with which she confronted the storms of life was reassuring to his more nervous temperament, soothing and allaying his fears and encouraging him to wait patiently for the fair weather sure to follow.

In 1849 he became a member of the American Association for the Advancement of Science. In 1851 he resigned his pastorate at Milton to resume geological work with Professor Rogers.

From 1853 to 1856 he was engaged in private and corporation surveys in Pennsylvania, and in the latter year published his first book, "Manual of Coal and Its Topography," a small volume of 224 pages finely illustrated by the author; now a very rare book and in great demand by geologists. That he believed this book conveyed a message of value which would add to his prestige and bring clients to his office is frankly stated in the first sentence of the preface, beginning "The author has planted this sapling for the future shade and ornament of his own office, but trusts that it may prove useful also, and perhaps agreeable to the public highway."

He was elected secretary of the American Iron Association in 1856, during the next two years was engaged in compiling statistics of the iron trade in the United States and in 1850 prepared and edited for the association an octavo volume of eight hundred pages entitled "The Iron Manufacturers' Guide," which for many years was a standard reference book.

In January, 1858, he was elected librarian of the American Philosophical Society and the next year became one of its four secretaries, which dual office he held until his resignation in 1884. He was elected vice-president in 1887, continuing in that office until 1897.

In 1859 he was elected professor of mining at the University of Pennsylvania, in 1872 was made professor of geology and mining,

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disposition, charitable and kindly, devoted to her husband and children and always deeply interested in his work and plans, and to her affectionate care and assistance must be credited his ability to turn out such enormous quantities of work well done.

and in 1886 professor emeritus of that chair. During his active work at the university he was for some years dean of the Towne Scientific School.

In 1869 he became editor of the *United States Railroad and Mining Register,* retaining this position until 1873, during which period he contributed to it a large number of editorials embracing political as well as technical essays and discussions.

From 1858 to 1874 he was largely engaged in geological work for corporations and private individuals, which was in fact his chief source of income, and without which he would have been unable to attend to the affairs of this society.

In the early sixties he overworked himself in private geological surveys in the oil fields, and in 1866 was compelled to go abroad and spend two years in regaining his health and strength.

In 1874 the commissioners of the Second Geological Survey appointed by Governor Hartranft to inaugurate a thorough resurvey of the state elected him state geologist. Putting aside a growing and lucrative professional practice, he devoted his whole time and energies to the supervision of the field forces and to the revision and editing of the reports of progress, comprising more than one hundred octavo volumes, and finally to the personal preparation of a final summary report.

The passage of the law inaugurating the Second Geological Survey of Pennsylvania was in response to a demand for accurate knowledge of our mineral resources, arising from the expansion of manufactures following the Civil War. At that time few of the states were prosecuting such surveys and those so engaged were hampered by lack of funds and scarcity of specialists trained to the work. No established mode of conducting such surveys had been formulated nor had any system been devised for a prompt publication of the results.

As organizer of this survey, Professor Lesley was thus confronted with problems upon a satisfactory solution of which the future of the survey surely would depend, for the appropriation made by the legislature was sufficient to cover two or three years at most, and unless the people were satisfied with the results there could be little if any hope of further appropriations. From the inception of the work it therefore was essential that reports be made and published each year, that these reports consist of great accumulations of facts describing the occurrence of ores, oils, coals and other valuable deposits in a form useful to the practical miner, and that the deduction of general laws, correlation of geological names, elaboration of geological structure, investigations of deposits of obscure origin and palæontological studies all must be deferred until the survey had been in progress for several years.

To meet these conditions, Professor Lesley devised the system adhered to throughout the life of the survey of publishing the material in "Reports of Progress," each report being an octavo volume usually confined to a certain district of county, that data covering any locality could be obtained from a single volume—now a system of recognized merit and since adopted by other governmental surveys.

In planning the organization and scope of the survey, the board of commissioners extended to him the utmost freedom, conferring upon him power to use his own methods, to select his assistants, to determine what work should and what should not be done, relying upon his judgment and ability to produce the best results at least cost, and standing loyally by him as staunch friends and supporters through all these years.

Almost immediately after his election he appointed as his principal assistants, Persifor Frazer, Frederick Prime, Jr., John H. Dewees, Franklin Platt, J. J. Stevenson, John F. Carll and Andrew Sherwood, geologists, and Dr. F. A. Genth and A. S. McCreath, chemists, each being assigned to a certain district or to special duties, and given one or two younger men as aids. With these as a nucleus, he gradually built up, chiefly by promotion from among the younger men, a large and efficient corps of trained workers, to whom he accorded the greatest latitude, encouraging them to originate, to devise new methods and theories, holding them responsible only that their work be well and accurately done. Probably no public organization was ever less bound by the red-tape of officialism than this survey corps, whose members he left untrammelled, unhampered, trusting each to do his duty, thus placing each in a position where he was driven to do his best, where he would be ashamed to do less.

He assumed the editing and proof-reading of nearly every report, generally contributing a preface discussing matters of most striking interest and disputed questions of geological nomenclature or theory. These prefaces are clear, concise expositions of the subjects with which they deal, so plainly written that the layman may read and understand, yet often defining the nicest possible distinction between opposing theories, ably and subtly reasoning for both sides—models of terse, forceful English.

Throughout this great task he never seemed to think of fame or honor to be won through it for himself, invariably giving credit to each assistant for the whole of his work, and reserving none for himself. In thus recalling his unselfishness, we are unavoidably reminded of the entire absence of anything like envy in his disposition. He never seemed conscious of professional jealousy, being utterly incapable of it himself and failing to realize the possibility of it in others. In this he towered far above the mass of his fellow scientists, with few indeed of equal stature. It was characteristic of the man to ignore as trivial the use of his work by others without proper acknowledgment, although he was himself scrupulously particular never to offend others in this way.

In devising necessary modifications of geological nomenclature he was most careful to adopt from names already used by other geologists those appropriate and in common use, hesitating and generally declining to formulate any system which would destroy or obscure the work of others.

While thus cautiously avoiding trespassing upon others and ignoring trespass upon his own domain, he was severe in denouncing charlatanism and scientific inaccuracy (which latter he regarded as an almost equal sin, and one of which he was most impatient), yet he was never bitter or vindictive, and when attacked by misrepresentation or captious criticisms seemed to feel it beneath his dignity and a waste of time to reply.

Through the efforts of the board of commissioners, appropriation after appropriation was secured for the continuance of the survey, but without resort to political influence or the methods of the lobbyist. He detested any appearance even of lobbying, holding that the survey should stand or fall upon its merits as appraised by the citizens of the state.

Never during the continuance of the survey were funds available sufficient for a complete geological and topographical survey, and he was obliged to use the more or less inaccurate county maps as the base upon which to lay down geological coloring and structure. This he greatly deplored, constantly grieving that he could not construct accurate maps and continually urging the inauguration of a topographical survey of the state. This latter object he never attained, but in one district he was able to construct maps more to his liking. This was in the anthracite coal-field where, through cooperation of the land and coal companies, the frame-work for an accurate mapping of the district became available, and there he developed the style of mapping he hoped to see extended to cover the whole state. This work was entrusted to Mr. Charles A. Ashburner, a talented young assistant (a member of this society, since deceased), who retired from the survey prior to its completion, but not before he had ably planned to realize Professor Lesley's high ideals of accuracy and geological completeness, and now after twenty years the anthracite mine sheets of this survey are unapproached, unrivalled, in beauty, accuracy and practical utility by any governmental survey publications, standing as present-day models of artistic, accurate geological mapping.

Through all these years he sacrificed much to the people of Pennsylvania, relinguishing the fees of a large professional practice, surrendering his compensation at the University of Pennsylvania, and even delegating to others many functions dear to his heart in this society, all that he might better serve his fellow citizens, set higher standards for geological work and prove its value in the practical development of his country.

At the last, he undertook the preparation of a " Final Summary Report," a digest of the numerous Reports of Progress. This herculean task was assumed at a period when nature was calling upon him to rest, but disregarding all warnings he set himself at it, determined that it should be done. Over 1,800 octavo pages tell us how well that work was done. It sets a standard for governmental reports. Nowhere can be found a more lucid exposition of facts and principles, unmarred by the assertion of personal theories, unbiased, thorough and complete. In the introductory chapters of the first volume, including especially the essay on "Geological Time," better perhaps than anywhere else we find the impress of his sign-manual; here are arguments, conceived and developed in a brain accustomed to exact scientific analysis, recorded with the clearness and simplicity of one writing for children, yet with the logic of a master in debate; the product of genius beyond praise.

But this great task he was not permitted to finish; his strength was insufficient, and after publishing two volumes and preparing a portion of the third and final volume (embracing in all over 1,800 octavo pages), he reluctantly delegated to others the writing of the final chapters.

In 1892 in failing health he removed to Milton, Massachusetts, where he lived until his death in 1903.

In addition to his membership in this society he was a corporate member of the National Academy of Sciences, Fellow of the American Association for the Advancement of Science, member of the Boston Natural History Society, of the American Oriental Society, of the Academy of Natural Sciences of Philadelphia, of the Oriental Club of Philadelphia, foreign member of the Geological Society of London, associate member of the Société Géologique du Nord, member of the Moscow Imperial Society of Naturalists, the Emden and Neufchatel Academies of Science, the Lille Academy of Natural Science, Foreign Honorary member of the American Academy of Arts and Sciences, member of the Union League Club of Philadelphia, and honorary member of the American Institute of Mining Engineers.

The degree of LL.D. was conferred upon him in 1878 by Trinity College, Dublin.

Aside from his work as state geologist of Pennsylvania, his most valuable contribution to the world of science was the discovery and enunciation of the principles governing the relation of structural geology to topography. He was the father of the science of topographic geology to which he early directed attention by illustrations prepared for the reports of the First Geological Survey of Pennsylvania of which Henry D. Rogers was the author, and by further elaboration of the subject in 1856 in his "Manual of Coal and Its Topography," wherein the relation of structural geology to topography was first clearly set forth, and a new method put into the hands of every field geologist whereby many difficult problems of structure may be solved by a mere inspection of the contour of the surface.

And now that he has passed away, as we review his record and ponder its messages, the thought that comes pregnant with the very essence of his life, is that he was a good man, who lived a goodly life, leaving to posterity the fruits of a most honorable career, a career marked by a beautiful devotion to duty extending through a long life well spent in the service of humanity. He helped to make the world better than he found it, striving ever after the realization of the most beneficent ideals, always aiming high, ever looking upward, exalting and ennobling those within the sphere of his influence and inspiring them to reach out after that which is noblest and best in life; through these he still lives, and through them the impress of his character to the benefit of humanity will descend from generation to generation and from generation to generation forever.