PROCEEDINGS

OF THE

Indiana Academy of Science

TWENTY-FIFTH ANNIVERSARY

1909

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EDITOR . . . . H. L. BRUNER

INDIANAPOLIS, IND.
1910

INDIANAPOLIS:
WM. B. BURFORD, PRINTER
1910

THE STATE OF INDIANA,
EXECUTIVE DEPARTMENT,
January 17, 1910.

Received by the Governor, examined and referred to the Auditor of State for verification of the financial statement.

OFFICE OF AUDITOR OF STATE,
INDIANAPOLIS, Feb. 7, 1910.

The within report, so far as the same relates to moneys drawn from the State Treasury, has been examined and found correct.

J. C. BILLHEIMER,
Auditor of State.

February 8, 1910.

Returned by the Auditor of State, with above certificate, and transmitted to Secretary of State for publication, upon the order of the Board of Commissioners of Public Printing and Binding.

MARK THISTLETHWAITE,
Secretary to the Governor.

Filed in the office of the Secretary of State of the State of Indiana, February 8, 1910.

FRED A. SIMS,
Secretary of State.

Received the within report and delivered to the printer February 9, 1910.

A. E. BUTLER,
Clerk Printing Board.

TABLE OF CONTENTS.

AN ACT TO PROVIDE FOR THE PUBLICATION OF THE REPORTS AND PAPERS OF THE INDIANA ACADEMY OF SCIENCE.

[Approved March 11, 1895.]

Preamble.

WHEREAS, The Indiana Academy of Science, a chartered scientific association, has embodied in its constitution a provision that it will, upon the request of the Governor, or of the several departments of the State government, through the Governor, and through its council as an advisory body, assist in the direction and execution of any investigation within its province, without pecuniary gain to the Academy, provided only that the necessary expenses of such investigation are borne by the State; and,

WHEREAS, The reports of the meetings of said Academy, with the several papers read before it, have very great educational, industrial and economic value, and should be preserved in permanent form; and

WHEREAS, The Constitution of the State makes it the duty of the General Assembly to encourage by all suitable means intellectual, scientific and agricultural improvement; therefore,

Publication of the Reports of the Indiana Academy of Science.

SECTION 1. *Be it enacted by the General Assembly of the State of Indiana*, That hereafter the annual reports of the meetings of the Indiana Academy of Science, beginning with the report for the year 1894, including all papers of scientific or economic value, presented at such meetings, after they shall have been edited and prepared for publication as hereinafter provided shall be published by and under the direction of the Commissioners of Public Printing and Binding.

Editing Reports.

SEC. 2. Said reports shall be edited and prepared for publication without expense to the State, by a corps of editors to be selected and appointed by the Indiana Academy of Science, who shall not, by reason of such service, have any claim against the State for compensation. The form, style of binding, paper, typography and manner and extent of illustration of such reports shall be determined by the editors, subject to the approval of the Commissioners of Public Printing and Stationery.

Number of Printed Reports.

Not less than 1,500 nor more than 3,000 copies of each of said reports shall be published, the size of the edition within said limits to be determined by the concurrent action of the editors and the Commissioners of Public Printing and Stationery: *Provided*, That not to exceed six hundred dollars ($600) shall be expended for such publication in any one year, and not to extend beyond 1896: *Provided*, That no sums shall be deemed to be appropriated for the year 1894. Proviso.

Sec. 3. All except three hundred copies of each volume of said reports shall be placed in the custody of the State Librarian, who shall furnish one copy thereof to each public library in the State, one copy to each university, college or normal school in the State, one copy to each high school in the State having a library, which shall make application therefor, and one copy to such other institutions, societies or persons as may be designated by the Academy through its editors or its council. The remaining three hundred copies shall be turned over to the Academy to be disposed of as it may determine. In order to provide for the preservation of the same it shall be the duty of the Custodian of the State House to provide and place at the disposal of the Academy one of the unoccupied rooms of the State House, to be designated as the office of the Indiana Academy of Science, wherein said copies of said reports belonging to the Academy, together with the original manuscripts, drawings, etc., thereof can be safely kept, and he shall also equip the same with the necessary shelving and furniture. Disposition of Reports.

Sec. 4. An emergency is hereby declared to exist for the immediate taking effect of this act, and it shall therefore take effect and be in force from and after its passage. Emergency.

APPROPRIATION FOR 1910-1911.

The appropriation for the publication of the proceedings of the Academy during the years 1910 and 1911 was increased by the legislature in the General Appropriation bill, approved March 9, 1909. That portion of the law fixing the amount of the appropriation for the Academy is herewith given in full:

For the Academy of Science: For the printing of the proceedings of the Indiana Academy of Science, twelve hundred dollars: *Provided*, That any unexpended balance in 1909 shall be available in 1910, and that any unexpended balance in 1910 shall be available in 1911. Academy of Science—Regular.

AN ACT FOR THE PROTECTION OF BIRDS, THEIR NESTS AND EGGS.

Sec. 602. Whoever kills, traps or has in his possession any wild bird, or whoever sells or offers the same for sale, or whoever destroys the nest or eggs of any wild bird, shall be deemed guilty of a misdemeanor and upon conviction thereof shall be fined not less than ten dollars nor more than twenty-five dollars: *Provided*, That the provisions of this section shall not apply to the following named game birds: The Anatidae, commonly called swans, geese, brant, river and sea duck; the Rallidae, commonly called rails, coots, mud-hens, gallinules; the Limicolae, commonly called shore birds, surf birds, plover, snipe, woodcock, sandpipers, tattlers and curlew; the Gallinae, commonly called wild turkeys, grouse, prairie chickens, quails and pheasants; nor to English or European house sparrows, crows, hawks or other birds of prey. Nor shall this section apply to persons taking birds, their nests or eggs, for scientific purposes, under permit, as provided in the next section.

Sec. 603. Permits may be granted by the Commissioner of Fisheries and Game to any properly accredited person, permitting the holder thereof to collect birds, their nests or eggs for strictly scientific purposes. In order to obtain such permit the applicant for the same must present to such Commissioner written testimonials from two well-known scientific men certifying to the good character and fitness of such applicant to be entrusted with such privilege, and pay to such Commissioner one dollar therefor and file with him a properly executed bond in the sum of two hundred dollars, payable to the State of Indiana, conditioned that he will obey the terms of such permit, and signed by at least two responsible citizens of the state as sureties. The bond may be forfeited, and the permit revoked upon proof to the satisfaction of such Commissioner that the holder of such permit has killed any bird or taken the nest or eggs of any bird for any other purpose than that named in this section.

Indiana Academy of Science.

OFFICERS, 1909-1910.

PRESIDENT

P. N. Evans.

VICE-PRESIDENT

C. R. Dryer.

SECRETARY

Geo. W. Benton.

ASSISTANT SECRETARY

A. J. Bigney.

PRESS SECRETARY

John W. Woodhams.

TREASURER

W. J. Moenkhaus.

EXECUTIVE COMMITTEE

P. N. Evans,
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G. W. Benton,
A. J. Bigney,
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A. L. Foley,
Glenn Culbertson,
D. M. Mottier,
Robert Hessler,
John S. Wright,
Carl L. Mees,
W. S. Blatchley,
H. W. Wiley,
M. B. Thomas,
D. W. Dennis,
C. H. Eigenmann,
C. A. Waldo,
Thomas Gray,
Stanley Coulter,
A. W. Butler,
W. A. Noyes,
J. C. Arthur,
O. P. Hay,
T. C. Mendenhall,
J. C. Branner,
J. P. D. John,
J. M. Coulter,
D. S. Jordan.

CURATORS

Botany........J. C. Arthur.
Ichthyology........C. H. Eigenmann.
Herpetology, Mammalogy, Ornithology........A. W. Butler.
Entomology........W. S. Blatchley.

COMMITTEES, 1909–1910.

PROGRAM.

J. P. Naylor, E. S. Johonnatt, J. S. Wright

MEMBERSHIP.

S. Coulter, A. W. Butler, M. B. Thomas

NOMINATIONS.

A. J. Bigney, L. J. Rettger, W. A. Cogshall

AUDITING.

W. J. Moenkhaus, G. W. Benton, W. S. Blatchley

STATE LIBRARY.

J. S. Wright, W. S. Blatchley, A. W. Butler
G. W. Benton.

RESTRICTION OF WEEDS AND DISEASES.

R. Hessler, J. N. Hurty, A. W. Butler
S. Coulter, D. M. Mottier.

DIRECTORS OF BIOLOGICAL SURVEY.

Stanley Coulter, C. R. Dryer, M. B. Thomas
C. H. Eigenmann, J. C. Arthur.

RELATIONS OF THE ACADEMY TO THE STATE.

R. W. McBride, M. B. Thomas, G. Culbertson
W. S. Blatchley.

DISTRIBUTION OF THE PROCEEDINGS.

J. S. Wright, H. L. Bruner, G. W. Benton
R. E. Lyons, J. H. Ransom.

PUBLICATION OF PROCEEDINGS.

H. L. Bruner, Editor, D. Bodine, D. M. Mottier.

OFFICERS OF THE INDIANA ACADEMY OF SCIENCE.

Years.	President.	Secretary.	Asst. Secretary.	Press Secretary.	Treasurer.
1885–1886	David S. Jordan	Amos W. Butler			O. P. Jenkins.
1886–1887	John M. Coulter	Amos W. Butler			O. P. Jenkins.
1887–1888	J. P. D. John	Amos W. Butler			O. P. Jenkins.
1888–1889	John C. Branner	Amos W. Butler			O. P. Jenkins.
1889–1890	T. C. Mendenhall	Amos W. Butler			O. P. Jenkins.
1890–1891	O. P. Hay	Amos W. Butler			O. P. Jenkins.
1891–1892	J. L. Campbell	Amos W. Butler			C. A. Waldo.
1892–1893	J. C. Arthur	Amos W. Butler	Stanley Coulter W. W. Norman		C. A. Waldo.
1893–1894	W. A. Noyes	C. A. Waldo	W. W. Norman		W. P. Shannon.
1894–1895	A. W. Butler	John S. Wright	A. J. Bigney		W. P. Shannon.
1895–1896	Stanley Coulter	John S. Wright	A. J. Bigney		W. P. Shannon.
1896–1897	Thomas Gray	John S. Wright	A. J. Bigney		W. P. Shannon.
1897–1898	C. A. Waldo	John S. Wright	A. J. Bigney	Geo. W. Benton	J. T. Scovell.
1898–1899	C. H. Eigenmann	John S. Wright	E. A. Schultze	Geo. W. Benton	J. T. Scovell.
1899–1900	D. W. Dennis	John S. Wright	E. A. Schultze	Geo. W. Benton	J. T. Scovell.
1900–1901	M. B. Thomas	John S. Wright	E. A. Schultze	Geo. W. Benton	J. T. Scovell.
1901–1902	Harvey W. Wiley	John S. Wright	Donaldson Bodine	Geo. W. Benton	J. T. Scovell.
1902–1903	W. S. Blatchley	John S. Wright	Donaldson Bodine	G. A. Abbott	W. A. McBeth.
1903–1904	C. L. Mees	John S. Wright	J. H. Ransom	G. A. Abbott	W. A. McBeth.
1904–1905	John S. Wright	Lynn B. McMullen	J. H. Ransom	G. A. Abbott	W. A. Mcbeth.
1905–1906	Robert Hessler	Lynn B. McMullen	J. H. Ransom	Charles R. Clark	W. A. McBeth.
1906–1907	D. M. Mottier	Lynn B. McMullen	J. H. Ransom	G. A. Abbott	W. A. McBeth.
1907–1908	Glenn Culbertson	J. H. Ransom	A. J. Bigney	G. A. Abbott	W. A. McBeth.
1908–1909	A. L. Foley	J. H. Ransom	A. J. Bigney	G. A. Abbott	W. A. McBeth.
1909–1910	P. N. Evans	Geo. W. Benton	A. J. Bigney	John W. Woodhams	W. J. Moenkhaus

CONSTITUTION.

ARTICLE I.

Section 1. This association shall be called the Indiana Academy of Science.

Sec. 2. The objects of this Academy shall be scientific research and the diffusion of knowledge concerning the various departments of science, to promote intercourse between men engaged in scientific work, especially in Indiana; to assist by investigation and discussion in developing and making known the material, educational and other resources and riches of the State; to arrange and prepare for publication such reports of investigation and discussions as may further the aims and objects of the Academy as set forth in these articles.

Whereas, The State has undertaken the publication of such proceedings, the Academy will, upon request of the Governor, or of one of the several departments of the State, through the Governor, act through its council as an advisory body in the direction and execution of any investigation within its province as stated. The necessary expenses incurred in the prosecution of such investigation are to be borne by the State; no pecuniary gain is to come to the Academy for its advice or direction of such investigation.

The regular proceedings of the Academy as published by the State shall become a public document.

ARTICLE II.

Section 1. Members of this Academy shall be honorary fellows, fellows, non-resident members or active members.

Sec. 2. Any person engaged in any department of scientific work, or in original research in any department of science, shall be eligible to active membership. Active members may be annual or life members. Annual members may be elected at any meeting of the Academy; they shall sign the constitution, pay an admission fee of two dollars and thereafter an annual fee of one dollar. Any person who shall at one time contribute fifty dollars to the funds of this Academy may be elected a life member of the Academy, free of assessment. Non-resident members may be elected from those who have been active members but who have removed from the State. In any case, a three-fourths vote of the members present shall elect to membership. Applications for membership in any of the foregoing classes shall be referred to a committee on application for membership. who shall consider such application and report to the Academy before the election.

Sec. 3. The members who are actively engaged in scientific work, who have recognized standing as scientific men, and who have been members of the Academy at least one year, may be recommended for nomination for election as fellows by three fellows or members personally acquainted with their work and character. Of members so nominated a number not exceeding five in one year may, on recommendation of the Executive Committee, be elected as fellows. At the meeting at which this is adopted, the members of the Executive Committee for 1894 and fifteen others shall be elected fellows, and those now honorary members shall become honorary fellows. Honorary fellows may be elected on account of special prominence in science, on the written recommendation of two members of the Academy. In any case a three-fourths vote of the members present shall elect.

ARTICLE III.

Section 1. The officers of this Academy shall be chosen by ballot at the annual meeting, and shall hold office one year. They shall consist of a President, Vice-President, Secretary, Assistant Secretary, Press Secretary and Treasurer, who shall perform the duties usually pertaining to their respective offices and in addition, with the ex-Presidents of the Academy, shall constitute an Executive Committee. The President shall, at each annual meeting, appoint two members to be a committee, which shall prepare the programs and have charge of the arrangements for all meetings for one year.

Sec. 2. The annual meeting of this Academy shall be held in the city of Indianapolis within the week following Christmas of each year, unless otherwise ordered by the Executive Committee. There shall also be a summer meeting at such time and place as may be decided upon by the Executive Committee. Other meetings may be called at the discretion of the Executive Committee. The past Presidents, together with the officers and Executive Committee, shall constitute the council of the academy, and represent it in the transaction of any necessary business not especially provided for in this constitution, in the interim between general meetings.

SEC. 3. This constitution may be altered or amended at any annual meeting by a three-fourths majority of the attending members of at least one year's standing. No question of amendment shall be decided on the day of its presentation.

BY-LAWS.

1. On motion, any special department of science shall be assigned to a curator, whose duty it shall be, with the assistance of the other members interested in the same department, to endeavor to advance knowledge in that particular department. Each curator shall report at such time and place as the Academy shall direct. These reports shall include a brief summary of the progress of the department during the year preceding the presentation of the report.

2. The President shall deliver a public address on the morning of one of the days of the meeting at the expiration of his term of office.

3. The Press Secretary shall attend to the securing of proper newspaper reports of the meetings and assist the Secretary.

4. No special meeting of the Academy shall be held without a notice of the same having been sent to the address of each member at least fifteen days before such meeting.

5. No bill against the Academy shall be paid without an order signed by the President and countersigned by the Secretary.

6. Members who shall allow their dues to remain unpaid for two years, having been annually notified of their arrearage by the Treasurer, shall have their names stricken from the roll.

7. Ten members shall constitute a quorum for the transaction of business.

MEMBERS.

FELLOWS.

†G. A. Abbott	*1908	Fargo, N. D.
R. J. Aley	1898	Indianapolis.
J. C. Arthur	1894	Lafayette.
J. W. Beede	1906	Bloomington.
George W. Benton	1896	Indianapolis.
A. J. Bigney	1897	Moores Hill.
Katherine Golden Bitting	1895	Lafayette.
W. S. Blatchley	1893	Indianapolis.
Donaldson Bodine	1899	Crawfordsville.
H. L. Bruner	1899	Indianapolis.
Severance Burrage	1898	Lafayette.
A. W. Butler	1893	Indianapolis.
W. A. Cogshall	1906	Bloomington.
†Mel. T. Cook	1902	Newark, Del.
†John M. Coulter	1893	Chicago, Ill.
Stanley Coulter	1893	Lafayette.
U. O. Cox	1908	Terre Haute.
Glenn Culbertson	1899	Hanover.
E. R. Cumings	1906	Bloomington.
S. C. Davisson	1908	Bloomington.
D. W. Dennis	1895	Richmond.
C. R. Dryer	1897	Terre Haute.
C. H. Eigenmann	1893	Bloomington.
Percy Norton Evans	1901	West Lafayette.
A. L. Foley	1897	Bloomington.
M. J. Golden	1899	Lafayette.
†W. F. M. Goss	1893	Urbana, Ill.
Thomas Gray (Died Dec. 19, 1908)	1893	Terre Haute.
A. S. Hathaway	1895	Terre Haute.
W. K. Hatt	1902	Lafayette.
Robert Hessler	1899	Logansport.

*Date of election.
†Non-resident.

†H. A. Huston	*1893	Baltimore, Md.
Edwin S. Johonnatt	1904	Terre Haute.
Robert E. Lyons	1896	Bloomington.
W. A. McBeth	1904	Terre Haute.
V. F. Marsters	1893	Santiago, Chili.
C. L. Mees	1894	Terre Haute.
†J. A. Miller	1904	Swarthmore, Pa.
W. J. Moenkhaus	1901	Bloomington.
D. M. Mottier	1893	Bloomington.
J. P. Naylor	1903	Greencastle.
†W. A. Noyes	1893	Urbana, Ill.
Rolla R. Ramsey	1906	Bloomington.
J. H. Ransom	1902	Lafayette.
L. J. Rettger	1896	Terre Haute.
David Rothrock	1906	Bloomington.
J. T. Scovell	1894	Terre Haute.
Albert Smith	1908	Lafayette.
†Alex Smith	1893	Chicago, Ill.
W. E. Stone	1893	Lafayette.
†Joseph Swain	1898	Swarthmore, Pa.
M. B. Thomas	1893	Crawfordsville.
†C. A. Waldo	1893	St. Louis, Mo.
†F. M. Webster	1894	Washington, D.C.
Jacob Westlund	1904	Lafayette.
†H. W. Wiley	1895	Washington, D..C.
W. W. Woollen	1908	Indianapolis.
John S. Wright	1894	Indianapolis.

*Date of election.
†Non-resident.

NON-RESIDENT MEMBERS.

George H. Ashley	Washington, D. C.
J. C. Branner	Stanford University, Cal.
M. A. Brannon	Grand Forks, N. D.
D. H. Campbell	Stanford University, Cal.
H. W. Clark	Washington, D. C.
H. B. Dorner	Urbana, Ill.
A. Wilmer Duff	Worcester, Mass.

B. W. Everman .... Washington, D. C.
W. A. Fiske .... Los Angeles, Cal.
C. W. Garrett .... Pi tsburg, Pa.
Charle H. Gilbert .... Stanford University, Cal.
C. W. Greene .... Columbia, Mo.
C. W. Hargitt .... Syracuse, N. Y.
O. P. Hay .... Washington, D. C.
Edward Hughes .... Stockton, Cal.
O. P. Jenkins .... Stanford University, Cal.
C. T. Knipp .... Urbana, Ill.
D. S. Jordan .... Stanford University, Cal.
J. S. Kingsley .... Tufts College, Mass.
D. T. MacDougal .... Tucson, Arizona.
L. B. McMullen .... Valley City, N. D.
T. C. Mendenhall .... Worcester, Mass.
J. F. Newsom .... Stanford University, Cal.
A. H. Purdue .... Fayetteville, Ark.
A. B. Reagan .... Orr, Minn.
J. R. Slonaker .... Stanford University, Cal.
Alfred Springer .... Cincinnati, Ohio.
Robert B. Warder (Deceased) .... Washington, D. C.
Ernest Walker .... Fayetteville, Ark.
G. W. Wilson .... Fayette, Ia.

ACTIVE MEMBERS.

C. E. Agnew .... Delphi.
L. E. Allison .... West Lafayette.
H. W. Anderson .... Ladoga.
Paul Anderson .... Crawfordsville.
H. F. Bain .... San Francisco, Cal.
Walter D. Baker .... Indianapolis.
Walter M. Baker .... Red Key.
Edward Hugh Bangs .... Indianapolis.
Howard J. Banker .... Greencastle.
H. E. Barnard .... Indianapolis,
W. H. Bates .... West Lafayette.
Guido Bell .... Indianapolis.

Lee F. Bennett . . . . Valparaiso.
Thomas Billings . . . . West Lafayette.
Harry Eldridge Bishop . . . . Indianapolis.
Lester Black . . . . Bloomington.
William N. Blanchard . . . . Greencast'e.
Charles S. Bond . . . . Richmond.
A. A. Bourke . . . . Edinburg.
Omer C. Boyer . . . . Lebanon.
H. C. Brandon . . . . Bloomington.
Fred J. Breeze . . . . Lafayette.
Chas. Brossmann . . . . Indianapo'is.
E. M. Br ce . . . . Terre Haute.
Wm. R. Butler . . . . Indianapolis.
Edward N. Canis . . . . Indianapolis.
E. Kate Carman . . . . Indianapolis.
Lewis Clinton Carson . . . . Detroit, Mich.
Herman S. Chamberlain (Deceased) . . . . Indianapolis.
E. J. Chansler . . . . Bicknell.
A. G. W. Childs . . . . Kokomo.
C. D. Christie . . . . Cincinnati, O.
J. H. Clark . . . . Connersville.
Otto O. Clayton . . . . Portland.
H. M. Clem . . . . Monroeville.
Charles Clickner . . . . Silverwood, R. D. No. 1.
Charles A. Coffey . . . . Petersburg.
William Clifford Cox . . . . Columbus.
J. A. Cragwall . . . . Crawfordsville.
M. E. Crowell . . . . Franklin.
Chas. M. Cunningham . . . . Indianapolis.
Lorenzo E. Daniels . . . . Laporte.
E. H. Davis . . . . West Lafayette.
Melvin K. Davis . . . . Terre Haute.
Charles C. Deam . . . . Indianapolis.
E. M. Deem . . . . Frankfort.
Harry F. Dietz . . . . Indianapolis.
James P. Dimonds . . . . Indianapolis.
Martha Doan . . . . Westfield.
J. P. Dolan . . . . Syracu e.

Hans Duden.................................Indianapolis.
Arthur E. Dunn.............................Logansport.
Herbert A. Dunn............................Logansport.
M. L. Durbin...............................Anderson.
J. B. Dutcher..............................Philadelphia, Penn.
Samuel E. Earp.............................Indianapolis.
A. A. Eberly...............................Nowata, Okla.
C. R. Eckler...............................Indianapolis.
Max Mapes Ellis............................Vincennes.
H. E. Enders...............................West Lafayette.
Samuel G. Evans............................Evansville.
William P. Felver..........................Logansport.
C. J. Fink.................................Crawfordsville.
M. L. Fisher...............................West Lafayette.
A. S. Fraley...............................Linden.
Austin Funk................................Jeffersonville.
John D. Gabel..............................North Madison.
Andrew W. Gamble...........................Logansport.
H. O. Garman...............................Indianapolis.
J. B. Garner...............................Crawfordsville.
Florence A. Gates..........................Wabash.
Robert G. Gillum...........................Terre Haute.
E. R. Glenn................................Brookville.
Frederic W. Gottlieb.......................Morristown.
Vernon Gould...............................Rochester.
Frank Cook Greene..........................New Albany.
Earl Grimes................................Russellv lle.
Walter L. Hahn.............................Springfield, S. D.
C. F. Harding..............................West Lafayette.
Mary T. Harman.............................State College, Pa.
Walter W. Hart.............................Indianapolis.
Victor Hendricks...........................St. Louis, Mo.
L. R. Hessler..............................Crawfordsville.
John P. Hetherington.......................Logansport.
C. E. Hiatt................................Philadelphia, Pa.
John E. Higdon.............................Indianapolis.
Frank R. Higgins...........................Terre Haute.
S. Bella Hilands...........................Madison.

John J. Hildebrandt..............................Logansport.
Geo. N. Hoffer..................................Lafayette.
G. E. Hoffman...................................Logansport.
Allen D. Hole...................................Richmond.
Lucius M. Hubbard...............................South Bend.
Martha Hunt.....................................Indianapolis.
O. F. Hunziker..................................West Lafayette.
John N. Hurty...................................Indianapolis.
Roscoe R. Hyde..................................Terre Haute.
J. Isenberger...................................Lebanon.
C. F. Jackson...................................Durham, N. H.
A. G. Johnson...................................Lafayette.
H. E. Johnson...................................Greenfield.
A. T. Jones.....................................West Lafayette.
W. J. Jones, Jr.................................West Lafayette.
O. L. Kelso.....................................Terre Haute.
A. M. Kenyon....................................West Lafayette.
Frank D. Kern...................................Lafayette.
L. V. Ludy......................................West Lafayette.
R. W. McBride...................................Indianapolis.
Richard C. McClaskey............................
T. S. McCulloch.................................Crawfordsville.
N. E. McIndoo...................................
Edward G. Mahin.................................West Lafayette.
James E. Manchester.............................Minneapolis, Minn.
Wilfred H. Manwaring............................Bloomington.
William Edgar Mason.............................Borden.
Clark Mick......................................Indianapolis.
A. R. Middleton.................................West Lafayette.
G. Rudolph Miller...............................Indianapolis.
F. A. Miller....................................Indianapolis.
Chas. R. Moore..................................West Lafayette.
Geo. T. Moore...................................St. Louis, Mo.
Richard Bishop Moore............................Indianapolis.
Frank K. Mowrer.................................Marion.
F. W. Muncie....................................Crawfordsville.
Fred Mutchler...................................Terre Haute.
Leslie C. Nanney................................Bedford.

Charles E. Newlin . . . . Indianapolis.
J. A. Nieuwland . . . . Notre Dame.
G. A. Osner . . . . Crawfordsville.
D. A. Owen . . . . Franklin.
Everett W. Owen . . . . Indianapolis.
Ferman L. Pickett . . . . Bloomington.
Rollo J. Pierce . . . . Richmond.
Ralph B. Polk . . . . Greenwood.
James A. Price . . . . Ft. Wayne.
W. H. Rankin . . . . Ithaca, N. Y.
C. A. Reddick . . . . Crawfordsville.
C. J. Reilly . . . . Syracuse.
Allen J. Reynolds . . . .
George L. Roberts . . . . Lafayette.
J. Schramm . . . . Crawfordsville.
E. A. Schultze . . . . Laurel.
Will Scott . . . . Bloomington.
Charles Wm. Shannon . . . . Brazil.
Fred Sillery . . . . Indianapolis.
Oscar W. Silvey . . . . W. Lafayette.
C. Piper Smith . . . . Logan, Utah.
Essie Alma Smith Shannon . . . . Bloomington.
E. R. Smith . . . . Indianapolis.
Geo. Spitzer . . . . Lafayette.
Brenton L. Steele . . . . Pullman, Wash.
Chas. Stoltz . . . . South Bend.
J. M. Stoddard . . . .
Milo H. Stuart . . . . Indianapolis.
Julius W. Sturmer . . . . Lafayette.
J. C. Taylor . . . . Logansport.
Albert W. Thompson . . . . Owensville.
A. D. Thorburn . . . . Indianapolis.
Iro C. Trueblood (Miss) . . . . Greencastle.
W. P. Turner . . . . West Lafayette.
Chas. A. Vallance . . . . Indianapolis.
J. M. Van Hook . . . . Bloomington.
W. B. Van Gorder . . . . Lyons.
H. S. Voorhees . . . . Ft. Wayne.

Frank B. Wade	Indianapolis.
Luther C. Weeks	West Lafayette.
Mason L. Weems	Valparaiso.
Daniel T. Weir.	Indianapolis.
James E. Weyant	Indianapolis.
Virges Wheeler	Montmorenci.
A. E. White	Connersville.
Alfred T. Wiancko	Lafayette.
William L. Woodburn	Bloomington.
John W. Woodhams	Indianapolis.
Herbert Milton Woollen	Indianapolis.
J. F. Woolsey	Cleveland, O.
G. A. Young	West Lafayette.
Jacob P. Young	Huntington.
L. E. Young	West Lafayette.
W. J. Young	Washington, D. C.
Lucy Youse	Terre Haute.
W. A. Zehring	West Lafayette.
Charles Zeleny	Urbana, Ill.

Fellows, resident	46
Non-resident	12
Members, active	183
Members, non-resident	30
Total	271

THOMAS GRAY.

Dr. Thomas Gray, a member of the Indiana Academy of Science since 1888, was President in 1897-8, died in Terre Haute, Ind., December 19, 1908.

He was born in Lochgelly, Scotland, February 4, 1850, received his early education in the schools of the district and, after serving an apprenticeship in handicraft, entered the University of Glasgow, graduating from the Mechanical Engineering course, with high honors, in 1874. After graduation he became Research Assistant to Lord Kelvin (Sir William Thompson). His work lay especially in the direction of absolute measurements in electricity and magnetism, electrical and heat conductivity of glasses of various compositions and the variation in conductivity of metals under stress. In 1878 he became Professor of Telegraph Engineering in the University of Tokio, Japan. While there he became interested in earthquake phenomena and invented several seismographs and investigated the elastic constants of many rocks. In 1881 he returned to Scotland, becoming Lord Kelvin's personal assistant, undertaking investigations in connection with practical problems in electricity then coming to the front. He developed and investigated methods for electrolytic measurements of electric currents and largely designed the well known Kelvin balances. He was Lord Kelvin's and Flemming Jenkins' representative as engineer for the Commercial Cable Companies and supervised the laying of the Bennett-Mackay transatlantic cables. In 1888 he came to Terre Haute, Ind., as professor of dynamic engineering in the Rose Polytechnic Institute, which position he held until his death. His investigational work was now mainly of an engineering character, too well known to recount. He was the author of several important works, the best known perhaps being the Smithsonian Physical Tables. The articles in the Encyclopedia Brittanica on telegraphs and telephones were from his pen. He also edited the definitions in electricity and magnetism for the Century Dictionary. He was the author of about sixty papers on scientific subjects, communicated to engineering societies and scientific journals. He was a member of most of the American scientific and engineering societies and held high offices in a number of them. His interest in the work of the Indiana Academy of Science made him a faithful and regular attendant at most of its meetings. On the roll of American and European scientists, his name stands high, and his contribution to science, as well as his work in the educational field while in this country, has been of the highest order.

Be it Resolved, That the Indiana Academy of Science recognize the services of Dr. Thomas Gray as investigator, experimentalist, teacher, and loyal supporter of the Academy by placing these resolutions and a sketch of his life upon the minutes of this meeting and print them in the volume of Proceedings.

The Committee: C. L. MEES.
A. W. BUTLER.
G. W. BENTON.

Adopted by the Indiana Academy of Science, in session in Indianapolis, Nov. 27, 1909.

W. H. RAGAN.

W. H. Ragan, for many years connected with the United States Department of Agriculture, and who recently died, was one of the charter members of the Indiana Academy of Science. He was one of that company, of which a number of members are here today, who were present at the first meeting. At that time he was a member of the faculty of DePauw University. He has had a deep interest in the progress of science, and especially in its application to horticulture, to which line of usefulness his life was devoted.

We make a tribute herewith to his memory.

The Committee: C. L. MEES.
A. W. BUTLER.
G. W. BENTON.

Adopted by the Indiana Academy of Science, in session in Indianapolis, Nov. 27, 1909.

TWENTY-FIFTH ANNUAL MEETING

INDIANA ACADEMY OF SCIENCE

CLAYPOOL HOTEL, INDIANAPOLIS, IND.

NOVEMBER 25, 26 AND 27, 1909

Officers and Ex-Officio Executive Committee

A. L. FOLEY, President
P. N. EVANS, Vice-President
J. H. RANSOM, Secretary
A. J. BIGNEY, Assistant Secretary
G. A. ABBOTT, Press Secretary
W. A. MCBETH, Treasurer

GLENN CULBERTSON
DAVID MOTTIER
ROBERT HESSLER
JOHN S. WRIGHT
C. L. MEES
W. S. BLATCHLEY
H. W. WILEY
D. W. DENNIS
C. H. EIGENMANN
C. A. WALDO
THOMAS GRAY
STANLEY COULTER
AMOS W. BUTLER
W. A. NOYES
M. B. THOMAS
J. C. ARTHUR
O. P. HAY
T. C. MENDENHALL
JOHN C. BRANNER
J. P. D. JOHN
JOHN M. COULTER
DAVID STARR JORDAN

The meetings of the Indiana Academy of Science Thursday evening, November 25th; Friday, November 26th, morning and afternoon; Saturday, November 27th, morning; and the informal dinner Thursday night, the luncheon Friday noon and the banquet Friday night, will be at the Claypool Hotel.

The rates quoted by the management are $2.00 per day and upward on the European plan and $4.00 per day and upward on the American plan. Where two or more persons occupy a room, the rates are $1.50 and upward per day, European plan, and $3.50 and upward per day, American plan. Hotel reservation and reservations for the banquet should be made at once.

A stereopticon will be provided.

Committee on 25th Meeting

AMOS W. BUTLER, Chairman

M. B. THOMAS
W. E. STONE
C. L. MEES
W. J. MOENKHAUS
H. L. BRUNER
J. P. NAYLOR

Local Committee

George W. Benton John S. Wright John W. Woodhams

OUTLINE OF GENERAL PROGRAM

Thursday, November 25

4:00 p. m. Meeting of the Executive Committee
6:30 p. m. Informal dinner
8:00 p. m. Opening session
Business
Address—"By Packtrain to the Tiptop of the United States in Quest of the Golden Trout," B. W. Evermann, U. S. Bureau of Fisheries, Washington D. C.

Friday November 26

9:00 a. m. Business
President's Address—"Recent Progress in Physics," Dr. A. L. Foley, Bloomington
Address—"Recent Progress in Chemistry," Dr. H. W. Wiley, Chief of the Bureau of Chemistry, U. S. Department of Agriculture. Washington, D. C.
Address—"Recent Progress in Botany," Dr. John M. Coulter, Department of Botany, Chicago University
Greetings from other societies
12:00 noon Informal luncheon
2:00 p. m. Address—"Darwin Fifty Years After," Dr. David Starr Jordan, President Leland Stanfard Jr. University, President American Association for the Advancement of Science
3:00 p. m. Section meetings
The Academy will meet in sections. A few papers, preferably those of historical character, will be read
8:00 p. m. Banquet—D. W. Dennis, Toastmaster

Saturday, November 27

9:00 a. m. Business
Address—"Methods and Materials Used in Soil Testing," H. A. Huston, Chicago
Address—"Federal Control of International and Interstate Waters," B. W. Evermann, U. S. Bureau of Fisheries
Address—"The Speed of Migration of Salmon in the Columbia River," Charles W. Greene, University of Missouri
Address—"Some Hoosier and Academy Experiences," C. A. Waldo, Washington University, St. Louis, Mo.

Suggestions: Plans for the Academy—

John S. Wright	W. E. Stone
Stanley Coulter	C. Leo Mees
H. E. Barnard	W. A. Cogshall

PAPERS TO BE READ

Unless otherwise stated, papers will be understood to be limited to fifteen minutes. The first circular of the Committee stated: "These papers will be presented, and while probably few of them will be read at the meeting, they will be printed in the Proceedings."

General

Thought Stimulation, Under What Conditions Does It Occur? 10 minutes ....Robert Hessler

Does Blood Tell? ....William B. Streeter Greensboro, N. C.

Hygiene of Indoor Swimming Pools, with Suggestions for Practical Disinfection. 25 minutes ....Severance Burrage

Indiana Problems in Sewage Disposal. 10 minutes..........R. L. Sackett

Defective Elementary Science ....William N. Heiney

Some Hoosier and Academy Experiences
C. A. Waldo, Washington University

Darwin Fifty Years After
David Starr Jordan, President Leland Stanford Jr. University

The Zia Mesa and Ruins ....Albert B. Reagan

That Erroneous Hiawatha ....Albert B. Reagan

The Medicinal Value of Eupatorium Perfoliatum ....A. J. Bigney

Chemistry

Methods and Materials Used in Soil Testing. 25 minutes
H. A. Huston, Chicago, Ill.

The Discovery of the Composition of Water (illustrated)
W. A. Noyes, University of Illinois

Molecular Rearrangements of Derivatives of Camphor........W. A. Noyes

Use of Refractometer in Dry Substance Estimation
A. Hugh Bryan, U. S. Bureau of Chemistry

Conductivity and Ionization of Solutions of Certain Salts in Ethyl Amine. 10 minutes ....E. G. Mahin

Recent Progress in Chemistry.
H. W. Wiley, Chief of the Bureau of Chemistry, U. S. Department of Agriculture

Electric Osmose. 15 minutes ....Harry N. Holmes

A Study of the Chemical Composition of Butter Fat
O. F. Hunziker and George Spitzer

On a New Complex Cyanogen Compound..................A. R. Middleton
The Determination of Endothermic Gases by Combustion...A. R. Middleton

Mathematics

Methods in Solid Analytics. 15 minutes..............Arthur S. Hathaway
Motion of n Bodies. 20 minutes......................Arthur S. Hathaway
Discussion of the Regular Inscribed Pentagon. 5 minutes...John C. Gregg
If the Bisectors of Two Angles of a Triangle are Equal, Those Angles are Equal. 5 minutes ....John C. Gregg

Physics

Direct Reading Accelerometers. 20 minutes ....C. R. Moore
Recent Work in Wood Physics. 10 minutes ....W. K. Hatt
Expansion of Paving Blocks. 10 minutes ....W. K. Hatt
Notes on the Strength of Concrete Building Blocks. 10 minutes H. H. Schofield
Slip of Riveted Joints. 10 minutes ....Albert Smith
Polarization of Cadmium Cells ....Rolla R. Ramsey
Investigation of a Point Discharge in a Magnetic Field....Oscar W. Silvey
The Tenacity of Gelatine ....Arthur L. Foley
Objections to LaPlace's Theory of Capillarity............Arthur L. Foley
Cohesion of Water as Modified by Certain Dissolved Salts. 10 minutes ....Edwin Morrison

Geology and Geography.

Some Features of Delta Formation. 15 minutes........Charles R. Dryer
A Physiographic Survey of an Area Near Terre Haute, Ind. 25 minutes ....Charles R. Dryer, Melvin K. Davis
The Collecting Area of the Waters of the Hot Springs of Hot Springs, Ark. 15 minutes ....A. H. Purdue, University of Arkansas
The Geographical and Geological Distribution of Some Pleistocene Mammals ....O. P. Hay, U. S. National Museum
On the Restoration of Skeletons of Fossil Vertebrates..........O. P. Hay
Where Do the Lance Creek ("Ceratops") Beds Belong, in the Cretaceous or Tertiary?..................................Oliver P. Hay
Paleontology and the Recapitulation Theory. 50 minutes....E. R. Cumings
The Tippecanoe, an Infantile Drainage System. 10 minutes..W. A. McBeth
Observations on Cyclones and Anti-Cyclones of North Temperate Latitudes. 10 minutes ....W. A. McBeth

Zoology.

A Paired Entoplastron in Trionyx and Its Significance. 15 minutes ....Henry H. Lane, Oklahoma State University

Physiological Explanation of the Psycho-Physical Law of Weber. 15 minutes ....Guido Bell
On the Nature and Source of Thrombin. 12 minutes..........L. J. Rettger
Federal Control of International and Interstate Waters
B. W. Evermann, U. S. Bureau of Fisheries
By Packtrain to the Tiptop of the United States in Quest of the Golden Trout (illustrated) ....B. W. Evermann
The History of Zoology in Indiana. 15 minutes.........C. H. Eigenmann
An Analytic Study of the Faunal Changes in Indiana. 25 minutes
Walter L. Hahn, South Dakota State Normal School
Some Notes on Parasites Found in Frogs in the Vicinity of St. Paul, Minn. 20 minutes..................H. L. Osborn, Hamlin University
The Mocking Bird About Moores Hill, Indiana..............A. J. Bigney
Cross-Fertilization Among Fishes......................W. J. Moenkhaus
Observations on Woodpeckers. 5 minutes...............John T. Campbell
Paroxysmal Hæmoglobinuria. 10 minutes................Oliver P. Terry
The Evolution of Insect Galls as Illustrated by the Genus Amphibolips ....Mel T. Cook, Delaware College
The Speed of Migration of Salmon in the Columbia River
Charles W. Greene, University of Missouri
Observations on Cerebral Localization
J. Rollin Slonaker, Leland Stanford Jr. University
The Nasal Muscles of Vertebrates........................H. L. Bruner

Botany.

Physiological Apparatus ....Frank M. Andrews
Some Monstrosities in Plants.........................Frank M. Andrews
A List of Algæ ....Frank M. Andrews
Re-Vegetation of the Salton Basin (illustrated)
D. T. MacDougal, Director Desert Laboratory, Tucson, Ariz.
Forest Conditions in Indiana. 15 minutes................Stanley Coulter
Additions to Indiana Flora, Number 4. 3 minutes......Charles C. Deam
The Development of the Reproductive Organs of Chara fragilis. 20 minutes ....George N. Hoffer
Right and Wrong Conceptions of Plant Rusts................J. C. Arthur
The Effect of Preservatives on the Development of Penicillium. 10 minutes ....Katherine Golden Bitting
Recent Progress in Botany...........John M. Coulter, Chicago University
Further Notes on Timothy Rust..........................Frank D. Kern

Editorial Notice.

All members of the Academy will doubtless be ready to assist in any efforts put forth having in view correct and early publication of the Proceedings. To this end the following conditions of publication are announced by the editor:

1. All papers to be included in the report of 1909 must be in the hands of the editor not later than December 15, 1909.

2. All papers should be typewritten as far as the nature of the subject will allow.

3. All tracings and maps should be drawn to correspond with the size of the page of the Proceedings, and must come within the following limits: 4½x7. If necessary, it may be made to cover two pages, or measure 8½x11.

4. Authors are especially requested to carefully mark and number all illustrations and to carefuly indicate in the MSS. the exact location of such illustrations.

5. To secure proper representation of mathematical work, authors are particularly cautioned to send in carefully traced figures on separate paper.

6. The limits of the appropriation require that all illustrations shall be in one color, and either photographs or etchings. As a consequence, all illustrations must be in black and white.

Resolution Providing for the Celebration of the Twenty-fifth Anniversary of the Indiana Academy of Science.

Resolved, That in view of the fact that the next meeting will be the Twenty-fifth Annual Meeting of this Academy, a special effort be made at that time to celebrate the quarter centennial of its organization.

That a committee of seven be appointed to have charge of the program and all necessary arrangements for such meeting.

That the time and place of the next meeting be left to said committee.

That an effort be made to have present all the living ex-presidents and all of the living charter members of the Academy. Also that all the universities, colleges and other educational institutions of the State, all scientific organizations, including the State Medical Society, Indiana Engineering Society, Indiana Section of the American Chemical Society, State Science Teachers' Association, and all individuals interested in scientific work and the press of the State be cordially invited to co-operate to make this a successful meeting and memorable occasion.

Adopted November 28, 1908.

The Beginning of the Indiana Academy of Science.

By Amos W. Butler.

In my early years the lack of association with persons who were interested in scientific pusuits and of opportunity to refer to books on scientific subjects was greatly felt. I planned to interest several persons in establishing a local society which would bring kindred spirits together. This resulted in the organization of the Brookville Society of Natural History in 1881. That year, for the first time, I attended the meeting of the American Association for the Advancement of Science at Cincinnati. There I had the pleasure of meeting many persons of whom I had only known by reading. This was the beginning of many acquaintances that have been permanent, helpful and inspiring. In my efforts to study local natural history I found it difficult to obtain information from students in other parts of the State. In talking with others I found they had had the same difficulty. In the winter of 1883-1884, the need of a State organization was strongly impressed upon me. Correspondence was begun with a number of persons whose names were prominent in scientific work of the State, and the majority of them favored such an organization. Among these were Dr. David Starr Jordan, Dr. J. P. D. John, Professors John M. Coulter, Stanley Coulter, Philip S. Baker, Daniel Kirkwood, Richard Owen and Oliver P. Jenkins. There were others who discouraged it. The subject was fresh in mind at the time of the meeting of the American Association for the Advancement of Science at Ann Arbor in 1884. There opportunity was given to talk the subject over, and for the first time I met Dr. John C. Branner, who had just been appointed professor of geology at Indiana University, and he strongly urged the formation of such a society. Finally it was decided to call a meeting to organize an Indiana Society. The Brookville Society of Natural History, as the most active organization of its kind in the State, was asked to take the initiative and call the first meeting. Accordingly that society appointed a committee for that purpose, consisting of Rev. David R. Moore, its president, Dr. S. P. Stoddard and Amos W. Butler. The meeting was called for Indianapolis on December 29, 1885. The plan was to have a series of papers on the status of different branches of science in Indiana. The meeting was held in the Marion County court house. The program included the following papers:

"Meteorology" ....Wm. H. Ragan
"Progress in the Study of Mammalogy in Indiana"........Edgar R. Quick
"Sketch of the Work Accomplished for Natural and Physicial Science in Indiana"..Richard Owen
"Sketch of C. S. Rafinesque"............................D. S. Jordan
"Work Done in Icthyology in Indiana"......................D. S. Jordan
"Work Done in Botany in Indiana"......................John M. Coulter
"Work Done in Physics in Indiana"..........................J. P. Naylor
"Work Done in Study of the Lower Invertebrates"........O. P. Jenkins
"Present Condition of the Study of Indiana Herpetology"......O. P. Hay
"The Study of Entomology in Indiana"......................P. S. Baker
"The Present Knowledge of Indiana Mineralogy".......Maurice Thompson
"Work Done for Geology in Indiana"..................Ryland T. Brown
"Chemistry" ....R. B. Warder
"The Present Condition of Indiana Conchology"..........David R. Moore
"Indiana Statistics" ....J. B. Conner
"The Past and Present of Indiana Ornithology"..........Amos W. Butler
"Geography" ....J. T. Scovell
"Astronomy" ....David Kirkwood

(Of these only Richard Owen and David Kirkwood were absent, and their papers were read by others.)

Dr. J. P. D. John was chosen president pro tem. There were about forty persons present, representing most of the educational institutions of the State, and including most of the scientific workers. Dr. David Starr Jordan was chosen first president and Amos W. Butler the first secretary. A constitution and by-laws were adopted. Since that time regular annual meetings have been held. All but one, which was held at Lafayette, have been held in Indianapolis, and until recently spring meetings at different points in the State. The first one of these was appropriately held at Brookville May 20-22, 1886.

The following persons are mentioned in minutes of December 29, 1885, as being present and taking part in the meeting:

J. P. D. John, Greencastle.
A. W. Butler, Brookville.
O. P. Jenkins, Greencastle.
J. C. Branner, Bloomington.
S. P. Stoddard, M.D., Brookville.
*W. H. Ragan, Greencastle.
E. R. Quick, Brookville.
D. R. Moore, Brookville.
D. S. Jordan, Bloomington.
J. M. Coulter, Crawfordsville.
J. P. Naylor, Bloomington.
O. P. Hay, Irvington.
*P. S. Baker, Greencastle.
*Maurice Thompson, Crawfordsville.
J. B. Conner, Indianapolis.
*T. B. Redding, New Castle.
*Ryland T. Brown, Indianapolis.
*R. B. Warder, Lafayette.
J. T. Scovell, Terre Haute.

*Deceased.

The following persons' names appear on the treasurer's book for that meeting, and they were probably present:

D. W. Dennis, Richmond.
*Joseph Moore, Richmond.
Stanley Coulter, Terre Haute.
B. W. Evermann, Bloomington.
S. E. Meek, Bloomington.
C. H. Eigenmann, Bloomington.
*J. L. Campbell, Crawfordsville.
D. A. Owen, Franklin.
C. R. Dryer, Fort Wayne.
A. J. Phinney, M. D., Muncie.
C. A. Waldo, Terre Haute.
C. W. Hargitt, Moores Hill.
*W. P. Shannon, Greensburg.
*T. J. McAvoy, Indianapolis.
L. D. Waterman, M. D., Indianapolis.
John Hurty, M. D., Indianapolis.
F. M. Webster, Lafayette.
*F. Stein, Indianapolis.

*Deceased.

Of about forty persons in attendance upon the first meeting, twelve are present at this meeting.

GREETINGS FROM INDIANA ASSOCIATIONS.

FROM THE INDIANA STATE TEACHERS' ASSOCIATION.

By Geo. W. Benton.

Mr. President and Members of the Academy: In the absence of Dr. Robert J. Aley, State Superintendent, and president-elect of the Indiana State Teachers' Association, it has devolved upon me and is my great privilege as retiring president to extend to you the greetings of the teachers of the State, and to congratulate you upon the completion of the series of notable meetings culminating in this anniversary.

It is peculiarly fitting that we do this in view of the importance of each of these societies, and of the part which each has had, and is destined to continue to perform in the life history of the State of Indiana.

The State Teachers' Association last December passed its fifty-fifth milestone, and in its uninterrupted history of fifty-four years has marked the successive stages of educational progress in the State, and has had an increasing influence in establishing standards and in directing the current of educational thought. Many of its officers and members have become prominent in the educational work of the State and nation, and many of them have enjoyed the privilege and honor of membership and active participation in the affairs of the Academy.

No less prominent in its own sphere, through the years of its activity, we recognize the importance of the great work which the Academy has done for the State and for the nation, in the spreading of scientific knowledge, in the encouragement of research, and in the inspiration of the younger generation of science teachers to greater effort and increased efficiency. We see in the Academy the most powerful agency in the solution of the great problem of fitting the highest development of scientific thought into the general scheme of education for all the people; and we confidently look forward to the achievements of the coming years of the Academy, believing that its services to the State and to education will continue to receive that recognition which it has so richly deserved in the past, and which we now so inadequately express.

The teachers of Indiana would consider me lacking in truth and courtesy, I am sure, should I fail to give expression to the deep pleasure and pride which we feel in the great history of the Academy, and in the exceptional capacity for usefulness with which it is so richly and generously endowed.

We greet you, and we bid you Godspeed!

FROM THE INDIANA MEDICAL ASSOCIATION.

By Dr. S. E. Earp.

Mr. Chairman: It surely is a pleasure as well as an honor to be chosen to appear before you for the Indiana Medical Association. I am the custodian of the good-will and hearty congratulations of the Indiana State Medical Association, but it is not the casket that assumes any importance; it is the jewel I bring you. Now, when we extend our greetings we desire that they shall have a cultured application and not a provincial one. Perhaps an explanation is in order.

One of your splendid members, Prof. Stanley Coulter, recently delivered an address before The Young Physicians' Club, of which I am a member—and I am a member by virtue of the fact that all physicians are young, but some are younger than others—and Prof. Coulter in his address said that culture is an instinctive appreciation of the very best, and that provincialism is *narrowness*, the antithesis of culture. And that whenever he had come in contact with a provincialist he had two impulses, first to laugh at him, and second to kill him. He had done the first, but so far he had been able to control himself and not do the second.

So we appreciate that everything concerning you is the very best, and that it is the very best who can appreciate the best in you.

The Indiana State Medical Association has passed its golden anniversary by ten years. It has 2,690 members in good standing, and yet there are not more than twenty-five who are members of this splendid body of yours. There ought to be more. There are men who are authorities in their line who should through this channel help themselves, help the association, help you and help the public by contributing what they possess. We must learn that, taking all the departments of science, "in union there is strength."

Another important factor is this: For a time in scientific medicine, on account of the number of medical institutions in this State, the interests were varied; but during the past year, for the first time in forty years, there has not only been an amalgamation of scientific medical interests, but there is complete unity. And now with the medical college we have here that is under the control of one of your best Universities, Indiana University, with its opportunities and facilities, we take it you will soon hear us rapping at your door, and we trust that the latch-string will be out.

We fully appreciate, as we congratulate you and bring our greetings, all that you have done and are doing for scientific medicine, and that it is valuable beyond price. Again I say, as I bring you our greetings, that we congratulate you most heartily.

FROM THE INDIANA HISTORICAL SOCIETY.

By J. P. Dunn.

I have been delegated by the Indiana Historical Society to extend its greetings to the Academy of Science. This is therefore an historic greeting; just what a scientific greeting should be, I am not quite certain. In the good old days that Dr. Coulter told about, I should think the proper thing was, "Have something with me," but in the course of the great progress that has been made in the last twenty years in the Pure Food Department, I do not know whether that would be safe. I judge that scientific people believe all the awful revelations that have been made, and that they are all on the water-wagon now.

There is one thing in which I think this society and the other learned societies of the State should be at a unit. We have a centennial in 1916. There has been some talk of having an exposition, but everybody knows that the recent expositions have been failures, and it would be a failure in Indiana. But it has been suggested that instead of this we erect a permanent memorial building devoted primarily to the preservation of the history of Indiana. This is being done now through the State librarian and the State museum, but we have not room enough. Mr. Blatchley has not room enough to do his work, and I understand valuable gifts have had to be refused on account of lack of room in that museum. There is also scientific work being handled in the State House by Dr. Hurty and Dr. Barnard, and we really need a building of this sort. These things ought to have ample quarters. I would like to see that centennial of 1916 celebrated by an ample building in which a museum and library could be housed, in which there would be room for laboratories and other work of the State, room for the Academy of Science, room for the Historical Society and all these other bodies.

I trust you will take that matter into consideration as you go out from here. Keep it in mind, and when you talk to your Representatives and Senators and people who have influence in the Legislature, lay it before them, and thus help in a work which I believe is of very great importance to the State of Indiana, both scientifically and historically. (Applause.)

FROM THE INDIANA BRANCH OF THE AMERICAN CHEMICAL SOCIETY.

By Prof. R. B. Moore.

Mr. President: As representative of the Indiana branch of the American Chemical Society I extend congratulations to the Academy upon its twenty-fifth anniversary. It is needless to argue the use of such a society in the State. It does a work which none of the national societies can do, and it is needless also to state that this work has been done well. Congratulations are especially in order, to those men who founded the Academy and have borne the burden of the work since that time.

I am also glad to see that the social life of the society is receiving sufficient attention at this meeting. We have little opportunity to get together during the year; it is all the more important therefore that the social side of our meeting should be emphasized.

The Indiana branch of the American Chemical Society extends to you congratulations and greetings. (Applause.)

FROM THE STATE PHYSICS TEACHERS' ASSOCIATION.

By Prof. J. P. Naylor.

Mr. President and Members of the Academy: I stand in the rather unfortunate position of belonging to the Committee of Arrangements for this meeting, and also representing one of the other societies. But I assure you that I did not make the assignment. The fact is I was simply held responsible for the presentation of the greetings of the State Physics Teachers' Association and tried to get a good man who could present the greeting in better words than I, although not in better spirit, I am sure.

As I look around over the faces of those present I see many members of the Physics Teachers' Association who are also members of the Academy, and it may occur to someone to ask why the Physics Teachers' Association should exist at all. The work in any science is many sided, and there are some things that can be done in the Indiana Academy and some things that can not be done. We, the physics teachers of the State, need to get together and compare notes. We want to know what the other man is doing and how he does it. This sort of work can not well be done by the Academy, for its province is rather along the line of investigation, and besides its program is always crowded; therefore the State Physics Teachers' Association.

Our association is, however, a sort of offspring of the Academy, and we look to it as the mother society. And as good children we come back at this time with our congratulations and hearty greetings, and hope for the Academy that the next two and a half decades may be even better than the past has been. We do not come like the Orientals, wishing that her shadow may never grow less but that her bright light may be ever enlarged, and that she may go on to larger accomplishments in the future. I bring you greetings. (Applause.)

FROM INDIANA SOCIETY OF ENGINEERS.

By Chas. Brossmann.

Mr. President and Gentlemen: I feel that is is an honor to address your meeting, and am glad to speak a few words of greeting on behalf of the Indiana Engineering Society.

On your program I notice the names of more than one engineer and subjects relating to engineering work. I feel that the scientist and engineer need no introduction, for they have ever worked either together or in sequence for the betterment of man and civilization.

On the vital questions relating to the physical development of our vast industrial system the scientist has made the work of the engineer possible.

The first step belongs to your work. You took the initiative and advanced radical though perhaps unappreciated theories, labored for years to prove them, and had to work and keep the courage of your convictions to establish your point beyond question.

Your reward has not usually come from a grateful public, but you have the reward of a greater knowledge.

I wish to mention one or two papers on your program, one "A List of Algæ." A list of algæ means nothing to a community, but when an entire water system becomes clogged with Crenothrix, they cry for the scientist to find the remedy.

The subject, "The Problem of Sewage Disposal," does not appeal to a city until the stench is apparent, then succor from scientist and engineer is needed.

Most of the papers to be read, touch upon the betterment of the human race, the conservation of its health, and the country's resources.

Today Dr. Von Lendenfeld investigates the organs of flight of the best flyers of the insect orders, Lepidoptera, Hymenoptera and Diptera. The public hears and smiles.

Tomorrow the Wrights fly for hours in the upper air. The public sees and gasps in wonder and amazement.

And so the scientist needs be the silent man. Carlyle says: "The noble silent men—scattered here and there—each in his department—

silently thinking—silently working, whom no morning newspaper makes mention of—they are the salt of the earth. A country that has none, or few of them, is in a bad way."

I am glad we have many in this country, and that this State is so well represented in the "silent men"—although perhaps they will not be so silent in the ensuing two days.

Gentlemen, I am pleased and honored in extending to you the greeting and good wishes of a brother society which appreciates its debt to science. The Indiana Society of Engineers greets you and wishes you a successful meeting.

FROM THE INDIANA ASSOCIATION OF SCIENCE AND MATHEMATICS TEACHERS.

By W. W. Hart.

Mr. Chairman: As I have sat here listening to the expressions of greeting on this occasion, I had the great pleasure of hearing the other gentlemen say the things I expected to say.

I feel that it is especially proper that our society should join with the other organizations today in expressing their interest in your Academy. In some respects, while not a child of the Academy, as is the Physics Teachers' Association, yet we might call ourselves a younger brother. Our interests are somewhat similar. We are interested in the sciences and mathematics, and I think that on that account we can appreciate better than others, possibly, the feeling of need which led to the organization of the Indiana Academy of Science twenty-five years ago. We are all of us working in a field in which we must look for sympathy, for encouragement, for inspiration, not to the public at large, because they frequently misunderstand us, but to our colleagues and fellow-workers. That, as I understand it, was one of the reasons for the organization of this society.

Also many of our number are directly indebted to some of you for the instruction and inspiration that led them to take up their life work. And we are all indebted to you for the standing you have given to scientific pursuits in the country at large.

So I bring to you today the most hearty congratulations upon your past history, and upon the glorious achievements of some of your number, and say that we wish you abundant success for the future. (Applause.)

FROM THE INDIANA AUDUBON SOCIETY.

By William Watson Woollen.

Mr. President. Ladies and Gentlemen: The first Audubon Society was organized in New York in 1886. Its purpose was "the protection of American birds, not used for food, from destruction for mercantile purposes." In 1889 it seemed to have accomplished the purpose for which it was organized and the movement died out.

A subsequent revival of the demand for birds for millinery purposes led to a re-awakening of sentiment on the subject, and in January, 1896, a State Audubon Society was organized in Massachusetts and in October of the same year one was organized in Pennsylvania. Such societies now exist in all of the states, except perhaps a half dozen, the object of their organization being the preservation of our birds which were fast being exterminated. It was thought that the people must be educated as to the worth of our birds, and these societies entered upon that work.

In April, 1898, principally through the instrumentality of the Indiana Academy of Science, the Indiana Audubon Society was organized. I have in my office the minutes of that meeting, at which I was present. In looking over the minutes of that meeting I find the society was mainly constituted of members of this Association.

The work which has been accomplished by the Audubon Societies of the country has been immense. I am not sure that I know of any other organization which, with as little money, has accomplished so much good. Its work for good has been of such a character as to attract the attention of the people of the country and especially the Department of Agriculture at Washington. The annual reports of that department have taken account of these societies and commended them for the work which they have accomplished. You all must be aware of the legislation that has been brought about through the influence of these societies, and especially the Lacey act passed by Congress for the protection and preservation of our birds.

I am glad to say that there is not a State in the Union which does not have its laws for the protection of our birds. In this State we have not lagged. We have placed upon our statute books the ideal law for that purpose, originally suggested by the American Ornithological Association.

Now, ladies and gentlemen, allow me to suggest in conclusion, that the membership roll of our Audubon Society contains but few names of the members of this association. We have a membership of about one hundred and fifty. Our first annual meetings under the provision of the constitution were held at Indianapolis. We learned, to our regret, however, that in this great and beautiful city there were very few people who were interested in this work. We changed our constitution. Since then we have gone to Franklin, Richmond, Shelbyville, Fort Wayne, and New Castle, where we have been received most cordially, and we hope we have done good.

Now, we bring our greetings to you, with the hope for future success, and that you will renew your love for this, one of your offspring. I have looked over the program arranged for this the twenty-fifth annual meeting of the Academy with its sixty-nine numbers and the additional numbers which Mr. Butler has read, and I find there are but two numbers which have any reference to our birds. These are No. 45, "The Mocking Bird in Indiana," and No. 47, "Observations in Woodpeckers." Now, ladies and gentlemen, to my way of thinking, this is not as it should be. I believe these other things which you have been writing and talking about are important, but of all of them the one particular thing which is of the greatest interest and value is the preservation of our birds. Without them you will have no occasion to talk about botany or any of the other things about which you have been writing and talking. It is the birds of our country to which we must look for its salvation. I thank you for the opportunity to say a word for them.

PLANS FOR THE INDIANA ACADEMY OF SCIENCE.

John S. Wright: Mr. Chairman, the committee made arrangements for several persons to speak of the plans for the Academy, with reference to future expansion and development. Now, most of the points that I had in mind have been covered very adequately by the speeches which have been made at different times during the meetings, particularly last night by Dr. John M. Coulter with reference to the social side of the Academy, making the members better acquainted with each other. I feel sure that the program committee will endeavor in planning the next meeting to emphasize the social aspect more and more. Possibly a smoker will serve very well to this end. I do not suppose we would attempt a banquet of as large proportions as the one we had last night. as that would entail too much effort.

I believe the Academy will do well to enlist the interest of men who are in industrial lines. There are within this State at the present time a great many men who are in industrial lines. Mr. Brossmann, who represents the Engineering Society, referred particularly yesterday morning to the interest that engineers and chemists have in scientific work, and that their work rests upon developments along the lines of science. These men in industrial lines may properly be enlisted in the Academy interests, and I am certain we can thus enlarge the number of those engaging in the work of the Academy.

I believe that is all I care to say, because other features of the Academy work will be mentioned by those who follow.

Dr. Stanley Coulter: Mr. Chairman. I think we are a unit upon the matter of the development of the social side of the Academy. It has occurred to me that one way in which that might be brought about would be to have the Executive Committee constitute itself a committee of introduction at each session, and make it a regular part of the program to introduce the new members to the older ones. I have frequently told young men that the only way for them to broaden out was by coming in contact with these older members, and they have come to the Academy meetings, stayed a day or two and have gone home without meeting a single one of them. We should certainly have some committee that would see that the young members are properly introduced to those with longer years of service in the State.

Another matter which should be taken up by the Academy and the Executive Committee is the length of time taken to print our reports. A man who is doing a bit of scientific work which is worth publishing, the preparation of which involves much time and labor, must wait eleven months for its appearance if he presents it to the Academy. A paper that may be of value at the time of its presentation, may not be worth nearly so much after a year has elapsed. You can not be sure that the thing you say today is the thing you would say in the same form a year from now. I think the Executive Committee should take this matter up in some definite way, and see that the proceedings are ready for distribution in less than a year from the date of meeting.

Another thing, it seems to me, that we need is that our programs should not be made up as they are now, in a comparatively haphazard fashion. In the past we had some programs that were really capital, and those who had these programs in charge would begin, say, in March or April to send the various members letters, suggesting that it would be a good time to arrange in their minds the subject they would present to the Academy, and thus, long before the Academy meeting the Executive Committee had in hand a well organized program.

In conclusion, I suggest: a recognition of the social side, an improvement in the methods of getting out our reports, so that they may be received very much more promptly than heretofore, and a return to the old method of having the Executive Committee, made up of the President, Secretary and Program Committee, feel that a large part of their work must be done before summer vacation if the meeting is to be a success. The request for my subject, under the present practice, always comes at a time when I am busier than at any other time of the year. As a consequence I send in some title that sounds well, and does not take much preparation, and trust in the main to the inspiration of the moment.

I am thoroughly in accord with Mr. Wright's suggestion that this organization is losing a very great element of strength in not having associated with it more closely the industrial scientists of the State. (Applause.)

Dr. H. E. Barnard (Indianapolis): Mr. President and Members, I cannot but feel that it is presumption for me, whose name was enrolled in the Academy but yesterday, to attempt to give advice to you who were at Brookville, and who have guided the Academy from its infancy through youth to manhood. But if there is a word I can say this morning it is to the older members, whom I would urge to give of their wisdom and advice to these young men, not only in lectures, but in heart-to-heart talks and fraternity with them. I wish to express my thanks to Dr. Coulter for his admirable toast last evening. More fraternity and fewer scientific papers I believe will be the key-note of the future of the society. Some of the papers stand a poor chance of being appreciated, because they are not understood; but we all want to know the men who write the papers, not for the papers, but for themselves. One may gain quite as much inspiration in the company of the worker in other fields as in association with his fellows, not in their papers, but in the social hour.

So I would urge more and more this fraternity among the members, especially at the spring meetings. That first gathering at the swimming pool has taken hold of me; it gives us a glimpse of an esprit de corps that will carry the Academy far, and make an Academy that we will be as proud of twenty-five years hence as we are today. (Applause.)

President W. E. Stone: Mr. Chairman and Members of the Academy, perhaps what I have to suggest will not permit of practical application, and yet as I have been in attendance upon this meeeting I am impressed with this thought about the Academy. It has become in a sense a child of the State; it owes something to the State as an organization. It represents a body of men who certainly have a great deal of influence in shaping the future of the State. Now, it occurs to me that the particular thing which this Academy can do is in the direction of shaping public appreciation of scientific methods and scientific spirit. You can not formulate that policy for immediate action, but I submit to you if it would not be a very valuable thing if the public at large, the press, business men and public officials, had a better conception of what scientific methods and the scientific spirit stand for. How much we should be spared in the press of the sensational talk of scientific attainment; how much we should economize in the administration of the affairs of city and state; how much more it would mean to the private affairs of our citizens if there was a conception of the idea that knowledge is to be gained on all of the common affairs of life which put into practice would result in efficiency and in economy.

Now, that is a matter of slow growth—public education. We are striving to bring people to a conception of that idea in all of our schools and colleges, and here is a public body which should be recognized as having influence and standing and weight in this State. What better service could it render in the course of a quarter century than to have promulgated steadily that notion of appreciation of scientific methods and scientific spirit? It is worth more than papers. It is the ultimate object of this Academy. It is the highest service it can render the State as a matter of public welfare and public education.

Now, that is very intangible, I realize, but I think it is an end worth thinking about. (Applause.)

Dr. C. L. Mees: It appears to me that the remarks I had prepared upon being notified to speak have been stolen by those who have preceded me. It is an old Chinese saying that it is dangerous to stoop down even to fasten your shoe strings in your neighbor's melon patch. So there is very little left for me to say.

I certainly am thoroughly in accord with all that has been said this morning, and by Dr. Coulter last evening, but there are one or two practical things which come to my mind now. Dr. Coulter referred to the fact that we are in danger of dissipation. Owing to the fact that the number of scientific workers in special lines in Indiana has increased very greatly in the last two years, papers presented to the Academy have become more and more technical in narrow specialties and the number capable of discussing them or even following them as presented was necessarily small and interest correspondingly flagged. This condition led to the formation of half a dozen or more of scientific societies made up of men especially interested along narrow lines of scientific research, commanding the interest and attendance of those having common interest and drawing their attention and membership from the Academy. Now the question is whether the Academy cannot devise some plan by which the work of these various societies could be co-ordinated and perhaps their meetings be arranged to occur about the same time as the Academy meeting. If the program of the Academy meeting could be somewhat shortened and the papers be made of more general interest, they would serve the purpose, as Dr. Stone has just intimated, of developing the scientific spirit, and then let the different societies meet and discuss the technical papers they may have to offer. I merely offer this as a suggestion, and do not know whether it would be practical.

There is another suggestion which possibly might be worked out. The American Institute of Electrical Engineers has tried a somewhat similar plan, that is, to have scholars from the various colleges where more or less graduate work is being done, attend meetings, and thus give them an enthusiasm which contact alone will bring, and publish their papers, if worthy, and interest them in the work of the Academy later on.

These are some of the practical points that come to my mind in connection with the future plans of this Academy. I believe the danger is now that, unless the character of the activity of the society is somewhat changed, we will become a sort of body which exists upon paper and in lists of membership, rather than in active work.

Mr. W. A. Cogshall (Bloomington): I have been very much interested in the statements during the last two days of the early work of the Academy—its early organization and membership, and in the large number of suggestions that have been made for the future of the Academy. I think most of these are good. It only remains to adopt some definite plans by which these suggestions can be put into something tangible. I do not know whether such plans can be worked out in the immediate future or not.

It seems to me the aim of this Academy is first to encourage scientific work among a good many who without the Academy would not do any such work. It does that to a certain extent. We have every year a long list of papers from men who do not belong to other scientific societies, and it is a good thing for them and a good thing for the State at large that these papers should be prepared and printed.

The other aim of the Academy, and which I believe to be the main one, is the bringing together of the scientific men of the State—not necessarily to hear the papers, as was very well said last night. I do not know that I should put the papers in quite so insignificant a place as was indicated, but we could well have the program the real excuse for meeting, and make that the frame-work of the whole thing. But I think a good deal of the scientific benefit is lost or perhaps not realized, by having such a large number of papers of such short duration. To my mind it takes a man who is a good deal better than the average to prepare a paper of five or ten minutes in length, that has anything in it, and if that is all there is to the paper, I do not know that it is really worth while to read it. I believe the whole work of the Academy could be much better carried on if we did not try to crowd sixty or seventy papers into one short meeting.

With the great number of things that have come into life since this Academy was organized, it is not possible for us to give two or three days continuously to a meeting of this kind very often, and so we could not have sixty or seventy papers. But if we could have papers that are long enough to be beneficial, and put them into a shorter space of time, we could then devote more time to the social element of the meeting. I do not believe we get much social benefit from the meeting, as it only happens once a year. We come up here and meet a few men and go back home, and in the course of a few months we have forgotten who these men were and where they came from and what sort of work they are particularly interested in. I believe we should have meetings which would not be too scientific very much oftener than once a year, which would serve to bring the members of this Academy into closer touch with each other.

I would suggest that we have, if possible, some sort of Academy headquarters here in Indianapolis, and that once a month or once in two months, or once a quarter, as may seem advisable, notices be sent out to the members that there will be a meeting. Have not over one or two papers, that could be presented after a little dinner or lunch. I think this would be well worth while.

I was very much interested yesterday in the statements of the Librarian of the State, in regard to the new building that is proposed. If by any possibility that building could be obtained through appropriation from the Legislature, a permanent headquarters for the Indiana Academy could be secured, a most excellent place for carrying out some such idea. It would give us a place for our library, and it seems to me it would be a benefit to the Academy on every side. It would bring the whole scientific body of the State of Indiana together often enough to get acquainted and keep acquainted.

I believe that some sort of permanent headquarters, more frequent meetings and shorter meetings, would give us the best results in this State.

BANQUET.

Friday Evening, November 29, 1909.

David W. Dennis, Toastmaster.

SPEAKERS.

David Starr Jordan.
Alfred Springer.
Glenn Culbertson.
M. H. Stuart.
John M. Coulter.
George T. Moore.
W. A. Noyes.
Chas. W. Greene.
B. W. Evermann.

MEMBERS AND THEIR FRIENDS PRESENT.

Andrews, F. M.
Bangs, E. H.
Barnard, H. E., and wife.
Barnhill, Dr. J. F., and wife.
Bennett, L. F.
Benton, G. W.
Bigney, A. J.
Bitting, Dr. A. W.
Bitting, Mrs. Katherine Golden.
Blanchard, W. M.
Blatchley, W. S.
Bodine, D.
Brayton, Dr. A. W.
Breeze, F. J.
Bross, Ernest.
Brossman, C.
Brown, D. C.
Brown, Hilton U.
Bruner, H. L.
Burrage, S.
Butler, A. W., and wife.
Bruce, E. M.
Carmen, E. K., Miss.
Cogshall, W. A.
Coulter, J. M.
Coulter, Stanley.
Cox, W. C.
Cox, U. O.
Culbertson, Glenn.
Daniels, L. E.
Deam, C. C.
Dennis, D. W.
Dillan, Miss F. E.
Dryer, C. R.
Dunn, J. P.
Earp, Dr. S. E.
Eigenmann, C. H.
Enders, H. E.
Evans, P. N.
Evermann, B. W.
Felver, W. P.
Foley, A. L.
Francis, J. R.
Gabel, J. D.
Golden, M. J.
Gottlieb, F. W.
Greene, C. W.
Greene, F. C.
Hadley, A. N.
Hankinson, T. L.
Hart, W. W.
Hathaway, A. S.
Hofer, G. N.
Hole, A. D.
Hyde, Roscoe.
Johnson, A. G.

Johnson, S.
Jordan, D. S.
Kenyon, A. M.
Kern, F. D.
King, R. M.
McBeth, W. A.
McBride, R. W.
Mees, C. L.
Millis, W. A.
Moenkhaus, W. J.
Montgomery, H. T.
Moore, G. T.
Moore, R. B.
Morrison, E.
Mowrer, F. K.
Noe, Fletcher M.
Noyes, W. A.
Pohlman, A. G.
Potter, Dr. Theodore.
Ransom, J. H.
Rettger, L. J.
Smith, E. R.
Springer, Dr. A.
Stoddard, Dr. S. P.
Stoltz, Charles.
Stoltz, Charles, Jr.
Stone, W. E.
Stuart, M. H.
Swift, L. B.
Taylor, F. B.
Thomas, M. B.
Thompson, Willis S.
Transeau, E. N.
Turner, W. P.
Van Gorder, W. B.
Waterman, Dr. L. D.
Weems, M. L.
Williamson, E. B.
Woodhams, John W.
Woollen, W. W.
Wright, John S., and wife.
Young, J. P.
Zimmer, H. E.

Dr. A. L. Foley: It seems to me the Program Committee has shown particularly good judgment in the program it has provided, and in no way has that good judgment been better shown than in the selection of the Toastmaster for this evening.

There is no man in Indiana who has had more influence upon the teachers of the State, upon the schools of the State; there is no man who has been closer to the hearts of his pupils. There is no man who has had more to do with the development of science in Indiana than has Professor David W. Dennis, of Earlham College, who will preside. (Applause.)

Prof. David W. Dennis: I am sure, ladies and gentlemen, that I wish more than any of you possibly can that all of that was true.

In science we have many of us been very lately instructed by an eminent Hoosier that nothing at all is settled, and I came to the conclusion this morning when recapitulation went overboard that perhaps it is so. But the records of the Indiana Academy of Science would furnish many exceptions to this rule. During these twenty-five years we have been settling a considerable number of questions; some of these have been settled so effectually that they have never come up again. For instance, many years ago—so many that I have forgotten the exact date—Dr. Jordan presented a discussion on "Fishing all the way from the Amazon to Greenland," and he said that the number of vertebræ in the fishes of the same species always increases with the latitude in which the fish is caught. He suggested that he knew no reason for it unless perhaps it is that life expresses itself in more vigorous terms at the pole than at the equator. But Prof. T. C. Mendenhall offered a theory that was received with much applause, and that everyone thought was right. He said the North always had more backbone than the South, anyway. (Laughter). So that is one question we have settled.

I remember also that twenty-four years ago our botanist presented to us what he was pleased to call a very important question. Several others have been presented that were more or less important, but this was really important, and it was, in general terms, the development of life from the plasmodium to the oak. He referred to the fact that mushrooms—I tried to get his exact words, but we did not publish in those days, so this is as I remember it—that mushrooms "are degenerates, mere driftwood cast up by the waves of life's ocean." Incidentally this idea was illustrated by another journey parallel to it, from the Amœba by way of the ascidian to man. In the discussion which followed, our zoölogist arose and said the ascidians "are degenerates, mere driftwood cast up by the waves of life's ocean;" so the status of the mushroom and the ascidian was settled.

We really took up some serious questions. I remember that Professor Waldo in a wide discussion of mathematical questions, had a good deal to say about parabolas, hyperbolas, asymtotes and other similar things; Professor Neff then followed with a paper dealing with the refinements of organic chemistry, which he illustrated with what appeared to be colored chalk; all of us were lost some of the time and some of us were lost all the time for some hours. This was followed by a glowing vision of creation from a Darwinian standpoint. It was an interesting occasion; we all understood and took on a benevolent expression. But the many things we used to teach that are discarded now were useful in their day. Carlyle says somewhere that the present time is "child and heir of all the past and parent of all the future," and I could not help thinking this morning when Prof. Coulter was talking, that as one after another these theories have been set aside, there has been a reason for the existence of each one, and it has called into existence something that is better than it was itself. Our criticism is constructive.

I believe scientific men—or at least if you will make it a little broader than that, the school-master today is the priest of today; and he is going to be the priest of the future. There were some questions submitted to the children of the schools in one of our cities; one of them was, "Where is Heaven?" In the answers one of the pupils (it was a girl, so there could not have been any malice in it) said that Heaven was said to be above the clouds, but she added that physical geography teaches that the atmosphere is only about forty-five miles high, and that even a very few miles up it is probably not possible for anybody to live, so Heaven could not be there at all. Whatever that child may have thought that was wrong or inadequate about Heaven, it is clear that she believed the things her teacher had taught her about the air. He, instead of her minister—if she had one—was her priest.

I happened to be present at the inception of this Society after Amos Butler brought it to us, and of course it would be very easy to continue these reminiscences; but that is not what the committee asked me to do, and I do not intend to do it. But this Society has been a great help to me and to all of us, not only in its meetings, but in the rambles we have had over all parts of Indiana in our Spring meetings. We went out to Fort Quiatanon and hunted beads the Indians had lost at the old trading post and were as happy when we found one as the Indians were sorry when they lost it; we have gone over the whole State getting acquainted with whatever of interest it had to offer. Even at the very first meeting down at Brookville, the home of the Academy, we went swimming, and naturally got acquainted with ourselves; saw ourselves in a sense in which others did not very often see us. (Laughter). These social occasions have been the best part to me, after all is said, of the meetings of the Academy from the beginning until now.

I have the pleasant and easy task of introducing first a man who needs no presentation to scientific men anywhere; a man who needs no title, but whose titles are so numerous that there would not be time to read them. He is an investigator and a teacher, was for a time the premier of Indiana teachers. He is an author to whom science owes much and man owes more; the man for whom the river Jordan was named. (Laughter). Dr. David Starr Jordan, President of Leland Stanford University. (Applause.)

Dr. David Starr Jordan: Mr. Toastmaster and President, Members of the Academy, Ladies and Gentlemen: It is a pretty hard thing to respond, impromptu, to all that. I only hope there is some of it that is not true. It is a very great pleasure to me to get back here, and yet that pleasure is not unmixed with a certain kind of pain. I was just remarking to Dr. Coulter that in the "fierce democracy" of this Indiana Academy "there was a Brutus once who would have brooked the eternal devil to take his seat in Rome" as easily as he would have sat for dinner in a dress-suit. But to see this "fierce democracy" in the brook at Brookville—it gives me a certain sense of pain. (Laughter.) And speaking of Brutus calls to my mind Marc Anthony, and I remember an occasion when a gentleman was called upon to speak, and he had only one speech which he said over and over, and just before going in he asked if anyone could give him the address of Marc Anthony. A friend said, "You know Anthony's style of life and the people he associated with; I should think his address would be at the same old place." (Laughter.)

I saw a statement not long ago by Henry Fairfield Osborn, that he did not think it possible for an American University to produce a Darwin, and the reasons he gave were that first, he—that is, the student nowadays—did not have to contend in his early life with something that was distasteful to him, as Darwin did; second, scientific men do not have the appreciation here that scientific men do in England; and third, that the scientific men of this country do not have the leisure to become such as Darwin was. It does not seem to me that these reasons are very good. I do not think, perhaps Darwin did not think, that any appreciable part of his greatness was due to the work in the University which he said was incredibly dull, and which led him to feel that he would never read a book on a certain subject afterwards. And as for appreciation in this country, you have just heard how scientific men are appreciated in Indiana, and it is even so everywhere we go. And so we have this kind of treatment, in America, whereas Darwin was named "gas" by his fellow-students, because he confined himself more or less to chemical experiments. And as for leisure, I know a great many scientific men of leisure who have never made any pretense to being Darwins on that account. It seems to me that Darwin was first made by heredity. There will never be another; you cannot get a man of high scientific rank and quality unless heredity starts the thing. You have to get the right kind of stock. There is no reason why the right kind of stock should not be found in Indiana, for there is such an amount of genius in this State that it spills over into all the other States. California is full of it that has been borrowed from Indiana, and so with the other States. The first thing, then, is heredity. The second thing is to be "up against it." We read in history that Darwin went to see horse races and watched them very closely; that he was interested in the beetles of England and gathered beetles in season and out of season. In other words, with all the scientific training a student gets he should be brought right up against nature; against the things that do not lie if you listen to what they have to say. Then the third thing. We read in the various historical sketches of Darwin that he "walked with Henslow," a man with enthusiasm, and this enthusiasm was passed from the teacher to him. I take it, then, that the making of a great man of science rests on these three things, and I do not think the other things have anything to do with it. I notice a man will do just as much when he has not any time, as he will when he has all the time there is.

Now, I think we have these elements to a greater or less extent in our modern Universities. Of course, heredity is not included, but the second element, that of coming up against it, is more or less within the power of every institution now. There was a time when institutions prided themselves that they did not let the students come up against any scientific knowledge. There was a time when the University teacher—an A. B. —was more interested in the song of the oriole than the students in his classes. But the Universities have recognized that defect. Now, the third element, "walking with Henslow." Jacques Loeb, of the University of Chicago, told me awhile ago that he received a very enthusiastic letter from a young man who said he wanted above all things to study the origin of life, and that he wanted above all things to study under Loeb and enjoy his fellowship. Then Loeb wrote back that, unfortunately, he had decided to go to California, and the young man wrote back: "Will you kindly turn my letter over to your successor?"

Now, to a large degree, young men are training themselves wrong. Instead of "walking with Henslow," they are going where they are hired for $200 to $500 a year. They are a bar to scientific research, for what professor can teach his students to do a thing which he cannot do himself? You may remember in the last number of the Atlantic Monthly, an article by Professor Showerman of the University of Wisconsin. The professor had worked for some time on the prefixes in P, of Plautus, he was then working on the suffixes in S, of Seneca, to be followed by the terminations in T of Terence. The point I want to get at is that this is not advanced work, and the student will not gain enthusiasm. I do not think we ought to mistake for advanced study this very elemental work, the things that are of no consequence, and just so far as we allow our young men to do this elementary work, so far will we find them going out as teachers without enthusiasm, and saying that it is impossible in this country ever to see another Darwin. (Applause.)

Mr. Dennis: The next speaker is a member of the Academy, and has been for eighteen years. He came to us from the neighboring State of Ohio, and we expect him this evening to bring the greetings of his native State to the Academy. He is the gentleman who in his earlier scientific career invented the torsion balance. At the present time his specialty is fermentation.

Dr. Alfred Springer, of Cincinnati.

Dr. Alfred Springer: Mr. Toastmaster, Ladies and Gentlemen: It certainly affords me great pleasure to be here with you this evening, and no little gratification to be permitted to address a body of men, many of whom have carved their names deeply in the records of scientific achievement. The achievements of those of you who have remained at home have become household words, and the fame of those who have left the State to spread such brightness as only science can convey, has loomed up conspicuously among many brilliant lights. Twenty years ago the American Association for the Advancement of Science, in looking over its list of eligible candidates, selected from your members T. C. Mendenhall as the man worthy to represent it as President. Chairmen for the various sections of the American Association have frequently been selected from the Indiana Academy on account of the good work they have done. As for the General Secretary of the American Association, where could a better and more popular one be found than in our own Amos. W. Butler? He graced that position in 1892, and ornithologically speaking, he was a "bird." (Laughter.) This year the American Association for the Advancement of Science has honored itself in selecting one of your past presidents for its President. No one who knows Dr. David Starr Jordan doubts but that he will add additional lustre to its already bright pages.

Permit me, as a delegate from the Cincinnati Section of the American Chemical Society, to congratulate you on the twenty-five years of your existence, and to bespeak for the future, if such a thing be possible, greater success than in the past. (Applause.)

Professor Dennis: The program committee wished a man to speak for the small college, and it has asked Professor Culbertson to do this. He was President of this Academy last year, and it is a fact that he has been a member of the Indiana State Legislature. I cannot understand how it came to pass, but will leave that for him to explain—it is true. If he occupies six minutes' time, he has obtained for us through the Legislature $100 a minute every year for all of that time, and I think he will be entitled to at least that much. Prof. Glenn Culbertson, of Hanover College.

Professor Glenn Culbertson: Mr. Toastmaster, Ladies and Gentlemen: I shall not attempt to explain how I came to the Legislature. I enjoyed the experience very much, but I do not know that I shall care to go through it again, so you had better be looking up another candidate if you want the appropriation continued two years longer. I was very much pleased to hear the expression this morning, but there really was not very much difficulty in getting the appropriation. And I want to say this in regard to that appropriation, that I did not do anything that was against my conscience in attempting to get it. If I had not felt that there were good papers presented to this Academy every year that ought to be published in its report, I should not have worked for this $600 additional appropriation.

My subject is "The small college in its relation to the Academy of Science." I think by going back twenty-five years in the history of the Indiana Academy of Science, every college in the State would come in that class. Since then, of course, some of them have moved forward into a higher class. I have been a member of the Academy for some fifteen or sixteen years, and it has been a great pleasure to come up here year after year to hear the papers read and the discussions entered into. They certainly have been an inspiration to me, and I take it they have to every man in a small institution in Indiana. We are spread out over a considerable territory, and we have a great deal of work to do. Dr. Jordan says that the more work a man has to do the more he will do, but it is true that if we have a great deal of work along different lines we do not have time to put in special work in preparing such papers as we have heard here year after year; nevertheless we have all done our part. Of course, we of the smaller colleges rather envy a good many of the teachers in larger institutions because of their ability and opportunity to pursue their work along certain lines, but there are compensations. We get a broader grasp of things in a certain way, and we have certain relationships that are very pleasant to us. I will admit that with some of the papers, all I can do is to look wise, but I have received a great benefit from a good many of them, and have gone back home resolved to understand more fully these things that are brought to our notice.

So far as the work of the small institutions of the State is concerned, you have only to look at the program to see that the small institutions have done their share in producing the scientific men that have been an honor to Indiana. We are very proud of them today.

I want to thank you for listening to the words I have spoken, but I think you can listen to better advantage to those who are to follow me.

Professor Dennis: Mr. Milo H. Stuart, of the Manual Training High School, has been requested by the committee to speak on the subject of High Schools. He was principal of the High School at St. Paul before coming here, and is certainly as well qualified to speak from that standpoint as any member of the Academy.

Professor Milo H. Stuart: Mr. Toastmaster, Ladies and Gentlemen: It is easy to see, in the splendid addresses to which we have been listening, why the Academy has endeared itself to the people of Indiana. I would be pleased to add other reminiscences if I could do so, but I am too late a recruit to make any contribution along that line.

Coming from the High School field, I naturally think of the work of the Academy from that standpoint. As we have heard these inspiring addresses today I have been thinking how fine it would be if every science teacher of the State of Indiana could have been induced to come to this fount of inspiration. I believe he would have gone back to his classes with fresh ardor.

We all remember when we left our Universities and got into original work, how great a pleasure it was to feel that we had contributed just a little to the volume of knowledge. The compensation that comes from that kind of labor is certainly very great, and it seems to me if the teachers of the State could come into touch with the people who are doing it, they would feel their load a great deal lighter. I know they would take back to their boys and girls inspiration that would fast make scientists out of them.

This Academy of Science marks its twenty-fifth milestone today, and its face is set toward the golden anniversary. I am reminded of the story of the Irishman who said he wished he knew just the spot where he would die. His brother asked him what he wanted to know that for, and he said if he knew the exact spot, he would spend the rest of his life keeping away from it. So I think the Indiana Academy of Science, through some of its officials, must have discovered the spot where it might die, and started in the opposite direction, and we are twenty-five years removed from that place tonight.

That leads me (with apologies to Tennyson) to conclude by saying, that

> Scientists may come and scientists may go,
> But the Academy goes on forever.

(Applause.)

Professor Dennis: Every word I said in introducing Dr. Jordan is true of the next speaker; every teacher in the state would forgive me for saying that after Dr. Jordan left us he became our premier. There was, however, one difference. Dr. Jordan, as President of the State University, had for his rule a motto "Die Luft der Freiheit weht."

The students hardly knew what this meant but finally concluded it was "No smoking in the buildings." Prof. Coulter succeeded Jordan and the first day he smoked in the office. (He sometimes smoked in those days.) The students made a bonfire of their best hats:—they had had but one rule and now they had none. Prof. John M. Coulter, of the University of Chicago.

Dr. John M. Coulter: Mr. Toastmaster and Friends: All these ancient and new members of the Academy, who have spoken, have about exhausted the subjects, and I hardly know where to find myself. One thing I had in mind when Dr. Jordan was suggesting that heredity perhaps determined in the first place whether a man was going to do anything or not, and that things that followed were more or less auxiliary. I remember to have heard Dr. Wiley some years ago raise the question why there were so many scientific men in this State as well as men who had achieved more or less distinction in other callings. He answered it then to his own satisfaction. I have never seen it tested, but he concluded that the men in Indiana who had made their mark in science or in any of the other professions were the men whose early life had been spent in the most forbidding parts of the State from an agricultural point of view, and that there was nothing to become interested in except education. Just how many scientific men were lined up in this roll-call, I do not know, but when this State is unable to produce anything else, it can produce distinguished men.

I suppose a charter member is expected to be more or less reminiscent. and there are two or three things that the other speakers have left unmentioned.

In its early days, twenty-five years ago, this Academy meant a great deal to those who were members, and for two or three reasons. I think Dr. Jordan and Amos Butler, for example, will bear me out in this. In the first place this State science was comparatively new; it was new to us, new to the State, and new to the country. We came together as a set of young men who were interested in a new thing with a sort of fine enthusiasm with respect to the unknown that is found everywhere. In the next place, the instruction in science, with which all of us were more or less concerned, was just as new. It was even newer, because in those days the position of science in the colleges we represented was more or less doubtful and some of the things we taught were often looked at askance. The whole situation in the matter of scientific instruction was in its very beginnings. This also gave us a fine enthusiasm, a sort of feeling of comradeship in a campaign. We felt the need of companionship, and we found it in the Academy. We would come here from our various colleges, full of enthusiasm, and talk over the problems, and this formed a nucleus of sentiment, an esprit du corps that first developed among us, and which has since developed and given to the Academy the place it now occupies in the State. I think perhaps a feature that sustained us, and that made as much for the solidarity of this Academy as any other, was that one of our first campaigns in the State was educational. Science was fighting for its life, for a place in the colleges. There was another association that met at the same time in Indianapolis, known as the "College Association," and one of the functions of the Academy was to lay plans to assault that "College Association." I remember distinctly one of the things we had to combat. There was a tendency to antagonize the intellectual tastes of the students in those days, and one of the old professors said he thought that the very thing a student needed was the thing he disliked the most. If he disliked mathematics, *make* him take it; if he disliked Greek, *make* him take it. That was one of the educational slogans at that day,—every student needs what he dislikes. I have an idea that no one thing could have brought us closer together in our community of interest than the discussion of these educational questions.

But today you are threatened by a danger that we did not encounter. Every interest brought us together; every impulse was to come here to meet friends and associates. Now the tendency is rather the other way. We are becoming more and more independent; we are becoming more and more narrow; and we are in greater danger of working apart than ever before in our history. Many fine men are growing up who have the very smallest amount of interest in anything that is going on outside of their own field, and as a consequence there is a tendency to segregation which I feel to be a thing that must be combated.

There are two dangers I wish to call to your attention, two dangers that reunions of this kind will help to correct. One of these is the matter of personality, the kind of personality that can only be developed in contact with men, that cannot be developed in connection with one's own theories and one's own way of looking at things. It is the kind of personality that influences men and is sympathetic with them, and can only be obtained by knowing men, thus gaining a very much wider range than is possible within the limits of one's own field. It seems to me that is one of the striking features that ought to be thought of in connection with this Academy. Frankly, I think that papers are relatively very unimportant things. I never saw very much inspiration in papers. The inspiration comes from association with men, and that is the thing to cultivate—this opportunity to associate one with another.

The other thing we are in danger of losing sight of, and which this Academy can correct, is the tendency to become narrow in our vision and lose our perspective of the whole general field, not only of science but also of education. You will find that as scientific men become less and less interested in other fields of work, as they grind their own grooves deeper and deeper, they become less and less effective as teachers and less and less influential with their students. You will find men with broad outlook, clear and wide vision, men with sympathy—and men can only get these things by coming in contact with larger fields than their own—are the men who win with students.

These two things we want in these days, men with sympathetic personality, with a broad view over science in general, with an appreciation of the work of others, and with larger view of education as well. I hear that the art of teaching is disappearing. It seems to me that the fine enthusiasm which a teacher must impart to his student, is in danger of disappearing from our scientific laboratories, which are too much in danger of becoming mere factories.

Your number is so small that you can really know one another and can know the work that is being done by one another, and that is just the sort of thing you need. You do not need to come here for training in science; the Academy is no place for training, it is for association and personal inspiration. (Applause.)

Professor Dennis: Ladies and Gentlemen: Some years ago "plankton" got into the reservoir of our waterworks at Richmond, and we were a unit that we could not get along with it there any longer, and when we set out to procure a remedy we found that such a remedy had been worked out by a member of this Academy, and this man is the one I will now call on to speak. He is a graduate of Wabash College. He is the inventor of a means of culture for the nitrifying bacteria of the soil, which invention he did not patent, but gave to the American people. This puts us all under obligations to him.

Mr. George T. Moore, of the Botanical Gardens of St. Louis. (Applause.)

In his response Mr. Moore called attention in a humorous way, to some of the advantages of scientific knowledge, and in conclusion presented the greetings and best wishes of the St. Louis Academy of Science and the Missouri Botanical Garden.

Professor Dennis: A number of telegrams and letters have been received since the adjournment, and I will ask Prof. Butler to read them now.

(The letter of Dr. Wiley is appended as it was the basis for action in the closing session on Saturday morning).

Washington, D. C., Nov. 22, 1909.

Mr. A. W. Butler, Indianapolis, Indiana.

Dear Mr. Butler—I have received from you and other members of the Academy of Science, cordial invitations to be present at the 25th anniversary meeting, November 25th-27th, 1909. Should I consult my personal desires I would surely accept the invitation. Just at this time, however, two extremely important cases are in preparation for trial before the United States courts, (1) the use of borax in foods, and (2) the use of peroxides of nitrogen in bleached flour. I am compelled to give every moment of my time to the preparation for these cases, the first one of which will be called in the federal court in Peoria on the 8th of December. I therefore am constrained by reason of these public duties to decline the invitation to be present at the meeting of the Academy of Science. I want to say, however, just one word to the members of the Academy, and that is a word of congratulation on the work which has been accomplished by the Indiana Academy of Science in the quarter of a century which has passed.

I do not believe that any state association in the country of a similar character has accomplished so much, nor has brought together a band of men more devoted to research, more single in purpose and more enthusiastic in the pursuit of scientific truth. Many of the members of the Association have from time to time gone out into other parts of the country to pursue their work in other States. Not one of them, I believe, has lost his love for the Academy nor parted with his devotion to its cause and welfare.

I have been reading lately some of the early history of Indiana in its political and literary development. I should like to suggest that some member of the Society, before the data are scattered and while it is still possible to derive from the mouths of living witnesses important facts, should write the history of early scientific education in Indiana, beginning with the work of the Owens at New Harmony, almost a hundred years ago, and bringing it up to the era of the establishment of the new science, say about to 1875, or 1880. To write the work of scientific research of Indiana in the last twenty-five years would be too much of an undertaking for any one man, but the greatest interest would attach to a history of the scientific development of Indiana from the time of its beginning, or a little after, up to the date which I have mentioned above. I feel sure that there are enthusiastic and industrious members of the Society who would undertake to do this, either by collaboration or by helping some one who would voluntarily assume the burden of the work. Scientific men of Indiana whose experience goes back of 1875 might contribute personal recollections of scientific development which would prove of intense interest. The scientific work of the early colleges of Indiana is worth the most careful study and would make interesting chapters in the history of those days when the study of science was not considered to be a requisite for a liberal education as it is at the present time. The story of the work of such men as R. T. Brown, E. T. Cox, Dr. Levette, John Coburn, and others of that class would make most interesting contributions to a work of this description. At the present time when there is so much interest in the early political and literary history of the State it seems to me the scientific history should not be neglected.

I had hoped to present and read some paper of a scientific character at the meeting, but as this is not to be, I should like to present in lieu thereof this suggestion, which I hope will be given due consideration, because if it can be carried out it will be historical as well as a scientific work which will prove of immeasurable interest in the near future, if not at the present time.

Let me close with the hope that this meeting may be all its promoters have intended it should be—a feast not only of science but of friendship—that it may result in the stronger cementation of the bonds which hold the love of the loyal Hoosiers firmly to the State, and excite a pride in the scientific work of Indiana which may rival that which so justly exists respecting its literary accomplishments.

Sincerely,

H. W. Wiley.

Professor Dennis: The Committee wishes to honor many more members of the Academy by asking them to speak to you this evening, but on account of the lateness of the hour we will have to restrict the number. I will now call on our old comrade, Prof. W. A. Noyes, of the University of Illinois.

Professor W. A. Noyes: Mr. Toastmaster, Ladies and Gentlemen: I have been resting very quietly and easily all the evening, not seeing my name on the program, and not having the slightest hint that I would be called upon. It is surely a very great pleasure to be here, and I would like to say just a word about the old times when the Academy started. I believe I was one of the charter members, and one of the things I remember of that time was the discussion in regard to the name that we should adopt. It was finally agreed, if I remember correctly on the recommendation of Dr. Jordan, that we should call it the Indiana Academy of Science, not the Indiana Academy of Sciences. I think that in his mind and in ours, as we selected that name, was the thought that after all there should be but one science, which is all-embracing, and I feel that as one of the ideals of the Academy it has been of the greatest value to us. As we come together in these meetings of the Indiana Academy, we feel that no matter how separated our lines of work may be, how different—so different sometimes that we can understand but little of each other's language—yet after all we are simply working in different parts of one great whole of scientific knowledge, and that it is our place to look at our part, our field, as merely one part of the whole, all parts of which may in some way or other touch our own. And this opportunity of seeing, of catching even a little glimpse of this work that is so far removed, perhaps, from our own, and the acquaintance of these men who are working in the different fields, is, it seems to me, one of the features of greatest value in these friendships and associations which we have made here in this Academy.

Professor Dennis: We shall now hear from Professor Charles W. Greene, of the Uinversity of Missouri.

Prof. Charles W. Greene: Mr. Toastmaster: It seems rather unfortunate that a man such as I, of no ability as a speaker, should be called upon, but I will do the best I can to express the feeling of enthusiasm and encouragement this meeting has given me. It has been a great pleasure to meet so many friends and to recall old times when the Academy first began, the time when at DePauw, through the genial personality of Professor Jenkins, we began to catch the scientific spirit. I remember my first meeting with the Academy was at Greencastle. We went out on a field excursion and we younger men were brought into intimate contact with the stimulating personal enthusiasm which always characterizes Indiana scientists.

I think one of the features of this meeting has been the showing of the great tolerance that has been developed in our scientific lines of thought. Dr. Coulter showed us that this morning. It is certainly very encouraging to the physiologist to learn that in the life of the plant, in its growth from the plasmodium, it is not predestined to go through any fixed and inflexible schedule of development. I felt at the time that probably the calm cold conservatism of morphology was yielding to the seductive charms of physiology as expressed in environment, that a new era in botany was still possiblie to us. That was not the old botany but a glimpse of the new.

Professor Dennis: Dr. Evermann for a long time a member of the Academy is with us and will tell us what members of the Academy are doing in Washington. He represents the Atlantic here as Dr. Jordan the Pacific. He gave us last night an account of a fishing trip to the "Tiptop of the United States" but he did not produce his "records or his instruments" or even his fishes; he gave us only fish stories. Perhaps he has the real article with him this evening. Dr. Barton Warren Evermann of the U. S. Fish Commission.

Dr. Barton W. Evermann: Mr. Toastmaster and Amos Butler—or the Indiana Academy—they mean the same thing. I have been looking at this program ever since I came into the room, and I notice what my friend, Dr. Coulter, also noticed, and mentioned in his remarks—the toast immediately following my name, which I fear bears some relation to what I have already said or what I may say in this meeting. "Lord, Lord, how this world is given to lying!" But I am glad Dr. Coulter noticed this and put in a disclaimer, thus relieving me to some extent of the suspicion that my fish stories were the only ones in mind.

I would like to say a word regarding those of the Indiana Academy who are now in Washington, and to tell you something of what they are doing. I noticed, perhaps you noticed, in a recent magazine, a long article on "The Plunderers of Washington." There were a dozen or more of them, and I am glad to say to you that there was not among these plunderers who were pictured in this article, any Washington member of the Indiana Academy. We all escaped that distinction at least! I think I can also say that no member of the Indiana Academy in Washington has been seriously involved in the Cook-Peary controversy. We have kept clear of that, also. If there is anything the Indianian learned long ago, it is to take care of himself and not to get into embarrassing situations needlessly. So in this case the members of the Indiana Academy have read the very interesting article by George Kennan in the Outlook which proved very conclusively that Dr. Cook did not have more than one-tenth of the pemmican necessary to enable him and his dogs to reach the North Pole. They took that for what it was worth, and waited for something further. Then in another magazine some man from the West had the whole thing figured out, showing that Kennan had Cook's dogs continuing to eat pemmican at the rate of a pound a day even after they were dead and the Indiana Academy people in Washington hope Kennan may be able to explain why and how they did such an unusual thing.

Several of your friends in Washington are engaged in very interesting work which has an important bearing upon matters in this State. Our good friend, Dr. Wiley, the most distinguished Washington member from this State, is still continuing his pure food work and trying to answer the question "What is whisky?" Dr. Hay, a former President of the Academy, and now in Washington, is trying to determine, no doubt for the benefit of the Academy, the age of the Ceratops beds in Wyoming, Idaho and Montana.

One matter that I think will be of some interest to you here in the Mississippi Valley, is that the Bureau of Fisheries is establishing a biological station at Fairport, Iowa, in the interest of pearls and the pearl button industry, a matter which will appeal to the ladies. There was established some few years ago a small button factory at Davenport. A

German came over and saw the great numbers of mussels in the Mississippi River, and thought they might make good buttons. He began experimenting and soon demonstrated that they were well adapted to this purpose, and now more than fifty thousand tons of these fresh-water mussels are used annually. This is a greater quantity than natural production can supply. The supply, of course, cannot keep up. Fifty thousand tons a year will soon use up the supply. The Bureau of Fisheries realized the possibility of an early depletion of the supply of shells and arranged with Professors Lefevre and Curtis of the University of Missouri to experiment and see if they could not develop a method for the artificial propagation of fresh-water mussels; and they have succeeded, so that the propagation of fresh-water mussels will soon be an easy proposition. Congress made an appropriation for a biological station in which these experiments may be carried forward. We have acquired sixty-five acres of land at Fairport, and the construction work is now going on at that place. It is the ambition of those who are particularly interested in that station to see there a station which will appeal to every biologist in the Mississippi basin. We want to make it a fresh-water biological station where any biologist of the Mississippi Valley or elsewhere may go and find the facilities and material for the study of any fresh-water biological problem in which he is interested; and the Bureau of Fisheries not only hopes you may avail yourselves of the advantage of the station when completed but most cordially invites you to do so.

Again on behalf of the Washington contingent I extend greetings to the Indiana Academy of Science. I thank you.

Professor Dennis: I hope you will permit me to take another minute. Reference has been made again and again to the large number of splendid men who have gone out from this Academy. It would be equally proper to refer to the large number of valuable men who have come into the Academy. Reference was made this morning by Mr. William Watson Woollen to the fact that the Audubon Society was an offspring of this Academy. I am sure the mother of that Society was necessity, and the father of that Society as well as of this was Amos Butler. I ask now that the Academy stand, and drink the health, in cold water, of Amos Butler, the father of the Indiana Academy of Science. (Applause.)

MINUTES OF THE TWENTY-FIFTH ANNUAL MEETING

Indiana Academy of Science

CLAYPOOL HOTEL, INDIANAPOLIS, INDIANA,
Nov. 25, 26, 27, 1909.

Friday Morning, November 26, 1909.

Meeting called to order by the President, Dr. A. L. Foley.

Reading of the minutes dispensed with.

Dr. Foley: We will now have the minutes of the Executive session of last evening.

Assistant Secretary Bigney: The Indiana Academy of Science met in the Claypool Hotel at four p. m., November 25th. Eleven members were present and several visiting members of the Academy.

Members present were: A. L. Foley, President; J. H. Ransom, Secretary; A. J. Bigney, Assistant Secretary; Robert Hassler; John S. Wright; Carl L. Mees; W. S. Blatchley; M. B. Thomas; C. H. Eigenmann; A. W. Butler; D. S. Jordan.

A. L. Foley, President of the Academy, in the chair.

The report of the Committee on the 25th meeting, by A. W. Butler, as printed on program, with several additional papers, was read.

G. W. Benton, J. S. Wright and J. W. Woodhams reported that all plans for the banquet had been made.

Membership Committee made no report. Report of State Library Committee was made by J. S. Wright. He stated that the Proceedings of the Academy were being cared for in good order and that many volumes had been bound.

No report from Committee on Weeds and Diseases.

No report from Directors of Biological Survey.

No report from Committee on Relations to the State.

Committee on Distribution of Proceedings reported through J. S. Wright. All work had been performed.

Editorial Committee, by H. L. Bruner, reported work done as ordered.

Report of Secretary on non-resident list was taken up. On motion it was decided to place only those members on the non-resident list who had done work of marked credit to the Academy. The list was passed on by the Executive Committee.

Deaths of Dr. Gray and W. H. Ragan reported. Committee on Resolutions appointed, consisting of C. L. Mees. A. W. Butler and G. W. Benton.

Bills of expense were reported by A. W. Butler. They were referred to Auditing Committee.

Foreign Exchange list ordered to be revised and printed in next report.

Summer meeting to be passed on tomorrow.

Committee on Fellows also to consider a list of Honorary Fellows.

It was voted to place $25.00 at the disposal of the Secretary for his official duties.

Resolution from California Academy of Science read.

Dr. Jordan extended greeting from the California Academy of Science, and thanks for books.

Committee of two on Fellows was ordered to be appointed by Academy.

Motion that the chairmen of Committees be retained, committees to be filled by chairmen.

Auditing, Membership, Program and Nominating Committees not to be covered by previous motion.

On motion G. W. Benton was chosen as another Assistant Secretary.

Adjourned.

J. H. RANSOM, Secretary.

A. J. BIGNEY, Assistant Secretary.

(Report adopted as read.)

Dr. Foley: I will now call on Mr. A. W. Butler to make a statement in regard to this meeting of the Academy.

Mr. A. W. Butler: Mr. Chairman, and Members of the Academy: The program as printed, and which I suppose the most of you have in your hands, has on it a list of sixty-three papers. There are six additional papers which have been added. One of these, a paper by Prof. M. B. Thomas, was omitted from the original list. The additions are as follows:

"The Wood Lot," M. B. Thomas.

"The Nasal Muscles of Vertebrates," H. L. Bruner.

"Streamers that Show Reversal of Curvature in the Corona of 1893," J. A. Miller.

"On a New Complex Copper Cyanogen Compound," A. R. Middleton.

"Determination of Endothermic Gases by Combustion," A. R. Middleton.

"That Erroneous Hiawatha," A. B. Reagan.

This brings the number of papers up to sixty-nine.

At the conclusion of the business of the meeting there will be responses from other State societies, some six or eight in number.

The program as printed indicates a banquet this evening, to which attention has been called, and the program for which will be announced later.

The program for tomorrow morning is also printed here, including four principal addresses, and suggestions as to plans for the Academy.

I want to say in behalf of the Committee on the Twenty-fifth Anniversary that we have been very much gratified by the interest that has been taken by the educational and scientific societies throughout the State. The Indiana Medical Association, the Historical Society, the Teachers' Association, and a number of other associations have by formal resolution recognized this twenty-fifth meeting, and several of them have appointed delegates to attend the meeting.

I would also like to call attention to the fact that we have had a very large number of congratulatory letters on the period we have arrived at in the history of this Society, and there are three I would like to call attention to. One is from one of the ex-Presidents whom we always delighted to honor, Mr. T. C. Mendenhall. He is at present in Europe in search of health, and as he cannot be present, sends his congratulations. Also a letter from Professor Goss, of the University of Illinois, who had expected to be present until he found that this date is the same as that of the dedication of their new Physics building, so he could not come. Also one from Prof. Kingsley, of Tufts College, Mass. These three letters are particularly earnest and cordial in their words of greeting.

We hope you will find everything arranged for your comfort and convenience, and beg to assure you that if anything has been overlooked or if there is anything you do not like in connection with the arrangements, we are sorry that such is the case. The Committee tried to do its best. (Applause.)

Dr. Foley: I will now call for reports from the different standing committees.

Program Committee, Mr. W. J. Moenkhaus, chairman: (This report included in the statement of Mr. Butler.)

Membership Committee:

(Moved and seconded that the Secretary cast the ballot of the Academy for the names read. Carried, and persons declared members upon signing of the Constitution and payment of dues.)

Treasurer's report, Mr. W. A. McBeth, Treasurer:

To the Indiana Academy of Science:

On hands, last report	$424 39	
Received dues and fees for 1909	95 50	
		$519 89
Expended as per receipts and vouchers		118 67
Balance on hand		$401 22

The papers and vouchers are ready for the Auditing Committee.

W. A. McBETH, Treasurer.

State Library Committee, J. S. Wright, chairman: (Postponed until later, when State Librarian Brown will make the report.)

Committee on Restriction of Weeds and Diseases: No report.

Directors of Bioligical Survey: No report.

Relations of Academy to the State: No report.

Distributions of Proceedings, J. S. Wright, chairman:

Mr. Wright: There is no special report to make. The Committee has the work in hand. We are now engaged in compiling a domestic exchange list.

Committee on Election of Fellows: Passed.

Report of Advertising Committee: (Included in statement of Mr. Butler.)

Report of Editor:

Mr. H. L. Bruner: The Proceedings for 1908 were published in the usual form. Each contributing author also received one hundred free reprints of his own article. (No reprints of abstracts were furnished.) The financial part of my report is as follows:

Balance in State Treasury from 1908	$244 98	
Appropriation for fiscal year 1909*	600 00	
Total		$844 98
Cost of Proceedings for 1908	$438 74	
Cost of reprints for 1908	85 68	
Total		524 42
Balance available for fiscal year 1910		$320 56
Appropriation for fiscal year 1910		1,200 00
Total available for printing the Proceedings of 1909		$1,520 56

I wish to call the attention of the members of the Academy to one or two matters. First in regard to the editorial statement on the program. We desire that papers be in the hands of the editor or secretary as early as possible, in order that the Proceedings may be gotten out more promptly than last year. Reprints will be furnished of all papers printed, excepting abstracts, and these may be furnished, if request is made. These reprints are paid for by the State Printing Board.

I desire to ask for suggestions as to changing the style of binding and improving the quality of the paper for the coming year.

I would also ask that each one sending a paper for publication should give his address on the paper, so proof can be sent and the reprints mailed. This is a very important thing and I hope it will not be overlooked.

Dr. Foley: Does anyone have any suggestions to make?

Mr. J. S. Wright: I am sorry to occupy so much time on the floor this morning, but I feel there is one thing that should be recognized, and that is the fact of the service rendered the Indiana Academy of Science by the past President, Mr. Glenn Culbertson, who succeeded in doubling the amount of money available for publishing. We now have $1,200 per

* The fiscal year 1909 began Oct. 1, 1908, and closed Sept. 30, 1909.

year, as against $600 before Mr. Culbertson took this in hand. I think this Academy owes him a debt of gratitude. (Applause.)

Dr. Foley: I wish to second what Mr. Wright has said. I also wish to point out another fact, that formerly any money left reverted to the State, while now it can be carried over until the next year.

Are there any other suggestions?

Mr. M. B. Thomas: It seems to me it would be best to improve the quality of the paper and printing, and possibly of the illustrations, but that this matter should be left to the Committee on Printing, of which Prof. Bruner is the chairman.

(Taken by consent.)

Mr. Wright: I move that the Academy extend a vote of thanks to Mr. Culbertson for his unusual service.

(Seconded and carried.)

Report of Resolutions Committee: No report at this time.

Mr. G. W. Benton: I would like to suggest that the Academy is under obligations to the press of the city for courtesies extended, in giving us column after column of space for advertising this meeting. We have been unusually privileged in this regard, and I think it is proper and courteous that we should recognize it in some definite way. Therefore I move that we extend a vote of thanks to the press of the city for courtesies extended to the Academy in announcing this Anniversary meeting.

(Seconded and carried.)

Dr. Stanley Coulter, (for the Membership Committee): It seems to me it would be remarkably pleasant if we could mark this twenty-fifth anniversary by a large increase in membership, and if you will see that applications are in the hands of the committee some time during the forenoon, we will report on them at the afternoon session, so the neophytes will have the feeling that they are full-fledged members.

After an announcement by the Treasurer in regard to payment of dues; and another by Mr. Benton regarding the banquet tickets, etc., Dr. Foley called on Mr. D. C. Brown, the State Librarian, to report in regard to the Academy and its relation to the State Library.

Prof. D. C. Brown: I am not a member of the Academy of Science, but as State Librarian I made an agreement with the Academy of Science by which the State Librarian is to classify, catalog and shelve the documents and reports belonging to the Academy, making them subject to removal by any members of the Academy, and subject to reference by the public. I am very greatly interested in having the State Library the center for reference of the entire State on every subject, and by the agreement made with the committee of your Academy two years ago this work has been begun and is progressing fairly well.

The agreement was that the catalog department of the State Library should, as fast as possible and as fast as funds would allow, proceed with this work. Up to the present time we have classified, cataloged, and made analytical catalogs of 143 volumes of domestic reports and 96 foreign reports, making a total of 239 volumes. These have all been bound, and there are about one hundred volumes at present ready to go to the bindery, some foreign and some domestic. These volumes are systematically cataloged and at the present time I have had them all bound alike in good buckram, with a certain kind of label on the back, with "Academy of Science" at the top and the library call number at the bottom. Inside, a label showing to whom the book belongs, and that it can be borrowed only by the members, but used for reference by the general public. I am not quite sure that it is advisable to bind all these books in exactly the same way, but it makes them easily understood when on the shelves. Members can tell instantly that that book belongs to the Academy of Science. A separate card list is also made in pencil and ink, and easily accessible at any moment.

I fancy you all understand that the binding is paid for by the library, with the understanding that if the Academy ever withdraws the books it must pay that amount, so the bills for binding are kept separate, and the public has the use of the books. The Academy would also have the right to have the cards that are made showing the books properly cataloged. Whether that will ever come, I do not know.

I am struggling as best I can for a State Library and Historical Museum, in which all the valuable records and scientific reports of the State can be kept, and in making the argument for that I have said that the Academy of Science would help.

I do not know that I can make any further statement about it, only to have it known to you that the reports are cataloged now about as fast as they come in. I have one request to make—that we may have a definite and correct list of your foreign exchanges, your domestic exchanges, and your membership. I have had considerable trouble about that, but have worked it out fairly well so far. The foreign exchanges are made through the Smithsonian Institute at Washington. The files of the reports sent to members are paid for by the Academy. The library pays for the others, and through the library they are distributed.

I am very anxious that the members come to the library, as their coming there to use these reports will make it known to the public that the reports are there and can be used.

I believe I have nothing further of interest, but I am very anxious to see you in the library. (Applause.)

Dr. Foley: I am sure I voice the sentiments of the Academy when I thank our Librarian for the efforts he has put forth in getting the Academy library in good shape, available for use.

The program calls for greetings from the various other scientific societies after the addresses of the morning. I am informed, however, that Mr. Brossmann, representing the Indiana Engineering Society, is here and cannot remain, therefore I will call upon Mr. Brossmann at the present time.

Mr. Brossmann's address will be found in full on page 44.

Dr. Foley: I might ask if there are any other representatives of societies here that cannot remain during the period. If so, we will have the greeting at this time.

There is just one other point that might be taken up at this time, and that is the question of a summer meeting. The question was mentioned at the Executive Committee meeting last evening, but was not settled. Are there any suggestions as to whether we shall or shall not have a summer meeting? I think the Program Committee would like to have an expression from the Academy. It does not wish to announce a meeting unless somebody meets. On the other hand, it does not wish to discontinue this meeting if it is the desire of any considerable number of members to continue them. What is the wish of the Academy?

If no one has any suggestions, I will call on Dr. S. E. Earp, who fears he may not be able to remain during the entire morning, to respond for the Indiana Medical Society.

Dr. Earp's remarks will be found in full on page 40.

(Mr. P. N. Evans, Vice-President, in the chair.)

Mr. Evans: We will now proceed with the regular order of business, and will hear the President's Address by Dr. A. L. Foley, of Bloomington.

Dr. Foley's address will be found on page 89.

Following the President's address:

Mr. Evans: Evidently this chair should be occupied by a physicist instead of a chemist, so I will vacate in favor of Dr. Foley. (Applause.)

Dr. Foley: It now gives me great pleasure to introduce one who needs no introduction, Dr. John M. Coulter, of Chicago University, who will speak to us on "Recent Progress in Botany." (Applause.)

Dr. Coulter's address will be found on page 101.

Following Dr. Coulter's address:

Dr. Foley: You will note from the program that Dr. Harvey Wiley was to have been here this morning to address us. I understand Dr. Barnard has a letter from Dr. Wiley. We would be glad to hear from Dr. Barnard.

Dr. H. E. Barnard: Mr. President, I just this morning received a communication from Dr. Wiley, in which he said he was engaged in the preparation of a very important case involving one of the basic principles of the Pure Food Law. He said if he came on here for four days, he did not know what would happen to the case, and that while he would be with us in spirit and thought, it would be impossible for him to leave his work in Washington to attend this convention. He sends to you his best wishes and hopes for a successful meeting.

Dr. Foley: You will note from the program that we now have greetings from several associations, scientific and otherwise, who have sent delegates to this association at this time. I will call first for the Indiana Teachers' Association, through its President, Mr. Geo. W. Benton.

(See page 39.)

Dr. Foley: We will now hear from the Indiana Branch of the American Chemical Society, through Mr. R. B. Moore.

(See page 42.)

Following the various society greetings the Academy adjourned until 2:00 p. m.

Saturday Morning, November 27, 1909.

Meeting called to order by President Foley.

(After asking the members who had not already done so to leave their names at the desk, so that a complete list of those in attendance at this meeting might be obtained, Dr. Foley called for the report of the Committee on Resolutions. Mr. C. L. Mees; chairman.)

For this report see page 24.

(Moved and carried that the report be adopted.)

Dr. Foley: It seems to me that the Academy is under great obligations to the Program Committee, especially to Mr. Butler, and I think a vote of thanks to this committee would be in order.

(Moved and carried that a vote of thanks be extended to the Program Committee, especially Mr. Butler, for the great amount of work that has been put on the program.)

REPORT OF NOMINATING COMMITTEE.

President, P. N. Evans, Lafayette.

Vice-President, Chas. R. Dryer, Terre Haute.

Secretary, George W. Benton, Indianapolis.

Assistant Secretary, A. J. Bigney, Moores Hill.

Treasurer, W. J. Moenkhaus, Bloomington.

Editor, H. L. Bruner, Indianapolis.

(Moved and carried that the report be accepted and that the Secretary cast the ballot of the Academy for these offcers.)

REPORT OF AUDITING COMMITTEE.

We have gone over the vouchers of the Treasurer, the Program Committee, and the Editor's Report, and find the sums have been done correctly.

W. J. MOENKHAUS, Chairman.

(Moved and carried that the report be adopted.)

REPORT OF COMMITTEE ON MEMBERSHIP.

Thirty-five additional names reported.

Applicants for Membership elected by vote of Academy, 1909.

Thomas Billings .... West Lafayette.
Earl Grimes .... Russellville.

Earl Rouse Glenn .... Brookville.
A. A. Bourke .... Edinburg.
Geo. Hall Ashley .... Indianapolis.
James Persons Dimonds.............. Washington, D. C.
Guido Bell .... Indianapolis.
Florence Anna Gates .... Wabash.
Oscar William Silvey .... Bloomington.
James E. Weyant .... Indianapolis.
John W. Woodhams .... Indianapolis.
Melvin Knolen Davis .... Terre Haute.
E. Kate Carman .... Indianapolis.
Paul Anderson .... Crawfordsville.
Howard J. Banker .... Greencastle.
Charles Alexander Vallam .... Indianapolis.
Thad. S. McCulloch .... Crawfordsville.
Frank Karlston Mowrer .... Marion.
E. M. Deem .... Frankfort.
Milo H. Stuart .... Indianapolis.
Charles Ruby Moore .... West Lafayette.
L. R. Hesler .... Crawfordsville.
Martha Hunt .... Indianapolis.
Brenton L. Steele .... Bloomington.
Alfred Theodore Wianco .... Lafayette.
Walter W. Hart .... Indianapolis.
Ira C. Trueblood, Miss............. Greencastle.
Luther Cornelius Weeks .... West Lafayette.
Fermen L. Pickett .... Bloomington.
William Logan Woodburn .... Bloomington.
Roscoe Raymond Hyde .... Terre Haute.
Chas. M. Cunningham, Dr............ Indianapolis.
Mason L. Weems .... Valparaiso.
Edward N. Canis .... Indianapolis.
G. A. Osner .... Crawfordsville.
Frederick W. Gottlieb .... Morristown.
Geo. T. Moore .... St. Louis.
Samuel E. Earp .... Indianapolis.
J. H. Clark ....
Leslie C. Nanney .... Bedford.
Everett W. Owen .... Indianapolis.
Geo. Spitzer .... West Lafayette.
Geo. N. Hoffer .... West Lafayette.
Julius Wm. Sturmer .... West Lafayette.
Virges Wheeler .... Montmorenci.
Harry F. Dietz .... Indianapolis.
Chas. Brossman .... Indianapolis.

A. D. Thornburn	Indianapolis.
Chas. Stiltz, M. D	South Bend.
Jacob P. Young	Huntington.
J. M. Van Hook	Bloomington.
Walter M. Baker	Red Key.
Wm. Reynolds Butler	Indianapolis.
W. H. Rankin	Ithaca, New York.
Omer C. Boyer	Lebanon.
W. M. Blanchard	Greencastle.

(Moved and carried that the Secretary cast the ballot of the Academy for these names, and that the persons be considered members after paying fees and signing the Constitution.)

Dr. Foley: I should like to bring up a matter at this time which was brought up yesterday, but we could not get an expression from the Academy. That is, in regard to the Summer meetings. Does this Academy want a Summer meeting? I think the Program Committee would like to have an expression from the members.

Dr. Stanley Coulter: I want to say that in twenty-five years' membership I have found that the Summer meeting is equivalent to about three Winter meetings in the way of uplift and encouragement. Of course, one of the objections is that a good many members—mathematicians, chemists and physicists—would not be specially interested in these Summer meetings. I would very much regret to see the Summer meeting abolished. If, however, it does not seem feasible, I presume it might be dropped. I move that the Program Committee be instructed to proceed with plans for the Summer meeting, and if in their judgment the signs are not favorable for a session, they be authorized to drop it.

W. A. McBeth: I want to second that motion. I remember with great pleasure the Spring meetings. I made it a point to attend them regularly, and through the fact that we had Spring meetings I have visited some very interesting points in Indiana which are hard to get to unless you particularly go there. The town of New Harmony was one of these places; it is full of historical associations. We went to Madison, to Bloomington, to many of the caves, and to various other points throughout the State where we would probably not have gone if it had not been for this particular attraction. Now, my own way of thinking is that if we would resolve to go to these Spring meetings they would be worth two of the Winter meetings to those who go. I am heartily in favor of resuming the Spring meetings.

(At the suggestion of Mr. Butler a standing vote was taken, which resulted unanimously in favor of resuming the Spring meetings.)

Mr. Butler: Mr. Chairman. We have a telegram of greeting from the Ohio Academy of Science, and I move that the Secretary be instructed to telegraph the greetings of the Indiana Academy in return.

(Taken by consent.)

Mr. Butler: In reference to the letter from Dr. Harvey Wiley read at the banquet last night, I move that a committee, consisting of Stanley Coulter, Harvey W. Wiley and C. H. Eigenmann be appointed to see that the suggestions in Dr. Wiley's letter in regard to obtaining some one to prepare a history of early science in Indiana, are carried out.

(Seconded.)

J. H. Ransom: I would like to amend that by adding Mr. A. W. Butler's name to that committee as a fourth member.

(Seconded.)

Stanley Coulter: I suggest that Mr. Butler be the first member instead of the fourth.

Mr. Butler: I think the purpose of the committee is simply to study the situation, and a smaller committee is better than a large one. The three first chosen are the proper members and would be able to do the work better than a larger committee.

(Amendment put and carried; motion as amended carried.)

Mr. Butler: I move that the Treasurer and Secretary be directed to notify all delinquent members that the constitutional rules against such will be enforced, by order of the Academy.

(Seconded and carried.)

Mr. Butler: Another matter I think should be acted upon by the Academy. The Editor this year has not put in any bill for expenses, and the expense of editing the Proceedings will probably be larger next year. I move that an appropriation of $25 be allowed the Editor for the expenses of this year and the year coming.

(Seconded and carried.)

Mr. J. S. Wright: In view of the fact that the Academy has received many favors from the Claypool Hotel in giving us this room without charge, and a room for the section meetings, and other courtesies, I move that we extend a vote of thanks to the management of the Claypool Hotel for courtesies shown the Academy.

(Seconded and carried.)

Dr. Foley: We will now take up the program of the morning. The first number is an address by Dr. B. W. Evermann, of the U. S. Bureau of Fisheries, on "Federal Control of International and Interstate Waters."

For Dr. Evermann's address see page 119.

Dr. Foley: The next paper is by Prof. Charles W. Greene, of the University of Missouri, on "The Speed of Migration of Salmon in the Columbia River."

An abstract of Professor Greene's address is given on page 125.

Dr. Foley: The last paper on the program, "Some Hoosier and Academy Experiences," is by C. A. Waldo, of the Washington University, St. Louis, but Mr. Waldo is not here. The first paper, "Methods and Materials Used in Soil Testing," is by H. A. Huston, of Chicago. Mr. Huston is not here, but his paper is, and it will take about fifteen minutes to read it. It is contrary to precedent that a paper should be read by anyone but the author. However, the Academy can change that, of course, at will. What shall we do with this paper?

(Moved and carried that the paper be read.)

For Professor Huston's address see page 111.

Dr. Foley: I am sure the members of the Academy would like to hear from anyone who has any suggestions to offer. This completes the list on the program, but we will be glad to hear from anyone else.

If you will pardon me, I would like to make a suggestion or two, one of which was made to me last evening.

Those of us who are members of the American Association know that when we register there, a number is given us corresponding to the name, address and business of the member. So all we need to do to find any man's pedigree is to refer to the number in the list, which is the registration list. Now, it seems to me that some scheme like that might be an advantage in connection with this Academy, so that any member can find out who the other man is. I know I am introduced to people a half-dozen at a time, whom I cannot place and name a few minutes afterwards. A great many people I find are like to me in that respect. We cannot associate names and faces after having been introduced to three or four persons at once. Perhaps some sort of a scheme might be adopted to advantage.

Another thing is that this meeting is the largest that we have ever had during my connection with the Academy, and the reason is evident. We have had men of national reputation to address us. I do not think this large attendance comes from the fact that this is an anniversary meeting, but from the fact that the program has been made worth while by having men who will draw people to the meeting.

You will note that the State is now doing our printing; we do not have to pay that ourselves, and you will note from the Treasurer's report that we have some money and that we are going to get more money, and we have nothing particular to do with this. Now, it seems to me that the Program Committee might arrange to bring one or two speakers here each year, speakers of national reputation, and spend some of this money for their expenses. If we could have some such program as we have had this year every year, with men like Dr. Jordan, and Dr. Coulter and Dr. Wiley, there is no question but what we would have a large attendance, and I think our funds will justify that. I merely offer these as suggestions.

H. L. Bruner: As editor of the Proceedings I would urge the importance of getting the manuscripts in as soon as possible. The fact that the Proceedings were late this year is due largely to the tardy reception of the papers by the editor. If the members, will turn over their papers promptly, I will see that they get into the hands of the printer as early as possible.

Dr. Foley: I want to second what Mr. Bruner has said. I was Editor one year.

This completes the program, unless the Academy wishes to take up some of the papers which are departmental. What is your will?

(Motion to adjourn.)

[PRESIDENT'S ADDRESS.]

Recent Developments in Physical Science.

[Publication No. 34.]

By Arthur L. Foley.

On this—the twenty-fifth—birthday of the Indiana Academy of Science, it is meet that we survey the progress made and take an inventory of stock on hand. Where were we? Where are we?

Comparing physical science of today with physical science of twenty-five years ago, I am forced to the conclusion that there has been a revolution.

In the first place there has been a revolution in the methods of teaching science. I would remind you that the physics laboratory of the University of Berlin was founded in 1863, the Cavendish laboratory of Cambridge in 1874. In 1871 Professor Trowbridge, of Harvard, was obliged to borrow some electrical measuring instruments, as the university had none of its own. It is not surprising, then, that a few years later—at the time the Indiana Academy of Science was founded—there were in the United States very few physics laboratories worthy of the name. Physics teaching in college and high school was chiefly from the text-book. Today a college which would offer work in physics without a laboratory would be considered a joke; and in order to be commissioned, a high school must have a certain minimum of laboratory equipment and the physics teacher must devote a part of his time to laboratory instruction.

In the second place there has been a complete change in the attitude of men of affairs toward the physics professor and his students. No longer do they consider us theoretical, and therefore impractical. No longer do they look with distrust or contempt on laboratory methods and data. No longer do they hold that apprenticeship and experience are sufficient for their needs. Today the large industrial concerns are establishing laboratories of their own and employing in them the best trained men they can command.

In the third place, there has been a revolution in some of our physical theories. By the term revolution I do not mean a destructive upheaval in which the work of the past has been repudiated and destroyed and a new order of things established. I mean that some of our ideas have undergone such a complete and rapid change that what some might term an evolution is really a revolution. Indeed, we have had two revolutionary periods within the life of this Academy.

The first came in 1887 with the epoch-making researches of Heinrich Hertz. Faraday had given us his theory of lines of force and the mathematicians had attacked it. Young and Fresnel had given us the undulatory theory of light and Laplace and Poisson had "befuddled us with their objections." Ampere had given a theory of magnetism, but Poisson and Weber had given two others. To explain an electric charge we could resort to the one-fluid theory, the two-fluid theory, the potential theory, the energy theory, the ether-strain theory. Maxwell had written a treatise on electricity which few could read and no one could fully understand. A distinguished French physicist said he understood everything in Maxwell's book except what was meant by a body charged with electricity. Maxwell had given us but a vague idea of electric displacements and displacement currents, because his ideas were bound up in equations without experimental verification, or even illustration.

Then came Hertz's researches, which confirmed the fundamental hypotheses of the Faraday-Maxwell theory and "annexed to the domain of electricity the territory of light and radiant heat." [1]"Many thinkers," said Lord Kelvin, "have helped to build up the nineteenth century school of *plenum*, one ether for light, heat, electricity and magnetism; and Hertz's electrical papers, given to the world in the last decade of the century, will be a permanent monument of the splendid consummation now realized." Some one has said that Hertz enthroned Maxwell in every chair of physics in Europe and America.

It appears that many of the ancient philosophers had a shadowy idea of a medium in space which they personified and called "Aether." According to Heriod, Aether was the son of Erebus and Night and the brother of Day. The Orphic hymns speak of Aether as the soul of the world, the animator of all things, the principle of life. The children of Aether and Day were the objects about us, the heavens with all their stars, the land, the sea. Aether was the lightest and most active form of matter and Day had the power of converting it into heavier matter. Plato speaks of the

[1]Kelvin. Introduction to Jones' translation of Hertz's "Electric Waves." Macmillan, 1893.

Aether as being a form of matter far purer and lighter than air, so light that its weight cannot be ascertained because distributed through infinite space.

During the fifteen years following the publication of Hertz's researches it is probable that greater homage was paid to Ether by modern physicists than was ever given it by the ancients. The ether was appealed to from every quarter. Light, radiant heat and electric waves were ether waves. An electric charge was an ether strain. An electric current was a phenomenon in the ether and not in the wire in which it appeared to flow. Magnetism and gravitation were phenomena of the ether. Matter itself became an aggregation of ether vortices. Ether and motion were expected to explain everything. Such terms as natural philosophy and physics were discarded by some of our text-book writers who adopted such titles as "Matter, Ether and Motion"; "Ether Physics"; "Ether Dynamics"; "The Mechanics of the Ether." Physics was defined as the science of motion.

The classical mechanics of LaGrange was built on what were considered fundamental concepts—mass, force, space and time. Hertz, in his treatise on mechanics published in 1894, endeavored to eliminate force and potential energy and reduce a universe to ether movement. Space and time were not fundamental ideas, but as Kant had said, were subjective notions. We measure time by a change of space relation; that is, a movement of a star, of the earth, of a clock hand. "In a world void of all kind of movement there would not be seen the slightest sequence in the internal state of substances. Hence the abolition of the relation of substances to one another carries with it the annihilation of sequence and of time." Thus everything was made to depend upon movement. The equations of motion became the chief instruments of physical research, and the criterion by which the results of experiments were interpreted. Galileo lost his professorship because he dared to dispute the authority of Aristotle. Daguerre was for a time placed in an asylum because he said he could take a picture on a tin plate. Galvani was ridiculed by his friends and dubbed "the frog's dancing master." Franklin's paper on lightning conductors was considered foolish, and refused publication by the Royal Society. Fifteen years ago it would have been almost as disastrous for a physicist to question the authority of LaGrange or Maxwell. Not only were the *results* of experiments subjected to mathematical analysis, the *direction* of scientific investigation was largely so determined. The question was first put to mechanics. If a positive answer was indicated the question was put to nature and the research went on. If the equations indicated a negative result the question was dropped and the research abandoned.

Physics was an *exact* science. Other sciences were not exact sciences because their theories and hypotheses could not be mathematically expressed—the relation between cause and effect was not expressible in algebraical symbols. Physics was an exact science whose fundamental principles had been discovered and its laws expressed by equations. All that remained to be done was to make more accurate measurements of physical quantities for use as coefficients and exponents.

Let me quote from the 1894 catalogue and later catalogues of one of the largest universities in the United States.

"While it is never safe to affirm that the future of physical science has no marvels in store. * * * it seems probable that most of the grand underlying principles have been firmly established and that further advances are to be sought chiefly in the rigorous application of these principles to all the phenomena which come under our notice. * * * An eminent scientist has remarked that the future truths of physical science are to be looked for in the sixth place of decimals." The foregoing is a verbatim quotation from the introductory statement preceding the list of courses in physics offered at one of our great universities, written, I think, in 1894. "Underlying principles firmly established," "Future truths in sixth decimal place," 1894. Then came the discovery of Roentgen rays, 1895; Becquerel rays, 1896; Zeeman effect, 1896; radium, 1898; atomic disintegration, the transformation of matter, the thermal effect of radioactivity, and intra atomic energy, 1903. I am unable to locate the sixth decimal idea in recent catalogues.

J. J. Thomson likens the discovery of Roentgen rays to the discovery of gold in a sparsely populated country. Workers come in large numbers to seek the gold, many of them finding that "the country has other products, other charms, perhaps even more valuable than the gold itself."

The chief value of Roentgen's discovery was not that it furnished us a new kind of light for the investigation of dark places, but in the fact that it led a host of workers to study vacuum tube discharges—the discharge of electricity in gases and the effects of such discharges on matter itself. The old dusty Crookes' tube was taken down from the far corner of the upper shelf and regarded with new interest. In a day it had ceased to be a forgotten, though curious, plaything, and had become a powerful instrument of research. It was before Roentgen's discovery that a well-known professor said to me that he considered it foolish for one to spend any part of his departmental appropriation for a vacuum; that when he paid out money he wanted something in return—not an empty space. And yet this man was familiar with the work of Faraday and of Crookes, both of whom with prophetic mind had foreseen and foretold. Let me quote from a lecture by Faraday on the significant subject "Radiant Matter."

[1] "I may now notice a peculiar progression in physical properties (of matter) accompanying changes of form, and which is perhaps sufficient to induce, in the inventive and sanguine philosopher, a considerable degree of belief in the association of the radiant form with the others in the set of changes I have mentioned.

"As we ascend from the solid to the fluid and gaseous states, physical properties diminish in number and variety, each state losing some of those which belong to the preceding state. * * * The varieties of density, hardness, opacity, color, elasticity and form, which render the number of solids and fluids almost infinite, are now supplied by a few slight variations in weight and some unimportant shades of color.

"To those, therefore, who admit the radiant form of matter, no difficulty exists in the simplicity of the properties it possesses * * * . They point out the greater exertions which nature makes at each step of the change and think that, consistently, it ought to be greatest in the passage from the gaseous to the radiant form." The lecture from which the foregoing is a quotation was delivered in 1816, when Faraday was but twenty-four years old.

Let me quote again, this time from a lecture by Sir William Crookes delivered sixty years later, more than thirty years ago, on the same subject—"Radiant Matter."

"In studying this fourth state of matter we seem at length to have within our grasp and obedient to our control the little indivisible particles which with good warrant are supposed to constitute the physical basis of the universe. We have seen that in some of its properties radiant matter is as material as this table, whilst in other properties it almost assumes the character of radiant energy. We have actually touched the borderland where matter and force seem to merge into one another, the shadowy realm

[1]Life and Letters of Faraday, Vol. 1, p. 308.

between known and unknown, which for me has always had peculiar temptations. I venture to think that the greatest scientific problems of the future will find their solution in this borderland, and even beyond; here, it seems to me, lie ultimate realities, subtle, far-reaching, wonderful."

The developments of the last few years have demonstrated that no truer prophecy was ever uttered, and the prophet Crookes has lived to witness and to take a part in its fulfillment.

The importance of the present rejuvenation of physical science does not consist alone in the abundance of the harvest. There have been abundant harvests in the past. Consider the decade which closed one hundred years ago. In 1798 Rumford boiled water by friction. In 1799 Davy melted ice by friction in a vacuum and Laplace published his work on mechanics. In 1800 Volta constructed the Voltaic pile, Nicholson and Carlisle decomposed water, Davy discovered the properties of laughing gas, and Herschel discovered dark heat rays. In 1801 Piazzi discovered the first asteroid, Ritter the chemical rays, and Young the interference of light. In 1802 Wedgewood and Davy made sun pictures by the action of light on silver chloride, and Wollaston discovered dark lines in the sun's spectrum. In 1808 Malus discovered polarization by reflection, Gay Lussac the combination of gases by multiple volumes, and Dalton the law of multiple proportions.

So great was the exhilaration and satisfaction produced by these discoveries that many scientists of that period appear to have become infected with something akin to the "sixth decimal" delusion. "Electricity," wrote the French scientist Haüy, "enriched by the labor of so many distinguished physicists, seems to have reached the term when a science has no more important steps before it, and only leaves to those who cultivate it the hope of confirming the discoveries of their predecessors and of casting a brighter light on the truths revealed." A statement which was almost immediately followed by the discoveries of Oersted, Ampere, Seebeck and Faraday. A statement which has been followed by the telegraph, the telephone, the dynamo, the motor, the electric light, the electric railway, the Roentgen rays, and the wireless telegraph and telephone.

If anyone today is disposed to criticise the men of science of other times because of their limited view, their complacent opinions and their intolerance of all that did not agree with theories they considered established, let him first read and ponder over what One spake about motes and beams.

The real significance of recent developments is in the fact that they change—in a way revolutionize—some of our ideas of things. And here let me say that proven facts and proposed theories should not be confused. A theory is simply a working hypothesis, invented for the purpose of explaining facts, to be discarded when facts are discovered with which the theory is not in harmony. A theory may explain many facts, it may be generally accepted, it may have survived for generations and be false. The phlogiston theory, the corpuscular theory are two examples. Shall we say that the theory of the indestructibility of matter and of the conservation of energy are two others?

The usual chemistry text-book would have us believe in the indestructibility of matter because the chemist can change the form of matter almost at will, and in all the chemical reactions there is no loss of weight. In replying to this argument I wish to make three points.

First. The balance, notwithstanding the statement of text-books, compares weights and not masses, and it is only because weight is assumed to be proportional to mass that we say we determine mass by the balance. What we really compare is the gravitational force which the earth exerts on two masses, and we have no a priori right to assume that this gravitational force is absolutely independent of the state or molecular arrangement of the attracted body. Why, for instance should we expect an absolutely uniform field of force about a crystal when that same crystal will, if placed in a proper solution, continue to grow symmetrically, and perhaps replace a broken-off corner before beginning its growth?

It is conceivable that there should be a loss of weight in chemical reactions and yet no destruction of matter. It is possible that mass and weight are not strictly proportional. If J. J. Thomson were not disposed to question the equation $w = m \cdot g$ he would not have experimented with a pendulum of radium, and he would not now be experimenting with a pendulum of uranium oxide.

In the second place there *is* an apparent change of weight in chemical reactions as has been shown by several experimenters, notably by Landolt,[1] who found a loss in forty-two out of fifty-four cases. The chemical reactions were brought about in sealed glass tubes which generally weighed less after the reactions than they weighed before. Later[2] it was found that some of these losses might be attributed to temperature and volume

[1] H. Landolt. Preuss. Akad. Wiss. Berlin, Sitz. Ber. 8, pp. 266-298, 1906.
[2] Landolt. Preuss. Akad. Wiss. Berlin, Sitz. Ber. 96, pp. 354-387, 1908.

changes. Whatever the testimony of the balance may have been, some of the reactions must have been accompanied by a loss of weight, for it has been proven by chemical means that such reactions are frequently attended by the escape of something through the walls of the glass tubes.[1] This loss is readily explained by the disintegration theory. If one wishes to explain it by assuming the diffusion of ordinary gases through the glass walls of the tube he must explain the fact that, in many cases, it was the heavy and least volatile substances that escaped fastest.

In the third place the element of time has been overlooked. Matter may be disintegrating, but at such a slow rate that in the limited time over which experiments have been extended the balance has failed to detect the change. As far as our experience goes the time of rotation of the earth is constant; but we know that it cannot be absolutely constant. The moon has slowed down until it takes a month to make one turn. To an ephemeral insect almost everything would appear to be eternal. With due respect for the balance and the wonderful work it has enabled chemists to do, it must be admitted that it is, comparatively, a very crude instrument. Let me prove it.

Suppose we fix the limit of sensibility of the balance at one one-thousandth of a milligram. Our books on chemistry tell us that 1 c c of gas, say hydrogen, at ordinary pressure contains 4×10^{19} molecules. The density of H being 896×10^{-7}, then 1 gm. of H would consist of $(4\times10^{19})\div(896\times10^{-7})$ molecules. Taking 112 as the ratio of the molecular weights of radium and H, then 1 gm. of radium would consist of $[(4\times10^{19})\div(896\times10^{-7})]\div112=4\times10^{22}$ molecules. Therefore .001 mgm. of radium would consist of 4×10^{16} molecules, and this would be the smallest possible number that our most sensitive balance could detect. If the gram of radium were disintegrating and its molecules escaping at the rate of a million per second it would require 4×10^{10} seconds = 463,000 days =1270 years for that gram of radium to lose in weight only the one-thousandth part of one milligram, all the while its molecules trooping away at the rate of a million per second.

The population of the earth is about 1,500 millions. The smallest number of molecules a balance will detect is 4×10^{16}, or about 26,600,000 times the population of the earth. We wonder if Mars is inhabited. If a Martian were to come to the earth to make an experiment to determine whether or not the earth is populated and he had no better instrument

[1] C. Zenghelis. Zeitschr. Phys. Chem. 65, 3, pp. 341-358, Jan. 5, 1909.

"for the detection of the existence of a man" than is the balance for a molecule, he would be obliged to go back and report the earth uninhabited. In fact his instrument for the man test would need to be 26,600,000 times as sensitive as the balance to give him even a hint of the probability of an earth population.

Thomson says that the smallest quantity of unelectrified matter ever detected is probably neon, and this was discovered by the spectroscope—not the balance. But the number of molecules of neon required to give a spectroscopic effect is about ten million million, or about 7,000 times the population of the earth. It has been shown that the presence of a single charged atom can be detected by electrical means. Thus the electroscope is millions of millions of times as sensitive as the spectroscope, which is itself in many cases far more sensitive than the balance. This explains, in part, why radium was discovered by physicists, and why physicists have been most active in all the work which has had to do with the theories of electricity and matter. If chemists wish to compete with physicists in this field of investigation they must adopt physical methods and apparatus or devise some of their own which shall be far more sensitive than the balance or spectroscope. Further, many of the great chemists of the world need to awake to the fact that there is something doing and that they are not doing it. Their indifference is surprising. Only three months ago one of them expressed the following sentiments in a paper read before the chemical section of the British Association. [1]* * * "Those who feel that the electron is possibly" (note the possibly) "but a figment of the imagination will remain satisfied with a symbolic system which has served us so long and so well as a means of giving expression to facts which we do not pretend to explain. * * * Until the credentials of the electron are placed on a higher plane of practical politics, until they are placed on a practical plane, we may well rest content with our present condition and admit frankly that our knowledge is insufficient to enable us even to venture on an explanation of valency." Think of it! We, the chemists, "remain content" in this day when, as the Hon. A. J. Balfour has said, the attempt to unify physical science and nature [2]"excites feelings of the most acute intellectual gratification. The satisfaction it gives is almost

[1]Scientific American Supplement. 63, No. 1761. P. 210, Oct. 2, 1909.

[2]"Reflections Suggested by the New Theory of Matter." Presidential Address, British Association for the advancement of Science, 1904. Science. 20 No. 504, pp. 257-266, Aug. 26, 1904.

aesthetic in its intensity and quality. We feel the same sort of pleasurable shock as when from the crest of some melancholy pass we first see far below the sudden glory of plain. river and mountain." "Rest content!" No wonder the Noebel prize in chemistry was awarded to Rutherford. a physicist.

As to the second principle, the conservation of energy, some have had misgivings. It was Kelvin, I believe. who said that radium placed the first question mark after this great principle. Many have refused to believe in the electron and disintegration theories because they saw, or thought they saw, in these theories a contradiction of the principle of energy conservation. Personally I do not see that there are necessarily any contradictions. But even if there were and we were therefore justified in rejecting the theories proposed to explain the facts. we certainly should not be justified in rejecting the facts themselves.

In this connection I am reminded of the story of a lawyer whose client was placed in jail for some very trivial offense. When the lawyer learned the nature of the charge he said to his client: "My friend, they cannot put you in jail on such a charge as that." "Yes. but they have." replied the prisoner. When our physicist says that radium cannot remain at a higher temperature than its surroundings and continue to radiate heat, as that would be contrary to the second law of thermodynamics, the answer is, Yes, but it does. When he says that it cannot continue to radiate energy without receiving energy from some other body. as that would be contrary to the principle of the conservation of energy. the answer is, Yes, but it does it.

When some one says that helium or carbon dioxide cannot appear in sealed tubes which contained no trace of these substances to begin with, the answer is, Yes, but they do.

Let us suppose that we have a mass of gunpowder and that it is possible to, and we do, cause it to explode. one grain at a time. each grain firing its neighbor as in the fuse of a firecracker. The temperature of the mass of gunpowder will be higher than its surroundings, and it will give off heat and other forms of energy and continue to do so as long as the powder lasts. No one would think of calling this an exception to the law of the conservation of energy or the second law of thermodynamics. The source of the energy is the atomic potential energy of the powder itself.

Let us suppose that we have a sphere with frictionless surface rotating at an enormous speed. Suppose that particles of matter are thrown off at frequent intervals. These particles, on account of their high speed. have considerable potential energy. Thus the sphere continues to give off energy without receiving any as long as any mass remains. The source of the energy is the kinetic energy stored in the sphere at the outset, of which energy we are conscious only when we have some method of detecting and slowing down the projected particles.

Thus the energy radiated by radium might be stored within the radium atom as potential energy and liberated by a sort of atomic—or subatomic—explosion. Or it might be stored as kinetic energy—of revolving electrons—and liberated gradually as these electrons escape from their orbits. It might be stored in both forms. In any case it is intra-atomic energy because stored *within* the atom itself and liberated only by atomic change—disintegration. In neither case would there be a violation of the principle of the conservation of energy or of the second law of thermodynamics. Sooner or later all the energy will have been radiated. The fact that the supply is destined to last so long is what appeals to us as wonderful. And so it is. The world is full of wonderful things to anyone who pauses long enough to think.

In this paper I have endeavored to give a general notion of the trend of thought and investigation in physical science rather than an enumeration and discussion of discoveries and theories. I might say, however, that there are strong reasons for believing in the molecular structure of electricity the electrical nature of matter, and the dependence of mass upon velocity. The theories of radioactivity and disintegration of matter are fairly well established. According to Ramsay, one of the most eminent chemists in the world, "we are on the brink of discovering the synthesis of atoms, which may lead to the discovery of the ordinary elements." Perhaps the dream of the alchemist is about to be realized. Certain it is that we are face to face with energies of which no one even dreamed a few years ago. Whether we call this energy intra-atomic, sub-atomic, interelemental or some other name, we know certainly that it exists, and that it exists in quantities far beyond the power of man's mind to comprehend. Man hopes some day, somewhere, somehow, to discover the means of unlocking this infinite storehouse. If this discovery is ever made, all the others which man has ever made will pale into insignificance beside it.

Lodge says of the one-pound shot and the one-hundred-pound shot which Galileo dropped from the top of the Leaning Tower, that "their simultaneous clang as they struck the ground together sounded the death knell of the old system of philosophy and heralded the birth of the new." The age of reverence for authority had passed away and the day of experimental investigation had dawned.

In a sense the discoveries of the past few years have resulted in a similar revolution. The revival of the experimental method has been complete. Accepted theories are being put to the test. What we have long regarded as proven facts are being questioned and, in many cases, challenged. There is no field of investigation which has not been cultivated anew.

In closing I wish to quote from the presidential address of J. J. Thomson[1] before the British Association at its last meeting. "The new discoveries made in physics the last few years, and the ideas and potentialities suggested by them, have had an effect upon the workers in that subject akin to that produced in literature by the Renaissance. Enthusiasm has been quickened and there is a hopeful, youthful, perhaps exuberant, spirit abroad which leads men to make with confidence experiments which would have been thought fantastic twenty years ago. It has quite dispelled the pessimistic feeling, not uncommon at that time, that all the interesting things had been discovered, and all that was left was to alter a decimal or two in some physical constant. There never was any justification for this feeling, there never were any signs of an approach to finality in science. The sum of knowledge is, at present at any rate, a diverging, not a converging series. As we conquer peak after peak we see regions in front of us full of interest and beauty, but we do not see our goal, we do not see the horizon; in the distance tower still higher peaks, which will yield to those who ascend them still wider prospects, and deepen the feeling, whose truth is emphasized by every advance in science, that 'Great are the works of the Lord.' "

[1]Scientific Am. Sup. 63, Nos. 1757 and 1758, pp. 154, 155 and 174–176. Sept. 4 and Sept. 11, 1909.

Recent Progress in Botany.

By John M. Coulter.

Mr. Chairman and Members of the Academy: When I face the Indiana Academy of Science at its twenty-fifth anniversary, I feel more like speaking of old times than upon any technical subject. However, perhaps some of these reminiscences may appear at the banquet tonight, and I will restrict myself just now to the program.

It is very hard for one who has not lived and worked through the period covered by the history of this Academy to appreciate the changes that have taken place in the science of botany. Those of you who have come into the subject during the last decade can hardly have a full appreciation of what you have missed and of what rapid development has taken place. At the time this Academy was being founded, almost all the instruction and investigation in botany was in taxonomy or classification, and that was chiefly restricted to the classification of flowering plants. I shall not weary you by recounting all of the important changes that have taken place since that time, but I wish to point out a few things that have impressed me.

The first impressive change is the tremendous development and differentiation of the subject during the period covered by the history of this Academy. In the background we have still the old historic field of taxonomy, which is being cultivated with greater zeal than ever. But the first change to note is the great development of the comparatively new science of morphology. In these days morphology has come to mean the structure and evolution of the plant kingdom as a whole, and its development has been little short of marvelous. Perhaps the first change from the old régime was brought about in this country by the appearance of Bessey's Botany in 1880, and from that date began the development of modern morphology in the United States.

In connection with the development of morphology there have grown up various expressions of it that have demanded special technique. The first of these to appear was that which is known as cytology. In collecting the facts in reference to the cell as a unit of structure, morphologists soon discovered that something must be known about cell structure, and thus a very special technique has been developed and is still developing. Cytology might be defined, therefore, as morphology at the limit of technique.

In more recent years there has been another outgrowth from morphology and still a part of it. For many years there had been what was recognized to be a great rubbish heap of facts called anatomy. For example, the classic "Comparative Anatomy of Phanerogams and Ferns," by De Bary, contains a mass of facts, but they are inchoate. Many of them were used in instruction, for in the early days of morphological instruction facts were simply collected without reference to their relationships. Presently, as morphology began to develop ideas, it was felt that these anatomical facts might mean something when organized; but in the absence of such organization they were largely abandoned in instruction. Recently, however, there has been rescued from this rubbish heap the new subject of vascular anatomy, which has become a tremendous instrument in the development of our knowledge of plant groups and of the evolution of vascular plants in particular. Thus vascular anatomy has greatly extended morphology, which at first chiefly concerned itself with the reproductive structures. It still remains for some one to organize in a similar way the vegetative structures outside of the vascular system, and then morphology for the first time will have its facts fairly in hand.

Under the shadow of this morphological development there appeared another growth known as pathology. The progress made in plant pathology during the period covered by the life of this Academy is familiar to many of its members. It began as morphology, but as it progressed it became more and more clear that it would have to join itself to physiology, and so pathology may be called a cross between morphology and physiology in its recent development.

Another great field that came in connection with this development of morphology, even more recently, is paleobotany. There has been such a subject ever since people have uncovered plant remains and their impressions in the rocks; but its method was to match fossil fragments with living plants, so that identification was always uncertain. The technique of today, however, has enabled us to secure knowledge of structures, and since vascular anatomy has been put upon a phylogenetic basis we have a key by which the relationships of these ancestral plants may be unlocked.

I can only mention the remarkable advance that has taken place in plant physiology, and also in the new subject of plant ecology. There should be added plant breeding, which has not only its important scientific aspects in connection with theories of heredity and the origin of species, but has also such enormous practical applications that it is reaching out into the needs of men.

This gives merely a glimpse of how the old science of botany, as it really was when this Academy was founded, has branched out into its present field of achievement. The student of twenty-five years ago who had studied botany in our colleges and learned just enough about gross morphology to be able to use Gray's "Manual" intelligently, and who regarded that to represent all there was in botany, would be astonished to see the development of today.

Following this outline of the expansion of botany in general, I wish to speak of three or four of the most notable advances made in my own special region of morphology, and that is the morphology of vascular plants. To me the most striking feature of morphological progress during the last twenty-five years has been the breaking down of the old barrier set up between what were called cryptogams and phanerogams, the barrier that separated fern plants from seed plants. Not only was this felt to be a solid barrier, but even in universities chairs of botany have been distinguished on the basis of this division of plants. If there is any place in the whole series of plants where there is no gap between great groups it is this very place. I can call attention only to two conspicuous facts that stand out in this connection. One is the discovery a few years ago that certain gymnosperms (cycads) possess fern-like swimming sperms, a feature that associates these seed plants very closely with ferns. The second is the discovery during the present decade of the great paleozoic group of fern-like seed plants. All are familiar with the fact that the coal vegetation was thought to be largely a fern vegetation because the preserved leaves looked like fern leaves; but it is now recognized that all of these great frond groups of the coal vegetation were seed-bearing plants. In fact, paleobotanists are sure now of only one family of paleozoic ferns.

Another fact of equal interest is the uncovering of the so-called mesozoic cycads. These have proved to be far removed from the other gymnosperms in their essential characters. We have a sort of national pride in the uncovering of this singular group, because the greatest deposits are in this country. The work of Wieland in revealing the rich deposits of these plants in the Black Hills region and in sectioning the cones with admirable skill and patience is well known. For the last five months Wieland has been exploring southern Mexico, and has discovered a section 2,000 feet in thickness that is packed with the remains of this peculiar group, making it undoubtedly the greatest deposit of these plants in the world. They are regarded now as of great interest because the peculiar structure of their cones has suggested the possibility that they may be a group of gymnosperms that has given rise to angiosperms.

Perhaps another notable change that deserves mention is the practical demonstration of the relationship between the two groups of angiosperms. It was thought once that the monocotyledons were the more primitive angiosperms, and that the dicotyledons were the more recent. We feel assured now that the monocotyledons have been derived from dicotyledons, for every monocotyledon starts with the vascular system of a dicotyledon; and if there is anything true in the old theory of recapitulation, the relationship of these two groups is evident.

Perhaps the most notable change in morphology is the change in mental attitude, and particularly in reference to the construction of phylogenies. I remember that at the early meetings of this Academy we were in the habit of constructing very complete and satisfactory phylogenies. We were sure just how one plant group descended from another. That is always easy when the facts are few; but now that facts are numerous, no one is able to construct a satisfactory phylogeny. No one imagines now that any living group has descended from any other living group.

Another marked advance is the change of mental attitude in connection with morphological work, in which morphology has clasped hands with physiology. I can only indicate some of its results. It has destroyed the old rigid categories. Botany was once largely an extensive system of terminology. Now we have passed from the days of terminology to the days of knowledge, and terminology no longer masquerades as knowledge. Not one of the old definitions has stood the test of experimental morphology. Experimental morphology has also helped to rid us of that old, Calvinistic notion of predestination in plant organs. Once it was thought that every primordium was destined to be one particular structure and nothing else. Now we know that a primordium may become almost anything under appropriate conditions, and is not destined to be some particular structure.

One of the most interesting recent results of experimental morphology has been that obtained in experimental work on heterospory. It has been shown that it is possible to develop megaspores from cells that ordinarily develop microspores. It is such results that are playing fast and loose with our old conceptions of rigidity of structure and function.

I can merely mention the field of plant physiology. If I speak of the changes that have taken place within the last twenty-five years, I must show the atmosphere in which we are living by assuring you that I am not the one to make such a presentation. In the old days one man taught all there was of botany, and probably he taught all there was of science. Today I have been compelled to ask a competent plant physiologist concerning the notable changes. He tells me that there are two conspicuous changes in the point of view. One is the gradual passing of the old vitalistic idea, which implied that there was some such thing as vital force that explained most things. Now the facts are explained, not in terms of vital force, but in terms of chemistry and physics. Another shifting point of view is a change from the old idea that form and structure are the result of some mysterious law of development, to the idea that form and structure are entirely expressions of the conditions under which growth has been conducted.

The very new field of ecology at present is in the condition of these other fields more than a decade ago. Young fields are largely jokes to the older ones; but there has been a change in ecology during the last few years. It has passed from the stage of inchoate observation, in which instruction in ecology could not be differentiated with distinctness from a holiday excursion, to methods of precision.

In conclusion, as one looks out over this great progress, he finds that it is all really an inevitable evolution from the stimulus that was given first by Hofmeister in 1898 to morphology, and ten years later by Charles Darwin to biology in general.

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Darwin Fifty Years After.

By Dr. David Starr Jordan.

Scientific men, as a rule, do not pay much attention to birthdays; but certain anniversaries have been impressed upon our minds of late, and in the last two years there have been many celebrations: The two hundredth anniversay of Linnaeus, and the one hundred and fiftieth of his great work, "Systema Naturæ"; the one hundredth anniversary of the birth of Agassiz, the greatest teacher of science; the one hundredth anniversay of the birthday of Charles Darwin, and the fifth anniversary of the publication of "The Origin of Species," the greatest landmark of the history of the nineteenth century. Twenty-five years ago we note another landmark of import to us. It was then that Amos Butler brought his Brookville academy to Indianapolis, where its first meeting was held on December 29, 1885. As I was just then elected president of Indiana University, the youngest of all the college presidents—and the greenest—being, therefore, by some preferred to the drier article, I was made president. With this came the suggestion that two others who, like myself, had fought each year on the bloody sands of the educational arena of Indiana—John Coulter and Harvey Wiley—would be my successors.

At that time the idea of evolution was in the air, the theory of descent, that the forms now living were created, not by mysterious power, but by the operation of natural selection and the survival of the fittest. It was my fortune to have been brought up as a student of Agassiz, having heard all his lectures on this subject, and inherited his prepossessions. It was my own studies of animals which led me little by little to become an evolutionist, and I have said that I went over to that view of the case about as graciously and as willingly as a cat which a boy draws across the carpet by its tail.

I remember it was out at Broad Ripple, just north of this city, where Copeland and myself first definitely decided that we were converts to Darwinism. The little sand darter in the river is a sort of perch, but differs from any others in having very few scales, and these very thin ones. We testified to our faith by an article in which we said that these little animals are derived from the scaly perches; that we did not know whether it has lost its scales because it buries itself in the sand and does not need them, or whether it buries itself in the sand because it has no scales and needs protection, or whether burying itself in the sand there has come to be a gradual selection of those whose scales are fewest and thinnest. Anyhow, we were sure of its origin, and that it was descended from some of the other forms of dwarf perch to that called the Johnny Darter.

Many men before Darwin had taught the theory of descent, but Darwin gave the first rational exposition of how it came about by natural processes. He showed that adaptation is the natural result of the survival of the adapted in the struggle for existence. Variation is everywhere among animals and plants. No two animals or plants are ever alike. There is everywhere a great wealth of life—more are born than can mature, and those survive and live who are able to fit themselves into the scheme of life. Darwin did not believe in evolution in vacuo, that is, evolution wholly independent of external circumstances and conditions, but this heresy that the laws of evolution, which are simply the way things come about, can produce evolution and divergence without any except metaphysical causes, still has a large body of followers. It is, in my judgment, one of the heresies of the present time.

In the evolution of any species in the rough-and-tumble of life, we have these four elements: Variation, heredity, selection and segregation. Variation is the starter. It is interwoven with the operation of heredity. The favorable variation survives, and the animal or plant possessing it gives rise to the next generation. This is selection.

The operation of isolation is this: A group becomes separated by some barrier which the individual can not cross. Little by little the species become separated into two or more species, one just as well adapted as the other. It is not often that differences between species are differences in adaptation. It is therefore not often that they are due to natural selection. The final difference, the final polishing or rounding off of the species giving it its distinctive minor character, is due to isolation. Variation and heredity are inside the individual. The incidents of selection and isolation are of the outside world. They are part of the modifying conditions of life. Without contact with the outside influences, in my belief, there is no evolution.

Darwin may be compared to an explorer in a new country. From some high point he makes a map of the country, locating its salient features, its rivers, lakes, peaks and cliffs. The detail must be worked out by those who come after. In the case of Darwin the map remains substantially as it was, although many have worked at the various details with which the modern chart is filling up. The discovery of the microscope has enabled us to frame a rational theory of heredity and to understand with some degree of certainty the physical basis of the functions of inheritance. The morphology of animals has been very fruitfully studied by many men. Many others have developed the history of past life on the earth, and we would have to have a theory of evolution to account for this, if Darwin had not furnished one already.

The three men most famous since Darwin are these: Wagner, Weissmann and Mendel. Mendel died before Darwin wrote and his work on the "Heredity of Peas" was forgotten until after Darwin's time, but has become a very important factor in our experimental studies of living forms in relation to inheritance. Wagner was the first one to lay adequate stress on the idea of isolation as a species-forming influence. His weakness was that he rejected selection as an element, assigning to isolation the impossible task of accounting for all the external phenomena in the origin of species. To Weismann we owe more than to any one else our present knowledge of heredity.

Theories of less importance are Eimer's orthogenesis, which has a good deal behind it, and which we shall probably accept if some genius will arise to tell us what it means. It rests on the fact that we have many long series of animals which seem to have progressively varied as time went on.

The study of the mutations of the evening primrose by De Vries has given many hints as to possibilities in plant breeding. I do not believe that the theory that species are mainly or largely formed by sudden mutations will survive the present generation of De Vries' followers, but the impulse given to experimental study of plants will long continue.

More than thirty years ago I used these words in Indianapolis:

"Darwin lies in Westminster Abbey, by the side of Isaac Newton, one of the noble men of the past whose life had made his own life possible. Of all who have written or spoken, by none has an unkind word been said. His was a gentle, patient and reverent spirit, and by his death has not only science, but our conception of Christ, been advanced and ennobled."

Methods and Materials Used in Soil Testing.

By H. A. Huston.

The consumption of commercial plant foods in the United States has reached approximately 5,000,000 tons and the cost to the consumer is nearly equal to the sum which we formerly paid for imported sugar, and which became the slogan in the campaign to establish the beet sugar industry in America—$100,000,000.

The industry is established, but by no means stationary. It has increased at least 50 per cent. during the past five years, a very high rate considering the magnitude of the business.

In the manufacture and control of these products there is employed a large number of chemists, and the Association of Official Agricultural Chemists, now over a quarter of a century old, was originally formed for devising suitable methods of analysis for these products. Thirty-three States have special laws for fertilizer inspection. The American Chemical Society recently organized a Division of Fertilizer Chemists, and most of our agricultural colleges and experiment stations devote a considerable amount of attention to the subject.

The farmer wants to know the facts about commercial plant foods and all officialdom, from the bureau chiefs of the National Department of Agriculture to the local speaker at the township farmers' institute, undertakes to enlighten him.

In those sections of the country where fertilizers have been longest used—along the Atlantic, the eastern gulf coast and the upper Ohio Valley—the experiment stations and control officials appreciate the magnitude and importance of the industry and understand its vital relation to crop production. In marked contrast to this is the state of affairs in the greater part of the great area drained by the Mississippi, where the most of our maize, wheat and oats are produced. Here we find also the curious combination of land rapidly increasing in money value and at the same time declining in productiveness, while the cost of farm labor is increasing. These circumstances cause the farmer to inquire how his crops may be increased and whether commercial plant foods may be profitable in this connection.

Some thirty-five years ago the winter wheat growers of the Ohio Valley began to use fertilizers, most of the material being the side products of the packing houses, mainly bone meal. Very profitable results were secured and the trade rapidly increased. In time acidulated goods were introduced, often being mixtures of equal parts of acid phosphate and bone. Later came the "complete" fertilizer, being ammonia 2, available phosphoric acid 8, and potash 2 per cent. This is still the so-called basal formula, that is, the one used as a starting point in calculating the trade value of goods with different formulas. About two-thirds of the fertilizer used in that section consist of complete fertilizer; the use of bone and ammoniated phosphate is declining and the use of mixtures of acid phosphate and potash is rapidly increasing. Common applications for wheat are from one to two hundred pounds per acre, and it is almost invariably applied with a fertilizer attachment at the same time the seed is sown. The efficiency of the fertilizer in securing a stand of clover, the seed of which is sown before the wheat starts its spring growth, is a point to which the farmers attach considerable importance and the increase in clover production may in part account for the reduction in the amount of nitrogen in the fertilizers now used as compared with that used at an earlier period.

The use of fertilizers gradually extend to other crops, but fully two thirds of the fertilizer sold in the Ohio Valley are used on winter wheat. The general tendency in composition has been to reduce the nitrogen and increase the potash, while the phosphoric acid has remained practically unchanged. Ready mixed brands are the rule, home mixing the rare exception.

It is, however, unnecessary to state that much of this plant food has been used in a most haphazard way and that both buyer and local seller knew little about the composition of the goods sold or their fitness for the crop or soil on which they were to be used.

The one thing which stood out very clearly was that they paid; that by their use good crops of wheat could be secured where unprofitable crops grew before; and that a stand of clover or grass could be secured, a suitable rotation of crops established and maintained, and that the cost of the fertilizer was returned many fold in the increase of wheat grain alone. Ten pounds of fertilizer costing from ten to fifteen cents produced on the average an increase of a bushel of wheat. This condition exists over much of the winter wheat belt extending from Kansas east and comprising an area of probably 200,000 square miles. These facts have existed too long and cover too much territory to be ascribed to local peculiarities of soil or season. The wheat grower knows that fertilizers pay. But as brands multiplied the question arose which is the more profitable, and many made simple tests of different brands in which the popularity of the local agent received more consideration than the amount and kind of plant food in the goods; they obtained the confusing results that might have been expected under these conditions. Better informed farmers applied to their experiment stations and agricultural colleges for aid, and in most cases were surprised to be told either that commercial plant foods did not pay or that they were unnecessary.

An examination of the records of field tests conducted by experiment stations in the winter wheat section shows that many experiments have been made, especially on wheat, and that most of them have been reported unprofitable. This apparent conflict between the results of practical and scientific agriculture has to some extent prevented the extension of the sale of plant food to territory where it was very much needed. One may fairly inquire why the results of the experimental field tests differ so widely from the results obtained in ordinary farm practice in the same sections.

First, we may consider certain things that are general in their nature. Many experiments are reported where relatively heavy applications of farm yard manure have been compared with applications of various brands and quantities of fertilizers without any clear statement or apparent knowledge of the composition of the latter. Such experiments are almost invariably reported as showing that manure is more profitable than the fertilizer, which is not strange in view of the fact that in the valuations the full cost of the fertilizer is charged up, while to the manure is charged only the cost of hauling. In such reports there is often a very clear intimation that the result is quite in line with the preconceived notions of the experimenter and that in discouraging the use of "expensive fertilizers" he is at least telling farmers what they like to hear even though it conflicts with what they need to know.

The method of application of the plant food is in many cases responsible for a considerable part of the difference observed between field practice and plot experiments. Application with the drill at the time of sowing small grains, which is the common method, frequently gives profitable results when the same amount and kind of fertilizer applied broadcast is unprofitable, and the same remark applies to light applications on maize.

One of the principal reasons for unprofitable results from plot tests is found in failure to make a distinction between the fertilization of crops producing high money values per acre, like truck and fruit, where the whole plant food supply may be profitably secured from chemical manures, and such crops as wheat, oats and maize, where the chemical fertilizers must be used to supplement and balance the supplies from the soil, farm yard and legume field. The cost of full rations of commercial nitrogen can only occasionally be recovered in the wheat crop and rarely if ever in the case of oats and maize. Double rations of phosphoric acid are often profitable and from one-half to full rations of potash. In most of the early plot experiments full rations were used, and sometimes the cost of the fertilizer for maize was greater than the total sum received for the crop even when the yields were good.

Perhaps the contrast between the plot tests and the farm practice can be shown better in the form of the amounts per acre and the formula. In some of the wheat plot tests extending over twenty years the fertilizer is the equivalent of 500 pounds per acre of goods having formula of nitrogen 10 per cent., phosphoric acid 5 per cent. and potash 6 per cent.; at the same time this series was started the common wheat fertilizer was 100 to 200 pounds per acre of 2-8-2, which has gradually changed to 2-8-6; nitrogen is sometimes increased to 3 per cent. The maize series of plots received the equivalent of 1,000 pounds per acre of a goods having a formula of nitrogen 12 per cent., phosphoric acid 4 per cent. and potash 6 per cent., while farm practice on maize uses 100 to 300 pounds per acre of goods having little or no nitrogen and containing from 5 to 10 per cent. phosphoric acid and 4 to 10 per cent. of potash. For clay soils a common maize fertilizer is 0-10-4, for loams 0-8-8 and for black sandy soils 0-6-10, while on the peat or muck soils 100 pounds per acre of muriate of potash or its equivalent in kainit are commonly used. A small amount of nitrogen is sometimes added, usually about 1 per cent.—rarely over 2.

The cost per acre of the maize fertilization would be about $30 for the plot work and from $1 to $4 per acre for the fertilizers commonly used. The cost per acre of the wheat fertilization would be about $15 for the plot work and from $1 to $3 per acre for the fertilizers commonly used.

In general it may be said that the fertilizers used on wheat and maize furnish about as much phosphoric acid as the crop removes, rarely as much as one half ration of potash and never over one-fifth ration of nitrogen, while the plot experiments have undertaken to supply full rations for a full crop, which is fully double an average crop.

The quantities of fertilizer used in the plot tests mentioned above seem quite absurd to the American grain grower, yet they are very conservative compared with another set inaugurated at about the same time in which 2,000 pounds of acid phosphate, 600 pounds of sulphate of potash and 600 pounds of sulphate of ammonia per acre were used, or with an extensive set of orchard experiments in which the plans called for the application of 40 pounds of muriate of potash with corresponding amounts of nitrogen and phosphates to each two year old tree.

In the case of the plot experiments conducted for the purpose of determining the value of the different plant foods, the excessive quantities have often caused a profit to be shown for only the particular plant food which was most deficient, while if more reasonable quantities had been used each would have shown a profit. It is not unusual to find reports of these experiments that recommend the use of a single plant food as all that is necessary merely because it was the one that chanced to give the largest profit.

As compared with this line of plot experiments with full rations we may, perhaps, devote a moment to results of plot experiments where amounts and formulas generally used in farm practice were taken as a basis.

On a typical worn clay wheat land an experiment was undertaken on the basis of 300 pounds per acre of goods containing nitrogen 3 per cent, available phosphoric acid 10 per cent and potash 6 per cent, each element being omitted in turn in the usual way.

The following results were obtained:

Fertilizers applied per acre. Equal to—	Yield, bushels per acre.	Reduction from Omitting			
		Nitrogen.	Phos. Acid.	Potash.	All.
300 lbs. 3–10–6	33.8	......	......	....	....
300 lbs. 0–10–6	29.1	4.7	......	....	....
300 lbs. 3– 0–6	7.6	......	26.2	....	....
300 lbs. 3–10–0	25.0	......	......	8.8	....
None	6.5	......	......	....	27.3

The nitrogen in the fertilizer cost per acre	$1 80
The phosphoric acid cost per acre	1 50
The potash cost per acre	1 10
The complete fertilizer cost per acre	$4 40

The nitrogen increased the crop 4.7 bushels at a cost of $1.80, the phosphoric acid increased it 26.2 bushels at a cost of $1.50, while the potash increased it 8.8 bushels at a cost of $1.10. As wheat sold at 90 cents per bushel it will be seen at a glance that all the plant foods were used at a profit, although, of course, we are not in a position to show that the combination is the one most profitable. Nor do we know that this was the most profitable amount. We do know that it was very profitable even neglecting the value of the increase in the straw and the very striking effect on the clover which followed the wheat.

The experiment is a typical one for soils in the winter wheat belt, and numerous others could be given showing results of just the same character and even more striking in profits.

The figures show how the lack of phosphoric acid limited the crop, and they serve to explain why bone gave such increases on these soils that for nearly a generation it was considered the only profitable thing to use.

In another series at a different place the amounts of the plant foods were varied, but the season was so unfavorable that the crop was limited by other considerations than plant food, the maximum crop being only about 13 bushels per acre and that of the unfertilized plots being only 2 bushels.

In these experiments the nitrogen is supplied from blood, the phosphoric acid from precipitated calcium phosphate free from gypsum, and the potash from muriate of potash, the purpose being to use materials exerting as little indirect effect as possible.

This matter is too often overlooked in planning such experiments, and for a considerable time the indirect effects may be so great as to mislead one who does not take them into consideration. Thus the gypsum in ordinary acid phosphate, amounting to about one-third of its weight and the sodium in the nitrate, may each release so much potash from zeolites in the soil that the plot with nitrate acid phosphate and potash may show little if any increase over that with nitrate and acid phosphate. Comparatively few experiments exist which have been conducted long enough and in such a way as to shed much light on the extent to which the indirect effects mask the direct effects.

In such cases one always turns to the admirable work at Rothamsted for help and the constantly increasing difference between the yields of plots 11 and 13 Broadbalk Field seem to show that the indirect effects are decreasing. The gypsum alone on plot 11 would theoretically release 90 pounds per acre of potash annually while the total annual application of potash on plot 13 is 100 pounds. The theoretical amount of potash that could be released by the bases in the minerals used on the fully fertilized plots at Rothamsted amounts to about 400 pounds of potash per acre annually while the potash applied in sulphate amounts to 100 pounds. While Director Hall has clearly pointed out the difference between the early years and the later, too many who use Rothamsted results to fortify their arguments simply take the average for the whole period and neglect to consider the results by decades.

Especially when we wish to secure indication of soil needs as promptly as possible should we take pains to use materials that will exert as little indirect effect as possible. By using blood as a source of nitrogen and gypsum free precipitated phosphates as the source of phosphoric acid we can remove most of these indirect effects and at the same time use materials easily secured and of high availability.

Another point that is never considered in planning the plot tests in the section under consideration is the marked difference in the fixing power of soils for plant foods and the firmness with which they hold them. This is roughly recognized in providing for an excess of phosphoric acid in commercial formulas but is seldom considered in plot tests.

The plot tests in most cases have simply been copied from plans made before the nitrogen gathering power of bacteria associated with legumes was understood and sometimes altered because of the injurious effect of the excessive nitrogen applications or too often abandoned altogether because the growth of the institution demanded the land for other purposes. The frequent changes in the staff of workers has also interfered seriously with both the conduct of the work and the interpretation of the results.

The conditions in the winter wheat section of the United States are such that large crops must be produced in order to realize a suitable return on the selling value of the land and the money spent for farm labor. The small grain crops are so related culturally with the clover crop that they are almost necessary in a rotation if we expect to utilize our most widely distributed legume as a source of nitrogen.

The chemical industries supplying plant foods and the purchaser of these products would both be greatly benefited by the inauguration at our experiment stations in the grain growing section of experiments properly planned to solve the question of the most profitable method of supplementing the plant food resources of the farms.

Up to the present time it must be confessed that the purely empirical methods of the fertilizer manufacturers have produced results that yield the farmer better returns than anything derived from the experiments started under the old system by the educational institutions in the grain growing section, but these are far from being the best obtainable. Both farmer and fertilizer manufacturer need the help of the educational institutions in the direction of securing facts relative to the most profitable methods of utilizing plant foods in the production of our great cereal crops —facts that will help and not discourage.

But such experiments must take into consideration

The kind of materials to use,

The avoidance of indirect effects,

The right methods of application,

The question of the most profitable amount, and finally

The rational interpretation of the results obtained.

German Kali Works, Chicago, Ill.

Federal Control of International and Interstate Waters.

By Barton W. Evermann.

Mr. President, Members of the Academy—I shall talk a very few minutes on this subject. The idea of federal control in matters pertaining to fisheries and game is a recent one, and one of recent and gradual development. I think perhaps the idea was first advanced in connection with the control of migratory birds. Ornithologists and others interested in the preservation of birds realized a number of years ago that the state laws of the various states were inadequate for the control of migratory birds. A bird today is in Louisiana or Alabama, tomorrow in Tennessee, next week in Kentucky, then Indiana, then Michigan, and the game laws in the different states are different. In some of these states there would be a law adequate for the protection of migratory birds as they went north or south, but in the next state into which they went there would be no law, so that migratory birds received very inadequate protection or no protection at all.

The first bill that was introduced into Congress that had any bearing on this question was introduced by George Shiras III, of Pittsburg. In this bill he proposed that the Federal government should take over the control of the regulations for protecting migratory birds. A little later the idea expanded and Mr. Shiras introduced a bill in Congress providing for the protection of migratory fishes. His attention had been called to the fact that in the Atlantic coast States there is no law adequate to protect the shad and other migratory fishes. The difficulty existed in all of the streams where migratory fishes came, but particularly in those streams which lie between two States and which are controlled by two or more States. The Potomac River was taken as an illustration. The laws of Virginia on one side and Maryland on the other were never the same, and at the same time it was legal to fish in one State and illegal in the other. The inevitable result was a series of evasions of the law by the fishermen of these States.

The Columbia River is another illustration, perhaps the most serious of all. There you have Montana, Idaho, Washington and Oregon, all concerned in the Columbia River. Idaho and Montana are not seriously interested in the salmon, but Washington and Oregon are both vitally interested in the salmon fisheries of that stream. But these two States have never been able to agree upon concurrent legislation which adequately protects the fisheries, and things have gone from bad to worse. Two years ago an effort was made by certain people interested to restrict the taking of salmon in the upper Columbia by cutting out the use of certain kinds of apparatus. This matter was referred to the people in Oregon, and at the same time those who were interested in the fisheries in the uppr Columbia had a similar question submitted to the people stopping fishing in the lower river, and a very curious result followed. The people said it would be a good thing to restrict fishing in both parts of the river, so both amendments carried, and the inevitable result followed that neither is enforced, illustrating very clearly the impossibility of two or more States agreeing upon adequate measures in questions of that kind.

Then the question came up as to the control of the fisheries in international waters. The question there has for many years been a serious one, particularly on Lake Erie. That lake has abutting on it four States on this side of the line—Michigan, Ohio, Pennsylvania and New York—and the province of Ontario on the other—five political units that are all interested in the fisheries of Lake Erie, and no two having the same laws, so that at one time it would be legal to fish at a certain distance from the shore and with certain apparatus off that narrow portion of Pennsylvania which fronts on Lake Erie, and just beyond that narrow strip in Ohio or New York it would be illegal, and there was constant difficulty to keep the fishermen of one State within the strip in which they had a right to fish; and the regulations on our side were in every case entirely different from those on the Canadian side, so that friction followed there. It was impossible for the individual States to handle this question, and in that way the question of federal control came up.

In addition to these questions, and of more recent development perhaps, has come the question of the desirability of federal control of interstate waters and other waters in the matter of public health. We have a good illustration of the necessity for this in the Potomac River. Washington City has sometimes suffered from an epidemic of typhoid fever, and investigation has shown again and again that the source of infection was not in the District of Columbia, but was brought from some place else; and carrying the investigation still further it has been proved on more than one occasion that Cumberland, Maryland, is responsible for at least some of the typhoid epidemics at Washington. The waters of the Potomac become infected at Cumberland, many miles above Washington, and the germs are carried from there and people infected. The District of Columbia, of course, is absolutely powerless in the premises; it can do nothing. The State of Maryland has done nothing, and the outlook is not encouraging. I do not believe Maryland will do anything to remedy the difficulty. It affects not only the District of Columbia, but every town between Cumberland and the District of Columbia, so that in that case the matter of public health is concerned in Maryland, the District of Columbia and Virginia.

A little more than a year ago the United States and Great Britain entered into a treaty providing for the appointment of an international Fisheries Commission, with power to draw up regulations governing the fisheries in international waters between the United States and Canada. That treaty specified the waters—from Passamaquoddy Bay on the east to Puget Sound on the west—taking in all of the Great Lakes except Michigan. As I see it, the principal point, the principal necessity for that treaty was to secure a set of uniform regulations for these waters. Under it, fishing on one side, in Canada, and in Ohio, Pennsylvania or New York, on the other, as far as Lake Erie is concerned would be the same. There would not be the conflicts which now exist. It does not seem to me that that treaty was necessary in order that the Federal government might take control of the fisheries in these waters, and for some reasons it would have been better if they could have brought about federal control of fisheries in these waters without entering into a treaty between the two countries. There may be some little risk in giving a foreign nation a hand in determining what shall be the regulations in the waters of Ohio, of Michigan, Pennsylvania or New York, and make it impossible for the United States to change the fisheries regulations on our side of the line without the consent of another country. But that may be laid aside as a matter of secondary importance.

One of the first men to become interested, to recognize the importance of the question of federal control in these matters was George Shiras III, a grandson of Chief Justice Shiras, an angler, sportsman and all-round naturalist, who is very much interested in the preservation of game and migratory birds. He first become interested in the protection of migratory birds, then fishes, and then in the larger question of all animal life in the streams which cannot receive adequate protection from individual States, and from that he has taken up the question of pollution of streams, and it has been shown by him and by others that the Federal government always had power to control interstate and international waters in all matters of navigation and fisheries and public health, because these three questions are larger than the interests of individual political units. The Federal government has exercised that power in the matter of navigation, but it has never exercised it in matters of fisheries or public health—the pollution of streams. But that it has that power and can exercise it whenever it wishes to do so, and that it is perfectly constitutional, I have no doubt in my mind, and I think the time is coming soon when it will be done. In this day when the question of public health is being agitated and considered so seriously, and when we understand more fully than we ever did before the sources of disease epidemics, when we realize more and more that the question is broader than the boundaries of a single State, it is clear that this question is a question which must be handled by the Federal government and cannot be handled by the individual States.

In the treaty between the United States and Great Britain, as you doubtless know, President Jordan was appointed commissioner representing the United States, and Prof. Edward E. Prince to represent Canada, and these two commissioners have gone over the boundary line from St. Johns to Vancouver, and at the end of last May they submitted their report to the respective governments, a report embracing a set of recommendations—some sixty-six in number—which they hope will control in a satisfactory way the fishing in international waters. That report will be made public, doubtless, soon after Congress meets. It will go to Congress and to Parliament, where the necessary provisions for enforcing these regulations will be provided. As it now stands, Canada already has the machinery which is needed to enforce the regulations on her side of the line. She has a very efficient system of patrol, facilities and men and means to enforce her fisheries regulations far better than they are enforced on this side of the line, particularly in Puget Sound. There is no such machinery on this side of the line for enforcing any set of fisheries regulations, because the matter has been and is now in the hands of the respective States. Each State has its own machinery; but under the terms of the treaty it would seem that the Federal government is morally bound to provide the necessary machinery for doing as well on this side of the line as Canada is doing on the other.

Now, if it turns out, as we believe it will, that this is the beginning of federal control in all of these large and important streams, then will come federal control not only of international waters, but interestate streams, and in all matters of pollution of any and all streams.

Mr. Shiras cites a number of cases: The State of Missouri vs. Chicago Drainage Canal, in which the decision of the court showed that the question is one larger than the State of Illinois and the State of Missouri, and that the Federal government must take it up. A similar case, Kansas City vs. The State of Colorado, the decision of the court pointed to the same view. And there is every reason to believe that the Supreme Court will uphold these decisions.

Bureau of Fisheries,
Washington, D. C.

The Speed of Migrating Salmon in the Columbia River.

By Chas. W. Greene.

(Abstract.)[1]

In the solution of this problem I devised a scheme whereby individual fishes could be given individual tags that would render identification absolutely certain if the fish should be recaptured. This plan was nothing more or less than the use of the conventional stock-marking aluminum buttons. These buttons are light and cannot be torn apart and they carry serial numbers on one face; on the other can be placed such special marks as one may select.

On August 14, 1908, I marked fifty-nine fish at Sand Island, just within the mouth of the Columbia River. These fish were liberated in the river in the hope that some would be retaken, and thus we might glean the story of their migration. The fish were marked by inserting numbered buttons through the caudal fin.

Seventeen of the fifty-nine fish liberated were retaken and reported to me; sixteen buttons were also returned to me. The fish were retaken along the river from a point four miles below where they were liberated up to the Dalles of the Columbia, just below Celilo Falls, a total distance of two hundred and fourteen miles. Near the upper limit quite a number of fish were taken and six of these had traveled a distance which, when rated, gives an average individual speed of from six and one-third to seven and one-half miles a day.

The following table is constructed to show the actual time from liberation to recapture, the distance covered, the probable time consumed in the straight-away run on a basis of the speed of number 76 (seven and one-half miles), and the days unaccounted for. My view is that these unaccounted days are chiefly spent in the lower estuary of the river in becoming acclimated to the fresh water.

[1] This investigation was undertaken in cooperation with the United States Bureau of Fisheries. This abstract is published by the consent of and with the approval of the U. S. Commissioner of Fish and Fisheries.

Species and Number.	Distance Traveled in Miles.	Days Out.	Days Required to Cover the Distance at a Speed of 7½ Miles a Day.	Days Unaccounted for, i. e., Available Acclimatization.
Silver 76	210	28	28	0
Silver 75	210	29	28	1
Silver 89	210	30	28	2
Silver 79	210	33	28	5
Silver 97	210	33	28	5
Steelhead 124	210	33	28	5
Chinook 80	15	11	2	9
Steelhead 98	210	52	28	24
Steelhead 125	70	35	9	26
Chinook 123	15	31	2	29
Silver 87	70	57	9	48
Chinook 113	4	6	0	6

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Columbia, Mo.

Thought Stimulation: Under What Conditions Does It Occur?

By Robert Hessler.

This is a subject of interest to nearly every one, but more especially to educated persons, as I found in discussing it with several hundred individuals. In a general way one may divide human beings into two classes: the educated and the uneducated. The uneducated usually pay but little attention to what is going on in the mind, what sort of thoughts they have, while on the other hand those who write or otherwise utilize their thoughts may pay much attention to the subject. Indeed, the latter may at times be worried because they can not think and cannot write, or because they "run out of" thoughts and vainly "rack their brains" for new ones. In the very beginning we must distinguish clearly between getting new thoughts or new ideas and the ability to write them out. In other words, to get the germ or plot of a story and then to write out the story to best advantage are two widely different things.

The difference between these two classes of individuals is shown very strongly in the matter of dreams. The ignorant pay considerable attention to their dreams, but only from the standpoint of "What does it mean?" They look upon a dream as an omen, while a writer may utilize a dream as a plot for a story, the dream being of actual value to him. Poets constantly tell about their dreams and of having dreamed. Again, we see this difference in attitude in the matter of the subconscious or automatic action of the mind, especially at night. There may be a great rush of thoughts. Many worry simply because they are unable to sleep on account of the "curious thoughts," while a writer may jot down a number of them and utilize them in his work.

The subject of thought stimulation may be studied from different standpoints, depending on the individual's occupation and training and the object of his investigation. Thus, the psychologist may approach it from a standpoint entirely different from that of the neurologist or of the alienist, while the viewpoint of a story writer may differ from all others. My standpoint may quite naturally be said to be mainly that of a physician interested in a study of chronic ill health as distinguished from actual disease. Many of the symptoms occurring in chronic ill health relate to disturbances in mental functioning, and hence must be given considerable attention.

If a physician desires to study normal individuals, that is, those who are neither sick nor diseased, he must go after his material. And here I might say that some, knowing my profession, have accused me of "talking shop." It is of course only those in search of a physician's services who come to him—and this paper may therefore be regarded as that of a seeker after knowledge, that is, a plea for more data from these in health. I hope that when it appears in the published proceedings some at least will take sufficient interest in the matter to give me their experiences and observations.

In regard to what I say here, it should be understood that this is simply a short abstract of a longer abstract. If I were to bring together all my data, and especially my case reports, they would make a large volume.

Classification of Data. For the purpose of classification as well as for convenience of study, I have divided my notes under several general subheads, as follows:

1. Simple observations on thought or mental stimulation before my days of medical schooling, such as any one not paying special attention to the subject might make.

2. Early days of medical practice. These notes are also rather simple, for it should be understood that in times past a medical student's attention was not called to the subject of mental influences.

3. Notes gathered while working among the insane.

4. Travel notes while in Europe, among them many relating to the environment of noted men and women, particularly of writers.

5. Notes, covering the last ten years, based on a systematic study of people in ill health, as distinguished from those afflicted with well-defined diseases. The bulk of my notes relate to this class of individuals.

6. Notes obtained from individuals who may be regarded as healthy, that is, not complaining of symptoms of ill health.

7. References to the literature, a comparatively small amount of data, chiefly incidental references found in biographies. This phase of the subject has been neglected, as it requires access to extensive libraries.

References to childhood are here omitted; my work concerns adults only. But we need only think of nightmare to realize how profoundly the mind of the child is influenced at times. I shall go over these subheads ery briefly, following the above classification.

People. Some people, or minds, with whom we come in contact stimulate us, just as there are those who depress us and many who do not affect us at all.

Books. These may also be classified according as they do or do not set us to thinking; some books act as decided mental stimuli.

Dreams. Dreams may be a source of mental stimulation to the intellectual, who may get some new ideas and utilize them. The ignorant dwell chiefly on the significance of dreams as good or bad omens. One can hardly realize what an important factor dreams are in the life of some people. This topic will be considered a little more fully later on.

Food and Drink. These have more or less influence on our well being and our thinking. One need only think of what often occurs after eating a late lobster salad or a welsh rarebit, when the thoughts are usually anything but agreeable. There is an old saying, "Who drinks beer, thinks beer"; and another, "One is what one eats."

Alcohol. Some know from personal experience whether alcohol excites or depresses the mind; certainly all have noticed the effects in others, how some individuals become greatly excited, with an active flow of words.

Tobacco should also be mentioned. Just how much truth there is in the claim of some men that they can think best while smoking or chewing is a question.

Anger, Jealousy, Resentment, or Grief, etc., may act as powerful stimuli.

Fright and Danger should also be mentioned; there may be a great rush of thoughts at what seems to be a critical moment.

Fever Fantasy. Those subject to colds and feverish conditions may have noticed in themselves the abnormal stimulation of thought at such times. The physician cannot avoid noticing it, especially in those delirious on account of fever.

Drug Stimulation. The most common is that of opium or its alkaloids, cocaine coming perhaps next. Hasheesh effects I have not observed. Not all persons are stimulated after the fashion of DeQuincey. Some brains are stimulated but little or not at all. The same is true of alcohol. The effect depends, moreover, largely on the dosage, varying from a more or less transient stimulation to complete stupor. It should be kept in mind that to a large extent anodynes that depress, such as acetanilid, are now used in place of opium.

Coffee. This is an active stimulant to some; many know that it will keep them awake at night, as night nurses. Some persons say coffee makes them dream. Literary workers may be actively stimulated by it, their thoughts flowing freely after its use.

Mania. An individual delirious in acute mania is a sight never to be forgotten. The delirium may continue for days, even for weeks, until the body is physically exhausted. Compared with this, the amount of mental work an ordinary brain worker does seems insignificant, and the idea of nervous prostration from mental overwork is made to appear ridiculous.

Recurrent Mania. This recurs at intervals, depending on the individual, after days, weeks, months, or even years.

Alternation of Mania and Melancholia. In this there are periods when the mind is very active, followed by periods of the opposite extreme. One of my insane patients during a period of exaltation had *Cacoethes scribendi*, the mania to write, and wrote me his autobiography; it would form a fair-sized book if put into print. He wrote continuously, did not even want to take time for meals or to sleep at night.

Chronic Mania. In this condition many individuals see visions and hold imaginary conversations; at times the brain is very active.

Dementia. At times when there is some disease producing fever there may be a transient lighting up of the mental faculties in dements, subsiding again with the subsidence of the fever. A study of such cases often sheds light on the mental processes in the normal, or sane.

Epilepsy. Epileptics about the time of an oncoming seizure may have active mental stimulation; the fact that some see visions is well known.

As a rule those confined in the hospitals for insane are quite demented, though there may be a transient mental improvement during or following some acute disease.

One of my unique experiences was the observation of the great mental improvement following the injection of erysipelas antitoxin[1] in an epileptic who was greatly demented; this mental stimulation, however, was only transient, subsiding on withholding the remedy, which proved too costly for continued use; in time there was a complete relapse.

Katatonia. Under this head I could write some lengthy accounts relating to mental stimulation from the use of desiccated thyroids.[2] Individuals who had been practically dead, both physically and mentally, had a veritable return to life under the use of thyroids.

Thyroid Medication or Stimulation. The above experiments were continued with different classes of patients to find out the limitations or usefulness of the new drug. This was quickly found.[3] In a chronic maniac it brought on acute maniacal disturbance and had to be discontinued. On the other hand, in individuals who were very dull and stupid on account of myxedema[4] all that was required to restore normal activity was the use of this remedy.

Redreaming Dreams. A personal experience while still living among the insane first directed my attention to dreams and the part they play in daily life. My experience in dreaming a dream over and over again during an attack of sore throat seemed so odd to me that I looked up the subject in the literature, and since then have questioned hundreds. I found nothing in the literature, and until recently did not meet any individuals who had had a similar experience—for this reason I gave a brief account in the *Psychological Review* for November, 1901. It may be added that while I dream much, but few dreams, comparatively, stand out vividly and are remembered next morning. An interesting study would be to seek the causation of dreams, why at times one dreams much and then again very little; likewise why certain periods of one's life rather than others are picked out, so to speak, by dreams.

Disease Influence on Mental Stimulation and Dreams. Here belongs a number of notes on cases in which the stimulating influence was

[1] Epilepsy and Erysipelas. Journal Amer. Med. Assn., May 14, 1898.
[2] Thyroid Medication. Indiana Medical Journal, June, 1896.
[3] Notes on Thyroid Medication. Ind. Med. Journal, Feb., 1898.
[4] Myxedema. Indiana Medical Journal, June, 1904.

noticed, as for instance in tonsillitis, when the mind becomes very active, with a great rush of thoughts, but without ability to hold them. After an attack of acute illness there may be a "clear brain" with active thinking. This can be explained by assuming that the brain was rested while the body was sick, or that it was stimulated by the disease or sickness, or by returning health, and now has a new set of thoughts.

Tuberculosis acts in many as a stimulant, producing especially cheerfulness and hopefulness, just the opposite from the next.

Acute Dyspeptic Attacks, as after the proverbial lobster salad. Here almost invariably the thoughts and the dreams are disagreeable, oppressive. Often it is less a question of the kind of food than of conditions under which the food is eaten. In the case of the lobster salad, the most favorable condition under which it is likely to produce disagreeable thoughts or dreaming is, in the opinion of some, a midnight lunch after attending a theater.

Chronic Dyspepsia. This to most of us brings up thoughts of pessimism, the effects thus standing opposite to those of tuberculosis. As a supposed classical example, Carlyle might be mentioned.

Atmosphere, the Air of Places. Literary people speak of the influence of atmosphere, but this may not at all refer to air conditions. On the other hand, physicians since the days of Hippocrates speak of the air of places. From a study of the subject one might almost come to the conclusion that the locality, the environment, has as much influence on thought stimulation as on the production of ill health and disease.

War Times. In my chronological account are some data relating to a friend whose regiment was called into camp on the breaking out of the Spanish-American War. The event was a great thought stimulant to him, especially when lying awake at night.

Travel. Next in order comes a mass of data based on a year's travel in Europe. The value of travel as a stimulant to the mind is recognized by everybody. The following is taken from my notes relating to this period:

"One day, at Heidelberg, I dropped into an inn for a bite to eat. I was going to sit down before a long empty table, when I was informed that it was a Stammtisch; that meant I had to take a seat elsewhere. While eating my modest meal, there was a rush of thoughts concerning the influence of the Stammtisch on the life of German thinkers, especially college professors, who frequently meet about such a table to exchange ideas, or get new ones, or both. Then my thoughts went to England, to its old coffee houses and the influence on the English writers who met there. That in turn brought to the mind the relative merits of beer and wine and coffee as aids to thought stimulation, and this again brought up the thought of the influence of tobacco smoke, whether this at bottom had anything to do with the matter, and that again brought me back to America, to our newspaper offices, where reporters often work in dingy offices densely filled with tobacco smoke and where many of the so-called 'pipe dreams' are concocted."

Health, Ill Health, Chronic Ill Health, Disease. During the last ten years I have been occupied more especially with adults in chronic ill health, as distinguished from real disease, and very naturally I have followed the subject of thought stimulation among this class. There is one very practical aspect, one, however, that is largely neglected by the average practitioner of medicine; that is, long sleepless nights during which the mind of a patient may be thinking all sorts of thoughts, usually disagreeable; if there is sleep there may be much disagreeable dreaming. The physician who is able to give patients of this kind restful nights is usually accomplishing something that his predecessors failed to do.

Individuals in chronic ill health often have very active minds and react acutely to certain drugs, such as opium, alcohol, caffeine; similarly to the salicylates, which are largely used in counteracting infection and inflammation. Many react acutely to the influence of travel. Thus while travel at home may disagree, travel in a foreign country may be beneficial. One can, of course, readily understand how in the case of literary persons one country may be preferred to another. But even common people who do not lead much of a mental life may notice the influence of travel, as when a farmer living in isolation complains of active dreaming or of restlessness at night after a trip to town. Among my case reports are at least four in which there was active stimulation of the mind while traveling on railways—in one case the thoughts or ideas were used in literary work. It may also be said that individuals with lively minds, literary people generally, react acutely to their surroundings, or to influences that scarcely produce an effect on the average man.

During the past few years I have been trying in my practice to distinguish between individuals who lead an active "Seelenleben" and those who do not. In a general way I can divide my cases (whether active minded or not) into four groups according to their ill health.

(a) Catarrh Victims, especially those subject to common colds and sore throat accompanied by disturbance in temperature. febrile condition, with more or less "fever fantasy," when all sorts of thoughts rush through the mind. If the individual is a writer and not too ill he may jot down some of these thoughts and utilize them. In some a recumbent position is an additional stimulating factor, and, indeed, people in health can often think best when reclining. One of my friends explained it by saying: "The pressure is equalized when lying down, there is less blood in the feet and more in the brain."

Catarrh victims may or may not be cheerfully excited—those infected with tuberculosis may be very cheerful and hopeful, the opposite of the next.

(b) Dyspeptics as we all know are usually pessimistic. One of my friends has said: "Beware of the literary critic who has dyspepsia or an acute dyspeptic attack, for he will see nothing to praise in your work or effort; all is gloom to him and mankind is going to the bow-wows." The depressed mental state may not last long in an acute attack, just as in the case of the boy who has colic from eating green apples, who thinks he is going to die, although he will be as well as usual the next day.

(c) So-called neurasthenics, known also as neurotics, and "the nervous." As a rule this class reacts acutely to environmental influences, and at night there may be insomnia with the mind actively at work. As to actual work, individuals vary greatly. Many have large thoughts but produce little; some are simply regarded as dreamers. What is commonly regarded as brain tire may really be motor tire; the brain is active enough, but there is no desire or little inclination for physical exertion necessary to write out the thoughts—a mental overstimulation with a motor paralysis, so to speak.

I have notes on one case, a man who would ordinarily be regarded as a neurasthenic, who dreams much and gets new ideas in his dreams, jotting them down in the dark at night, in bed. But frequently he finds in the morning that he has no notes, for, after a dream that he wants to record, he dreams further that he is recording it or has made an entry on his scratch tablet, and then sleeps on; all has been a dream. Sometimes on awakening he retains an indistinct idea of the dream which he wanted to record.

The influence of environment may be very marked in this group, as already mentioned. Some men can do their best work in the city, others in the country. I have a curious account of a writer who habitually ran out of ideas and then went to the nearby large city to spend a day, or rather night, for he would lie awake in the dark, in his room at some large hotel, filling scratch tablets with all sorts of thoughts or ideas that came to him. It would be interesting to know whether there was any marked change in blood pressure, whether he may not have belonged to the next group.

I shall refrain from citing more such cases, for to make reports valuable they should give a lot of details, or we may be wholly unable to draw conclusions regarding possible causes. In a general way it may be said that the more details in a case report the better.

(d) A group of cases that may be called cardio-vascular, in which there are disturbances in the blood pressure. At times of a high blood pressure there may be great mental activity. Brief mention may be made of a few cases.

Mrs. A. Middle-aged woman with a persistently high blood pressure, rarely under 200 mm, and often much above that, even to 250 mm. Complains of the mind being very active, all sorts of "komische Gedanken" passing through the brain; but at times of unusually high pressure the thoughts are anything but comical, the "Gedankenflucht" being the opposite; she at times fears enacting a tragedy. When I add that my own pressure runs from 100 to 110 mm, the significance of a pressure of 250 mm will be better understood.

Mrs. B. Elderly woman, gloomy and worrying thoughts both on account of ill health and possible financial difficulties. To distract her mind, to change the trend of her thoughts, her relatives nightly took her to a crowded revival meeting, but it was quickly found that conditions grew worse, and that the rush of thoughts seemed to prevent sleep altogether. She came to me and I found a high blood pressure. Simple medication and remaining away from the meetings caused the high pressure to disappear within a week, and the mental disturbance to subside, followed by a philosophical state of mind with cessation of worry.

Neither of these two individuals is intellectual; they do not utilize their thoughts.

Mrs. C. Middle-aged intellectual woman. Great rush of ideas at times of occasional high blood pressure, especially at night, often practically sleepless on this account. In the day time felt too fatigued, tired out, to be inclined to exert herself physically, but the mind would perhaps be very active. Often had "bright thoughts" at night and wanted to get up and jot them down, for she was unable to recall them the next morning, but her physician had told her not to do this, as it would aggravate her insomnia. When she came to me, I promptly advised her to jot down her thoughts, that with a little practice she could do this in the dark; at the same time I instituted measures to reduce the blood pressure —and when the pressure went down the automatic action of the brain ceased and sound sleep returned. How to bring down a high blood pressure is a medical question that need not be discussed here.

Mr. D. Middle aged man in whom a tendency to increased blood pressure gradually developed, along with much dreaming at night and subconscious mental activity, the thoughts coming at such times being utilized in his work. Problems and matters awaiting solution would be taken up and worked out at such times. This subconscious activity was always orderly, entirely different from that of dreams, for in the latter there were all sorts of incongruities and anachronisms. A change in environment caused the high pressure to subside and with it the subconscious mental activity, but the dreaming continued as formerly. Now and then there is a period, or it may be but a single night, of automatic activity, and the question is to find out the why and the wherefore of this activity.

Mr. E. The most literary man in a small community; past middle age; mind always at work. Came to me complaining of symptoms of ill health. I suspected cardio-vascular disturbance and on examination found a high blood pressure. I at once proposed a systematic examination, with health supervision. But to be literary does not necessarily imply the possession of good common sense, and instead of following my advice, given him at length, he adopted an easier and simpler course; he changed doctors. He went to a man who merely gave him a little medicine. A short time ago he died suddenly of cerebral hemorrhage.[1]

Ordinary people when they have a rush of thoughts at night may simply worry because they are not able to sleep, whereas the brain worker who utilizes his thoughts may welcome at least an occasional such rush

[1] There is a possibility that in this case arterio-sclerosis had set in, but I am incined to believe there was none at the time he came to me. It should be kept in mind that in this paper I am excluding children and the aged, as well as those afflicted with well-defined diseases or pathologic processes.

of thoughts, because it may furnish him material, data, plots. He may even seek to bring about this condition, or what is commonly called "inspiration." In this connection I might mention one case which may shed some light. A middle-aged literary woman had been complaining of disturbed heart action, marked especially by arrythmia. In order to correct the difficulty, her physician prescribed digitalis in larger doses than is usual. In a short time her mind became very active, with sleeplessness at night and with a great rush of thoughts. She then came to me and I promptly had her discontinue the digitalis, when the mental excitement subsided. The supposed heart disturbance itself was treated by methods other than drug treatment.

To what extent high blood pressure is a factor in thought stimulation in normal individuals I am unable to say. To study that will require "material." If, as earlier stated, the physician wants to study those in health he must go to them, and seek out those whom he thinks suitable for his work. Moreover, a physician never has that complete control over his "material" as the biologist. He can take up or leave off work at any moment; the physician must get the consent of his patients. Even the hospital physician has a great advantage in this respect.

It would seem a natural and simple inference that the increased circulation in the brain stimulates the cells, and thereby stimulates thought—and then at once the question arises, What brings on increased blood pressure?

Borderland Cases. Just where the normal shades off into the abnormal or where "perfect health" changes into "ill health" is often difficult to determine. There is no norm, there are no standards; what agrees with one may disagree with another. I will mention a few more factors which in some individuals play a role in thought stimulation.

Music. The mind or imagination of some people is strongly excited by music. When one critically studies cases he may be able to make distinctions between the influence of grand opera and rag time music, and whether the music is heard indoor or out of doors, as on a street corner or in a park.

Theater. Attending a play may bring on a lively "play of the imagination."

Church. A merchant once told me that he did his best business thinking or planning while apparently listening to a long sermon. And I know of a college student whose thoughts were most active while "listening to a sermon." Such stimulation is known to but few, while the opposite, drowsiness, is known to nearly everybody. Perhaps the "constitution" has something to do with it. I have notes on a preacher who gets his ideas for his next sermon a week ahead. If he fails to get them on Sunday night, he probably gets them at the time of the midweek prayer meeting. Local option meetings also seem to excite some—is it the enthusiasm?

Storms. Among my case reports are some of individuals whose minds were set agoing during the prevalence of a storm; if at night, there was much restlessness and sleeplessness with a rush of thoughts. An inquiry into details often leads to curious results.

Weather Changes should also be mentioned. The state of the weather is by many supposed to have an influence. I should especially like to hear from those who have made any observations along this line.

Books. Books, as a source of thought stimulation or of inspiration, are generally classified as good or bad, ancient or modern, new or old. To the average reader a book is simply a book, but those who utilize their thoughts or bright ideas may be able to make distinctions. Reading between the lines, an individual with a vivid imagination may get all sorts of new ideas, he may get more out of them than the author put in.

Lectures differ greatly in their stimulating influence. To some an occasional lecture may be helpful, while repeated lectures may fail to stimulate, or one may say there is overstimulation and the mind fails to retain the impressions. We all know how the lectures of instructors vary; some stimulate the students, others do not.

Barber-Shop Influences. One of my old patients, who lived at home all the time, went once a week to the barber shop, and then complained of insomnia with much dreaming at night. (But to make the story more complete it should be added that he was a chronic consumptive and that much coughing accompanied the insomnia and dreaming—some might regard this as a relationship of cause and effect.)

I recall a statement in a French reader, "Nothing refreshes the mind like having the hair dressed." A man is supposed to have made the remark—I mention this here as a possible factor in mental stimulation in women, as they often spend much time in dressing the hair. Perhaps that statement is on a level with that of the poet who spoke about "scratching the head, thinking the thoughts would come," etc.

Exercise may be an essential to a writer or sedentary thinker, as for the man who writes all forenoon and puts in the afternoon walking, riding, rowing, gardening, etc. Here one would have to distinguish between properly working up ideas and getting new ones, between resting the brain by a different occupation and getting new thoughts while so occupied; the new thoughts may perhaps come involuntarily while physically employed.

Baths of various kinds seem to be a stimulant to some persons.

"Being in Harness" is an important factor, as in the case of the business man who could not think, could not plan, while on a vacation, but the moment he returned to his dingy office his mind became very active. One man of affairs told me he would rather wear out than rust out, meaning that although he felt better physically while away from his old occupation his mind was dull; he would rather not feel so well bodily than to have ennui and boredom.

Subconscious Mental Activity.

Perhaps the most interesting phase of the whole subject is that of so-called subconscious cerebration, with its various synonyms, such as automatic cerebration, unconscious cerebration, etc. This form of mental activity is to be clearly distinguished from conscious activity on the one hand and from dreaming on the other; it is neither. Thus, while writing these notes, an old patient to whom the question was put gave me a good illustration.

This woman is a clerk in a county treasurer's office (I am not naming the county). Ordinarily she does not dream, or so lightly that few of the dreams are recalled the next morning. She has what may be considered good health, but at times does complain of some minor ills. Twice a year she works under great stress, at taxpaying time, when from early morning till late at night she is at the office, taking in money and receipting for it. After a day or two of this hard work she continues the work at night, "in her mind," to the exclusion of sound or refreshing sleep—the mind automatically and in spite of all her efforts to prevent going over and over the work of the day. On account of the loss of sleep, etc., she begins to suffer in health, and feels sure that at times if there were a few more days of it she would break down. But she admits one advantage of this automatic action of the mind or brain: Errors are constantly occurring, and when the books are balanced at night no one can account for the various discrepancies, and, of course, there is worry. Now in her "night work," during this automatic cerebration, she generally "sees" just where the discrepancies are and the next morning is usually able to make the corrections promptly.

She has some well-defined ideas regarding causes, that is, of the conditions under which such activity comes on, and I shall consider her remarks later on in summing up "causes" and "supposed causes."

Asked about dreams, she said they occurred in the winter time, rarely in the summer—the exceptions usually being times of actual illness.

Another patient told me that as a boy in school he worked out his mathematical problems while in bed at night. After he left school this form of mental activity largely disappeared and now only occasionally returns; he utilizes it in planning his business affairs.

Insomnia. After a wakeful period at night, perhaps of an hour or two, there may gradually come repose, and then when one is about to fall asleep, subconscious mental activity may come on with a flow of thoughts, perhaps valuable in one's work. Then comes the conscious thought, "If I don't jot down these thoughts or ideas they will be lost; if I do write, then the composure to sleep will disappear and I will again be wakeful and sleepless. Shall I write or not? Shall I put the thoughts on paper or get the sleep?" While undecided, sleep may come on, there may even be a dreaming that the thoughts have been written; the mind is relieved and deep sleep follows. In the morning nothing is remembered of the train of thoughts. If, however, they were written out, then on awakening the whole occurrence likely comes vividly to mind, or at least there are notes more or less clearly decipherable. This may also occur in the morning when one is about to turn over for another nap, and then this mental activity is confused with dreaming, but the coherency of ideas enables us to distinguish.

Sleepless nights of active minded people who utilize their thoughts are often due to the fact that they do not want to let go of the thoughts that come. They lie awake, thinking about them, or they will be kept awake by the very act of writing them down. When the mind is relieved and sleep is about to come, there may be another train of thought, and this too must be disposed of. This may recur over and over, and as a result there is a sleepless night. Insomnia is usually considered the bane of the brain worker, but perhaps after all it has its compensations.

Some individuals can distinguish very clearly between dreaming and subconscious mental activity. Some who utilize their thoughts refer to the latter as "inspiration," and in their attempts to bring on such a condition have tried all sorts of experiments. In reading biography one at times comes across statements that seem to refer to this condition of mind, as when Voltaire or Pope in the middle of the night called for his clerk or stenographer to take down a train of thought. This form of mental activity occurs in all kinds of persons, but as already mentioned is most marked in brain workers. The question naturally arises, What is back of it all? What produces this form of mental activity? By gathering a large mass of data one may be able to arrive at some conclusions. One can not solve the problem from a study of books, it must be studied in living persons whom one can question about details and antecedents.

Here again my own observations have been confined mainly to those in ill health. To what extent automatic mental activity is a question of medicine and to what extent a problem in psychology may largely depend on the individual studied, as well as on the student—on his knowledge and purpose. But we should not forget that the modern psychologist studies and investigates largely by the use of instruments, in his laboratory.

To study the influence of blood pressure requires the use of a sphygmograph, and that means that the study of thought stimulation due to the changes in blood pressure is beyond the man who makes but simple observations. The man not connected with a laboratory might, of course, seek out a physician who makes blood pressure tests and would interest himself in the subject.

On the other hand, auto-observations of what is going on in one's own mind are or can readily be made by any one who will take the trouble to observe, no apparatus being required, unless it be a watch or clock to note the time of day or night and a fever thermometer in the case of those who have "fever fantasy"—which may or may not be distinguishable from the mental activity unaccompanied by fever or disturbance in the temperature of the body. At times the mental stimulation may be wholly out of proportion to the rise of temperature, and I have had cases where there seemed to be a high temperature, judging by the redness of the face and the complaints of the patient, and yet the thermometer failed to reveal any elevation of temperature. One has to distinguish between "feeling feverish" and having a real fever, that is, an actual elevation of temperature.

Just now a fad has spread over the country which gives undue importance to this form of mental activity in the treatment of ill health and the cure of diseases. It would seem that there are two kinds of psychotherapists, the real and the pseudo. The former limit themselves to so-called neuroses and functional disturbances, while the latter ascribe subconscious mental activity to practically everything—except perhaps to the healing of broken bones.

I have already referred to the fact that some individuals make sharp distinctions between dreaming and subconscious mental activity or subconscious cerebration. I myself believe these are two different processes but one will have to give close attention to what is going on in the mind to enable him to discriminate. As to the possible existence of a "subconscious mind," as an entity, that is another question. Perhaps it is synonymous with the "soul" of the old philosophers.

QUESTIONING ABOUT DREAMS. In questioning people about dreams one quickly learns to divide dreamers into three classes.

There are those who "wonder what it means," who are constantly speculating on the significance of a dream. Some will tell of having heard some one telling of seeing a certain event in his dream and found that very thing to have actually occurred at the time and place indicated in the dream. They will tell of it in detail, if one listens, and then ask. Now how do you explain that? Personally, I have never had such a dream, one in which I "saw in my dream" events or incidents that actually happened, either at the instant the dream occurred or the next day or next week, or at any time. Neither have I met a single individual who had such a dream or "foresaw" an unusual event. When we consider that out of thousands and thousands of dreams some one may have noticed such an incident, we must conclude that it was simply a coincidence, as where during a thunderstorm at night a relative or a friend exposed to the storm "is seen," either struck by lightning or being near the place where it did strike.

Now it would seem quite natural for one when awakened by the peal of thunder to think of his relative, and the sudden thought may be mistaken for a dream. Even if it were a real dream and "came true" it must still be regarded as a coincidence, as one instance out of thousands of dreams the rest of which did not come true. We hear of the particular one, and as just remarked, at second hand, or even a number of removes from the original source, to the neglect of all dreams that did not come true. At times we see a mention in the newspapers of dreams that "came true."

A second class is composed of those who pay no attention whatever to dreams, and also those whom one can not interest in the subject at all, who may even express disgust at the very idea of giving a dream a second thought. This class is as unsatisfactory to the student as the other.

Those who do give some attention to dreams and may be made to take additional interest when their attention is called to the subject form a third class—the class I have in mind in this paper. They are comparatively few—but, as in other things, to the few we owe our increase in knowledge.

Out of the long list of factors and conditions enumerated in this paper only a few, perhaps only one or two, may play an important role in the life of any one person; to him, however, they may be essential. As an example, we have the man who requires the quiet of the country, or on the other hand, the man who requires the stimulation of city life.

In asking for data one can put the question in several ways: In the case of those who have occasional periods when the mind is very active, we can ask, "Under what conditions does this occur?" "What causes the mind to become thus active?" While in those whose minds are nearly always active, but where there are occasional intervals of inactivity, when a man says, "I can't think," we may ask, "Why not?"

As an addendum may be mentioned several other factors that stimulate the mind and bring on thoughts.

Trying It on the Audience—"for further inspiration." I recall how Dr. Jordan used to do this before his classes in Evolution, as he himself told us. I have often wondered how much inspiration he got from a dull class.

An Assigned Task, as a factor, as where a member of the Academy sends in his title, and as the time for the meeting approaches gradually "gets busy," knowing that his paper must be ready at a certain time—for instance, myself during the last few days.

Finally may be mentioned the Annual Academy Meetings as a source of stimulation and of inspiration, especially to those of us who live in isolation. This is a factor in thought stimulation not to be undervalued.

Hygiene of Indoor Swimming Pools, with Suggestions for Practical Disinfection.

By Severance Burrage.

The "Ole Swimmin' Hole" of our boyhood days is doomed. The favorite spot in pond or stream to which we used to go after school for a good swim and play, with no thought for the microbe in the water nor the bathing suit for our bodies, is, for the boy of today almost unknown, and for the boy of the future will be but an unrealizable dream. With the advance of civilization these swimming holes are being replaced by public bath-houses, and to these, or to gymnasiums that are provided with swimming tanks, the boys must go for their swim. The streams and ponds have become polluted to such an extent that it is dangerous for the boys to bathe therein. This is the result of the increase in population, coupled with the great carelessness of individuals and communities in the disposal of wastes. This replacement of the natural swimming pool by the indoor swimming pool may carry with it new unhygienic conditions, and it is a discussion of these conditions and their elimination that forms the purpose of this paper.

CONSTRUCTION OF INDOOR SWIMMING POOLS.

One of the first requirements in the sanitary construction of the indoor swimming pool is that it must be so constructed that it may be easily cleaned. To this end the surface of the lining material of the pool should be very smooth, such a surface as is provided by glazed tiles so laid as to avoid all cracks and crevices. At the angles formed by the meeting of the sides and floor of the pool, curved tiling should be used, which would give the same result in the border of the pool flooring as is obtained in the angleless baseboard in up-to-date hospitals and operating rooms. The almost universal deposit of a slimy sediment in these pools, even when the water is comparatively clear, makes it necessary to provide for an easy and complete cleaning. A concrete or cement lining, made as smooth as it is possible to make it, furnishes a surface that is difficult to keep clean.

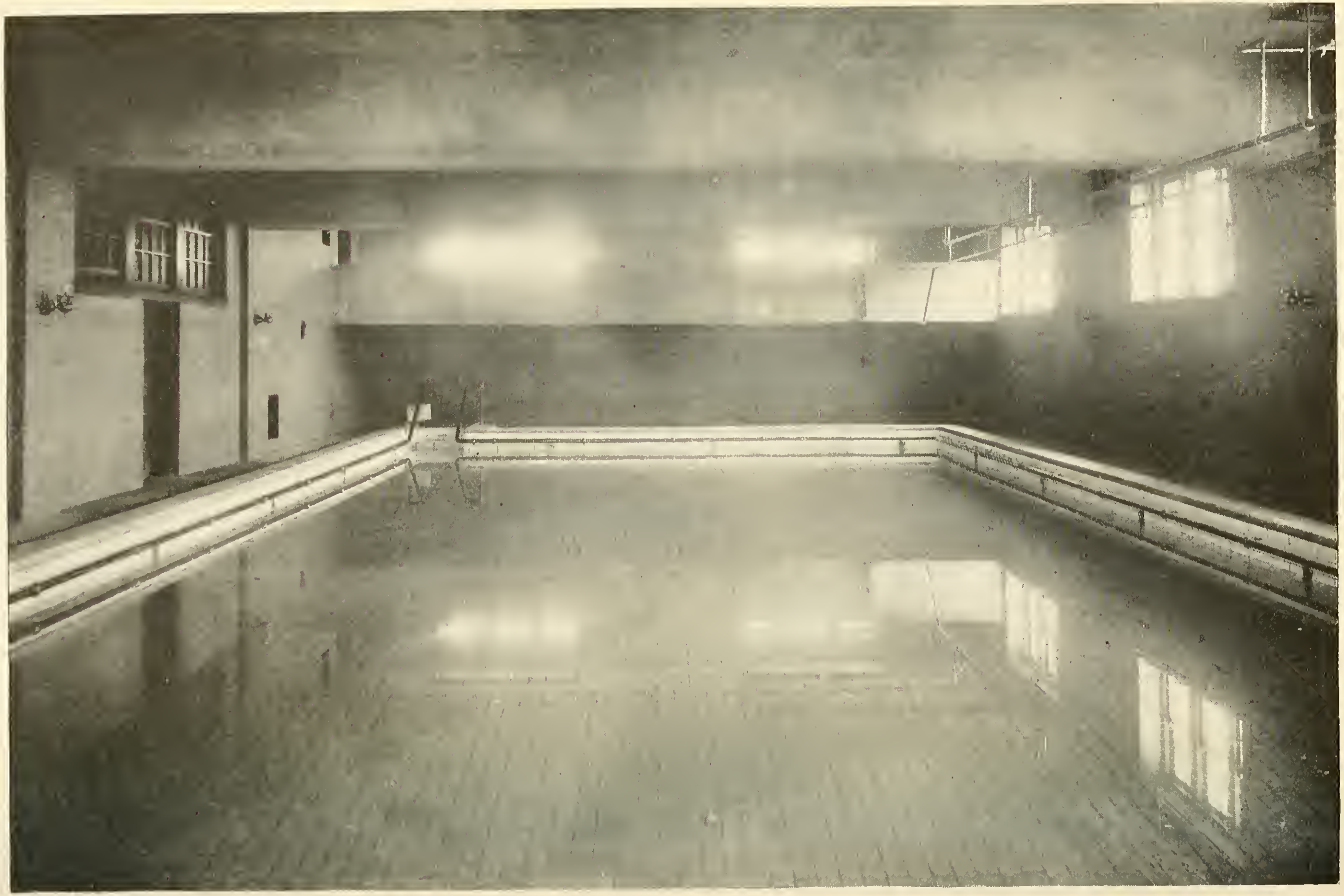

Fig. 1. Swimming Pool, Memorial Gymnasium, Purdue University.

A swimming pool lined with the glazed tile referred to above is shown in Fig. 1, Purdue University Memorial Gymnasium, 1909.

In addition to the outlets for the water in the bottom of the pool, it is advisable to have, at the overflow point, a sufficient number of outlets, or a trough extending all around the pool, so that when a scum or dirt collects on the surface of the water, the upper layers may be drawn off without necessarily emptying the whole pool.

Fig. 2. Men's Swimming Bath, Leeds, England. (Lighted by sky-light only.)
By courtesy of "Modern Sanitation."

The floor of the pool room should be so drained that water dripping from bathers who have come out of the pool can not collect in puddles, and, furthermore, such water should drain, not back into the pool, but into the overflow waste pipes.

The Water Supply.

The water supplied to the swimming pool must be pure, and every possible means used to keep it so during and after its use by the bathers. The nearest approach to an ideal water supply for an indoor swimming pool would be the provision for a pure water to start with, and a continuous change of water, during the use of the pool, the rate of this change being governed by the number of bathers in the pool. In most cases this is out of the question on account of the expense.

The water of these pools is not exposed to the many purifying factors that affect out-door waters. The pool is usually located in the basement or in buildings the interior of which the direct rays of the sun seldom reach. Thus one of the most important factors in the purification of natural waters is removed. It is true that the water does get some aeration while the bathers are stirring it up, but because of the constant contamination at such times, this aeration cannot be counted upon as very much of a purifying factor. During the times when the pool is not being used, when the water is stagnant, no purification is taking place. On the contrary, bacteriological tests have shown that there is an increase in the bacterial content, particularly if the water has been warmed up to a temperature of 75 degrees Fahr. or over. There is considerable sedimentation during such times, but if this sediment remains in the bottom of the pool to be stirred up when the bathers next use the water, this cannot be looked upon as purification.

The cold plunge at the Fleischmann baths, New York City[1] has "enormous windows of plate glass facing south and the medicine of the sun and the glory of the sky." (Fig. 6.) Comparing this elegant sunny pool room with the condition in our average basement swimming pools,

Fig. 3. Women's Swimming Baths, Leeds, England. (Direct sunlight rarely reaches water.) By courtesy of "Modern Sanitation."

it makes the latter look dark and gloomy. The pool room at the Purdue gymnasium is on the south side of the building, and the windows are large for a basement room, and yet even this does not get the necessary sunlight for purposes of purification of the water.

At the swimming baths at Leeds, England (Cookridge street), the skylight is used for lighting the rooms, but even here the effect is none too brilliant. (Figs. 3 and 4.)[2]

[1] Lucy Cleveland, Modern Sanitation, Jan., 1908.

[2] Henry Gray, Modern Sanitation, Oct., 1909.

Pollution of Water by Bathers.

A bacteriological study of the water used by a bather at the Victoria Baths at Bonn,[1] shows well the character and amount of pollution that may take place in public baths. The test was made on a stoker (Heizer), who was made to wash in a tub for three minutes, using no soap. Before the test, the bath water contained 24 bacteria in a cubic centimeter, and no *Bacillus coli*. After the three minute washing, the bath water contained 1,900 bacteria and 40 *Bacillus coli* in each cubic centimeter.

Bacteriological tests made by the writer on the water of the swimming pool in the new memorial gymnasium at Purdue University demonstrated the presence of 930 bacteria per cubic centimeter in the water of the pool before being used by the bathers. After use by about thirty bathers, all of whom were supposed to have taken a soap shower before entering the pool, the bacterial content was 109,200 per cubic centimeter. Tests were made for *Bacillus coli*, and the results were consistently positive after the pool had been used. The water immediately after cleaning the pool and refilling gave consistently negative results for *Bacillus coli*.

The available literature gave almost no data as to bacteriological analyses of swimming pool waters.[2]

DISEASE DANGERS IN SWIMMING POOLS.

There are great chances for the dissemination of germ diseases through indoor swimming pools. The results of the bacteriological tests given in the preceding paragraphs, which showed the constant presence of the *Bacillus coli* in the water used by bathers, demonstrates the possibility of intestinal diseases, particularly typhoid fever. While bathers do not swallow the water intentionally, it is next to impossible to avoid getting some water into the nose and mouth, which would ultimately reach the intestinal tract. One does not have to be sick or to have any symptoms of typhoid fever to disseminate the germs of that disease. This is well shown in the notorious case of "Typhoid Mary" in New York.[3]

Diseases of the respiratory tract have an unusual chance to be spread in the swimming pool. The bather with incipient tuberculosis, pneumonia

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[1] Zur Hygiene der Hallenschwimmbade. Dr. Selter. Aus dem Hygienischen Inst. der Univ. Bonn. Rundschau. Dec. 1, 1908.

[2] Hesse, Dresden. Zeitschrift f. Hyg. Bd. 25. Eden, Berlin. Arch. f. Hyg. Bd. 19. Koslik, Gratz. Diese Zeitschr. 1898, S. 361.

[3] Whipple. Typhoid Fever.

or tonsillitis, with his sputtering, coughing, snorting and spitting, would undoubtedly infect the water with the specific germs of those diseases. Ordinary colds and sore throats following the plunge bath are frequently laid to the effects of the bath, while in most cases such results are undoubtedly due to germ infection. One of the factors which lead the writer to take up this subject was an epidemic of colds among the users of the Purdue swimming pool this fall.

Venereal diseases could be transmitted through the agency of the swimming pool. One case of gonorrhoea could infect many eyes in a crowded swimming pool.

It is practically impossible to compel the bathers to submit to a complete medical inspection and physical examination before they are allowed to enter the pool, and yet from many points of view this would be a most desirable thing.

The least that can be done for the protection of the bathers is to insist that certain rules be strictly adhered to. For example, such rules as the following are posted prominently in the Purdue gymnasium:

TAKE A SOAP SHOWER BEFORE ENTERING POOL.

All gymnasium privileges will be denied persons affected by any contagious or communicable disease.

All persons MUST take a soap shower before entering the pool.

All persons using the pool MUST wear bathing suits or trunks.

Of course facilities must be provided for the required showers, and each person should provide his own towel and soap.

In the Central Baths, Bradford, England, special arrangements are provided for washing the feet,[1] a most desirable thing as a prerequisite to the use of the pool. (See Fig. 7.)

PRACTICAL PURIFICATION OF WATER IN SWIMMING POOLS.

The amount and character of the pollution in swimming pool waters point very clearly to the need of some practical process of purification. In most cases it is too expensive to have a continuous change of water, and

[1] Centralized Public Baths. Bertha H. Smith, Modern Sanitation. November, 1909.

in some too expensive to change the water once or twice a week. At the Central Baths, Bradford, England, the water is filtered. The expense of pumping the water and caring for the filter does not make the filtration process a particularly economical one.

It occurred to the writer that some chemical, as copper sulphate or chloride of lime, both of which are being used extensively in the purification of sewage and sewage polluted waters, might be used in the treatment of swimming pool waters with but small expense. Inquiries in many directions and a careful search in available literature resulted in but scant information. A single reference[1] reported the use of a chemical, an "electrolytic fluid," by the medical officer of health of the metropolitan borough

Fig. 5. Plunge, East 23d St. Public Bath, New York City. (A fairly well lighted indoor pool.) By courtesy of "Modern Sanitation."

of Poplar, Mr. F. W. Alexander. This fluid is obtained by the electrolysis of a solution containing magnesium chloride, the result being a solution of magnesium hypochlorite. Treatment of water in swimming baths by this fluid was thought to be simple, economical and efficient, bacteriological tests on water so treated giving sterile results.

Before finding this reference the writer had conducted a series of tests on the water of the swimming pool at the Purdue gymnasium, using chloride of lime.

Commercial chloride of lime (bleaching powder) is usually manufactured by passing dry chlorine gas over freshly slaked lime, the chlorine

[1] Scientific American Suppl. No. 1765, Oct. 30, 1909.

being obtained by the electrolysis of salt. This chloride of lime is composed largely of calcium hypochlorite. When added to water this hypochlorite dissolves, leaving a residue of calcium hydrate and calcium carbonate. Both of these substances are entirely harmless factors in a bath water. The oxidizing power of the commercial chloride of lime is represented by about 35 per cent of available chlorine. It is nascent oxygen that is the purifying factor, not the chlorine.

The capacity of the Purdue swimming pool is 85,000 gallons, and 680 grams of the chloride of lime were used at each dose. This would be about the equivalent of 20 pounds to the million gallons. Before starting the experiment with the chemical, bacteriological analyses were made of the water for a week, the pool being emptied twice. No attempt was made to keep track of the number of bathers in the pool.

The following table shows the results of the analyses for the week before using the chemical dosage, compared with the results of the analyses for the week while the dosage was going on. During the latter week, that is, while the tank was being dosed with the chemical, the water was not changed at all.

The method of applying the chemical was to sprinkle it on the surface of the water in the pool. This was easily done with one trip around the edge, throwing the powder as one walked. The time occupied in this process was less than two minutes.

Date.	*No. of Bacteria per c. c.*	*B. Coli.*
Monday, November 1, pool just filled	560	None
Evening, after use	6,160	Present
Tuesday a. m., November 2	20,650	Present
Evening, after use	37,600	Present
Wednesday a. m., November 3	27,800	Present
Evening, after use	60,500	Present
Tank emptied.		
Thursday a. m., just after filling	930	None
Evening, after use	8,500	Present
Friday a. m., temperature of water 85 Fahr	109,200	Present
Evening, after use	106,400	Present
Saturday a. m., November 6	118,000	Present
Evening, after use	140,000	Present

Monday, November 15, pool freshly filled........	780	None
Evening, after use...............................	23.100	Present
Pool dosed with 680 grams chloride lime.		
Tuesday, November 16, a. m.......................	26	None
Evening, after use...............................	12,000	Present
Pool dosed with chloride of lime.		
Wednesday a. m., November 17.....................	14	None
Evening, after use (no sample).		
Pool dosed with chloride of lime.		
Thursday a. m., November 18, water had not been changed as was usually done...............	9	None
Evening, after use (no sample).		
Pool not dosed.		
Friday a. m., November 19.........................	11,200	Present
Evening, after use...............................	20,500	Present
Dosed with chloride of lime.		
Saturday, November 20............................	18	None
Evening (no sample).		

A study of the results shown on this table indicates that the effect of the chloride of lime treatment is almost complete sterilization. The samples of water taken the morning after the water had been dosed in no case showed more than 26 bacteria per cubic centimeter. And what I believe to be a very important factor is that the general average of the bacteria is lower, much lower, than during the week when the chemical was not used. The effect of stopping the dosage is prettily shown in the Friday morning sample, November 19. The pool is used by the "coeds" and faculty ladies on Thursday evenings, and it was inconvenient for the writer to arrange to have the sample collected. No arrangement was made to have the chemical applied.

SUMMARY AND CONCLUSIONS.

There are certain dangers to health in the indoor swimming pools. The construction of the pools, the enforcement or neglect of rules governing those who use the pools, the proper attention to the water supply, as to its purity before use by the bathers and after use, all have a direct bearing on the extent of these dangers.

On account of the expense it is practically impossible to provide for a continuous change of the water. The filtration of the pool water after use also involves some trouble and expense. The use of certain disin-

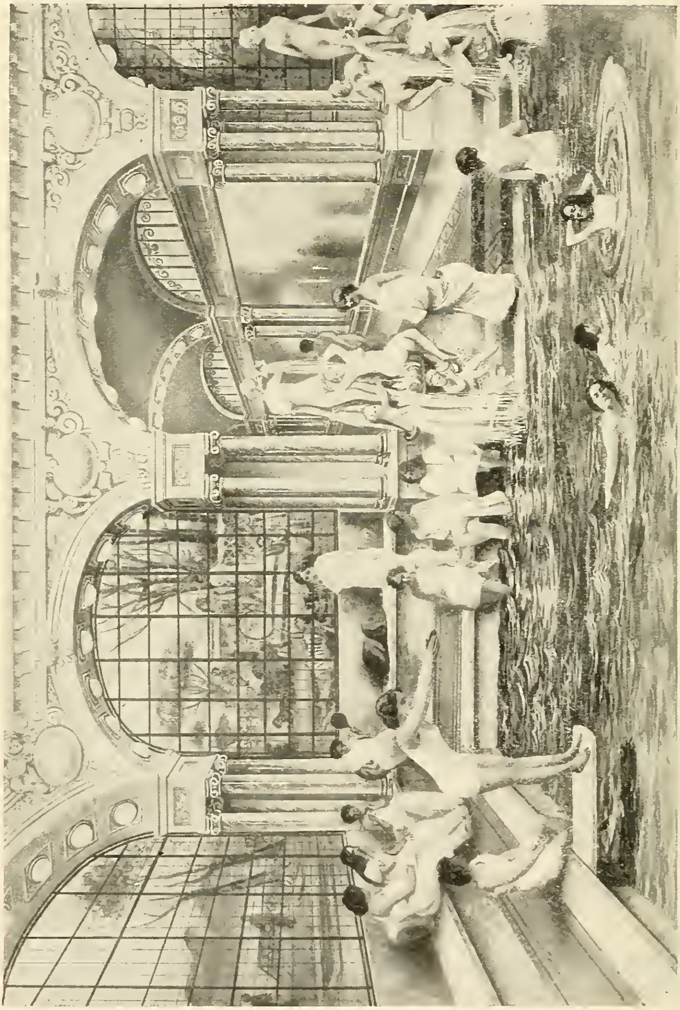

Fig.6. Plunge, Fleischmann Baths, New York City. Women's Hour. (Showing unusual lighting for an indoor pool.) By courtesy of "Modern Sanitation."

fectants would seem to be more simple and economical. The writer would criticise the liquid or fluid chemicals as being harder to apply to the pool water. They would have to be thoroughly stirred into the water. The substance used by the writer, chloride of lime, is sprinkled on the surface of the water, and it to a great extent distributes itself by sinking through the water. The results of the bacteriological tests certainly indicate that the substance has a very great purifying power.

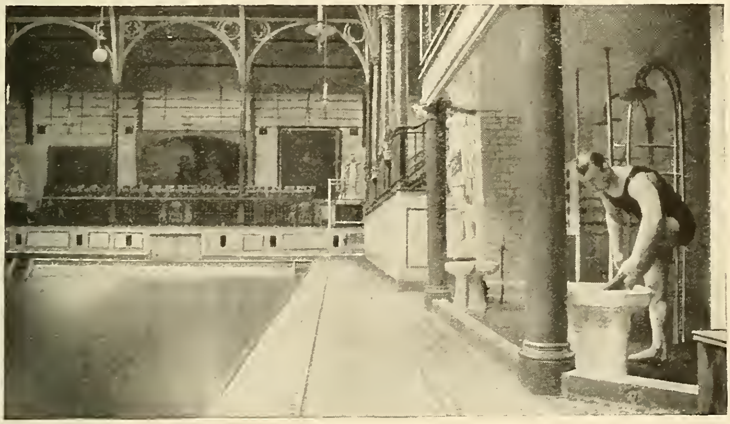

Fig. 7. Facilities for foot-bath before entering plunge. Bradford, England. By courtesy of "Modern Sanitation."

The indoor swimming pool is a valuable hygienic factor in our public baths and gymnasiums. It makes the bath attractive to many who would otherwise look upon bathing as a bore. Anything which will tend to make the boys and girls, youths and maidens, men and women bathe more frequently is desirable. But the swimming pool has its dangers, and most of these depend not upon the users of the pools alone, but much more on the construction and management of them. Therefore we must look to the builders and directors of our baths and gymnasiums for the satisfactory hygiene and sanitation of indoor swimming pools.

The Problem of Sewage Purification in Indiana.

By R. L. Sackett.

CONDITIONS.

As the population of a city increases, the difficulties of obtaining a sufficient water supply which is free from contamination by sewage becomes more and more difficult. It is now a well established fact that sewage-contaminated waters are to a considerable extent the cause of summer complaint and other bowel troubles, besides the more dangerous disease, typhoid fever. The extensive death rate among children is in some measure chargeable to impure water.

There are very few large cities that are able to obtain a ground water of satisfactory quality and quantity. We are therefore driven to the use of surface water.

OBJECT OF SEWAGE PURIFICATION.

Large volumes of sewage are discharged into the White, the Wabash and Ohio rivers and their branches, also into Lake Michigan, by the cities situated near them. In order to maintain a stream in a condition approaching normal purity, methods for the purification of sewage are applied, so that the resulting effluent discharged into a stream is purer. This purification is obtained by some method of oxidation which will remove the putrefactive material or highly organized food on which pathogenic bacteria live.

Sewage purification is a relative matter, and absolute purity of the effluent is practically impossible and generally is unnecessary. The problem, then, is to adapt available means to the conditions in order to economically defend the people against water-born diseases.

Dilution may be considered a process of purification, and therefore the larger the volume of pure water available in a stream the lower the percentage of purification required, for wherever there is running water not already contaminated, oxygen is present and some purification takes place; vegetation and sedimentation also assist.

The old theory that a stream would purify itself in a flow of ten miles was a dangerous one, because it depended distinctly on conditions.

In many instances no doubt typhoid has been carried thirty miles by a river. and then has caused a serious epidemic.

PROCESS OF PURIFICATION.

While a certain amount of purification takes place in a septic tank, its office is rather that of changing the organic matter from the condition of suspension to one of solution. Hence it is now more frequently called a hydrolytic tank. It is, however. important in that it makes the succeeding processes of nitrification easier and permits of much more rapid treatment than would otherwise be possible.

The second step is one of several types of filtration. First, we might place the slow sand filter, which was usually some 3 or 4 feet deep; over the surface sewage flowed either continuously or intermittently, the latter being the more efficient method.

A second form was the contact filter, which was a tight tank filled with broken stone. coal or hard clinker. This tank was filled with sewage from the bottom, and after a time was emptied automatically.

The third and most successful type of filter is formed of stone, about one-half inch in diameter. Over the surface sewage is sprayed or sprinkled periodically by automatic syphons.

After filtration there is still left some organic matter. but, if the process is successful, it does not cause putrification. It is quite probable that some bacteria pass through the filter and thus gain access to the stream. Hence it has been proposed that where a high degree of purification is necessary the effluent from the filters should be sterilized.

PURIFICATION PLANTS IN INDIANA.

Two or three tanks were installed in Indiana some ten years ago. and a set of four small sand filters was at one time (about 1900) in operation at Indiana Harbor, but has since been abandoned.

The oldest plant still in operation is at the Eastern Indiana Hospital for the Insane near Richmond. It consists of a concrete tank and intermittent sand filters. It treats the sewage of about 1,000 people and leaves the stream into which the effluent flows in a very satisfactory condition. It was built in 1901 at an expense of $9,000. The cost of operation has been negligible.

The second plant of any size was built at the Southern Hospital near Evansville. It was a chemical precipitation plant using lime or soda ash. After precipitation in large concrete tanks the sludge was pumped to a press; the resulting cake of organic matter was dumped into a cistern made for the purpose. The cost of the plant was originally $18,000, and the cost of operation about $1,200 per year.

It was replaced in 1905 by a three-step process which included tanks, stone filters and finally intermittent sand filters.

The conditions here required a high degree of purification. The population is approximately 1,000, and the cost of operation is probably less than $200 per year. The cost of reconstructing the plant was $10,000.

In 1908 the city of Bloomington constructed a system of sanitary sewers and installed a purification plant consisting of a central concrete tank and two series of stone filters, the latter being sprinkling filters.

Angola, a city of about three thousand population, is now constructing a system of sewers and a sewage purification plant consisting of sedimentation tanks, stone filters for the first treatment and sand filters for the final. The city will build a second plant next year. The cost of the two plants will be about $20,000. The cost of operation of these small municipal plants will be watched with interest, as it will determine in some measure the details of future designs.

The city of Laporte, with a population of 12,000 and rapidly growing, is completing plans for a system of sewers and is providing for sewage purification.

The city of Shelbyville is also constructing a system of sanitary sewers, and the entire town looks forward to sewage purification at some time.

There is no question but that the educational propaganda which the State Board of Health has been pursuing is bearing fruit. The state institutions themselves are with a few exceptions well provided with a good water supply and sewage purification plants where they would otherwise prove a danger to neighboring communities.

There can be no doubt but that this movement toward pure water will have a measurable effect upon the morbidity and mortality of the State.

Purdue University.
Lafayette, Ind.

That Erroneous Hiawatha.

By Albert B. Reagan.

Hiawatha, hero of Longfellow's poem of the same name, is not recognized as a hero god by the Chippewa Indians. Neither the name nor the person designated occurs in the mythology of these people. Their god is Menibusha, and the only word approaching Hiawatha is Ket'-che-wah'-sah, which means "afar off." Through the kindness of the Indian missionary and court-agency interpreter here, Rev. Frank H. Pequette, who is himself a Chippewa and has lived and preached in various parts of the Chippewa country, I quote his own summing up of this subject:

"When a white man asks an American who is the greatest man of his country he answers, 'George Washington.' But I am here to declare that Hiawatha is not the hero of my race. This personage is unknown to the Chippewa Indian.

"The Indian lad sitting in the forest with his bow and arrow, observing the trees and the sky and the sand and the water of the Great Lakes and the animals and the fishes, asks himself, 'Who made these?' He cannot answer the question himself, so he asks the old medicine man of his tribe. 'Menibusha,' answers the sage. 'Menibusha made the earth, sky, the sun, moon and stars, and the wild things and the fishes, and he made you also, my son.' So says the medicine man of the tribe. Menibusha also made the land, the island-continental surface on which we now live. He is the first brother of all mankind and now lives in the East.

"All the Indians before they became Christians (that is, all Chippewas) supposed that Menibusha was the Supreme, the greatest man and god of his nation. And when the first white man asked the Indian the question who was their greatest personage the Indian replied, 'Menibusha.' 'Where does he live?' of course asked the white man. 'Ket'-che-wah'-sah,' replied the Indian—meaning (that he was) afar off. The white man's ears were not tuned to the Indian sounds used in pronunciation and he caught it Hiawatha, which did not mean god, but 'afar off'; and one great white fellow, Longfellow by name, wrote our legends with this unknown Hiawatha. But this Hiawatha is not known to us Indians."

Medicinal Value of Eupatorium Perfoliatum.

By A. J. Bigney.

Eupatorium perfoliatum, commonly known as thoroughwort, or boneset, is a well-known plant, yet its real medicinal value is not as well known as it should be. This plant varies from two to four feet in height, blossoming in August and September, and is abundant in flat and swampy lands. It seldom grows in hilly sections. Nature seems to have made provision for the curing of the diseases prevalent in certain regions. In swampy countries chills, malarial, intermittent, typhoid and other fevers are common. Since boneset occurs in these localities and is particularly valuable for curing such diseases it seems to substantiate the above statement.

The blossoms, small branches and leaves are the parts generally used. It has four medicinal properties—an emetic, a tonic, a light laxative and a diaphoretic. As a diaphoretic it should be taken hot just before retiring. This is specially helpful for colds and fevers. For restoring the powers of the stomach it is better to take boneset cold.

For the diseases already mentioned boneset has been known and used as a home remedy for a long time. In the so-called la grippe it has not been used very extensively so far as I can learn. Some prominent physicians say it is almost a specific for it.

My experience in its use dates from the first appearance of la grippe in this community, about 1889. As soon as the symptoms begin a teacupful of the infusion of the boneset is taken just before retiring. This produces some perspiration, strengthens the nerves, regulates the digestive organs, thus giving the body an opportunity to increase the building up of the system, and in this way the resisting power is sufficient to overcome the disease. Occasionally the next day some of the cold infusion may be taken, always before meals, for, after eating, the emetic power may predominate. The next night the hot solution should be taken. Usually this kind of treatment will cure the disease without going to bed at all. This treatment should be taken early in the development of the disease in order to get the best results.

The first time of taking it, it should not be very strong until a person finds out its action on his stomach, for the emetic influence is exerted much stronger in some persons than in others. If one can retain it, it matters but little about the strength of it. It is made as ordinary tea.

I have thoroughly tested it in my own case when la grippe has been making its invasion, and as a result I have never yet had a regular siege of the disease. My own family has tried it time and again with splendid results. Some people cannot take it at all because of its emetic effect. I have given my neighbors the benefit of my experience. While its results are always good, yet in some persons the results will not be as marked as in others.

The students of Moores Hill College have been very willing to respond to my desire to have them test it. Students have come into the class-room with the symptoms strongly developed, and on being advised to take this remedy have actually taken it that night. They would report that the results were even better than they could have expected in so short a time. They would not even have to stop work. Scores of reports could be given, but I do not think it necessary. The best way will be to test it for yourself. It can be secured from your druggist if it does not grow in your locality. An extract of boneset is made, but I have had no experience with it.

I am pretty thoroughly convinced that nearly every case of la grippe can be cured by this remedy if taken early in the development of the disease.

Moores Hill College,
Moores Hill, Ind.

Refractive Index as a Measure of Dry Substance in Saccharine Products.*

By A. Hugh Bryan.

Dry substance determinations are the most difficult determinations a a chemist has to make, and again one of the most important. In sugar materials, containing many organic substances and also inorganic salts, various reactions and changes are going on when the sample is heated in the course of making a dry substance determination. Varying degrees of heat also tend to decompose these substances. Also, the length of time of heating is a very important factor. The accepted method for sugar compounds, where accurate results are desired, is the loss of weight at 70° C. when heated in vacuum. It has been found at that temperature that levulose shows little, if any, decomposition. Sugar chemists of Germany modify that procedure by drying at 65° to 70° C. in the air until all visible water is gone, and then heat for from 2 to 4 hours at 105° C. in vacuum, it being claimed that by first drying and then heating to 105° in vacuum, no sugar is decomposed. It is a fact, however, that if one makes two determinations of moisture on the same sample at different times, it is more than likely that the results will not check. Differences of as high as 0.5% have been noted, especially where the substance under examination is high in reducing sugars. It can hardly be expected to obtain a method for determining moisture accurately without a direct determination of this by drying. Such a procedure takes time, and at its best, so far, gives only approximate results.

The refractometer was first tried in sugar work by Strohmer (Zeit Ruben Zuckerind., Vol. 21, p. 256) in 1884 and again in 1886 by Muller (Ibid. Vol. 37, p. 91). They showing that the index depended on the concentration of solution. The latter investigator gave a table for estimating the dry substance of beet juices from the refractive index. Again in 1901, Stolle published a table for the above. All of these used the old forms of instruments. Tolman and Smith,[1] using the heatable prism instrument, such as is used today, and pictured later in this paper, found

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[1]Jour. Amer. Chem. Soc. (1906), *28*, 1476.

that for equal concentrations, all sugars have about the same index of refraction. Main[1] published tables of water content from refractive index in 1907, and called attention to the accuracy of this method, as compared with the true dry substance. Since that time the literature has been full of articles on this method of determining the dry substance.

All authors, with but few exceptions, claim much for this method as a quick one and yielding comparable results. They all agree that the results so obtained are nearer the true dry substance than by obtaining the dry substance from the specific gravity. The substances dissolved along with the sugar seemingly have a closer refractive index to sugar than specific gravity.

Working on syrups of various origins, I obtained the following average figures.[2] The method for true dry substance was loss of weight in vacuum at 70° C. The table of Prinsen Geerligs, and also his corrections for temperature were used. These are given later in this paper.

IN CASE OF MAPLE SYRUP.

Thirteen samples were examined. In only one case was the refractometer dry substance higher than the true, and in all others the true dry substance was higher. This difference ranged from 0.20% to 1.34% with an average of 0.50%.

WITH CANE SYRUP.

Ten samples were examined. In three cases the refractometer dry substance was higher than the true by 0.16%, 0.34%, 0.62%. The other cases range from 0.24% to 0.93%, or an average difference on the whole of 0.29%.

HONEYS.

Twenty-four samples were examined. In 2 cases the refractometer dry substance was higher than the true by 0.21% and 0.91%. In all the rest it was lower by from 1.15% to 2.52%, with an average of 1.45%. This is the greatest difference noted. One of three causes or all may account for this large difference. (1) The actual dry substance may not be right, viz., this product may not give up all of its water at 70° in vacuum, or, (2) the dextrin of the honey may change the refractive index of the

[1]Inter. Sugar J. (1907), *9*, 481.

[2]NOTE. See Jour. Amer. Chem. Soc. (1908), *30*, 1443, for a previous paper on this subject by the author.

whole, or, (3) the values given in the table for dry substance from refractive index may not be right.

COMMERCIAL GLUCOSE.

The two samples examined show the refractive index dry substance higher by 0.27% than the real dry substance. The closeness of these readings would tend to disprove the second cause for honey.

CANE MOLASSES.

Seventeen samples were examined. In 3 cases the refractometer dry substance was higher than the true by 0.16%, 0.39%, and 0.59%. In all the rest it was lower by from 0.38% to 1.53%. The average difference was 0.79%.

BEET MOLASSES.

Fifteen samples were examined. In all cases the true dry substance was higher than the refractometer. The difference varied from 0.38% to 1.83%, with an average of 1.08%. When the original substance was diluted one-half with water, and a reading made on this, the dry substance obtained was doubled. The results showed 5 cases where the refractive index dry substance was higher than the true by from 0.25% to 0.53%. In all other cases, the true was the highest by from 0.39% to 1.62%, with an average of 0.36%. It is seen then by dilution, the average difference between the true dry substance and refractometer has dropped from 1.08% to 0.36%. The results then are nearer the true dry substance. This comes about by being able to get a clearer field and thereby a closer reading.

However, later work has shown that this dilution with water, even though it has brought the dry substance by refractometer nearer the true dry substance, introduces a serious error. When water is added to molasses there is a contraction in volume.

This contraction has been taken into account in the construction of specific gravity and refractometer table for pure sugars so that solutions of the latter, whether mixed with water or a sugar syrup, will give the correct percentage of solids either by specific gravity or refractive index.

The impurities, however, which accompany sugars in solution in molasses, have not only a different specific gravity than sugar, but also a different contraction co-efficient, so that the solution diluted with water shows a different specific gravity or refractive index than that calculated from tables for pure sugars.

To reduce these variations of contraction to the minimum, a concentrated pure sugar solution is used as a dilutant. Results obtained with some cane molasses samples show the error that is introduced by the water dilution and also the effect of the sugar dilution.

Sample No.	Undiluted Molasses.	Diluted Half With—	
		Water.	Sugar Sol.
1	80.57	83.24	80.91
2	72.32	72.94	72.21
3	77.92	78.44	77.91
4	73.92	75.34	73.81
5	82.05	84.44	82.41

In the undiluted form all of these can be easily read. The half dilution with water is anywhere from .62% to 2.7% higher than undiluted while the half dilution with sugar solution varies from 0.0 to 0.3%.

Tischtschenko (Z. des Vereins Deut. Zuckerind., Feb. 1909, 103), calls attention to this possible error in the determination and recommends the use of a pure sugar solution. Von Lippman corroborates the results (Deut. Zuckerind., 34, 1909, 401). It therefore behooves us to use sugar solution in diluting our dark colored solution in preference to water. The formula for calculating the dry substance when using a concentrated sugar solution as a dilutant is:

$$X = \frac{(A + B)\,C - BD}{A}$$

in which X=% dry substance of the original sample, (A) the grams of the original substance mixed with (B) the grams of concentrated pure sugar solution, (C) the % dry substance of the mixture obtained from its refractive index, and D= the % dry substance of the pure sugar solution obtained from its refractive index. The method of procedure is simply the preparation of a concentrated granulated sugar solution and mixing in a small beaker a weighed quantity of this with a weighed quantity of the original solution or sample, and taking refractive index of the mixture.

Summarizing the average results, we find that the refractometer dry substance is higher than the true.

	Per Cent.
The difference in case of maple syrup	0.50
The difference in case of cane syrup	0.29
The difference in case of honeys	1.45
The difference in case of glucose	0.27
The difference in case of cane molasses	0.79
The difference in case of beet molasses	1.08
The difference in case of beet molasses (half)	0.36

With the exception of the honeys and possibly cane molasses, also beet molasses undiluted, the differences are well within the error of determination of water by actual drying. By half dilution, the beet molasses is brought within the limits, and where dilution with sugar solution tried this difference would be cut down considerably. Cane molasses, showing 0.79%, might be considered within the limits, as a true moisture content on this material is a difficult task. Honeys are, then, the only ones whose difference is large, but it is hoped that with the work now being carried on, the reason for this difference will be obtained and a method for procedure be established for this grade of substance. However, there is one thing to be said in regard to the refractometer, that it is possible to obtain duplicate results that are identical, and different investigators should obtain identical results, which is a condition that does not exist with the other methods for dry substance determination in use now. The refractometer method has the advantage of being quick and not losing accuracy by speed, and then only small portions are necessary for a determination.

The method of making a dry substance determination is substantially this: The instrument (Fig. 1) is placed so that the light falls on the mirror (R) and this is turned on its axis to reflect the light up through the prism (B) and (A). The source of light can be daylight, but a better one is a 32 or higher candle power lamp. The tubular (D) is connected by rubber tubing to the source of water supply of constant temperature and the other tubular (E) has a rubber overflow connection. The thermometer is placed in its socket. The optical parts of the instrument are turned forward on the stand (a). By turning the catch (V) the prism B is swung open on (C) from prism (A) and a few drops of the solution to be examined is placed on the prism (A). Enough of the solution should be added so that on closing the prism (B) on (A) a part of the liquid is forced out. The optical parts are brought back into their original place.

The arm carrying the magnifying glass (L) should be down to the 1.3 end of the scale. Then by looking through the eyepiece (F), focusing the cross-hairs into plain view, the arm (L) is moved until a bright color appears in the lower half of the field. By turning the milled screw (M)

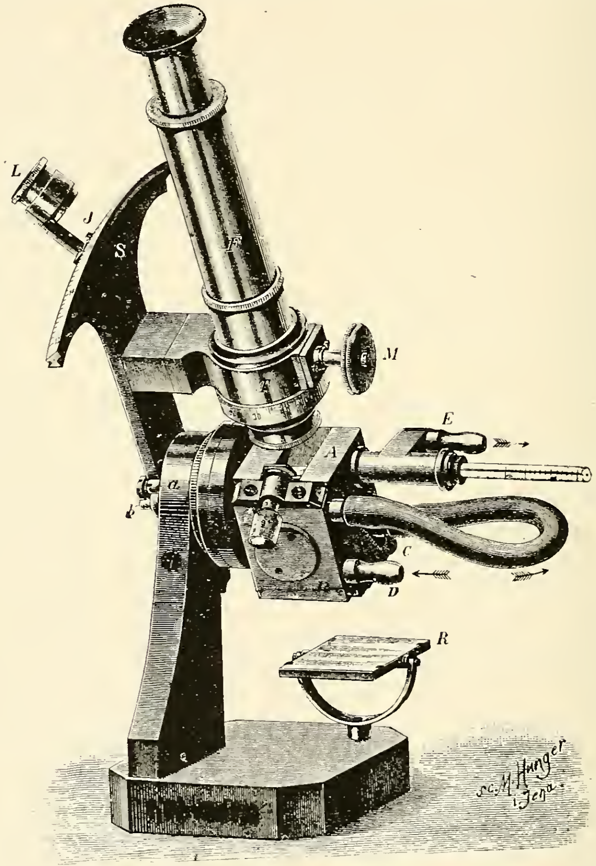

Fig. 1.

of the compensator the line of color is made more distinct; viz., there is sharpness of line dividing the light and the dark field. By moving the arm farther this line is brought up to a point where it coincides with the intersection of the cross-hairs. At this point, the index of refraction is read on the scale (J). At the same time the temperature is read on the thermometer.

The instrument should be tested first with water and its accuracy established thereby: Ref. Ind. of water at 20° being 1.3330. The substance to be examined should be at about the same temperature at which the readings are to be made. Therefore waiting a few minutes after applying the liquid to the prism, it could be considered that this is at the same temperature.

Tables have been prepared for converting the refractometer readings to dry substance or per cent. of moisture. To some of these previous reference has been made. Hugh Main, in the International Sugar Journal of 1907, Vol. 9, page 481, gives a table covering this. The readings are to be made at 20° C. Geerligs has published a table also for dry substance from the refractive index. The temperature of the reading is 28° C. and he has also prepared a table for corrections for other temperatures than 28°. These are now given:

Geerlig's Table for Dry Substance in Sugar-House Products by Abbè Refractometer, at 28° C.

(Intern. Sugar J., *10*, 69-70.)

Index.	Per Cent. Dry Substance.	Decimals.	
1.3335	1	0.0001=0.05	0.0010=0.75
1.3349	2	0.0002=0.1	0.0011=0.8
1.3364	3	0.0003=0.2	0.0012=0.8
1.3379	4	0.0004=0.25	0.0013=0.85
1.3394	5	0.0005=0.3	0.0014=0.9
1.3409	6	0.0006=0.4	0.0015=1.0
1.3424	7	0.0007=0.5	
1.3439	8	0.0008=0.6	
1.3454	9	0.0009=0.7	
1.3469	10		
1.3484	11	0.0001=0.05	
1.3500	12	0.0002=0.1	
1.3516	13	0.0003=0.2	
1.3530	14	0.0004=0.25	
1.3546	15	0.0005=0.3	
1.3562	16	0.0006=0.4	
1.3578	17	0.0007=0.45	
1.3594	18	0.0008=0.5	
1.3611	19	0.0009=0.6	
1.3627	20	0.0010=0.65	
1.3644	21	0.0011=0.7	
1.3661	22	0.0012=0.75	
1.3678	23	0.0013=0.8	
1.3695	24	0.0014=0.85	
1.3712	25	0.0015=0.9	
1.3729	26	0.0016=0.95	
1.3746	27	0.0001=0.05	0.0012=0.6
1.3764	28	0.0002=0.1	0.0013=0.65
1.3782	29	0.0003=0.15	0.0014=0.7
1.3800	30	0.0004=0.2	0.0015=0.75
1.3818	31	0.0005=0.25	0.0016=0.8
1.3836	32	0.0006=0.3	0.0017=0.85
1.3854	33	0.0007=0.35	0.0018=0.9
1.3872	34	0.0008=0.4	0.0019=0.95
1.3890	35	0.0009=0.45	0.0020=1.0
1.3909	36	0.0010=0.5	0.0021=1.0
1.3928	37	0.0011=0.55	
1.3947	38		
1.3966	39		
1.3984	40		
1.4003	41		
1.4023	42	0.0001=0.05	0.0012=0.6
1.4043	43	0.0002=0.1	0.0013=0.65
1.4063	44	0.0003=0.15	0.0014=0.7
1.4083	45	0.0004=0.2	0.0015=0.75

Index.	Per Cent. Dry Substance.	Decimals.	
1.4104	46	0.0005=0.25	0.0016=0.8
1.4124	47	0.0006=0.3	0.0017=0.85
1.4145	48	0.0007=0.35	0.0018=0.9
1.4166	49	0.0008=0.4	0.0019=0.95
1.4186	50	0.0009=0.45	0.0020=1.0
1.4207	51	0.0010=0.5	0.0021=1.0
1.4228	52	0.0011=0.55	
1.4249	53		
1.4270	54		
1.4292	55	0.0001=0.05	0.0013=0.55
1.4314	56	0.0002=0.1	0.0014=0.6
1.4337	57	0.0003=0.1	0.0015=0.65
1.4359	58	0.0004=0.15	0.0016=0.7
1.4382	59	0.0005=0.2	0.0017=0.75
1.4405	60	0.0006=0.25	0.0018=0.8
1.4428	61	0.0007=0.3	0.0019=0.85
1.4451	62	0.0008=0.35	0.0020=0.9
1.4474	63	0.0009=0.4	0.0021=0.9
1.4497	64	0.0010=0.45	0.0022=0.95
1.4520	65	0.0011=0.5	0.0023=1.0
1.4543	66	0.0012=0.5	0.0024=1.0
1.4567	67		
1.4591	68		
1.4615	69		
1.4639	70		
1.4663	71		
1.4687	72		
1.4711	73	0.0001=0.0	0.0015=0.55
1.4736	74	0.0002=0.05	0.0016=0.6
1.4761	75	0.0003=0.1	0.0017=0.65
1.4786	76	0.0004=0.15	0.0018=0.65
1.4811	77	0.0005=0.2	0.0019=0.7
1.4836	78	0.0006=0.2	0.0020=0.75
1.4862	79	0.0007=0.25	0.0021=0.8
1.4888	80	0.0008=0.3	0.0022=0.8
1.4914	81	0.0009=0.35	0.0023=0.85
1.4940	82	0.0010=0.35	0.0024=0.9
1.4966	83	0.0011=0.4	0.0025=0.9
1.4992	84	0.0012=0.45	0.0026=0.95
1.5019	85	0.0013=0.5	0.0027=1.0
1.5046	86	0.0014=0.5	0.0028=1.0
1.5073	87		
1.5100	88		
1.5127	89		
1.5155	90		

Table of Corrections for the Temperature.

Temperature of the Prisms in ° C.	Dry Substance.												
	0	5	10	15	20	25	30	40	50	60	70	80	90
	Subtract.												
20	0.53	0.54	0.55	0.56	0.57	0.58	0.60	0.62	0.64	0.62	0.61	0.60	0.58
21	0.46	0.47	0.48	0.49	0.50	0.51	0.52	0.54	0.56	0.54	0.53	0.52	0.50
22	0.40	0.41	0.42	0.42	0.43	0.44	0.45	0.47	0.48	0.47	0.46	0.45	0.44
23	0.33	0.33	0.34	0.35	0.36	0.37	0.38	0.39	0.40	0.39	0.38	0.38	0.38
24	0.26	0.26	0.27	0.28	0.28	0.29	0.30	0.31	0.32	0.31	0.31	0.30	0.30
25	0 20	0.20	0.21	0.21	0.22	0.22	0.23	0.23	0.24	0.23	0.23	0.23	0.22
26	0.12	0.12	0.13	0.14	0.14	0.14	0.15	0.15	0.16	0.16	0.16	0.15	0.14
27	0.07	0.07	0.07	0.07	0.07	0.07	0.08	0.08	0.08	0.08	0.08	0.08	0.07
	Add.												
29	0.07	0.07	0.07	0.07	0.07	0.07	0.08	0.08	0.08	0.08	0.08	0.08	0.07
30	0.12	0.12	0.13	0.14	0.14	0.14	0.15	0.15	0.16	0.16	0.16	0.15	0.14
31	0.20	0.20	0.21	0.21	0.22	0.22	0.23	0.23	0.24	0.23	0.23	0.23	0.22
32	0.26	0.26	0.27	0.28	0.28	0.29	0.30	0.31	0.32	0.31	0.31	0.30	0.30
33	0.33	0.33	0.34	0.35	0.36	0.37	0.38	0.39	0.40	0.39	0.38	0.38	0.38
34	0.40	0.41	0.42	0.42	0.43	0.44	0.45	0.47	0 48	0.47	0.46	0.45	0.44
35	0.46	0.47	0.48	0.49	0.50	0.51	0.52	0.51	0.56	0.54	0.53	0.52	0.50

Example: Desired, the dry substance of a sample whose refractive index is 1.4589 taken at 26° temperature. The nearest index is 1.4567, which equals 67% then 1.4589 minus 1.4567 (the nearest value in the table lower than it) =.0022. In the decimal column opposite look for .0022 and one finds a value of 0.95. So the reading is 67.95 but at a temperature of 26° (from the table of corrections) .16 must be subtracted or the correct dry substance would be 67.79. In like manner the dry substance of a sample with a refractive index of 1.5021 at 28° C. would be 85.05, and one of 1.3802 at 28° would be 30.1, and one of 1.3655 at 33° C. would be 22.06.

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Conductivity of Certain Salts in Ethyl Amine.

By Edward G. Mahin.

The conductivities of silver nitrate, lithium chloride and ammonium chloride in ethyl amine were measured by Shinn,[1] who showed that the molecular conductivities change with dilution in an unexpected manner. In the case of silver nitrate the molecular conductivity decreases with dilution until V=75.15, this being the highest dilution used. The molecular conductivity of lithium chloride increases with dilution until V=0.867, then decreases until V=21.08, after which it apparently slightly increases. The molecular conductivity of ammonium chloride decreases with dilution until V=18.24, after which it increases. These facts would not seem remarkable were it not for the concluding words of the author's paper. After summarizing the results of his experimental work, he says:

"From the standpoint of the theory of electrolytic dissociation the electrical behavior of solutions in primary and secondary amines and in amides, so far as such solutions have been studied, is inexplicable. The facts that for one and the same solute the conductivities of solutions may not only be very large or very small, but may increase or decrease with dilution, or attain maximum or minimum values depending upon the specific nature of the solvent, suggest that the role of the solvent in the process of the transmission of an electric current through a solution is, in all probability, a very active rather than an indifferent one, and does not materially differ from that of the solute. In such event, the prevalent conception of 'molecular conductivity' becomes not only meaningless, but misleading."

In arriving at the conclusions here indicated it would seem that the author has overlooked certain facts which may not only serve to explain the apparent departure from the dilution laws, but which would make this departure seem inevitable. It has long been known that the aliphatic amines are strongly basic substances, forming simple salts analogous to the ammonium salts, as well as complex metallic salts which are analogous to those where hydrogen of the ammonium radicle is substituted by a metal. Indeed, this salt formation is to be expected since the ali-

[1] J. Phys. Ch., *11*, 537.

phatic primary amines are members of a series of mono-substituted ammonias, of which the basicity is greater than that of the mother substance.

Köhler[1] isolated a salt having the composition represented by the formula $C_2H_5NH_2.HgCl$. Müller[2] investigated double salts of ethyl amine with palladium, Jorgensen[3] those with platinum, Carson and Norton[4] those with uranium, Bailey[5] with vanadium, Bonnefoi[6] with lithium chloride, and Hoffman and Marburg[7] with mercuric chloride. In most cases more than one salt was produced by varying the proportions of ethyl amine and the simple salt used. Hoffman and Marburg isolated and studied the compounds $C_2H_5NH_2.HgCl_2$, $(C_2H_5NH_2)_2HgCl_2$ and $C_2H_5NH.HgCl$. Bonnefoi found that by leading the vapor of ethyl amine over dry lithium chloride various double salts were produced, the proportion of the constituents depending upon the temperature. The following compounds were formed under the conditions indicated:

Temp.	*Formula.*	*Heat of Formation, Calories.*
70°	$C_2H_5NH_2$ .LiCl	+ 13834
58°–70°	$(C_2H_5NH_2)_2$ .LiCl	+ 24817
Ord. to 58°	$(C_2H_5NH_2)_3$ .LiCl	+ 35387

It seems probable, in the light of these facts, that at still lower temperatures other compounds will be present, having a still higher ratio of ethyl amine to the original salt; this should be particularly true with regard to solutions in liquid, anhydrous ethyl amine. In other words, we are here dealing with an application of the mass law, where the temperature and mass of the reacting substances are to be considered in the attempt to solve the problem regarding the composition of the resulting compound. We should expect that any solution would contain several compounds of the constituents, having a certain average composition which would depend upon the temperature and degree of dilution.

Shinn[8] tested, in an approximate but not quantitative manner, the action of ethyl amine upon 14 salts, concerning which the following résumé is here given:

[1] Ber., *12*, 2323.
[2] Ann., *86*, 366.
[3] J. pr. Ch., (2) *33*, 517.
[4] Am. Ch. J., *10*, 220.
[5] J. Ch. Soc., *45*, 693.
[6] C. r., *129*, 1257.
[7] Ann., *305*, 191.
[8] Loc. cit.

NH_4Cl. . . .Very soluble with evolution of ammonia.
LiCl . . . . .Soluble.
$FeCl_3$. . . . .Slightly soluble.
$SnCl_2$. . . . .Insoluble, unchanged.
$CoCl_2$ . . . .Reacts with evolution of heat, forming greenish yellow precipitate.
$PbBr_2$ . . . .Reacts, forming white precipitate which afterward redissolves.
KI . . . . . .Insoluble, unchanged.
CdI_2. . . . . .Reacts, forming white, insoluble precipitate.
AgCN . . . .Slightly soluble.
$Hg(CN)_2$. .Slightly soluble.
$AgNO_3$ . . .Soluble with evolution of much heat.
$NaNO_3$ . . .Insoluble, unchanged.
$Pb(NO_3)_2$. .Reacts, forming white, insoluble precipitate.

It is thus seen that, in all cases where the salt dissolves appreciably, there is evidence of chemical action, either through the evolution of heat or the formation of a precipitate, or both. In the case of ammonium chloride the well known action of evolution of ammonia was observed. There is, therefore, every reason for expecting that complex salts will be formed in every case excepting the last, where no doubt ethyl amine hydrochloride is produced, as Shinn has pointed out. If this be true, the question still remains as to whether the reaction is complete as soon as the salt is all in solution, so that henceforth all physical properties will be those of a solution in ethyl amine of a definite double or complex salt, changing with dilution only with respect to the degree of ionization. With the investigations of Hofmann and Marburg and of Bonnefoi in mind, the answer to this question would certainly be negative. We should expect that the ratio of ethyl amine to simple salt combined with it would not only change with lowering of temperature, but that it would increase with decreasing concentration, because as dilution progresses the ratio of amine to salt in solution increases. If the conductivity of the complex salt is much less than that of the simple salt the change in molecular conductivity with change in concentration would be the resultant of two influences, i. e., change in ionization and change in complexity of the ions. The migration velocity of a complex cation containing one or more molecules of ethyl amine could not be very high, and it is not likely that such a compound would possess a very high degree of ionization. This fact would then result in a more or less gradual tendency toward falling off in the molecular conductivity with increasing dilution, since we are actually dealing not only with more complex compounds, whose ionization is probably less than that of the simpler ones, but also with more complex ions, whose velocity is probably less than that of the simpler ones. If, however, the ionization resulting from dilution proceeds at a greater rate than does the change in complexity, increase in molecular conductivity would then be the rule. This actually happens for a certain range in the case of lithium chloride, then later the increasing complexity of the ions perhaps gains the ascendancy and molecular conductivity decreases with further dilution. The effect of dilution upon molecular conductivity will necessarily be somewhat complicated, if the preceding reasoning is correct, involving at least the following changes: (a) Increase in molecular complexity, through increase in the active mass of ethyl amine, (b) change (probably decrease) in *ionization constant* because of increasing complexity, (c) increase in ionization of any one form, since at any dilution a considerable number of different complexes are probably present, and (d) probable decrease in migration velocity on account of increasing complexity of the ions.

This would seem to be merely another special case of the influence of solvate formation upon conductivity, and such influences have long been known. The formation of hydrates, for example, has a very marked effect upon the conductivity and upon the lowering of freezing point and vapor pressure of aqueous solutions.

In the case of solutions of ammonium chloride in ethyl amine it is by no means certain that the entire amount of salt is converted at once into ethyl amine hydrochloride when brought into a solution of any concentration. We should certainly expect that equilibrium will result when a certain amount of ammonium chloride remains as such in the solution, this amount becoming smaller as dilution proceeds. The molecular conductivity will then depend upon (a) the ratio of ethyl amine hydrochloride to ammonium chloride, (b) the relative ionization constants of the two compounds and (c) the relative migration velocities of the two (or more) cations involved.

The theory of electrolytic dissociation has proven of so great value to physical chemistry and has piloted the way to so many valuable investigations that one cannot fail to realize its importance. This does not mean that its imperfections should be ignored or that there should be any cessation in the search for facts which will test it to the extreme. But so many supposed objections have been urged against it that, on closer investigation, have been found to entirely conform to the theory or to require only minor modifications, that we hesitate to accept such a sweeping statement as that contained in Shinn's paper. The facts cited do not necessarily conflict with the theory—indeed, they would seem to point to the truth of the theory. What is needed is more experimental evidence covering these points.

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A Study of the Chemical Composition of Butter Fat, and Its Relation to the Composition of Butter.

By O. F. Hunziker and George Spitzer.

SUMMARY OF AUTHENTIC KNOWLEDGE OF THE COMPOSITION OF BUTTER FAT.

Milk fat or butter fat consists of triglycerides of fatty acids. The fatty acids of butter fat are monobasic and have the general formula C_nH_{2n+1} CO OH, except oleic acid, which is a non-saturated acid belonging to the acrylic series with the general formula C_nH_{2n-1} CO OH. The triglycerides of butter fat do not exist as glycerides of one fatty acid, but as a mixture of several acid radicals combined with glycerin. Glycerin is a triatomic alcohol, $C_3H_5(OH)_3$. Theoretically, therefore, the milk fat could contain triglycerides of the fatty acids present, that is, there could be tributyrin, triolein, tristearin, etc. In reality no such combination exists. Just in what order the triglycerides are present has not been definitely established. The acids present in butter fat are butyric, caproic, caprylic, capric, lauric, myristic, palmitic, oleic and stearic.

Bell[1] holds that butter fat consists of mixed glycerides, glycerides in the molecule of which the glycerol is combined with three different acid radicals forming a compound having the following composition:

$$C_3H_5\begin{cases} OC_4H_7O \\ OC_{16}H_{31}O \\ OC_{18}H_{33}O \end{cases}$$

This theory is supported by the fact that the glycerol forms triacid compounds and not compounds of one acid, which theoretically could be possible. If the glycerol formed monoacid compounds, butter fat would contain glyceryl tributyrates, caproates, stearates, etc.

SOLUBILITY OF BUTTER FATS IN ALCOHOL.

If butter fat is dissolved in alcohol, from 1.1 to 3.3 per cent of the fat goes into solution, the solubility depending on the temperature of the alcohol. If tributyrin existed in butter fat, all of the tributyrin would

[1] The Chemistry of Foods, Vol. II, page 44.

go into solution.[1] Analyses of the portion soluble in alcohol show that this is not the case. Tables I and II give the value of the constants as determined for the portion soluble in cold alcohol, the portion not soluble in cold alcohol, but soluble in hot alcohol, and the portion not soluble in either hot or cold alcohol.

TABLE I.

	Portion Soluble in Alcohol at 20 deg. C	Portion Not Soluble in Cold Alcohol, but Soluble in Alcohol at 75 deg. C.	Portion Not Soluble in Either Hot or Cold Alcohol.
Reichert-Meissl Number	48.1	29.6	20.7
Melting point	16.9°C.	31.5°C.	36.0°C.
Soluble acids (as Butyric)	9.79%	6.60%	4.26%

TABLE II.

	At 20°C.	At 75°C.
Solubility of butter fat in 95 % alcohol	1.1%	3.3%

The melting point of the portion soluble in alcohol at 20° C. is 16.9° C., while that of the portion not soluble in either hot or cold alcohol is 36° C., showing a difference of 19.1° C. The Reichert-Meissl No. in the portion soluble in alcohol is 48.1, in the portion not soluble in alcohol it is 20.7, showing a difference of 27.3. Since only 1.1 per cent of the fat is soluble in cold alcohol, this would indicate that no tributyrin exists in butter fat. This fact becomes still more evident by an examination of the molecular weight of the glycerides soluble in alcohol and those not soluble in alcohol, as calculated from the figures in Table VII.

[1] Cochran, "Action of Alcohol on Butter Fats," Analyst, Vol. 13, page 55. Lewkowitsch, "Oils, Fats and Waxes," Vol. II, page 675, 1909.

The saponification of a neutral fat yields a perfectly definite compound. This saponification takes place according to the following reaction for an ester with three molecules of acid combined with the radical of a trihydric alcohol:

$$C_3H_5\begin{cases}R_1\\R_2\\R_3\end{cases}+3KOH=C_3H_5(OH)_3+K(R_1+R_2+R_3)$$

The molecular weight of the triglyceride is calculated as follows: Determine the per cent of KOH required to saponify the fat, and divide the molecular weight of 3(KOH) by the per cent thus obtained, or multiply the saponification equivalent by three.

Thus, from the figures in Table VII the saponification equivalent of the portion soluble in alcohol was found to be 216.5. This multiplied by 3 is 649.5. This equals the molecular weight of the triglyceride.

The saponification equivalent of the portion not soluble in alcohol was found to be 260.9; this multiplied by 3 equals the molecular weight, 782.7. The molecular weight of butyrin $C_3H_5(C_4H_7O)_3$ is 302, while the molecular weight of the triglycerides of the soft portion is 649.5.

The fact that only 1 per cent of the butter fat was dissolved in cold alcohol shows clearly the absence of tributyrin, otherwise the per cent of alcohol-soluble fat would be higher. The soft portion must, therefore, be made up of mixed glycerides of the acids found in butter fat, the acids having a low melting point and a low molecular weight predominating.

FRACTIONAL SEPARATION OR CRYSTALLIZATION OF BUTTER FATS.

The same condition presents itself if butter fat is subjected to fractional separation. When butter fat is exposed to a temperature below the melting point of the harder glycerides, the softer glycerides separate from the harder glycerides. When this process is repeated by lowering the temperature after each separation, a separation can be effected whereby the constants differ widely from the original mixed glycerides.[1] Table III shows the variation of the constants of the fats thus separated. The butter used in this experiment was made in March.

[1] Richmond Dairy Chemistry, page 37.

TABLE III.

Composition of Portions of the Butter Fat Obtained by Fractional Separation.

	Original Butter Fat.	Soft Portion.	Hard Portion.
Reichert-Meissl Number	29.06	32.65	26.74
Iodine number	34.97	42.10	30.11
Koetts. saponification number	229.5	233.87	228.8
Refractive index at 40° C.	Reading 44.1 1.4552	Reading 45.1 1.456	Reading 43.1 1.4546
Melting point	34° C.	14.5°C.	37.8°C.
Per cent insoluble acids	88.76	87.89	89.47
Melting point of insoluble acids	40°C.	36.5°C.	42.5°C.
Per cent soluble acids (as butyric)	5.89	6.67	5.46
Koetts. saponification number insoluble acids	219.5	221.35	218.8
Iodine number insoluble acids	37.36	45.05	33.48

Later in the season (in May) another sample of butter was treated similarly, separating the liquid from the solid portions of the fat, and the constants were determined as shown in Table IV.

TABLE IV.

Composition of Portions of the Butter Fat Obtained by Fractional Separation.

	Original Butter Fat.	Soft Portion.	Hard Portion.
Reichert-Meissl number	30.00	33.85	24.66
Iodine number	39.82	43.55	33.08
Koetts. saponification number	230.1	232.78	226.4
Refractive index at 40° C.	Reading 44 1.4552	Reading 44.8 1.4558	Reading 43 1.4545
Melting point	32.5°C.	13.2°C.	38.1°C.
Per cent insoluble acids	87.54	86.67	88.64
Melting point insoluble acids	39.2°C.	35.3°C.	42.4°C.
Per cent soluble acids (as Butyric)	6.09	6.90	5.17
Koetts. saponification number insoluble acids	220.53	221.6	218.7
Iodine number insoluble acids	42.14	46.2	35.66
Per cent glycerin	12.58	12.89	12.33

Tables III and IV show that the soft portions contain more volatile or soluble acids, also a greater per cent of oleic acid in combination with the glycerol base than the hard portions. The melting point of the soft portions was 22.8° C. and 24.9° C., respectively, lower than the melting point of the hard portions.

The difference in the melting points between the soft and hard portions of the insoluble fatty acids was not as great as that of the soft and hard portion of the glycerides from which the insoluble acids were derived. The reason for this must lie in the fact that the soluble fatty acids have been removed and that, therefore, the melting points of the different portions of the insoluble fatty acids depend almost entirely on the per cent of oleic acid present.

The soft portion of the glycerides is made up of a higher per cent of acids with a lower melting point, i. e., oleic and soluble acids. The soluble acids have a very low melting point. Therefore, even a slight increase in the per cent of soluble acids must cause a lowering of the melting point.

Tables V–A and V–B show a comparison of the iodine number of the soft and hard portions of the glycerides and of the insoluble acids derived from the glycerides. The iodine number of the soft and hard portions of the insoluble acids is higher than that of the corresponding portions of the glycerides of the butter fat. This is natural. The soluble acids and glycerin have been removed from the glycerides, raising the per cent. of the remaining constituents of the insoluble acids above that in the glycerides.

TABLE V–A.

Iodine No. of Soft and Hard Portions of Butter Fat.

	Soft Portion Iodine Number.	Hard Portion Iodine Number.	Soft Portion Per cent Olein.	Hard Portion Per cent Olein.	Gain Iodine Number.	Gain Per cent Olein.	Per cent Gain Olein of Soft Portion Over Hard Portion
From table III........	42.10	30.11	48.83	34.92	11.99	13.72	39.31
From table IV.........	43.55	33.08	50.518	38.37	10.47	12.148	31.6

TABLE V–B.

Iodine No. of Insoluble Acids of Soft and Hard Portions.

	Soft Portion Iodine Number.	Hard Portion Iodine Number.	Soft Portion Per cent Olein.	Hard Portion Per cent Olein.	Gain Iodine Number.	Gain Per cent Olein.	Per cent Gain Olein of Soft Portion Over Hard Portion.
From table III........	45.05	33.48	52.25	38.84	11.57	13.41	34.5
From table IV.........	46.2	35.66	53.59	41.36	10.54	12.23	29.5

CONCERNING THE SOLUBLE FATTY ACIDS.

Table VI shows the per cent of soluble fatty acids and glycerin in the soft and hard portions of butter fat. The soft portion contained 2.06 per cent more soluble acids and .56 per cent more glycerin than the hard portion, as obtained from data in Table IV.

TABLE VI.

Per Cent of Soluble Acids and Glycerin in Soft and Hard Portions.

	Soft Portion.	Hard Portion.	Gain.
Per cent soluble acids..........	8.23	6.17	2.06
Per cent Glycerol..........	12.89	12.33	.56

The soluble acids were calculated on the basis of a mean molecular weight of 104.5. This molecular weight was calculated from the amount of glycerides of the soluble acids and other data taken from Table IV.

The glycerol (C_3H_5) is calculated from the per cent of soluble acids, mean molecular weight 104.5. From this calculation the per cent of glycerin $C_3H_5\ (OH)_3$ is readily determined.

The general formula for one molecule of a triglyceride is $C_3H_5(R)_3$, where R stands for mixed acid radicals R_3=104.5X3=313.5; allowing for the basic hydrogen C_3H_2=38, then the molecular weight of the triglyceride $C_3H_5(R)_3$ is 351.5.

$$351.5 : 38 = 8.23 : X$$

$$X = .888\%\ C_3H_2$$

From these results the per cent of glycerin is calculated as follows, the molecular weight of glycerin being 92:

$$38 : 92 = .888 : X$$

$$X = 2.14$$

This is the per cent of glycerin combined with the soluble acids of the soft portion.

Likewise, the per cent of the glycerin combined with the soluble acids of the hard portion is calculated:

$$351.5 : 38 = 6.17 : X$$

$$X = .888\%\ C_3H_2$$

$$38 : 92 = .666 : X$$

$$X = 1.61$$

This is the per cent of glycerin combined with the soluble acids of the hard portion.

The difference between the per cent of glycerin combined with the per cent of soluble acids of the soft portion and the per cent of glycerin combined with the per cent soluble acids of the hard portion, then, is 2.14—1.61=.53%. This agrees closely with the difference of the glycerin between hard and soft portions as shown by analyses. (See Table VI.)

The per cent of glycerin combined with the insoluble acids is nearly the same in both soft and hard portions, because the per cent of insoluble acids in the soft and hard portions differs very little. Also the variation in the composition of the insoluble acids would not materially affect the molecular weight. Therefore, it is reasonable to expect that nearly the same per cent of glycerin is combined with the insoluble acids of both the soft and the hard portions.

RELATION OF COMPOSITION OF BUTTER FAT SOLUBLE AND INSOLUBLE IN ALCOHOL TO COMPOSITION OF SOFT AND HARD PORTIONS OF FAT OBTAINED BY FRACTIONAL SEPARATION.

A comparison of the constants of the soft and hard portions with the constants of the fats soluble and insoluble in alcohol shows a close relation. The results are summarized in Table VII.

TABLE VII.

Showing the Variation of the Constants of the Soluble and Insoluble Portions in Alcohol, Also of the Soft and Hard Portions of Butter Fat Taken for the Experiment.

	A			B		
	Alcohol—Soluble Portion.	Alcohol—Insoluble Portion.	Original Butter Fat.	Soft Portion.	Hard Portion.	Original Butter Fat.
Reichert-Meissl number	48.1	20.7	27.70	33.85	24.66	30.00
Melting point	16.9°C.	36.°C.	33.5°C.	13.2°C.	38.1°C.	32.5°C.
Iodine number	34.07	39.75	37.63	43.55	33.08	39.82
Koetts. saponification number	259.14	215.06	227.4	232.78	226.4	230.1
Saponification equivalent	216.5	260.9	246.79	241.1	248.3	244.0
Refractive index at 40° C.	Reading 42.7 1.4543	Reading 45.6 1.4563	Reading 44.4 1.4555	Reading 44.8 1.4558	Reading 43 1.4545	Reading 44 1.4552
Per cent soluble acids (as Butyric)	9.792	4.26	6.60	6.90	5.17	6.09

These data give the composition of the portions of fat soluble in alcohol and of the original butter fat; also the composition of the soft and hard portions of butter fat separated by fractional crystallization and of the original butter fat. The samples A and B of butter fat used for the two experiments were not taken from the same lot of butter.

The Reichert-Meissl No. is distinctly higher in the fat soluble in alcohol and in the fat of the soft portion, than it is in the fat insoluble in alcohol and in the fat of the hard portion, as well as in the original fat.

The melting point is lowest in both the fat soluble in alcohol and in the fat of the soft portion.

On the other hand, the iodine number is lowest in the fat soluble in alcohol and highest in the fat of the soft portion.

The figures in the above table show the influence of the constants on the melting point of butter fat. The portion of fat insoluble in alcohol and the original fat from which the above portion was taken show a decidedly higher iodine number than the portion soluble in alcohol. If the melting point depended solely on the iodine number, the melting point of the fat insoluble in alcohol and of the original butter fat would be distinctly lower than the melting point of the portion soluble in alcohol. Table VII shows that this is not the case. The melting point of the portion insoluble in alcohol and of the original butter fat is a great deal higher (19.1° C. and 16.6° C., respectively, higher) than the melting point of the fat soluble in alcohol. The only factor to which this fact can be attributed is the high Reichert-Meissl No. in the case of the fat soluble in alcohol, as compared with the low Reichert-Meissl No. of the fat insoluble in alcohol and of the original butter fat. These results make it perfectly clear that the softness or hardness (melting point) of butter fat is dependent to a great degree on the per cent of soluble fatty acids present.

This table further shows, as stated in the previous chapters, that butter fat is a mixture of triglycerides of different fatty acids. The soft portion is the result of mechanical separation at different temperatures. It, therefore, contains more glycerides combined with acids of low melting points including oleic and soluble acids. Furthermore, the fat soluble in alcohol represents glycerides of acids soluble in alcohol. Since it is known that some of the glycerides of the soluble acids are soluble in alcohol, we can assume that some of the molecules in butter fat are made up of the glycerides containing a larger proportion of the soluble acids than others.

CONDITIONS AFFECTING THE COMPOSITION OF BUTTER FAT.

The composition of butter fat varies with the season of the year. A series of analyses of butter fat of butter made during each of the twelve months of the year, yielded the results summarized in Table VIII.

The results in Table VIII show that the Reichert-Meissl number was lowest in October, increasing steadily until it reached its maximum in March. After March it dropped abruptly, holding about its own till July, then taking a second drop and declining slightly toward October.

TABLE VIII.

Effect of the Season of Year on the Composition of Butter Fat.

	Reichert Meissl Number.	Iodine Number.	Melting Point.
January	30.03	31.20	33.4° C.
February	30.58	31.97	33.5° C.
March	31.30	31.94	33.5° C.
April	29.35	35.83	33.3° C.
May	29.55	36.48	32.5° C.
June	29.56	38.23	32.45° C.
July	28.90	37.10	31.9° C.
August	27.13	38.99	32.1° C.
September	27.19	35.36	33.0° C.
October	26.54	34.27	33.2° C.
November	28.36	30.65	33.4° C.
December	29.62	30.30	33.6° C.

The Iodine number was lowest in December, increasing slightly toward and including March; rising abruptly in April and continuing to rise up to and including June, then gradually declining toward October and dropping suddenly in November, followed by a slight drop in December.

The melting point followed, in general, the Iodine number reversedly. It was lowest in mid-summer when the Iodine number was highest, and it reached its maximum in December, when the Iodine number was lowest. The variations of the melting point, however, were not so abrupt as those of the Iodine number. A careful study of Table VIII suggests that, at times, the variations in the melting point may have been influenced strongly by the Reichert-Meissl number.

Experimental data produced in this country and abroad show unmistakably that the feed which the cows receive influences the per cent of olein in butter. Such feeds as cottonseed meal, bran, corn, overripe dry fodders, etc., when fed in excess, tend to decrease the per cent of olein, while linseed meal, gluten feeds, succulent pasture grasses, etc., are conducive of raising the per cent of olein.

The volatile fatty acids do not seem to be appreciably affected by the feed the cows receive. They are influenced, however, by the period of lactation as shown in Tables IX and X.[1]

TABLE IX.

Showing the Effect of the Period of Lactation on the Milk Fats.

TIME.	Reichert-Meissl Number.	Soluble Acids.	Insoluble Acids.
1st month	32.41	7.39	87.26
2d month	29.48	7.07	87.99
3d month	29.95	7.08	87.90
4th month	29.97	7.11	87.72
5th month	29.56	7.00	87.72
6th month	29.21	6.82	88.19
7th month	28.06	6.45	88.4
8th month	25.32	5.84	88.6
9th month	25.45	6.01	88.5
10th month	27.45	6.26	88.1

[1] Hunziker, Proceedings of the Indiana Academy of Science, 1908, page 144.

TABLE X.

Showing Effect of the Period of Lactation on the Milk Fats.

TIME.	Reichert-Meissl Number.	Soluble Acids. Per Cent.	Insoluble Acids. Per Cent.
1st month	36.68	8.20	86.76
2d month	35.75	8.09	86.74
3d month	33.19	7.59	86.99
4th month	33.80	7.56	86.95
5th month	33.63	7.47	87.10
6th month	33.57	7.55	86.94
7th month	32.72	7.49	86.99
8th month	31.63	7.25	87.41
9th month	31.98	7.10	87.50
10th month	32.03	7.12	87.46
11th month	26.64	6.50	88.20
12th month	30.48	8.86	87.69

The data in Tables IX and X represent results of experiments with three cows whose period of lactation commenced in October and November respectively. They were fed on a uniform ration throughout the entire period of lactation with the exception that in July (the 9th month after calving) they were turned out on pasture.

The above tables clearly show that the soluble fatty acids are highest immediately after parturition, or at the beginning of the period of lactation. Slight irregularities excepted, they decreased as the period of lactation advanced and were lowest toward the close of the period of lactation.

It so happens that in most localities the majority of the cows drop their calves in late spring, at a time when they also change from dry feed to succulent pasture. This explains why in early summer both the per cent of volatile fatty acids and the per cent of oleic acids increase and the melting point decreases.

RELATION OF COMPOSITION OF BUTTER FAT TO COMPOSITION OF BUTTER.

During late spring and early summer, at a time when, as shown above, the Reichert-Meissl number and the Iodine number are high and the melting point is low, the butter-maker experiences usually considerable difficulty in manufacturing butter with a reasonably low moisture content. This coincidence has suggested to the writers that there may be a more or less intimate relation betwen the melting point of the butter fats and their power to absorb water during the process of butter-making. A series of experiments was, therefore, conducted bearing on this point. The results are shown in Table XI.

TABLE XI.

Per Cent of Moisture Retained by Soft and Hard Fats Churned Separately.

	Per Cent Water.		Per Cent Increase of Soft Over Hard
	Soft Fats.	Hard Fats.	
March butter	43.84	24.76	77.02
May butter	50.62	24.78	104.28
Average	47.23	24.77	90.65

Table XI covers experiments in which soft and hard portions of butter fat (butter fat with a low and a high melting point) were separated from one another by fractional crystallization of the fats and by pressure. The soft and hard portions were churned separately under identical conditions, adding the same amount of water to each churning and churning at the same temperature.

Twelve separate churnings were made each, the March butter and the May butter. In the March butter the per cent increase of the moisture of the soft fats over that of the hard fats was 77.02. In the May butter the per cent increase of the moisture of the soft fats over that of the hard fats was 104.28. These figures unmistakably show that the soft fats are capable of taking up a great deal more moisture than the hard fats. They, therefore, can leave little doubt that the material increase in the moisture content of butter made in early summer is due to the increase in the soft fats it contains.

The moisture-retaining property of the fats is largely dependent on their melting point. The lower the melting point, the greater is their power to mix with and retain water. Since the glycerides of the oleic and soluble fatty acids have a low melting point, it is reasonable that any increase in the per cent of these glycerides tends to increase the water-retaining properties of the butter.

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On a New Complex Copper Cyanogen Compound.

By A. R. Middleton.

(Preliminary Note.)

When a cold concentrated solution of KCN is added to a cold concentrated solution of cupric chloride or sulphate, but not nitrate, greenish brown cupric cyanide is precipitated; the precipitate dissolves on further addition of KCN with formation of a claret red to violet red compound, much resembling potassium permanganate solution. Further addition of KCN destroys the color, with precipitation of white cuprous cyanide (presumably), which then dissolves in excess of KCN. First addition of concentrated cupric salt solution, or the solid salt, to concentrated KCN solution produces a brilliant violet color, instantly destroyed by further addition and quickly disappearing on standing. Further additions of copper salt give the red compound, provided the solution is kept nearly at 0°; otherwise cyanogen is evolved and the red compound is not formed. If the solutions are too concentrated or too dilute, the red compound is not formed. Solutions about one-half saturated appear to give the compound most readily and in largest amount.

Search through the available literature has revealed no reference to such a compound. It is quite unstable, decomposing to a brown solution on standing in a warm room over night; is instantly decomposed by strong and weak acids and bases and by pyridine; soluble in alcohol, but insoluble in chloroform, ether, benzene, toluene and carbon tetrachloride. Attempts to crystallize out the compound are in progress, and at the time of writing appear promising. The method pursued is as follows: Solid $CuCl_2.2H_2O$ was added in small amounts to KCN solution about one-half saturated, with constant shaking in ice water. After the red color reached a maximum, the solution was filtered, three volumes of 95% alcohol added and placed in the icebox in an exhausted desiccator. After 24 hours white opalescent scales separated, which, after washing with alcohol and ether and drying, present a metallic appearance somewhat resembling tinfoil. These contain copper and may be cuprous cyanide. The solution retained its red color unchanged and it is hoped that the compound can be crystallized out in form suitable for analysis.

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Determination of Endothermic Gases by Combustion.

By A. R. Middleton.

Endothermic gases such as ethylene and acetylene, even when mixed with sufficient air to form an explosive mixture, may be accurately and safely determined by combustion in a gas pipette provided the following conditions are observed: (1) Presence of a considerable excess of oxygen; (2) admixture with an exothermic gas; (3) slow admission of the combustible gases to the combustion pipette; (4) application of heat from below on the entering combustible gases; (5) reduced pressure. These conditions are secured by using a Winkler-Dennis gas combustion pipette, the platinum spiral being placed as near the juncture of the capillary with the pipette as possible without endangering the glass; mixing the endothermic gases with one to two volumes of pure hydrogen; and slowly leading this mixture into oxygen instead of the reverse as is usually done in combustion of the methane and hydrogen of illuminating gas.

The combustion is carried out as follows: The hydrogen used as a diluent is generated in a Hempel hydrogen pipette from zinc free from carbon; the requisite quantity is drawn into a burette, measured and transferred to a mercury pipette; a measured volume of acetylene or ethylene is then driven over into the hydrogen and the gases thus mixed drawn back into the burette. About 10 cc. more than the theoretical amount of oxygen required for the combustion is measured and transferred to the combustion pipette. The burette containing the mixed combustible gases over mercury is connected with the pipette and the level bulb of the latter so placed that the oxygen in the pipette is under a reduced pressure of one or two centimeters of mercury. The current is then turned on and the resistance so adjusted that the spiral is maintained at a bright red heat. The pinch-cock on the rubber connection of the burette with the capillary arm of the pipette is opened; the expansion of the oxygen by the heat from the spiral approximately balances the reduced pressure and little or no gas enters the pipette on opening the pinch-cock. The screw pinch-cock on the connecting tube of the burette and its leveling tube is then slightly opened and so adjusted that the flow of gas into the pipette is about 2 cc. per minute. After proper adjustment is effected the apparatus requires no further attention until the combustion is ended.

If the inflow of combustible gases much exceeds the rate prescribed, a series of small explosions is likely to occur at the juncture of the capillary side-arm with the pipette, traces of carbon deposition are evident and the results are slightly low.

Some analyses of acetylene and explosive mixtures of acetylene with air are appended:

Exp. No.	C_2H_2, cc.	H_2, cc.	O_2, cc.	Res. cc.	After KOH cc.	CO_2, cc.	O_2 Consumed, cc.	C_2H_2, %.
1	20.0	30.0	80.0	55.0	15.0	40.0	65.0	100.0
2	10.0	30.0	54.2	34.3	34.2	20.1	40.0	100.5
3	2.0	50.0	52.6	26.8	22.8	4.0	29.8	100.0
4	30.0	30.0	100.0	70.3	10.8	59.5	89.2	99.2
5	30.0	15.0	100.0	77.8	18.4	59.4	81.6	99.0
6	15.0	30.0	69.0	46.6	16.6	30.0	52.4	100.0
7	15.0	30.0	70.0	47.4	17.4	30.0	52.6	100.0
8	10.2	25.0	50.9	33.4	13.0	20.4	37.9	100.0

Explosive mixtures of air and acetylene:

1	15.0	30.0	50.6	35.2	13.4	21.8	....	72.6
2	15.3	30.0	51.0	34.6	12.5	22.1	....	72.2

Absorption by fuming sulphuric acid gave 72.0% and 72.3%.

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Methods in Solid Analytics.

By Arthur S. Hathaway.

Define the "vector" $[h, k, m]$ as the carrier of the point $(x, y, z) = P$, to the point $(x + h, y + k, z + m) = Q$, and show that the distance and direction cosines of the displacement PQ are given by functions of the vector called its *tensor* and *unit*, $T[h, k, m] = \sqrt{(h^2 + k^2 + m^2)} = n$, $U[h, k, m] = [h/n, k/n, m/n]$.

Interpret the sum $[h, k, m] + [h', k', m'] = [h + h', k + k', m + m']$ as a resultant displacement, $PQ + QR = PR$, and the product $n[h, k, m] = [nh, nk, nm]$, as a repetition of the displacement.

Define the linear functions of $q = [x, y, z]$ as the "scalars" or "vectors" whose values or components are linear homogeneous functions of the components of q, such as $ax + by + cz$, etc. Hence, for a linear function Fq, $F(q + r) = Fq + Fr$, $nFq = F(nq)$.

Hence, for a bi-linear function Fqr, $F(aq + a'q', br + b'r') = abFqr + ab'Fqr' + a'bFq'r + a'b'Fq'r'$.

A special scalar and vector bilinear function of $q = [x, y, z]$, $q' = [x', y', z']$ are defined.

$$Sqq' = xx' + yy' + zz' = Sq'q.$$

$$Vqq' = [yz' - zy', zx' - xz', xy' - yx'] = -Vq'q.$$

If θ be the angle between the displacements q, q', these functions are interpreted as,

$Sqq' = Tq \,.\, Tq' \,.\, \cos\theta$. $TVqq' = Tq \,.\, Tq' \,.\, \sin\theta$; and Vqq' is a displacement perpendicular to both q and q', in the same sense as the axis OZ is perpendicular to OX and OY, *i. e.*, on one side or the other of the plane XOY.

The use of this material is illustrated in the following examples:

$A = (2, 3, -1)$, $B = (3, 5, 1)$, $C = (8, 5, 2)$, $D = (5, 7, 11)$.

1. Find the lengths and direction cosines of AB, AC, AD.

   Ans. $TAB = 3$, $UAB = [\frac{1}{3}, \frac{2}{3}, \frac{2}{3}]$, etc.

2. Find $\cos BAC$. Ans. $SUABUAC = \frac{16}{21}$.

3. Find area of ABC and volume of $ABCD$.

   Ans. $\frac{1}{2}\, TVABAC = \frac{1}{2}\, 185$, $\frac{1}{6}\, SADVABAC = -13$.

4. Find the cosine of the diedral angle $C - AB - D$.

   Ans. $SUVABACUVABAD = \frac{-1}{37\sqrt{10}}$

5. Find the sine of the angle between AD and the plane ABC.

   Ans. $SUADUVABAC = -\frac{6}{\sqrt{185}}$.

6. Find the projection of AB on CD and the distance between them.

$$\text{Ans.}\quad SABUCD = \frac{19}{\sqrt{94}},\ SADUVABCD = \frac{78}{\sqrt{485}}.$$

7. Find the equation of the line AB.

$$\text{Ans.}\quad AP = tAB, \text{ or } \frac{x-2}{1} = \frac{y-3}{2} = \frac{z+1}{2}\ (= t).$$

8. Find the equation of the plane ABC.

$$\text{Ans.}\quad SAPVABAC = 2x + 9y - 10z - 41 = 0.$$

(a) The distance from this plane to (x', y', z') is $SAP'UVABAC$, or

$$\frac{(2x' + 9y' - 10z' - 41)}{\sqrt{185}}.$$

9. The vector whose tensor and components are the moments of AB about C and about axes through C parallel to OX, OY, OZ, is $VCAAB = [2, 9, -10]$.

10. The work done by CD in making the displacement AB is $SABCD = 19$.

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Motion of N Bodies.

By Arthur S. Hathaway.

The relative motion of n bodies, in any order of space, and subject to any law of mutual action, is given by

$$(1)\quad \ddot{\phi} = \phi\pi$$

where φ is a matrix which transforms n determining points of a reference space of order $n - 1$ into the positions of the n bodies, and π is a self-conjugate matrix, depending solely upon the ratios of the mutual reactions to the corresponding mutual distances.

The matrix ϕ is of order $n - 1$, if the motion of the bodies is within the reference space, and ϕ', the conjugate of ϕ, annuls every direction of the reference space exterior to the space of the moving bodies. If the space which contains the moving bodies be greater than $n - 1$'st order the matrix ϕ must be of the same order, but must annul all directions outside of the reference space.

The reduced equations of motion are,

$$(2)\quad (\dot{\psi} + W)\,\psi^{-1}\,(\dot{\psi} - W) = 2\,(\ddot{\psi} - \psi\pi - \pi\psi),$$

$$(3)\quad \dot{W} = \pi\psi - \psi\pi,$$

where $\psi = \phi'\phi$, a function of the mutual distances, and W is a skew conjugate matrix, whose elements are to be found from the quadratic equations between them in (2), and thence substituted in the remaining equations of (2) and in (3), giving a certain number of reduced equations of second and third order.

Another equation which is linear in the elements of W enables us to find the reduced equations in third and fourth orders,

$$(4)\quad D_t\,(\ddot{\psi} - \psi\pi - \pi\psi) = \pi\dot{\psi} + \dot{\psi}\pi + W\pi - \pi W.$$

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Direct Reading Accelerometers.

By C. R. Moore.

Every person is more or less familiar with the subject of acceleration or deceleration—changes of velocity—whether or not the laws governing the same or the mathematical expressions therefor are understood. Such everyday occurrences as passengers swaying to and fro partially suspended from street car straps, the hurry up that accompanies one's movements as he tries to reach the car door just as the motorman stops the car, are examples which prove this. Changes in the rates of motion are essential to all forms of transportation, and the more rapidly a car or train can be brought up to speed (or stopped) the shorter will be the time required between two points when a given number of stops must be made. Railway trains, street and interurban cars are therefore started and stopped as quickly as is consistent with reasonable comfort, in response to the demand of the traveling public for fast time.

It is the purpose of this paper to discuss briefly the laws of motion, and to describe a new device for measuring the rate of change of velocity, showing results of tests recently conducted in the Electrical Laboratories at Purdue University.

The author realizes at the outset that the subject of acceleration measurement is an old one and is rather reluctant to lay claim before this body of scientists that what is offered herein is new. However as far as his knowledge goes this device has not been used previous to this time. The scheme is brought to your attention for whatever consideration it may merit.

Before discussing accelerometers in detail, a brief study of just what is meant by acceleration and deceleration may be of value.

In Fig. 1 curve "D" shows distances plotted against time, the distances being taken as ordinates and the time as the abscissæ. The car is to be thought of as moving from a certain point "O," distances "d" being measured from that point at the end of the any time "td." It will be noted that during the first few time units after the car starts the distance passed through each successive unit is greater than that passed through during the preceding unit of time, i. e. the rate of motion is increasing. At the

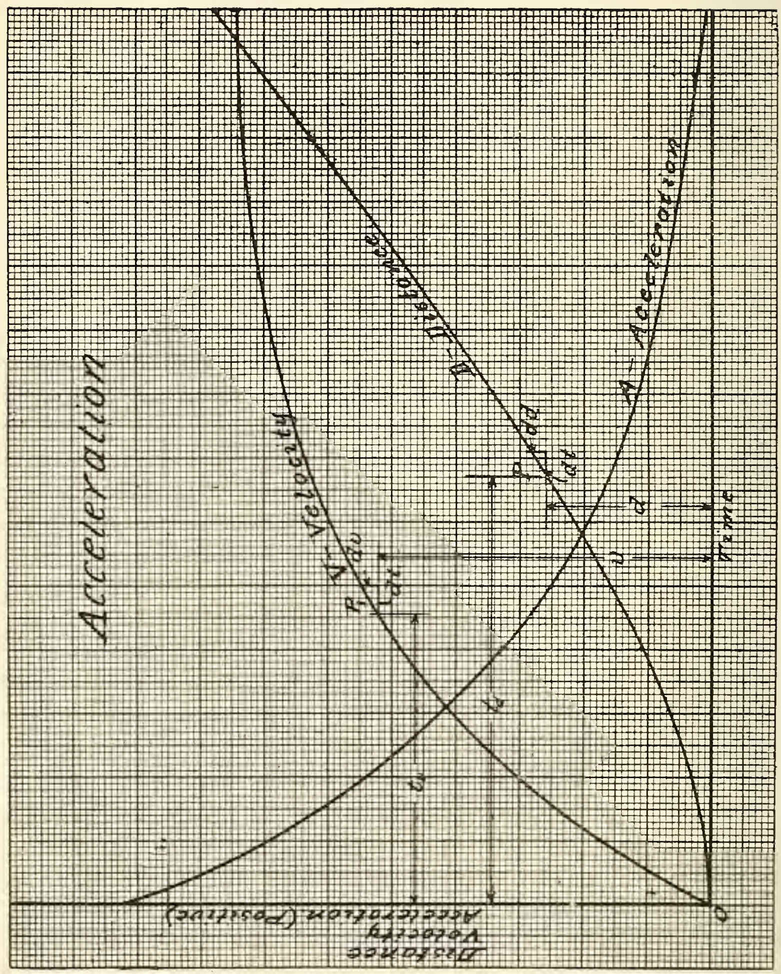
Acceleration
V - Velocity
D - Distance
A - Acceleration
Time
Distance
Velocity
Acceleration (Positive)
O

Fig. 1.

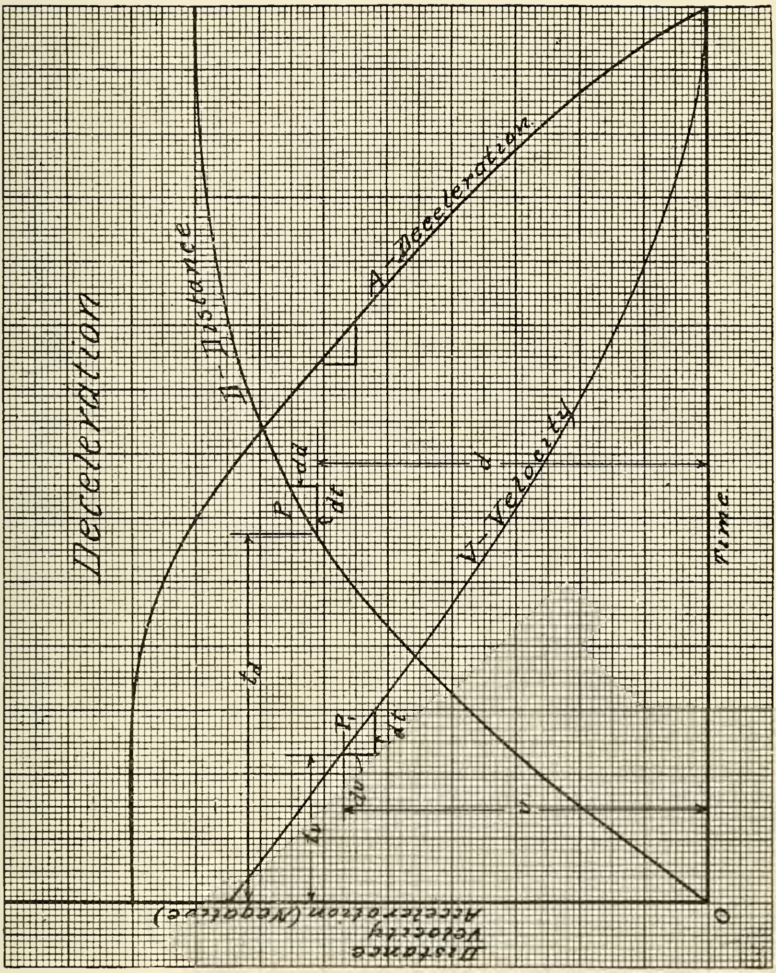
Deceleration
D-Distance
A-Deceleration
V-Velocity
time
Distance
Velocity
Acceleration (Negative)

Fig. 2.

end of a certain time, however, equal increments of time show equal increments of distance. The curve then becomes straight because the rate of motion has become constant.

Velocity or the average rate of motion is defined as the space passed over divided by the time required for passage. The average velocity through any point then may be found by dividing small increments of distance by the corresponding increments of time. By taking these increments sufficiently small we may make the average velocity approach the true instantaneous velocity through any given point, as closely as we please. At the limit or when the increments become zero these velocities are equal.

Near the point "P" on the distance curves shown in Figs. 1 and 2 are drawn small triangles having for their vertical components small distances "dd" and for their horizontal components the corresponding increments of time "dt." From the above definition the average velocity for the space passed over designated by the small triangle will be $v = \frac{dd}{dt}$.

By taking this triangle very small the average velocity may be made to very closely approximate the instantaneous velocity at the point "P." It is also to be noted that the ratio $\frac{dd}{dt}$ is the expression for the tangent of the angle included between the line "dt" and that portion of the curve which completes the triangle. Values proportional to "v" may therefore be found at any point on the distance curve by drawing a tangent line at that point and finding the tangent of the angle between this line and the horizontal. Plotting these values multiplied by a constant gives the velocity curves "V" (See Figs. 1 and 2). From this curve we are able to determine the velocity of the car at any time "t."

By scanning curve "V" we note that the velocities for different time values until that time is reached where the distance curve became a straight line. At this point the tangent values become constant and the velocity curve becomes horizontal.

Just as velocity may be determined by dividing space passed over by the time required, so may the acceleration be determined by dividing the velocity change by the time required to make the change. The statements relative to average and instantaneous velocity also hold for average and instantaneous values of acceleration. We may therefore write $a = \frac{dv}{dt}$

as the general expression for acceleration when derived from the velocity-time curve. As before, this expression denotes tangent values so that the acceleration curve may be obtained from the velocity curve in the same manner as the velocity curve was obtained from the distance curve. It is interesting to note that the acceleration curve reaches the X-axis at the same time the velocity curve becomes horizontal and at the same time the distance curve becomes straight. This is shown mathematically as follows:

$$v = \frac{dd}{dt} \qquad a = \frac{dv}{dt} = \frac{d^2d}{dt^2} = 0 \text{ for } v = \text{a constant.}$$

or the value of "v" can be variable only so long as the distance time curve is not straight, and unless "v" is a variable the second derivative of the distance cure will be zero.

Physicists learned early that weight could not be taken as a standard of force on account of the variation of gravity with location on the earth's surface. Knowing however that force was required to change the velocity of a body it developed that when the amount of substance—mass—in a given body was known $(m = \frac{w}{g})$ the force needed to give it a definite change in velocity in a given time was a definite function of these two quantities. The familiar expression for this is, Force = mass × acceleration.

The equation is valuable to scientists and engineers alike. Using unit mass and unit acceleration, the scientist finds thereby a unit force which is constant. (The equation of the pendulum gives him the acceleration due to gravity at any point so that mass may be easily determined.) Knowing the masses involved in a given car or machine, the engineer is able to predetermine the torque necessary at the motor shaft to bring the same up to speed in a given time. This information is valuable for purposes of design.

After the apparatus has been assembled it is sometimes necessary to determine their performance. The mass being known it remains to measure the acceleration to see if the motors meet the requirements.

This measurement of acceleration has been attempted in many ways. A few of the more important schemes will now be considered. Accelerometers employing a freely moving mass of some sort have been most used. Dr. Sheldon's device is of this type, using a suspended weight carrying a pointer at the bottom (fastened thereto by rods) which plays over a scale.

The mass being free to move is sensitive to changes of velocity and the scale may be calibrated to read acceleration directly. The calibration is fairly simple and the device is not difficult to construct.

Another device working on the same principle consists of a "U" tube partially filled with mercury so placed that its plane is parallel to the motion of the car. It is obvious that changes of velocity will cause the mercury to rise in one side of the tube and to fall in the other. The more quickly these changes occur the greater will be the difference between the heights of the mercury in the two portions of the tube. The tube may therefore be calibrated to read acceleration directly.

Again the accelerometer takes the form of a slightly inclined track upon which rolls a ball. This track is made to extend in both directions and has a short level portion at the middle. Changes of velocity cause the ball to move one way or the other along the track. This device is difficult to read and is not very accurate.

All of these accelerometers are confined to horizontal motions and if the track be other than level corrections must be made therefor. This involves a great deal of labor and expense so that while the devices are simple in themselves their use is complicated. It is next to impossible to make them self-recording.

Another apparatus for reading acceleration consists of two magnetically actuated markers so arranged that dots may be made by each of them on a sheet of paper moved at a uniform rate of motion. The magnet of one of these pointers has its circuit closed through battery at regular time intervals by a clock. The other pointer has its magnet operated on a circuit which is closed through battery a definite number of times per revolution of the car wheel. From the record made by these pointers the acceleration at any time may be determined. This apparatus also involves a great deal of labor and expense and is seldom used.

The accelerometer which is the subject of this paper depends for its operation entirely upon electrical phenomena and is independent of its own location, motion or position. It will therefore read acceleration vertically or at any angle as well as in the horizontal direction. No corrections are necessary and it may easily be made self-recording. It is not difficult to calibrate and is permanent.

The circuit as originally conceived is shown in Fig. 3 in which "B" is an electric condenser, "C" an ordinary high grade direct current volt-

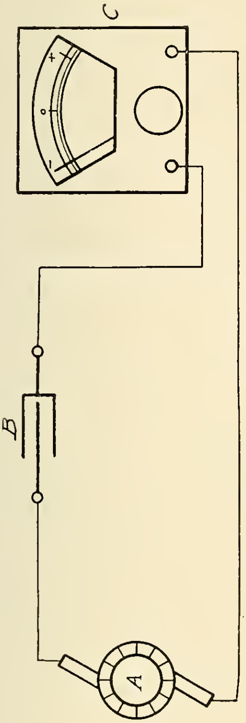

Fig 3.

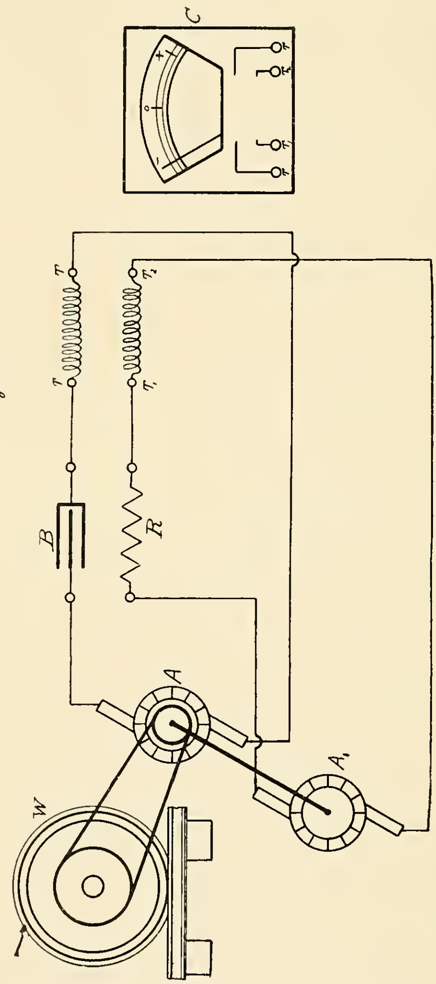

Fig. 4.

meter (with the extra resistance removed) and "A" is a direct current magneto generator having permanent magnet fields.

The equation of the condenser is Q=EC; where Q is the quantity of electricity in Coulombs (ampere seconds), E is the voltage impressed, and C is the capacity in farads of the condenser. Studying this equation we find that if E is increased uniformly the quantity of charge Q on the condenser plates will also increase uniformly. Since Q is increasing uniformly with respect to time, the inflow of current is at a constant rate. i. e., $i = \frac{dq}{dt}$. Likewise a constantly decreasing E will give a constant outflow of current. However, as soon as E reaches a fixed value all current flow in the circuit ceases since it is one property of the electric condenser to arrest the flow of direct current. (The terms "inflow" and "outflow" refer to those condenser plates that are directly connected to the instrument terminal. Of course as much current flows on to one set of plates as flows off of the other plates, the current in the line having a definite direction during an increase of voltage and the opposite direction during a decrease of voltage.) The magnitude of these currents are shown by the direct current instrument which consists merely of a coil swinging in a uniform magnetic field. So long then as the voltage is changing uniformly the instrument will read a constant value returning to zero only when E ceases changing. It follows that if E does not change uniformly the instrument will not read a constant value but that its indications will be proportional to the instantaneous rate of change of the voltage. The direct current magneto is so designed that its voltage is directly proportional to its speed, so that changes of voltage at its terminals can only occur as a result of changes in speed. Therefore the instrument reads the rate of change of speed, i. e. acceleration whether positive or negative.

In a preceding paragraph it was implied that an electric condenser allows no current to pass when the voltage E has reached a fixed value. This would be a fact if an ideal condenser could be made. but it is a well known fact that there is always some leakage even in the best condensers. This means that the dielectric has a definite value of resistance which varies with different conditions and substances, and according to Ohm's law the leakage current will be $I = \frac{E}{R}$. This state of affairs renders our ideal circuit incorrect for any speed above zero because the instrument gets a small current in a definite direction that is practically proportional to speed, and even if the voltages were constant—acceleration zero—the instrument could not return to its zero position.

The circuit must therefore be modified to compensate for this small leakage current, as is shown in Fig. 4. A second direct current magneto (or another commutator on the original machine) is arranged so that it can feed current through a high resistance to another coil on the moving element of the instrument. This second coil is wound over the first and works in the same magnetic field. The current is passed through it in such a direction that the torque produced thereby opposes the torque of the original coil. By adjusting the high resistance these torques may be made equal and the instrument will read zero for any constant value of voltage within reasonable limits. This allows the charging currents to actuate the instrument entirely independent of the leakage current and condensers of reasonable cost may be employed.

In Fig. 4 the second generator is shown at A_1, the high resistance at R, and the second coil on the moving element of the instrument has its terminals shown at T_1 and T_2. These terminals are also shown in the separate sketch of the instrument C. It will be noted that the pair of magnetos are shown belted to a car axle. When this is done changes in the rate of motion of the car will produce changes in the voltages of the magnetos so that the instrument may be calibrated to read accelerations in terms of feet per second per second, as well as in terms of revolutions per second per second.

Figures 5 to 11 show the results obtained recently from tests on this type of accelerometer. Three curves (Figs. 5, 6 and 7) show positive acceleration, and three (Figs. 8, 9 and 10) show negative acceleration.

The experimental apparatus with which these results were obtained was made up as follows: the direct current machine in the condenser curcuit was a separately excited generator of about 500 watts capacity having a normal speed of 1,800 R. P. M. The fields were excited from storage battery, about 140 milamperes being used. At 1,800 R. P. M. this excitation gave about 50 volts at the terminals. Since the field was constant and no appreciable current was taken from the armature the voltage remained directly proportional to the speed. The condensers had a combined capacity of about 65 micro-farads and were of the ordinary paper type. The instrument used was home made and very imperfect. Its moving element was very heavy, its frictional error large and the damping effect

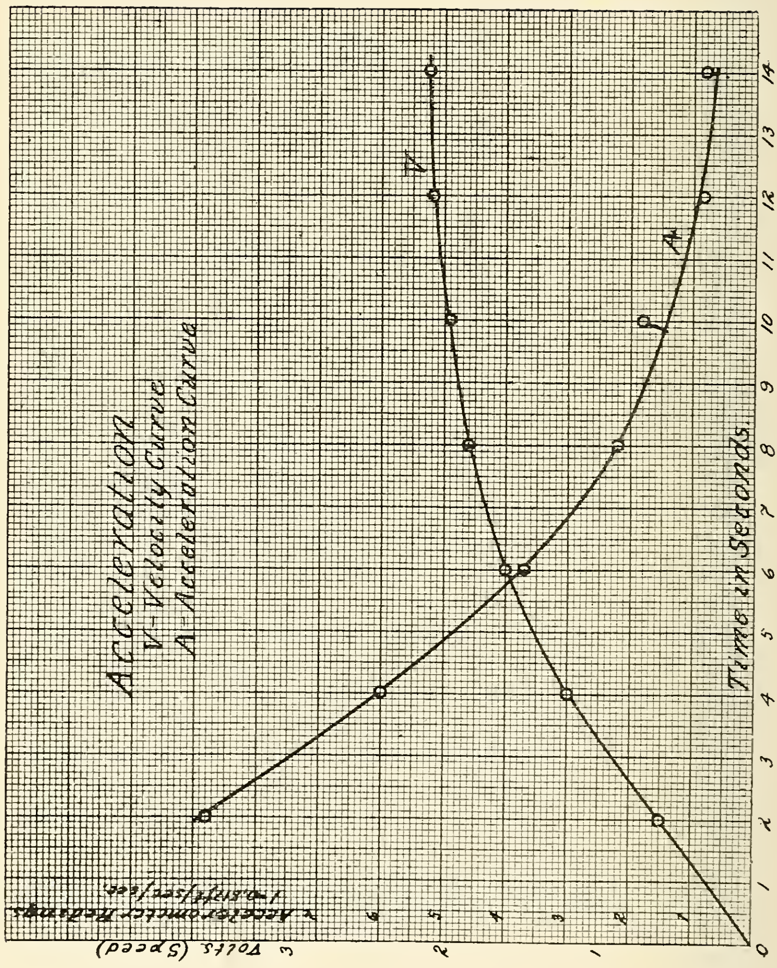
Acceleration
V-Velocity Curve
A-Acceleration Curve
Time in Seconds
Volts (Speed)
Accelerometer Readings
V
A

Fig. 5.

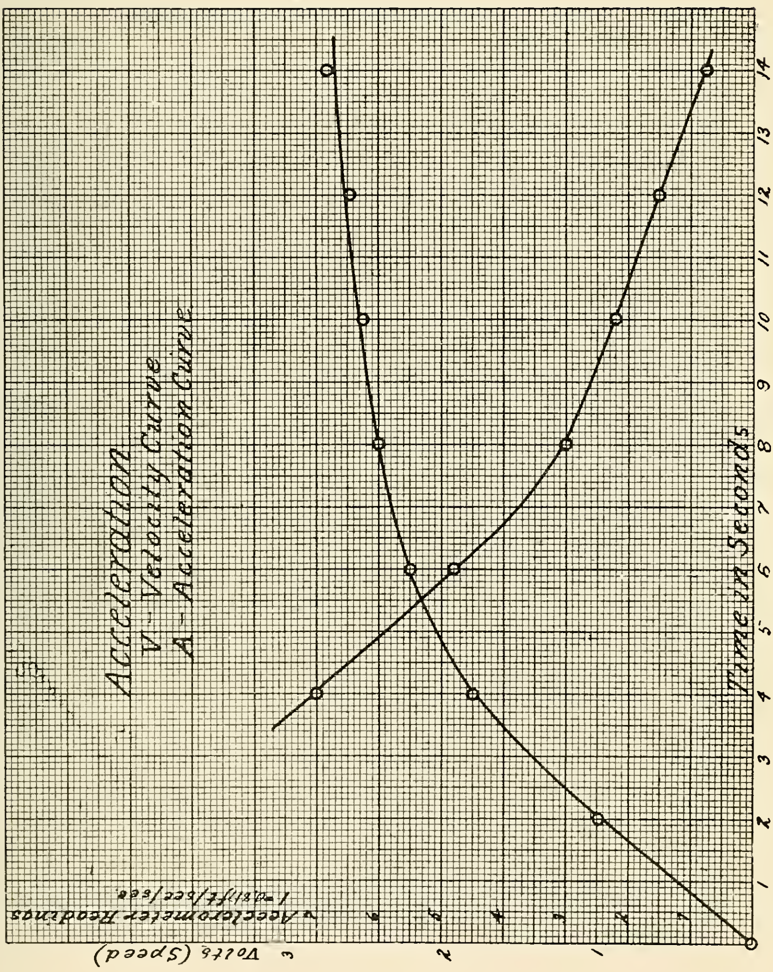
Acceleration
V - Velocity Curve
A - Acceleration Curve
Time in Seconds
Volts (Speed)
Accelerometer Readings

Fig. 6.

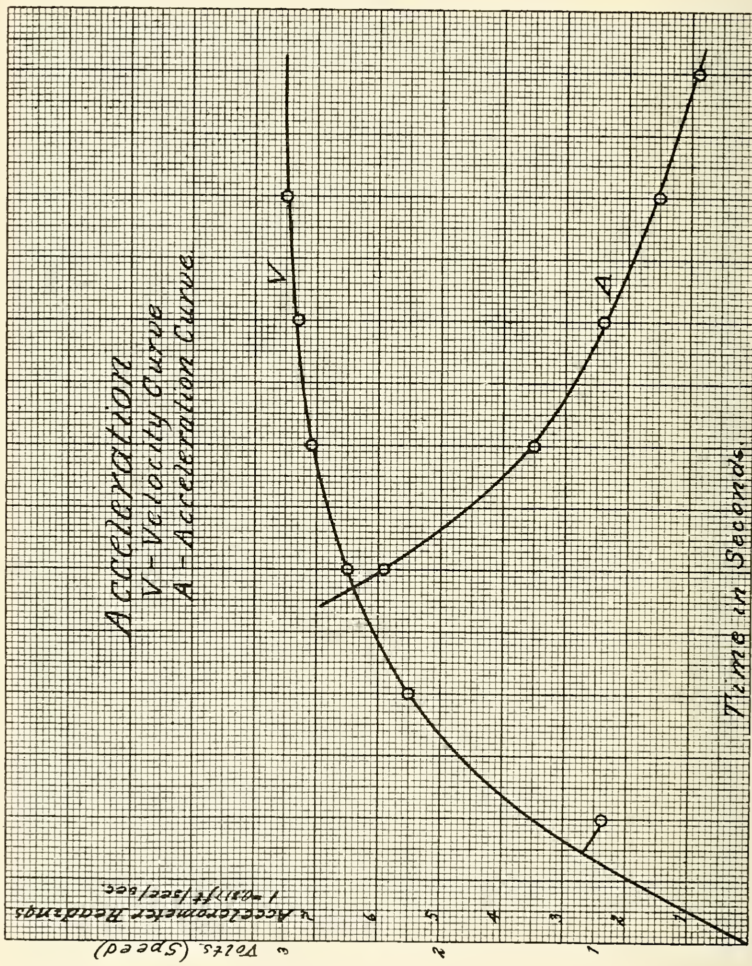
Acceleration
V - Velocity Curve
A - Acceleration Curve
V
A
Time in Seconds
Volts (Speed)
Accelerometer Readings

Fig. 7.

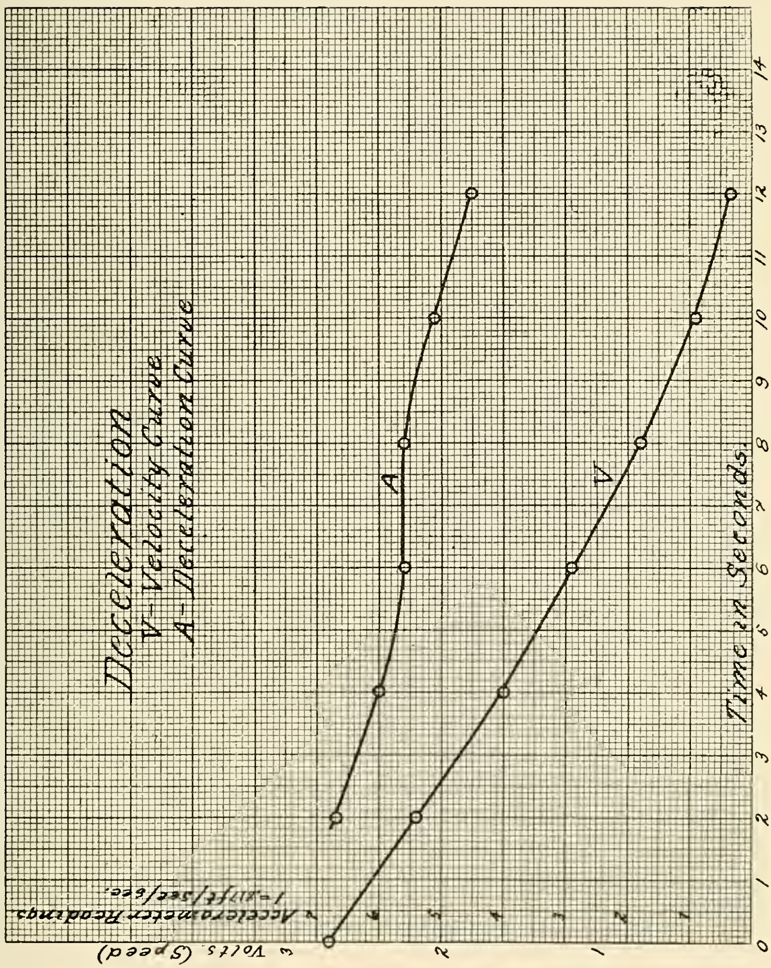
Deceleration
V-Velocity Curve
A-Deceleration Curve
A
V
Time in Seconds
Volts (Speed)
Accelerometer Readings

Fig. 8.

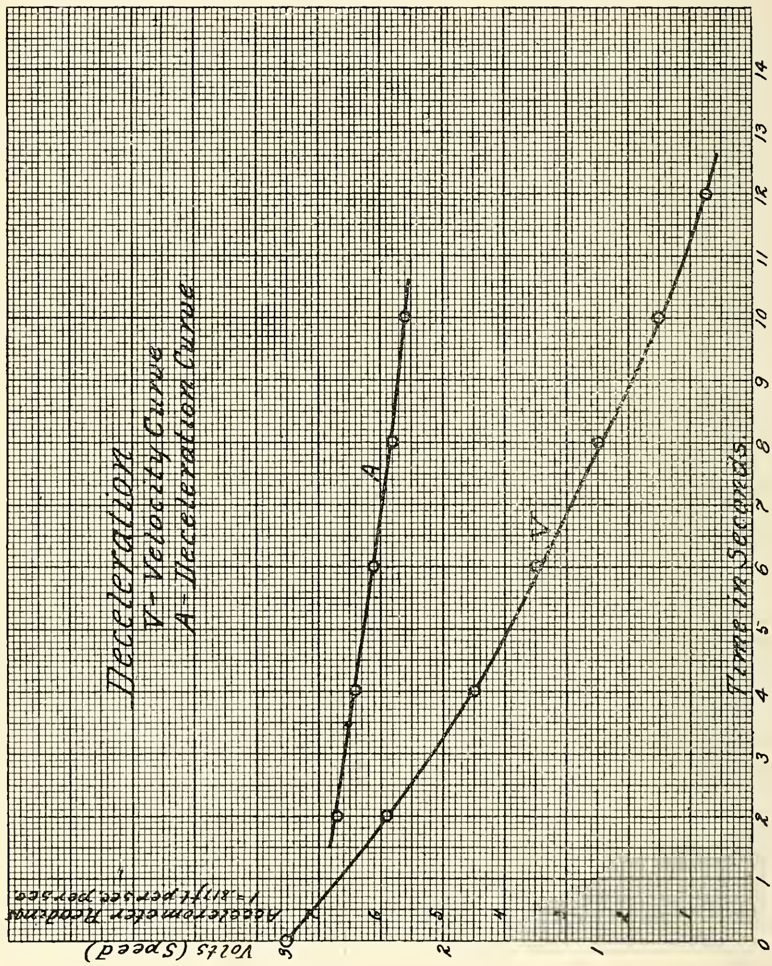
Deceleration
V- Velocity Curve
A- Deceleration Curve
A
V
Time in Seconds
Volts (Speed)
Accelerometer Readings

Fig. 9.

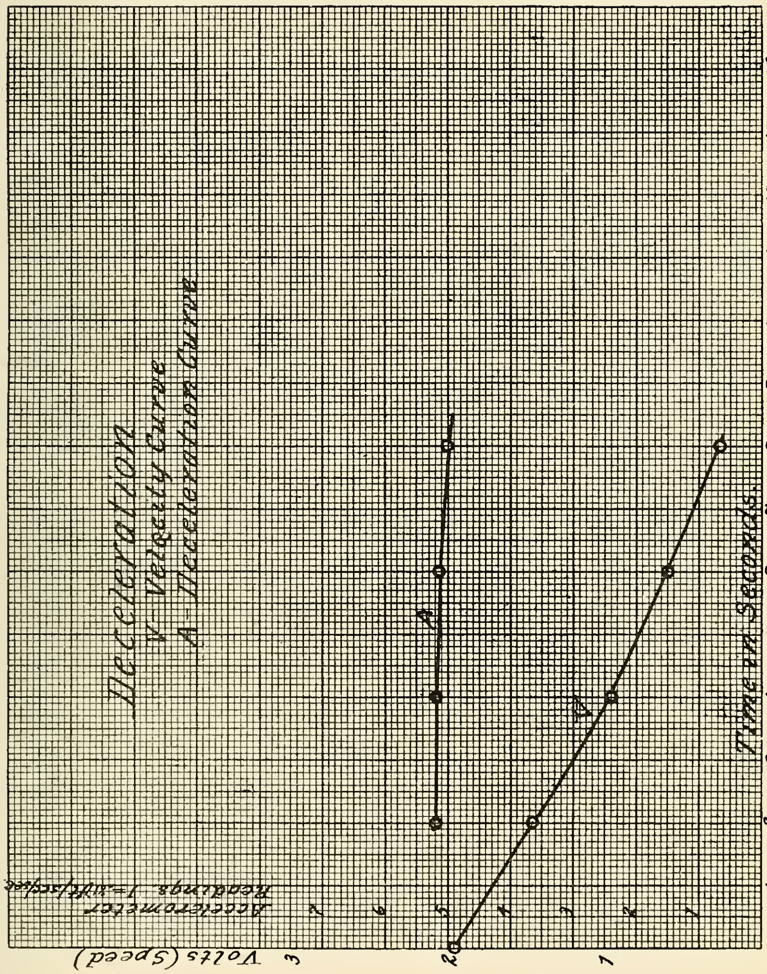
Deceleration
V-Velocity Curve
A-Deceleration Curve
Time in Seconds
Volts (Speed)
Accelerometer
Readings

Fig. 10.

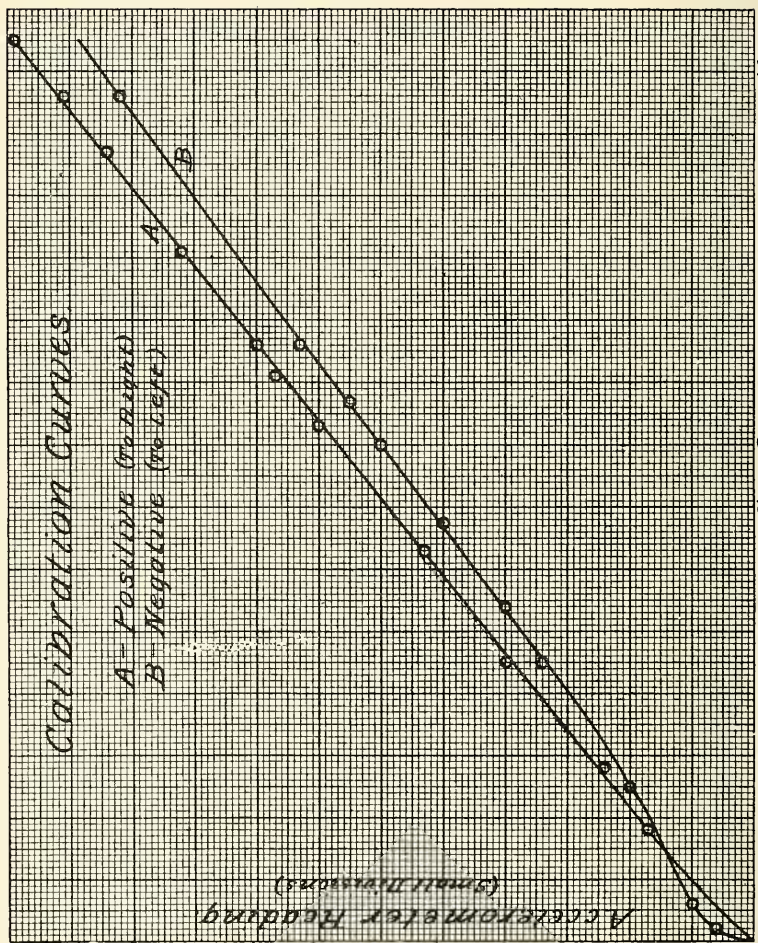
Calibration Curves
A- Positive
B- Negative
A
B
Accelerometer Reading
(Small Divisions)

Fig. 11.

poor. Its calibration curves are shown in Fig. 11. These imperfections account for the variation in its calibration constant as will be stated later. The resistance circuit contained a three-volt, 1,800 R. P. M. magneto (permanent fields) directly connected to the motor shaft, as was the generator in the condenser circuit. The resistance employed was of the ordinary box type.

Acceleration was obtained by impressing suddenly a fixed voltage on the driving motor and reading values of speed and the accelerometer every two seconds. Deceleration was obtained by opening the motor switch and reading speed and the accelerometer every two seconds. The speed readings were secured by attaching a voltmeter to the three-volt magneto. Some of the readings thus taken are shown in Figs. 5 to 10 which are self-explanatory.

Scanning these curves brings out their similarity to the mathematical curves on Figs. 1 and 2.

Calibration is effected by drawing tangents at various points on the speed time curve and dividing the accelerometer reading at this point by the value of the tangent of the angle between this line and the horizontal. This quotient should be constant. Now by noting actual voltage and the corresponding speed the number of volts per revolution may be obtained. Our tangent value indicates volts change in a given time "t" which may now be reduced to revolutions change in the same time. If the generators be belted to a car axle the wheels of which have a known diameter this revolution change may be reduced to the corresponding change of linear velocity in the given time "t."

For the tests herein described, however, the instrument scale was arbitrarily drawn and, with the particular circuit set up, each small division corresponds to an acceleration of 0.33 revolutions per second per second. If it had been used on an interurban car having 24" wheels its scale would indicate 0.817 feet per second per second per small division. This value could be reduced to a workable figure by using a larger condenser, a higher voltage and a more sensitive voltmeter.

These calibration values varied from 15 to 25 revolutions per second per second per small scale division on account of imperfections in the instruments and the small readings made necessary by having insufficient capacity.

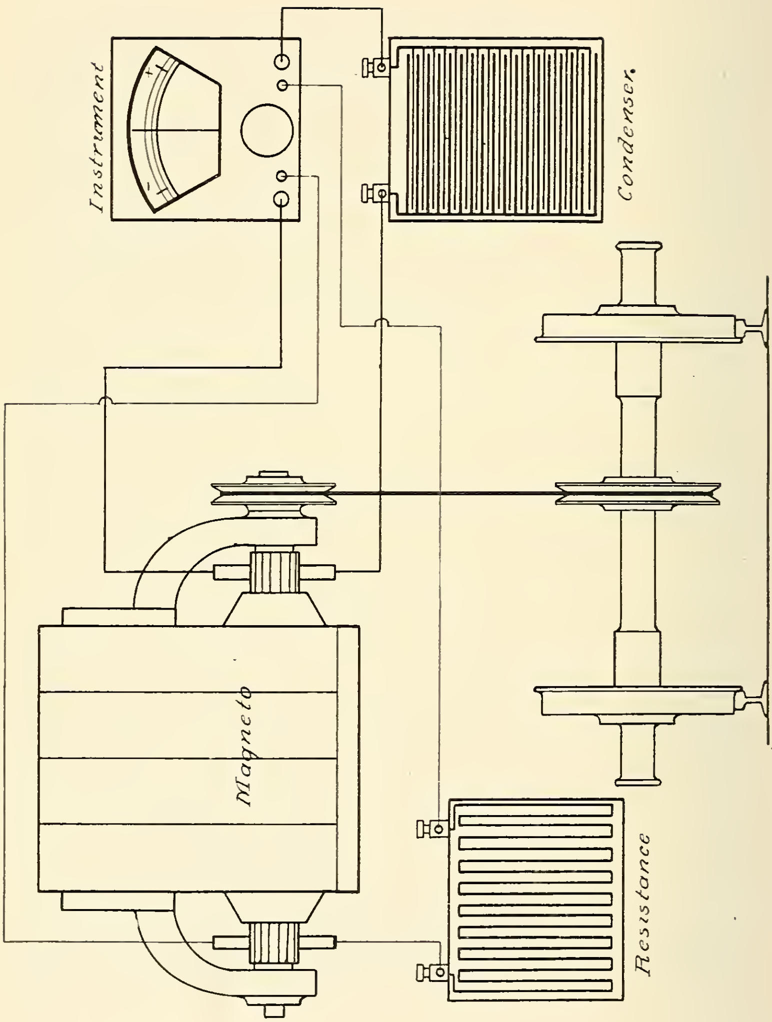

Fig. 12.

Almost any condenser when suddenly discharged if allowed to stand a few minutes will experience a rise in potential at its terminals. This rise is due to what is known as the residual charge. This phenomenon is explained as follows: When a condenser is charged its dielectric is strained and being non-homogeneous the strains are unequal. (By strain is meant the actual compression of the plates.) When discharged these strains are relieved but they do not decrease at the same rate, so that some parts of the dielectric become strained in the opposite sense and balance those parts which are slower in acting. The condenseer is then apparently discharged, but after standing a while these strains tend to diminish and usually there is a resultant strain set up. This resultant strain is due to the fact that while the forces were originally balanced at the end of the first discharge, yet the distances are unequal and in nonhomogeneous materials stress is seldom proportional to strain.

The condenser may now be discharged again and after a time may show still another rise of potential. In the apparatus herein described this effect is entirely negligible, for the reason that the condenser is never charged or discharged suddenly, some few seconds being required to complete the action.

In all condensers there is also some absorption, but with good condensers used at the voltages proposed for this apparatus this effect is also quite negligible, and we may with certainty say that for a given voltage change at any part of the potential range equal quantities of electricity pass through the instrument.

With an instrument giving a uniform scale therefore we have an apparatus which will show equal increments of readings for equal rates of change of velocity, i. e. a direct reading accelerometer.

Fig. 12 shows the apparatus as assembled for use in railway work. The double commutator magneto is here shown belted directly to the car axle. It is obvious that the readings of the instrument are unaffected by grades or side tiltings of the car.

The apparatus may be made self-recording by employing a recording instrument instead of an indicating one, as shown in Fig. 13. These recorders may be obtained in the market and are very sensitive and reliable. The record is made by placing a pen on the end of the voltmeter pointer, the whole being pulled down upon a sheet of paper moving at a uniform rate of motion by means of a small magnet whose circuit is closed through battery by a clock. The record is thus made automatically and needs no correction.

The accelerometer may be made self-contained and is easily transferred from one car to another.

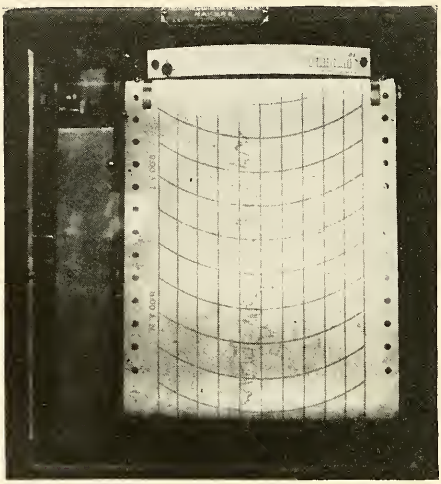

Fig. 13.

Before closing, the author wishes to express his appreciation of the efforts of Messrs. F. C. Weaver, G. T. Shoemaker and E. E. Thomas, members of the present Senior Electrical Class at Purdue University, whose kindly assistance made this paper possible.

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Some Notes on the Strength of Concrete Building Blocks.

By H. H. Scofield.

The concrete building block industry is rapidly assuming an important position and is now established on a firm basis among the other industries supplying building materials.

Reinforced concrete is now very largely used and seems to be the best form of concrete used for floors, beams and columns, but the concrete block seems to be the form of concrete most adaptable for use in the walls of residences and other buildings.

The industry has grown so rapidly in the past few years that standard specifications for their manufacture and use have been adopted by the National Association of Cement Users and by many cities of the United States. The need for proper specifications was brought about mainly on account of the large number of inferior blocks placed on the market by irresponsible manufacturers. The causes for this are various, such as: a desire for higher profits brought about by using inferior ingredients; ignorance as to the best methods of using the materials at hand, careless workmanship and improper treatment as to storage, etc.

The specification for crushing strength as called for by most specifications is so high that it can be filled only by the best methods and the best material, and although it is many times more than a block will ever be called upon to stand in actual use in a wall, yet it insures a block which is strong, dense and thereby water-proof, with clean-cut, smooth edges, and a block which will endure for ages.

The following are some items which enter into the making of good concrete blocks:

In the selection of a cement, a maker has two alternatives. He must either use a first-class, standard brand of known excellence, or he may use the competitive brands on the market, thereby getting lower prices. In the latter case, he should have each shipment sampled and tested by a reliable testing laboratory.

An unsound cement may not show up till the block is in the wall or for years after, but it is practically inevitable that the block will finally crack and disintegrate. Some cement companies take proper precautions in the treatment of raw materials and storage of the finished product. such that very rarely does an unsound cement leave their mill. Other companies, in the rush of business, do not properly mix and grind their raw material and finished cement, and do not store the cement long enough for the hydration of the free lime present. These are conditions that tend to place more or less unsound cement on the market. The future of the concrete block manifestly depends to a great extent upon the use of a sound cement.

For maximum strength in concrete, the cement must be finely ground, but fine grinding is expensive and consequently this part of the manufacture is often slighted. The cement should also be slow setting, as a cement that reaches its initial set in two or three hours will be stronger at the end of seven days or a month than a quicker setting cement that reaches its initial set in forty or fifty minutes.

The cement to be used in concrete blocks should in all cases pass the specifications of the American Society for Testing Materials.

Too frequently the reason for poor concrete is ascribed to poor cement. and no thought is given the other materials entering in, namely; sand. gravel or broken stone. The selection and proportioning of the aggregate for the best concrete is very important in the building block industry. It is well known that the proportions of cement, sand and stone which will give the densest mixture of concrete will also give the highest strength. It is also recognized that a rich, dense mixture of concrete is the most nearly waterproof that concrete alone can be made. So that for a strong, water-proof block, it is important that the cement and aggregate be properly proportioned. This may be done by actual trial mixtures to determine the densest concrete. An aggregate containing coarse stones and sand has greater density than sand alone and consequently is better for use in concrete blocks.

According to Wm. B. Fuller, an eminent authority on concrete, the most nearly perfect gradation of sizes of particles in an aggregate may best be known by the process of mechanical analysis and subsequent reproportioning. In case the business warrants it, samples of the gravel should be submitted to a reliable testing laboratory for mechanical analysis to determine the proper proportions, A dirty gravel or one that contains impurities should be washed. This will not only improve the strength of the concrete, but will make a more uniform and desirable color for the finished block.

It is now agreed that cement hardens by a process of crystallization of the active elements. Water must be present for the crystallizing to go on. Therefore it is necessary that the proper amount of water be used in mixing the concrete. This, by some authorities, is from 8 to 18 per cent. Also it is necessary that after moulding, the block must not be allowed to dry out, as no subsequent addition of water will give perfect crystallization. Some makers cure their blocks in a steam bath, thereby insuring constant moisture. The economical value of steaming concrete blocks is a subject for experiment as yet. Most specifications limit the time after making at which blocks may be used in the wall, so that the increased speed of hardening by the steam process is not so important.

The specification for crushing strength of concrete blocks, in most cases, is 1,000 pounds per square inch of gross area, no allowance being made for the hollow spaces. The block must reach this strength in 28 or 30 days after making.

The city of Indianapolis has recently adopted specifications for concrete building blocks, and the results of the first series of tests for the block makers of that city by the Laboratory for Testing Materials of Purdue University, indicate a chance for improvement.

Of 75 tests of blocks, supposed to have been made under the specifications, only 28 per cent. passed the specification for crushing strength, and the average age of these was 41 days instead of 30. Similar results have no doubt been found in all cities which have adopted a building block ordinance. However, under the influence of these somewhat rigorous specifications, it is to be expected that the quality of the product on the market will greatly improve. This in itself will strengthen the industry for those makers who are content to manufacture good blocks at a reasonable profit.

Purdue University,
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Polarization of Cadmium Cells.

By R. R. Ramsey.

While working on another problem (Phys. Rev. Vol. 16, p. 105) it was noted that the E. M. F. of a cadmium cell was greatly decreased and at times apparently reversed after a considerable quantity of electricity had passed through it.

To investigate the cause of this phenomenon the experiments described below were undertaken. Work of a similar nature has been carried out by F. E. Smith (Phil. Trans. Roy. Soc. Lon., Series A, Vol. 207, p. 393); by S. J. Barnett (Phys. Rev. Vol. 18, p. 104, 1904), and by P. I. Wold (Phys. Rev., Vol. 27, p. 132, 1909). However, in their experiments the time of polarization was comparatively small, the attention of the investigators being directed to the initial polarization or to the rate of recovery. In my work I have attempted to find the cause of this polarization.

Cells were constructed of the H type and according to the accepted formula for cadmium cells. The chemicals used were C. P. chemicals of commerce. With ordinary care a cell could be obtained whose E. M. F. did not differ more than .001 volt from the standard value. Measurements of E. M. F. were made by means of a potentiometer. At times where rapid measurements were desirable and great accuracy was not necessary a voltmeter was used, the readings being corrected for the internal resistance of the cell. Current was measured with a milliameter and time was measured with a watch. At first it was thought that the polarization was a surface effect, that a relation existed between the area of the surface of the electrode and the quantity of electricity required to polarize a cell to some standard amount. Cells were made with electrodes of various diameters. The current was noted at stated intervals, so that the total quantity could be caluclated. This was found to differ in different cells, but it appeared to depend more upon the past history of the cell than upon the electrode surface exposed.

It was found that after a cell has been polarized once and has regained its normal E. M. F. again it required less quantity of electricity to polarize it than it did during the first run. A cell with three legs was made. Two of the legs were filled with mercury and the third was filled with cadmium amalgam. Connection was made to the amalgam terminal and to one of the mercury terminals and current passed until the cells were polarized. Measurements were made by means of the potentiometer, and it was found that the E. M. F. between the unpolarized mercury terminal and the cadmium terminal was normal, while the polarized mercury terminal gave a very small value, showing the polarization to be at the mercury terminal. Measurements were made between polarized cells and unpolarized cells by connecting the two cells together by means of a siphon filled with cadmium sulphate solution. In every case it was found that the polarized mercury terminal gave low values, while the polarized cadmium terminal gave normal values when connected to unpolarized mercury terminals, never deviating more than could be explained by concentration and temperature effects.

A cell (5) was short circuited for some days and part of the mercury was removed with a pipette, washed and filtered through a pinhole and made the mercury terminal of a new cell (6) from which the mercurous sulphate was omitted. The E. M. F. was measured from time to time and the recovery noted. The following table gives the results.

	E. M. F.	
	(5)	(6)
March 9, 5:15 p. m.	0.1308	0.1290
March 10, 9:00 a. m.	.1320	.1307
March 10, 3:45 p. m.	.1363	.1310
March 12, 9:20 a. m.	.1488	.1339
March 13, 10:15 a. m.	.1675	.1322
March 14	1.0222	.1317
March 15	1.0242	.1335
May 14	1.0146	.0691
June 8	1.0177	.0533
August 26	1.0189	.0637
September 24	1.0150	.0462

The above table shows that cell (5), which contained mercurous sulphate, recovered its E. M. F. in a few days, while (6) remained polarized for six months. The results show the E. M. F. in March to be greater than the later values. This may be due to the cadmium sulphate solution not being concentrated in the early observations or to some constant error of the potentiometer. The table shows that the polarization is due to something in the mercury which can not be washed or filtered out. But is removed by mercurous sulphate. The mercury from cell (6) was taken out and placed in a tube and sparked by a large electric machine. Cadmium lines were very distinct in the spectrum. Thus it would seem that polarization is caused by cadmium being deposited in the mercury and that the recovery is due to the removal of the cadmium by the mercurous sulphate.

Indiana University,
Bloomington, Ind.

An Investigation of a Point Discharge in a Magnetic Field.

By Oscar William Silvey.

Since the announcement of the magnetic deflection of the electric arc and of the path of the particles of a vacuum tube discharge, there has been some investigation of the electric discharge in a magnetic field at atmospheric pressure.

Among the first of these investigations was that of Precht,[1] who found that when a spark passed transverse to the lines of force in a magnetic field, between a point anode and a blunt cathode, there was a deviation of the path of the spark, especially from the middle portion of the spark gap to the cathode, the spark increased in brightness, and there was a decrease in the fall of potential between the electrodes. Also, if the electrodes were separated farther until a brush discharge existed between them, the stream showed a deflection, the potential between the points decreased, and the brush often changed into a spark discharge, when the electro-magnets producing the field were excited. In case of the glow discharge, where there existed a small brush at the anode and a bright spot on the cathode, with the intervening space dark, the spot moved up or down according to the electrodynamic laws, when the field was magnetized first in one direction and then in the other.

In case a point cathode was used with a blunt anode, the spark was deflected and the potential raised, when the magnet was excited the spark discharge being often changed to a brush.

[2]H. E. Schaeffer has recently studied the effect of the magnetic field on the spark discharge of an induction coil in each of the following types of spark:

"1. The spark obtained when neither capacity nor self-induction had been introduced into the secondary circuit of the induction coil.

"2. The spark obtained when a capacity of 0.005 to 0.012 microfarads had been introduced into the secondary circuit.

[1] J. Precht, Wied. Annalen (66-4, pp. 676, 697), 1898.
[2] H. E. Schaeffer, Astro-Physical Journal (28, pp. 121-149), Sept., 1908.

"3. The spark obtained when a capacity of .0005 to .012 M. F. and a self-induction of 0.003 henrys has been introduced into the secondary circuit."

In this study it was found that "when the magnetic field was parallel to the spark length, the first type of spark presented two sheets of vapor in the form of spirals. In a field at right angles to the spark length this vapor is in the form of two semicircular sheets, one being on each side of the spark gap in a plane perpendicular to the direction of the magnetic field.

"In the second type of spark (if the capacity did not exceed .002 M. F.) and in the third type brilliant spiral threads in a parallel field and brilliant circular threads in a transverse field took the place of the spiral and circular sheets respectively. In the first and second types of spark the bundle of threads across the gap could not be deflected by a magnetic field of 12,000 gausses. In the third type the metallic vapor and the threads across the gap were deflected in a very strong field and in a manner analogous to that of the circular and spiral threads. Reversing the direction of the magnetic field, or that of the current through the primary of the induction coil, changes the position of the sheets and of their ends. Decreasing the current through the primary or lengthening the spark gap sufficiently, causes one sheet or one set of threads to disappear."

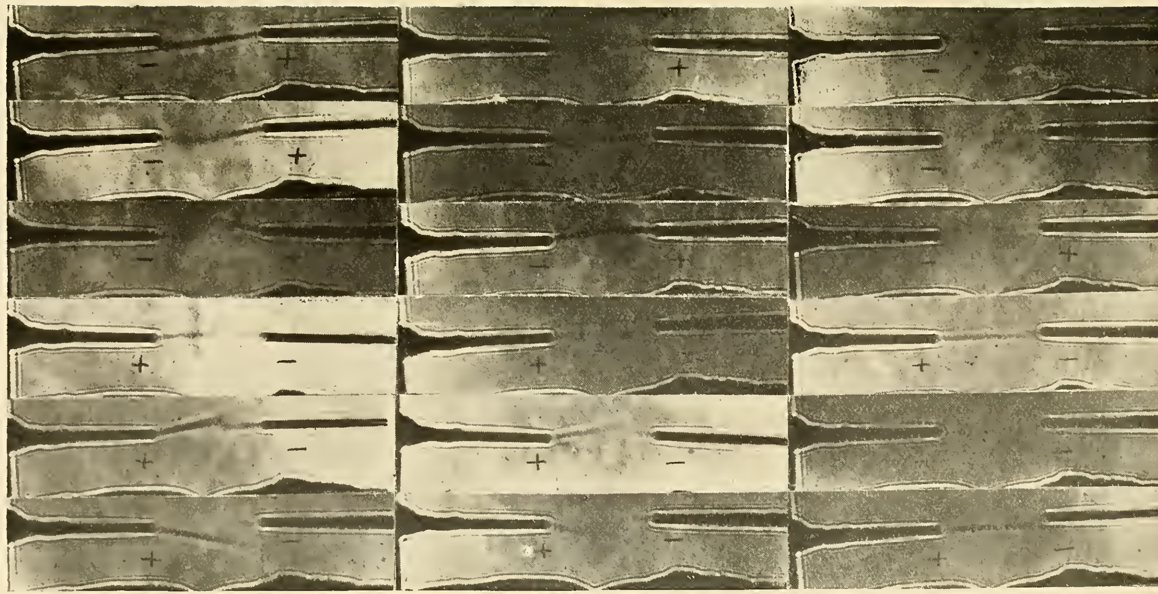
The different parts of the deflected spark were analyzed by the spectroscope, and it was found that the "Circular sheet of the first type of spark gave a spectrum of nitrogen bands, while the central threads showed that of the metallic lines and the air lines. The second type gave the same spectrum of bright air lines, and fainter metallic lines, for both circular threads and central threads. The third type showed the same spectrum (air lines) for all metals used as electrodes. The spectrum of the circular threads showed the arc lines in addition to the air lines."

By means of a rotating mirror, the velocity of the circular threads of the spark was determined, and from this a value for $\frac{E}{M}$ calculated.

Prof. A. L. Foley[1] passed transversely through a long tube which served as a pinhole camera an electric discharge and observed that when a photographic plate was placed at the opposite end of the tube from the pinhole, the plate after exposure showed a shadow picture of a stream

[1] Not yet published.

No. 1.
No. 2.
No. 3.
No. 4.
No. 5.
No. 6.



A. B. C.

No. 1. Current first direction; magnetism (none).
No. 2. " " " " 1st direction.
No. 3. " " " " 2d "

No. 4. Current second direction; magnetism (none).
No. 5. " " " " 1st direction.
No. 6. " " " " 2d "

between the points which were used as electrodes. The picture of this stream was surrounded by interference or diffraction fringes, in some ways resembling the fringes about the solid points themselves.

The principal object of the present investigation was to study the effect of a magnetic field upon this stream and to study the character of the particles composing it.

The apparatus used was that constructed by Prof. Foley and Mr. Haseman for the investigation of interference fringes about a point discharge, air streams, and vapor streams. It consisted of a wooden tube 6.87 meters long (Fig. 1). One part 20.3x20.3x230 cm. was made to telescope over another part 15.2x15.2x457 cm. This provided a means of separating the two parts for adjusting the points and magnets. Another portion (E., Figs. 1 and 3) containing a plate holder (F) was made to fit over the end. The tube was painted a dead black inside, and at intervals screens (Fig. 9) were placed throughout the tube so that no light would be reflected from the sides. An opening was made in the lower side of the tube beneath the points and through this opening a magnet was introduced so that the lines of force were perpendicular to the direction of the line of discharge. During the latter part of the experiment a similar opening was cut in the top of the tube and a second magnet placed above the first one so that like poles faced each other. Figs. 4, 5, 6 and 8, show the successive attempts to increase the field strength. The end of the tube (C) was closed by a cap which shut out all light except from a pin hole, as shown by Fig. 2. A circular disc with holes of various sizes provided a means of regulating the amount of light. A is a 90° arc lamp, the crater of which is focussed on the pin hole by the lens B.

Light was shut out of the tube by placing a piece of black card board in front of the pin hole. When a photograph was to be taken, if the discharge was a silent or brush, the slide (S) was drawn from over the plate, and after the tube had come to rest, the card board was removed until the plate was sufficiently exposed. In case of the spark discharge which fogged the plate if exposed too long, the card board was first removed and the exposure made by withdrawing the slide.

The points first used were made of sharply pointed brass pins 0.61 mm. in diameter and 3 cm. long. In the latter part of the experiment the brass pins were replaced by steel millinery needles 0.70 mm. in

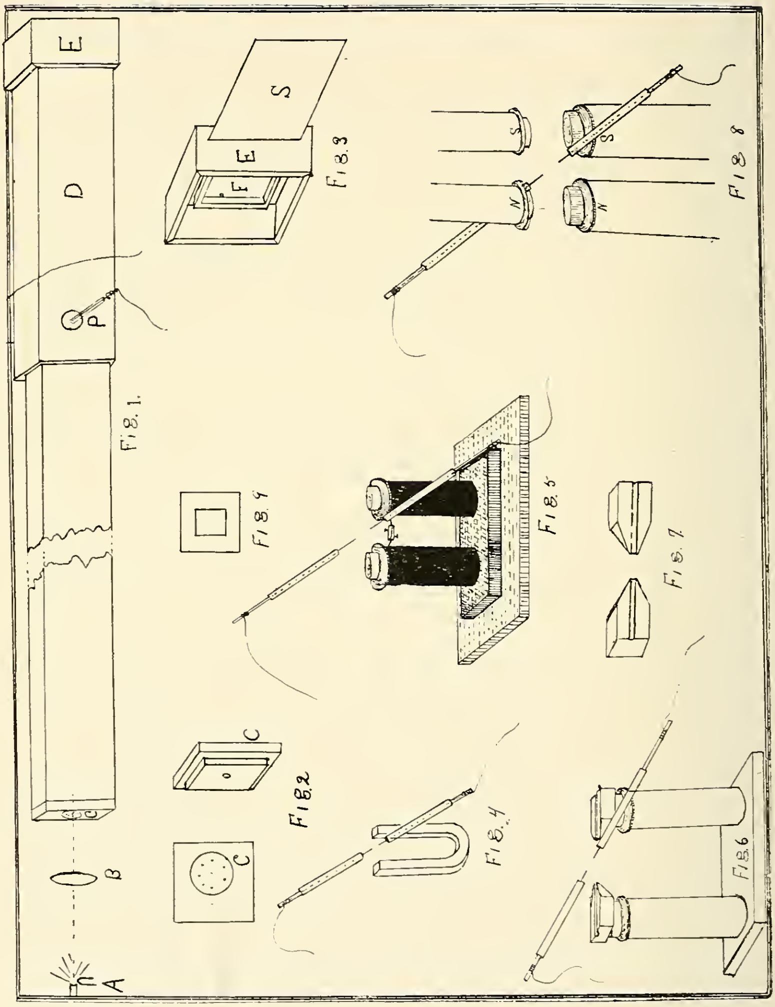


Fig. 1.
Fig. 2
Fig. 3
Fig. 4
Fig. 5
Fig. 6
Fig. 7
Fig. 8
Fig. 9
A
B
C
D
E
F
P
S
N

diameter and 5.2 cm. long. They were soldered into the ends of brass rods 0.5 cm. in diameter. The rods were placed in glass tubes and held firm by sealing wax at the two ends of the tubes. The points were charged by means of a four-mica-plate Wagner static machine (the Leyden jars had been removed), which was run by an electric motor with a rheostat in circuit for regulating the speed. The rods extended through the sides of the camera as shown by (P) Fig. 1, so that the points were near its axis. The points were about 15.5 mm. apart for the first three series of photographs and about 17 mm. apart for the last four series.

For the first series of photographs the magnet extended through the lower side of the tube directly below the points and was placed so that the tops of the pole pieces were about 0.5 cm. below the points. When the separable pole pieces, Fig. 7, were used they were covered with a layer of sealing wax about 3 mm. thick on all sides except the one facing the magnet cores, to prevent sparks passing to the magnet from the points.

As a preparation for the experiment the simpler part of Precht's work was repeated (i. e., apparatus was set up containing one point and one blunt electrode in the same position shown by the points in Fig. 6). The deflection of spark, brush and glow discharge were easily observed in a semi-darkened room when a transverse field was produced by exciting the magnets. Some cases were observed in which the discharge was transformed from one type into another, but no measurements were made of the potential, nor determination made of the signs of the charge on the points to see if they accorded with the results given by Precht.

The magnets and points were then placed in the tube as described and photographic records made of the discharge. The silent discharge was first studied. To produce the magnetic field a permanent horseshoe magnet was first used, and although it was strong enough to blow out the arc of an arc lamp, the photographs taken showed no deflection of the stream. It was then replaced by an electro-magnet, Fig. 5, later pole pieces, Fig. 7, were placed as shown in Fig. 6, and finally two electro-magnets placed in opposition, Fig. 8, in attempts to produce a field sufficiently strong to deflect the stream. The magnets were weak compared with those used by Precht and H. E. Schaeffer. The field measured only about 1,000 gausses as used in Figs. 5 and 6, and only about 1,500 gausses as used in Fig. 8. None of the photographs taken of the silent discharge showed any deflection when the magnets were excited.

Seven series of photographs were then taken.

A—Is a visible spark discharge.

B—Is a Brush discharge (a violent stream extended about 0.8 cm. from the positive point. The negative point showed only a bright speck).

C—The glow or silent discharge. (Nothing was visible between the points in the darkened tube. Each point showed a bright speck.)

D—Spark discharge representing the highest speed of the machine and highest potential between the points.

E—Spark discharge, representing the lowest speed of the machine at which a visible spark passed between the points. A lower speed would have caused the spark to change to brush.

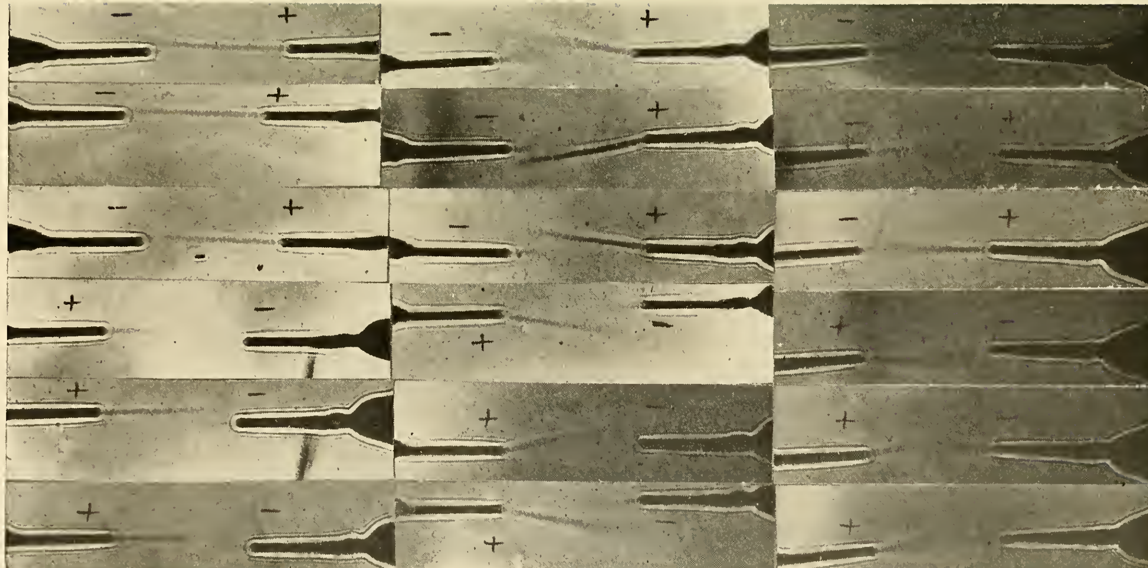
F—Silent discharge, same as C.

G—Silent discharge, same as C. Deflected by a stream of air issuing from below the points.

The different series in decreasing order of their potential as represented by the relative speed of the machine are D, A, E, B (C, F or G). Series A, B and C were taken with magnet and pole pieces as represented in Fig. 6. The magnetic field strength was about 1,000 lines per sq. cm. in the region of the points. The points were 15.7 mm. apart. Series D, E and F were taken with the magnets as shown in Fig. 8. The magnetic field strength in the neighborhood of the points was about 1,500 lines per sq. cm.

The six numbers of each series, A, B, C, D, E and F, were taken in succession as rapidly as possible, it requiring 20 or 30 minutes to complete the series. In the photograph the longer stream is the one from the positive terminal and the shorter one the stream from the negative electrode. If the positive stream is from right to left it is designated as "first direction," if from left to right, as "second direction." Nos. 1, 2 and 3 then show current in the "first direction," while Nos. 4, 5 and 6 show current in the "second direction." If the magnets were excited so that the direction of the lines of force were from the front to the back of the photograph (i. e., after correcting for the reversal in direction caused by printing from the plate), the direction of magnetism is designated as "first direction," and those with the lines of force from back to front of the page are designated as magnetized in the "second direction."

No. 1.
No. 2.
No. 3.
No. 4.
No. 5.
No. 6.



D E F

No. 1. Current first direction; magnetism (none).
No. 2. " " " " 1st direction.
No. 3. " " " " 2d "

No. 4. Current second direction; magnetism (none).
No. 5. " " " " 1st direction.
No. 6. " " " " 2d "

Following then this plan, Nos. 1 and 4 show the current when the magnets are not excited. Nos. 2 and 5 show the current in a field of the "first direction," and Nos. 3 and 6 show it in a field of the "second direction." It may be observed from the photographs that the streams in series A, B, D and E are deflected as if they were flexible conductors bearing a current, in so far as direction of deflection is concerned, thus indicating that the stream is one of charged particles.

But some characteristics of the photographed stream are hard to explain on the theory that the air is ionized and that the stream consists of charged particles. The glow discharge and the negative stream in all cases show no deflection in a field of 1,500 gausses. Also the stream goes in a straight line after leaving the point instead of following a curved path to the opposite electrode, and there seems to be no connection or joining of the negative and positive streams. In some ways it acts as the air and vapor streams investigated by Professor Foley and Mr. Haseman. In case of the silent discharge, where the machine was run at its lowest possible speed and the potential was the lowest, the stream retains the same size as far as it can be traced. In series B there is not much change in the width of the stream. Series E shows the stream growing broader as the distance increases from the electrode. Series A shows a still greater broadening and D an even greater dispersion. The greater pressure in the stream no doubt accompanies the greater potential difference, and therefore accompanies the greater dispersion of the stream, as was shown to be true in case of air and vapor streams by Professor Foley and Mr. Haseman. Series E and B show a greater deflection than any other series, and since B was the highest potential brush discharge and E the lowest potential spark discharge which could be obtained without a transformation of the type of discharge, these few photographs indicate that the greatest magnetic deflection is produced when the discharge is on the verge of changing from one type into the other. Enough photographs were not taken to verify this, however.

It will be observed in Nos. 1 and 4 of the series E that the stream does not always pass along a line directly between the points, even when the discharge takes place outside a magnetic field. In the observations made such cases were in a minority, the discharge as a rule passing directly between the points or nearly so. The cause of its deviation in these few cases was not learned.

Also, very often when adjusting the speed to obtain photographs for series B and E the discharge would change from one type to the other when the magnets were excited. Precht found that this was the case, but these observations can hardly be compared with his, since point electrodes were used in this case, while he used one point and one blunt electrode. In all cases observed where a change occurred, if a brush discharge in a nonmagnetic field passed above or below a line directly between the points as shown by the spark discharge E, 1 and 4, and the magnets were excited to deflect the stream in such a way as to make the path of discharge shorter, it changed to a spark discharge. Or if a spark discharge passed directly between the points and was deflected it changed to a brush. In all observed cases (possibly 25 or 30) the transformation could be explained by the change of distance.

The series G shows the effect of an air current on the path of discharge. The air current was led into the camera through the bottom side by means of a glass tube 2.25 cm. in diameter so that the mouth of the glass tube was 2.2 cm. below the points, and flowed at the rate of about 1,200 c. c. per second. Nos. 1 and 3 show the discharge without the air current, and Nos. 2 and 4 show deflection by the air current. It differs from the deflection produced by the magnetic field in that the greater deflection here is with the negative stream. This indicates that the pressure is not as great in the negative stream as in the positive, which agrees with the work of [1]S. Arrhenius, who measured the torsion produced by a suspended wire cross with points bent at right angles to point in the same direction and found that the torsion produced by the negatively charged wire was less than the positively charged wire, which was more clearly shown the lower the potential. (Note—It was intended to show a photograph with current in second direction, deflected by an air current. G 4, which should have shown this, shows a current in the same direction as G 2, which was due to a reversal of polarity of the machine. The error was not observed until the apparatus was torn down.)

Series H shows photographs of the points when the poles of the machine were placed close enough for a spark to pass between them. It was found that when a spark passed between the poles of the machine there was a violet stream (brush) between the points. This violet stream did not usually pass directly from one point to the other, but was curved with

[1] S. Arrhenius (Annal. Phys. Chem. 63, pp. 305-313), 1897.

No. 1.

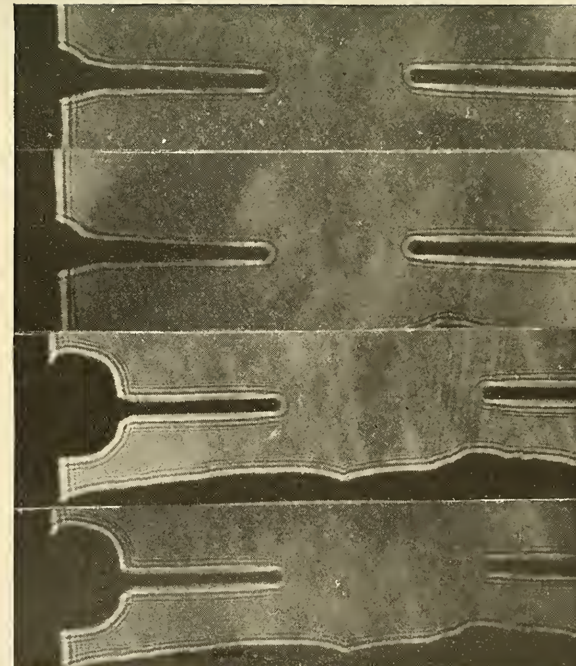
No. 2.

No. 3.

No. 4.

G

Nos. (1 and 3) G. Without air stream.
Nos. (2 and 4) G. With " "



H

H. Spark between poles of the machine, violet brush between points.

the two ends connected to the needles, not always at the points. When the magnets were excited there was no deflection observed in a field of 1,500 gausses. The photographs taken show nothing between the points.

Before putting on the cap containing the plate to take the photographs in series D a pencil drawing was made of the general form of each spark as seen from the end of the tube. Fig. 10 is a blue-print taken from these drawings, which shows that the direction of the spark as it leaves the electrode has the same direction as the photographed stream.

The width of the streams was measured in the proximity of the point with a micrometer microscope, and it was found that the width was independent of the potential between the points. The measurement was made between the outer edges of the central dark band. It will also be noticed in series D that the negative stream is almost as plain and almost as long as the positive stream.

The photographs of series E show plainly the interference fringes as described by Professor Foley. Although no special pains were taken to show these fringes in any of the work, one or two can be seen on each photograph.

SUMMARY OF RESULTS.

1. The positive stream between the points for a spark or brush discharge was deflected by a magnetic field as low as 1,000 gausses, the direction of deflection being in accordance with electro-dynamic laws.

2. The stream for glow discharge and the negative stream in any case were not deflected by a field of 1,500 gausses.

3. The direction of the photographed stream for a spark discharge as it leaves the point is the same as the visible direction of the spark.

4. The size of the stream at the points is independent of the potential between the points.

5. The stream was deflected by an air current, the negative being deflected more than the positive.

6. The stream for the high potential spark increased in width as the distance from the point increased, while the stream for the glow discharge retained its original size as far as it could be traced.

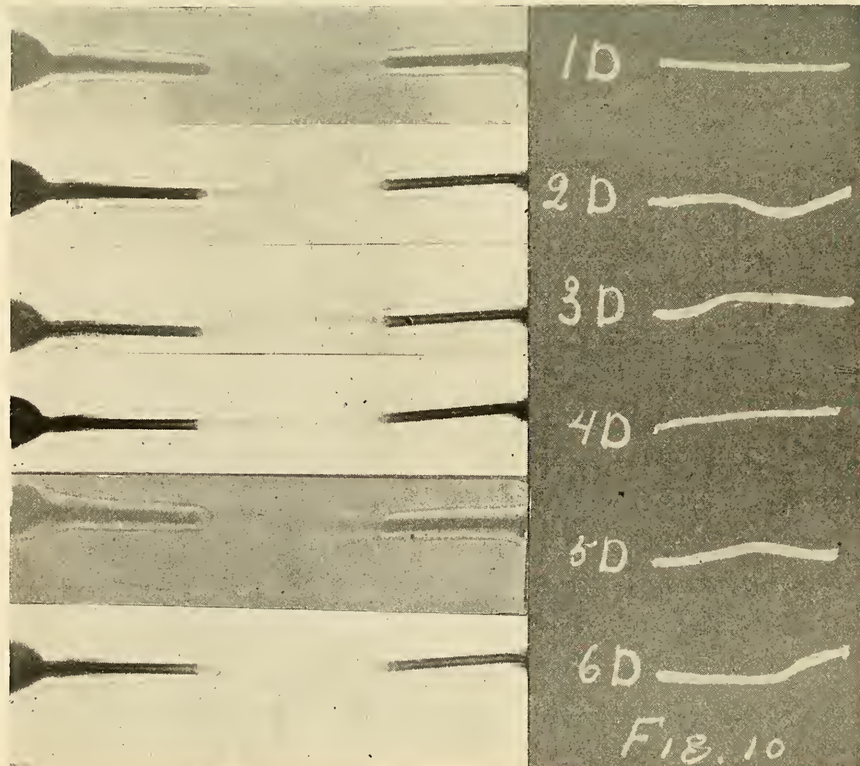
7. Some of the data indicate that the stream photographed is one of the ionized air particles, the stream as a whole having an index of refraction different from that of the surrounding air, due to its pressure.

If this is the case, however, the silent discharge stream and the negative stream should have been deflected also. This might possibly be done with a stronger field. Also, the stream, if it consists of charged particles, should terminate on the opposite electrode, which is very seldom the case.

The above investigation was suggested by Professor Arthur L. Foley of Indiana University. I wish to thank him and Professor R. R. Ramsey for their helpful suggestions during the course of the investigation.

Physics Laboratory of Indiana University.
Bloomington, Ind.

No. 1 D.
No. 2 D.
No. 3 D.
No. 4 D.
No. 5 D.
No. 6 D.



The Tenacity of Gelatine.

[Publication No. 35.]

By Arthur L. Foley.

Some years ago the author called attention to the fact that the cohesive forces of gelatine must be considerably greater than those of glass in order that a single drop of gelatine in drying and contracting on a glass plate may pull a ring or disk of glass from the plate.[1] The forces here exhibited are apparently greater than shown in the common, though not well known, process of producing chipped glass by flowing a pane of glass with gelatine and allowing the gelatine to dry. Inasmuch as the author could not find in any of the literature at hand any recorded values of the tensile strength of gelatine, he requested one of his students to attempt to determine its value. Several plans were tried, the one giving the best results being as follows:

Gelatine threads were drawn out between the ends of small wooden sticks (about the size of a match) after dipping one end of each in melted gelatine. The diameter of a thread was varied by varying the size and temperature of the gelatine drop, the thickness of the fluid, the length of the thread and the time spent in drawing it. To the other end of the wooden sticks there had been attached previously small wire hooks for suspending the upper end of the threads and for attaching a small cloth sack to the lower end. Into this sack dust shot were slowly run until the thread broke. The cross-section of the thread was then measured at the point of break.

When the section of a thread was regular its cross-sction was calculated from the diameters measured by a micrometer microscope. Threads of irregular cross-section were placed under a microscope with a camera lucida attachment and a tracing made of the perimeter. The area of the tracing was measured with a planimeter and the area of the section of the thread itself calculated from the known magnifying power of the microscope and attachment.

When glass threads are drawn they are usually almost cylindrical. Gelatine threads also are probably approximately cylindrical at the time

[1] Note on the Molecular Forces in Gelatine. Science, Vol. 23, p. 790, May 18, 1906.

of drawing, but they are not so after hardening and drying. The cross-section usually becomes quite irregular. This indicates a condition of internal strain which acts to lower the breaking strength of the thread. Coarse threads would be subject to greater strains than fine threads, and therefore we should expect them to show a smaller tensile strength than the fine threads. This is in accord with the results of experiment as shown by the data of Table I.

TABLE 1.

Cross Section in sq. mm.	Breaking Strength in grams.	Tensile Strength in Kgm. per sq. cm.
.000835	27.6	3,305
.000919	21.5	2,340
.001002	21.1	2,106
.001334	37.2	2,778
.001670	35.0	2,096
.002610	58.2	2,230
.004524	96.5	2,133
.006729	138.1	2,052
.013920	211.4	1,519
.035900	400.1	1,114
.130300	800.0	614
.264900	2,850.0	1,076
.608900	5,600.0	919
1.709200	6,100.0	357

Inasmuch as the error in measuring the breaking strength of any particular thread was relatively small and all strains tended to decrease that strength as measured, I have included in Table I only maximum readings; that is, readings which gave the greatest values of the tensile strength for the several sizes of threads. Average readings gave results some twenty per cent lower.

Curve A of Fig. 1 is a plot of some of the individual maximum readings of Table I. It will be observed that the measured tensile strength increases very rapidly as the threads are made thinner. A similar increase takes place in wires and glass threads, and is attributed to a "skin effect." This increase is shown in Curve B, Fig. 1, which represents the results obtained by drawing out glass threads and determining their tensile strength by the method already described. That the increase in the case of glass threads was due in part only to the "skin effect" was indicated by the fact that the tensile strength measurements for threads of different sizes were made more nearly uniform by carefully annealing the threads. This was done by hanging the threads in a vertical iron tube with a small weight attached to the lower end of each thread to keep it straight. The entire tube was then brought to a temperature slightly below the melting point of glass and maintained at that temperature for one hour, after which it was allowed to cool slowly in the asbestos jacket surrounding it.

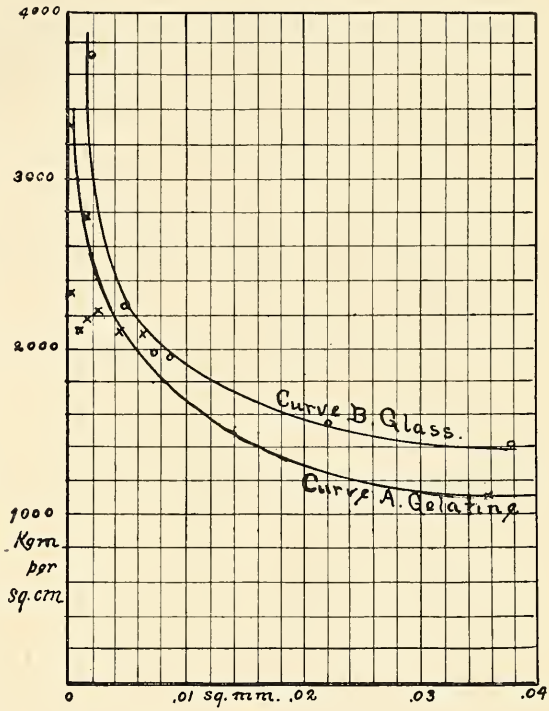


Fig. 1.

An attempt was made to anneal the gelatine threads by hanging them in a moist atmosphere in an enameling oven. Various oven temperatures were tried with little permanent effect on the threads. The author inclines to the view that the "skin effect" does not account for the larger values of the tensile strength shown by the finer gelatine threads, but that it is due chiefly to the cause already suggested—the greater uniformity in the finer threads.

The highest value of the tensile strength obtained for glass threads was 3,652 kilograms per square centimeter, while the maximum for gelatine threads was 3,305 kilograms per square centimeter. It is evident that these values cannot represent the true values or the relative values of the tenacity of glass and gelatine. It may be that the internal strains set up in the gelatine threads were such that the tensile strengths as determined in this experiment were always too low. Or it may be that the "skin effect" in glass threads gave values far beyond the tenacity of glass in a plate. Further experiments along this line are in progress.

The strength of the gelatine threads was found to increase for a few hours after drawing, and then to decrease—especially when the thread was exposed for a day or two to a dry atmosphere. Impurities tended to weaken the threads, Curve A of Fig. 2 showing the tensile strength of threads of ordinary glue. Curve B those of gelatine containing six per cent of potassium chlorate, and Curve C those of supposedly pure gelatine. Gelatine containing six per cent of potassium alum gave a curve similar to Curve B; that is, the tensile strength of the gelatine was diminished by the salt. Still it must be greater than the tenacity of the glass, for in chipping glass about six per cent of some easily crystallizable salt is mixed with the glue in order to produce the peculiar fern-

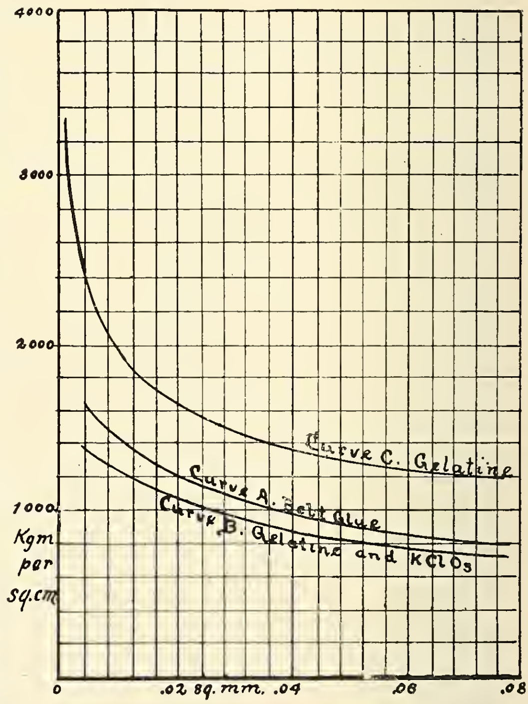


Fig. 2.

like forms which give chipped glass its decorative effect. Evidently there are forces involved here other than cohesion and adhesion, as these terms are commonly used.

Most of the experimental work of this investigation was done by Mr. Elmer J. Harrel, now of the high school of St. Paul, Minn.

Physics Laboratory of Indiana University,
Bloomington, Ind

Effect of Certain Dissolved Salts Upon the Cohesion of Water.

By Edwin Morrison.

Cohesion is defined as "that force which holds molecules of the same kind together." This force is very manifest in all solids, giving rise to such properties as hardness, brittleness, malleability, ductility, tensile strength, etc. Although not so apparent, all liquids manifest the same kind of an attractive force between molecules. Surface tension and the phenomenon of capillarity are due in a measure to cohesion of the molecules. That molecules of water are held together by means of cohesion can be demonstrated by bringing a clean, horizontal disk of glass in contact with the surface of water and then adding sufficient force to pull the disk away from the water. In case the surface of the disk is wet when it comes away from the water we know that the force applied has separated two films of water, each equal in area to that of the disk.

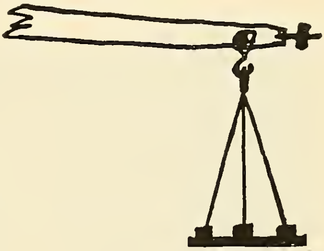


Fig. 1.

Probably Gay-Lussac first experimented upon this force and established the commonly accepted data of 526.875 dynes per square cm. Gay-Lussac used a glass disk supported by three guy cords as shown in Fig. 1.

The author designed and constructed a piece of apparatus for measuring cohesion of water and other liquids and reported the same to the Iowa Academy of Science in 1904. This apparatus consists of a round glass disk 10.6898 cm. in diameter mounted upon an accurately constructed cone 10.5 cm. high. with an eyelet in the apex for suspending the cone from the hook of a specific gravity balance. A cut of this apparatus is shown in Fig. 2.

In 1905 the author carefully worked out and reported to the Iowa Academy of Science the value of the cohesion of water as follows:

Data.—Diameter of the glass disk.

1 Measurement	10.662 cm.
2 Measurement	10.698 cm.
3 Measurement	10.727 cm.
4 Measurement	10.694 cm.
5 Measurement	10.645 cm.
6 Measurement	10.674 cm.
7 Measurement	10.702 cm.
Average	10.6898 cm.

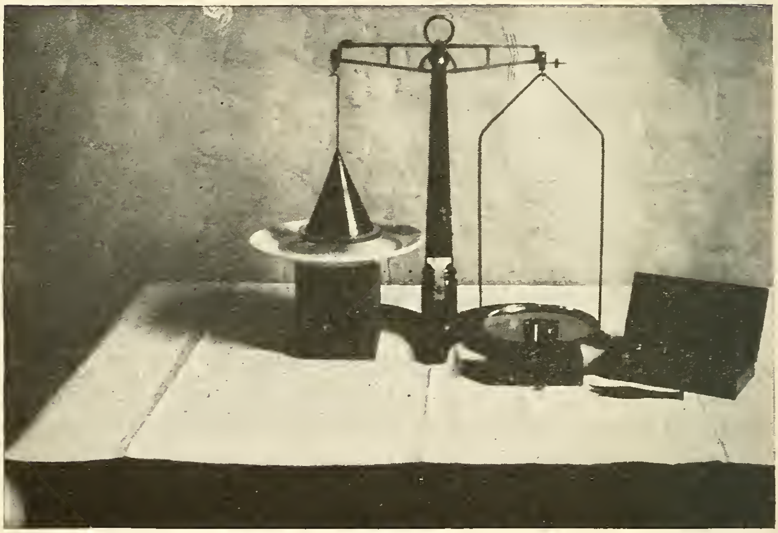


Fig. 2.

Test No. 1.—The number of grams to separate the disk from water at 4° C.

Trial 1	48.725
Trial 2	48.730
Trial 3	48.725
Trial 4	48.733
Average	48.728

Test No. 2.—The number of grams to separate the disk from water at 7° C.

Trial 1	48.710
Trial 2	48.715
Trial 3	48.725
Trial 4	48.730
Average	48.720

Test No. 3.—The number of grams to separate the disk from water at 7° C.

Trial 1	48.630
Trial 2	48.640
Trial 3	48.655
Trial 4	48.675
Average	48.650

The diameter of the disk being 10.6898 cm., the radius being 5.3449 cm., the area is 89.7200 square cm. In the first test given above it required 0.5431 g. to separate one square cm. of water. In the second 0.5430 g. and in the third 0.5421 g. The average of the three tests is 0.5427 g. per square cm., which is equal to 531.846 dynes per square cm.

In comparing these results with those of Gay-Lussac we find that he used a disk which was 11.86 cm. in diameter, and that it required 49.40 g. to separate the disk from water, or 526.875 dynes per square cm.

At this point it may be well to state the precautions taken in the experiment. First, in order to insure that the water used was chemically pure, ordinary laboratory distilled water was redistilled in Jena glass vessels in the presence of sulphuric acid and potassium dichromate. Second, the disk was thoroughly cleansed by washing in a solution of potassium dichromate and sulphuric acid; then in alcohol; then the disk was dried in a current of air and washed again in redistilled water. Third, a delicate laboratory balance with a rider weight was used in the experiment.

At the time the above data on the cohesion of water was worked out it was suggested that certain dissolved salts have a marked effect upon the cohesion of water. It is the purpose now to note some of these effects.

A number of solutions of certain salts in distilled water have been tested by means of the same glass disk as used in the cohesion of water experiment. The first solution tested was that of sodium chloride. Six solutions were prepared by dissolving each of the following number of grams of salt in 200 cc. of distilled water: 7.82 g., 15.64 g., 31.28 g., 46.92 g., 62.56 g., 72 g. (saturated solution).

Six solutions each of copper sulphate and sugar were prepared in the same way as in the case of sodium chloride, and each solution was tested for the number of grams to separate the liquid films.

The results for the eighteen different solutions are tabulated as follows:

First.—The number of grams to separate the disk from the solutions when 7.82 g. of each of the three materials were dissolved in 200 cc. of water.

Trial.	Sodium Chloride.	Copper Sulphate.	Sugar.
1	42.45	48.40	48.50
2	42.50	48.45	48.52
3	42.50	48.47	48.48
Mean.	42.48	48.44	48.50

Second.—The number of grams to separate the disk from the solution when 15.64 g. of each of the three materials were dissolved in 200 cc. of water.

Trial.	Sodium Chloride.	Copper Sulphate.	Sugar.
1	42.15	49.20	50.50
2	42.00	49.30	50.52
3	41.95	49.35	50.51
Mean.	42.03	49.28	50.51

Third.—The number of grams to separate the disk from the solutions when 31.28 g. of each of the three materials were dissolved in 200 cc. of water.

Trial.	Sodium Chloride.	Copper Sulphate.	Sugar.
1	46.39	50.35	51.50
2	46.30	50.37	57.49
3	....	50.35	51.51
Mean.	46.345	50.356	51.50

Fourth.—The number of grams to separate the disk from the solutions when 46.92 g. of each of the three materials were dissolved in 200 cc. of water.

Trial.	Sodium Chloride.	Copper Sulphate.	Sugar.
1	50.00	51.00	53.10
2	50.02	51.05	53.10
3	50.01	51.07	53.50
Mean.	50.01	51.06	53.26

Fifth.—The number of grams to separate the disk from the solutions when 62.56 g. of each of the three materials were dissolved in 200 cc. of water.

Trial.	Sodium Chloride.	Copper Sulphate.	Sugar.
1	50.90	51.50	55.70
2	50.85	51.45	55.80
3	51.05	51.25	55.75
Mean.	50.90	51.46	55.75

Note. The copper sulphate solution was a saturated solution.

Sixth.—The number of grams to separate the disk from the solutions when each of the three materials were saturated solutions at the normal temperature.

Trial.	Sodium Chloride.	Copper Sulphate.	Sugar.
1	50.92	....	57.00
2	51.05	....	56.95
3	50.90	....	57.10
Mean.	50.96	51.46	56.99

These results for each of the three dissolved salts may be plotted graphically by using the number of grams concentration as abscissas and grams to separate the disk as ordinates.

Conclusions.—First, the above data seem to indicate that within certain limits the cohesion of water with dissolved salts in it is a function of the concentration.

Second, as far as tested all dilute solutions of salts in water render the cohesion of the solution less than that of pure water.

Third, so far as tested the dilute strongly basic salts produce a greater decrease in the cohesion of the solution from that of pure water than the nonbasic salts.

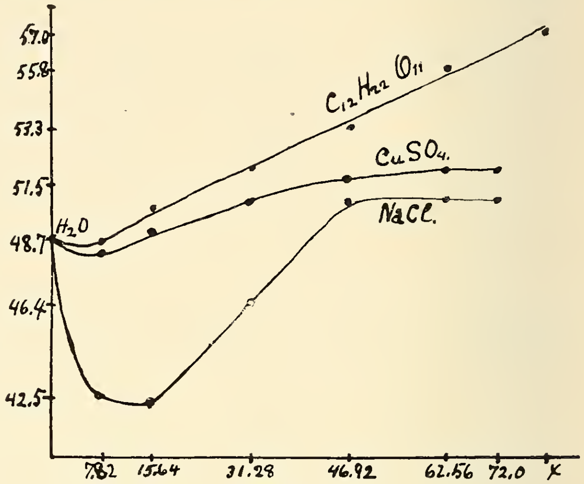


Fig. 3.

Fourth, it is also noted that before the point of saturation is reached in the strongly basic solutions, increased concentration does not produce increased cohesion.

Tests are in progress with various other salts than the ones referred to above. Also tests are in progress in which other solvents than water are being used.

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Some Features of Delta Formation.

By Charles R. Dryer.

In August and September, 1902, the writer spent some weeks among the western Finger lakes in Livingston and Ontario counties, New York. Along the shores of Hemlock Lake his attention was attracted by many recently formed deltas which seemed to present unusual features. Each delta was a semi-circular pile of fine shale shingle symmetrically arranged around the mouth of a little gully formed by a wet weather stream. The level top stood about two feet above the lake surface and was bounded by a bank of shale which sloped downward about three feet in six to a mud line under water. The wash of waves had cut at the top of the slope a vertical cliff six inches high. The land side was bounded by a very steep bank of stratified shale, a portion of the general lake shore, which is almost everywhere precipitous. From the mouth of the gully a groove a foot wide and six inches deep extended straight out half way or more across the top of the delta, but in no case reached the water's edge. Along the sides of the groove lay sticks of wood and fragments of shale of relatively large size. One medium sized delta measured thirty-one feet by twenty-six in diameter. No camera was at hand, but sketches were made from which a rough model was constructed and photographed. (Fig. 1.)

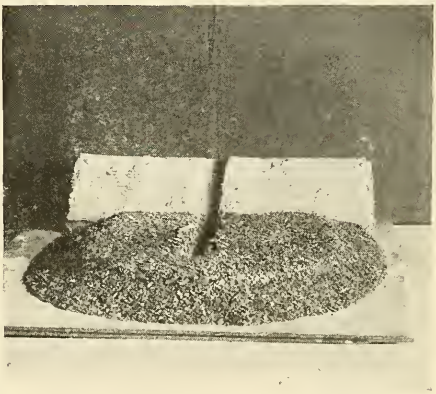


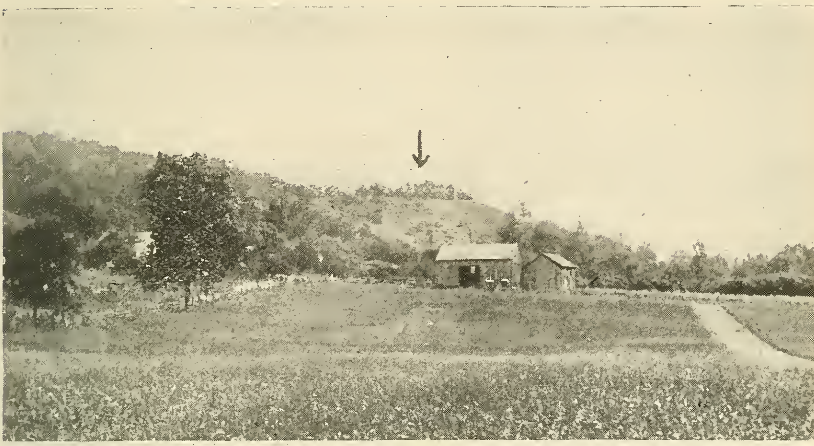
Fig. 1. Model of Torrential Delta in Shale Gravel.

The interpretation of the phenomena seemed plain. These deltas were built during an exceptionally violent storm which filled the gully with a rushing torrent and raised the level of the lake. The force of the stream was abruptly checked at lake level and its load was deposited in the form of a fan-like delta. Toward the last of the storm the stream striking the flat top of the delta dug out the groove for a few feet, but was deflected upward and spread out into a thin sheet before reaching the edge. This interpretation was confirmed by the records of rainfall and lake level kept by the Rochester water works at the foot of the lake, which is the source of public supply for that city. These records are as follows:

Date.	*Hours.*	*Rainfall.*	*Lake Level Above Datum.*
July 5	12:30–7:00 p. m.	.921 in.	1.736 ft.
" 6	In the night.	2.349 "	2.926 "
" 7	10:00–11:00 p. m.	.546 "	3.106 "
" 8	....	....	3.126 "
" 18	8:00–11:00 a. m.	.101 "	2.176 "
" 19	In the night.	1.397 "	2.296 "
....	2:00–6:00 p. m.	.607 "	....
" 20	In the night.	.864 "	2.546 "
" 22	....	....	3.186 "

These deltas were begun during the heavy rains of July 5-7, when 3.816 inches of rain fell and the lake rose 1.39 feet, most of the work being done in the night of July 6, when 2.35 inches of rain fell. They were completed July 18-20, when 2.97 inches of rain fell and the lake rose 1.01 feet. These miniature torrential deltas furnish suggestions for the interpretation of similar but larger features which mark the shore lines of the temporary glacial lakes formerly occupying the Finger lake valleys.

A similar flat-topped, steep-sided feature caught the writer's eye on the east side of Honeoye Lake. Projecting from the steep hillside like a bracket it rose 200 feet above the lake, suggesting by its bold and symmetrical outlines an artificial origin similar to that of the dump pile of a mine (Fig. 2). It proved to be a torrential delta built at the mouth of Briggs gull. Its finely curved front slope, about 150 high, is as steep as the material will lie. Its flat top is traversed by a channel twenty feet wide and three feet deep which extends to the edge and is continued by a similar groove in the steep face. The southern side cut away by the main stream shows characteristic foreset beds of sand containing large fragments of shale near the top. Briggs gull now drains a basin of about six square miles. A heavy rain with rapid melting of ice or a sudden diversion of drainage by the breaking of an ice dam in glacial times may have enabled the stream to build this delta in a few days or weeks. Briggs delta helps to account for the anomalous distribution of glacial lake deltas. Similar features are numerous in the Finger lake valleys. Not their presence but their absence from the former mouths of many streams seems the chief problem. Why do not deltas occur on all of the hundreds of streams that score the valley sides? Why did one stream a mile or two long build a delta a few rods in area while a much longer stream near by



A.

B.

Fig. 2. Briggs Delta. A. From below. B. From above.

built none? The answer seems to be that such features have no prolonged history, but owe their existence to a single local and brief accident of drainage which did not affect neighboring streams.

The largest delta of this class observed in the region, at Bristol Springs on the west side of the Canandaigua lake valley, was built in the Naples-Middlesex glacial lake at one nearly static or slowly subsiding level. The top, about one-half by one-quarter of a mile in area, is smooth and gently sloping forward from the 1,200 to the 1,100-foot level. The surface material is very coarse, containing rounded cobbles up to six inches in diameter, often with little admixture of finer sediment. This delta was built by a stream from the Bristol valley, which during the process must have drained a loaded ice lobe and not a lake.

Such simple, flat-topped, steep-sided deltas, resembling the bastion of a fort or an abutment prepared by a daring engineer from which to spring

Fig, 3. Naples Delta. Two Upper Levels. A kettlehole in the woods.

the arch of a bridge, are formed rapidly by strong or torrential streams and are composed of relatively coarse materials. From their striking and characteristic form and position they may be called *bracket deltas.*

Garlinghouse delta, a few miles south of Naples, does not project like a bastion from the face of the valley wall but fills a niche a mile deep and half a mile wide, the walls of which rise sharply 500 feet above its surface. The niche now receives two or three insignificant brooks, but one of them comes from a gap in the wall which opens northward to the upper Honeoye valley. This gap probably once transmitted a strong stream from the ice front but a few miles distant. This delta may be the only one of its kind, and if so, belongs in a class by itself—that of *niche deltas.*

Compound deltas built at several different levels are numerous in the Finger lake region. Coy Glen delta near Ithaca, a fine specimen of the type, rises from the Cayuga valley to a height of 700 feet like a giant staircase of seven steep, convex risers and as many flat treads, each of which has been evenly bisected by the stream. Such deltas are formed in waters the level of which is alternately standing and falling, the upper step being the oldest. They may be distinguished as *step deltas.*

The delta above Naples rises 400 feet and has a basal periphery of more than two miles. (Fig. 3). Seven levels are distinguishable, of which the upper three are the most conspicuous. The greater part of its mass

Fig. 4. Pitted Surface of Morainal Delta.

was brought by a stream which flowed out of the Honeoye valley from the west and was the outlet of the glacial Honeoye Lake. Streams which flow out of lakes cannot, as a rule, have sufficient load to build large deltas, and the question at once arises, how could the outlet of a lake build one in this case? Its construction was not a matter of a brief period but continued through the whole life history of the Naples-Middlesex glacial lake, into which the stream emptied. The presence of this delta is evidence, so far as it goes, that during that period no lake existed in the Honeoye valley. The area of the land which could have been drained to this delta is insignificant, and we are apparently forced to the conclusion that it was built by drainage from a drift-loaded ice mass. This inference is sustained by the occurrence upon the highest level of a sharp kettle hole 300 feet in diameter and 25 feet deep, marking the place where a detached ice block stranded and melted.

The occurrence of kettle holes in deltas is not uncommon.[1] A remarkable case of this kind has been described by the writer where an area of ten acres of delta surface is thickly pitted with small kettles.[2] (Fig. 4.) This delta is the joint product of a land stream and a valley glacier which contributed ice blocks and an undetermined portion of the permanent material. There are probably many intermediate forms between such a *morainal delta* and one due wholly to stream work.

Fig. 5. Outer Face of Morainal Delta. Fan in front of notch.

When lake waters are withdrawn the bisection of a delta may result in the formation of an alluvial fan in front of it. This gives a characteristic combination of *notched delta and fan.* (Fig. 5). The fan of Mill Creek at the foot of Honeoye Lake is a mile in diameter, and is responsible for the existence of the lake, to which it acts as a dam. The fan of Canadice outlet bears a similar relation to Hemlock Lake, which, however, is too deep to owe its existence wholly to that cause.

Deltas occasionally take the form of long, narrow ridges upon one or both sides of a stream, resembling the natural levees in "the goosefoot"

[1] Fairchild, Journal of Geology, Vol. 6, p. 589.
[2] Bulletin Geological Society of America, Vol. 15, p. 457.

of the Mississippi. Normally the point where a tributary valley joins a larger one is marked by a notch in the wall of the latter, but in some cases a bisected spur appears instead. The delta of Canadice outlet, mentioned above, furnishes a good example. A delta at Lake Warren level, near East Bloomfield, what is left of it, has the form of a single narrow tongue more than a half mile long. Such lateral deposits of a stream may be called *levee deltas.*

Hanging deltas have been the chief guides to geologists in mapping the outline of temporary glacial lakes, but they are worthy of more careful study as simple and well displayed specimens of constructional shore forms.[3]

State Normal School,
Terre Haute, Ind.

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[3] Nearly all the features mentioned in this paper may be found upon the Wayland, Naples, Honeoye and Canandaigua sheets of the Topographic Map of United States.

A Physiographic Survey of an Area Near Terre Haute, Indiana.

By Chas. R. Dryer and Melvin K. Davis.

The Survey.—In the summer of 1909 the senior author of this paper, in despair of living long enough to receive any help from the U. S. Geological Survey or from the State of Indiana, resolved to try what might be done by his own students toward a serviceable topographic survey of the area around Terre Haute. Four young men and two young women were ambitious enough to undertake the work. For a base map the atlas of Vigo county was used and found to be very poor, in fact a disgrace to the surveyor, the draughtsman, the printer and the whole community concerned. We simply made the best of it. The profiles of three railroad lines traversing the region were obtained, and other base lines and points were determined with a surveyor's level. Most of the topographic work was done with the hand level and staff. It was found possible to require that no discrepancy between different lines of levels should exceed one foot. Highways and divides were followed and section and other cross-country lines were run wherever necessary. About two days a week for six weeks were spent in the field, and the result was found to be worth while. While surveying was being done the location of particular features was noted in order that no time would be lost when their special study should come. The map drawn by the junior author of this paper from the data thus secured has proved adequate for the purpose in view.

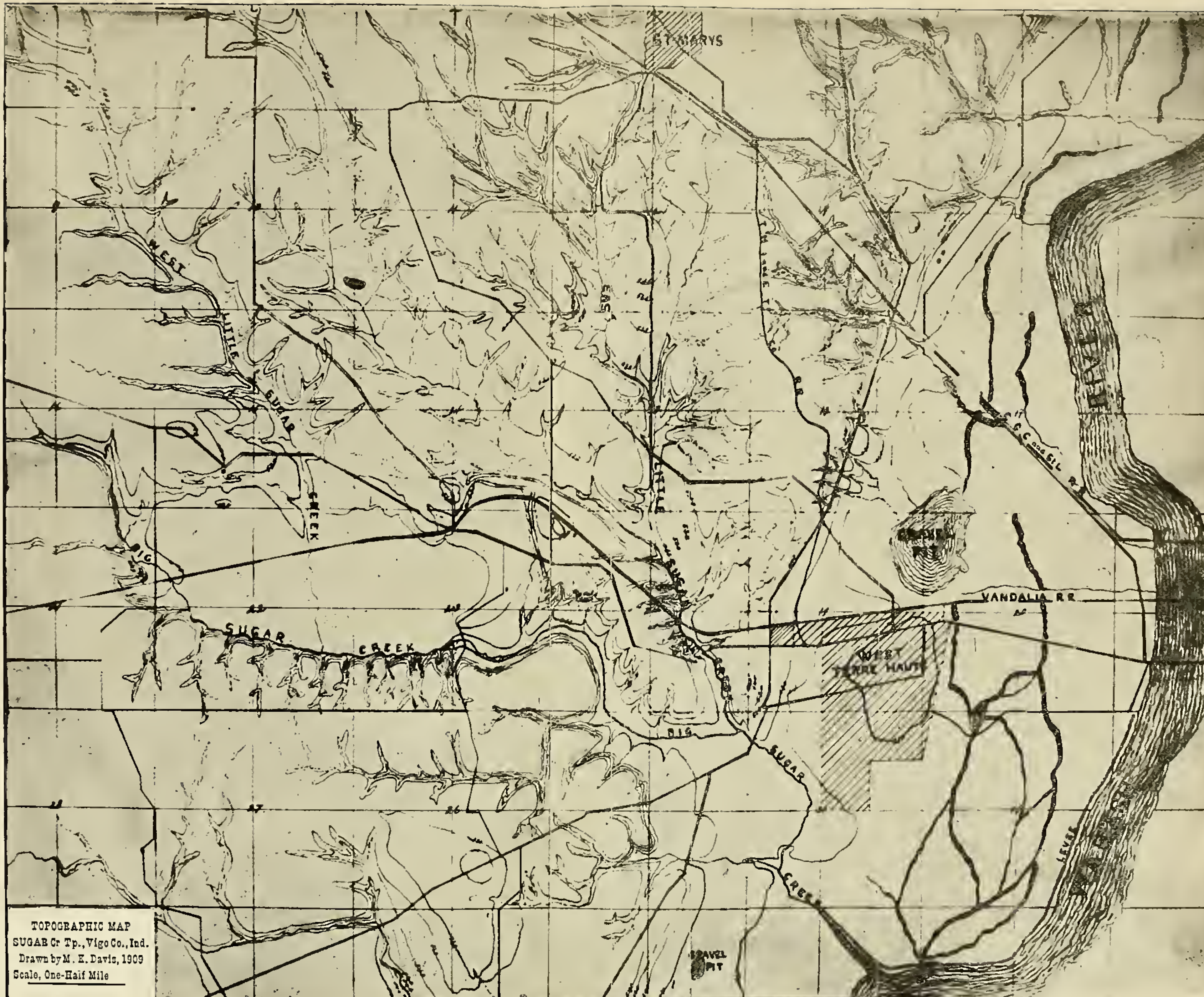
General Description.—The area surveyed is immediately west of Terre Haute and comprises about 25 square miles in Sugar Creek township, Vigo county, Indiana. It is bounded on the east by the Wabash river and includes a portion of its flood plain. West of the Wabash bluffs, here eighty to one hundred feet high, the area consists of an originally smooth upland of glacial drift 540 to 560 feet above sea level, which has been submaturely dissected by the branches of Sugar creek. The remnants of the original surface have been reduced to the scrap-tin outline characteristic of the leaves of the pin oak. The larger valleys are flat bottomed and contain alluvial filling to a depth of 40 or more feet. The drilling of a well at Vandalia mine No. 81, section 24, showed the deposit to be 40 feet deep. The slopes of valley sides are generally steep and the ravines of the ultimate tributaries are exceedingly narrow and sharp. The depth of the glacial drift is generally from 40 to 60 feet, and the streams only here and there touch bed rock.

Many beds of recent conglomerate appear along west Little Sugar creek. The principal valleys are preglacial, with a base level determined by the level of the preglacial Wabash, which was 60 or 70 feet lower than the present river. These valleys were filled with drift which the post-glacial streams have scarcely half removed. The drainage has developed by headward erosion into an intricate, dendritic system of insequent branches which penetrate to nearly every acre of the area. Judging from the position of large trees there is reason to believe that the lines of drainage were well defined before vegetation sprang up.

Stratigraphy.—The underlying bed rocks of the area are the shales of the coal measures with several workable seams of coal, the uppermost of which outcrops along the foot of the Wabash bluffs. The shales above the coal form about one-half the height of the bluffs. The upper half consists of glacial drift. Intercalated with the shales are several thin strata of limestone, two of which exercise a notable influence upon the topography. Below the 500-foot level a tough, flinty limestone four or five feet thick has resisted river erosion to such an extent as to form a terrace between the Wabash flood plain and bluff, in some places 500 feet wide and 20 feet above the plain. We call it the flinty limestone. A similar but less silicious limestone lies about thirty feet higher. In section 31 the waters of the Sugar creek system have cut a gap in these strata and reach the Wabash at grade.

The glacial drift belongs to the Illinoian drift sheet of Leverett and lies just outside the border of the Shelbyville moraine. The mass of it consists of a tough boulder clay, weathering on exposed faces into roughly hexagonal columns and containing numerous striated and faceted boulders of moderate size. Large boulders are rare. In some places the till is one-half fine gravel. There are occasional thin partings of sand. In a railroad cut about one mile to the north of the area surveyed buried logs of wood up to nine inches in diameter are numerous along a level horizon, but no difference can be discovered between the overlying and the underlying till. In the south bluff of Sugar creek beds of laminated silt are intercalated in the till and point to the occurrence of an interglacial interval of notable extent. The upper four or five feet of drift often con-


WEST
BIG
C.C. and St.L. R.R.
RIVER
GRAVEL PIT
VANDALIA R.R.
LEVEE
TOPOGRAPHIC MAP
SUGAR Cr Tp., Vigo Co., Ind.
Drawn by M. E. Davis, 1909
Scale, One-Half Mile



ST MARYS
WEST
LITTLE
SUGAR
CREEK
EAST
LITTLE
BIG
SUGAR
CREEK
GRAVEL PIT
VANDALIA R.R.
WEST TERRE HAUTE
BIG
SUGAR
CREEK
C.C. and St.L. R.R.
LEVEE
GRAVEL PIT
TOPOGRAPHIC MAP
SUGAR Cr Tp., Vigo Co., Ind.
Drawn by M. E. Davis, 1909
Scale, One-Half Mile

sist of a deep red, pebbleless and structureless loam, the origin of which is an unsolved problem. The red loam, even upon moderate slopes, gullies rapidly and has greatly facilitated the dissection of the region.

The Wabash Plain, two miles in width, presents the usual flood plain features of levees and bayous. At one point, S. W. ¼ of S. E. ¼ of section 8, shale outcrops in the midst of the alluvium. Before the valley was filled this was an island in the river with deep water all around it. The valley filling in some places is 80 feet deep, and consists of sand and gravel carried into the valley by water and floating ice. At a railroad gravel pit in section 36 twenty-five feet of fine gravel is exposed, with an occasional stratum containing enough clay to resist rain wash and cause the formation of earth pillars two or three feet high. A terrace of coarse, roughly stratified gravel formerly occupied an area about one mile by one-half opposite the city of Terre Haute and was an island at high water. The town of West Terre Haute stands upon the southern half of it. The northern half has been entirely removed by the railroad companies and excavated to the level of low water in the river. The remaining surface is from 15 to 25 feet above the plain.

The Sugar Creek Drainage System presents several peculiar features and furnishes some of the most interesting problems of the area. The small tributaries of the Wabash are usually of the parallel type, not combining into systems, the main streams flowing nearly at right angles to the Wabash. The Sugar creek system is fan-shaped, consisting of four principal streams which converge southward and eastward to a junction and pass out through a single gap upon the Wabash plain. East Little Sugar creek flows southward seven miles parallel with the Wabash river and about one mile west of the bluff. In sections 25 and 26 a nameless stream flows about one mile eastward, turns northward one-half mile and again eastward, both bends being right angles. This seems to be due to harder material in the stream's course.

Sugar Creek Lake.—At the western border of the area surveyed the valley of Sugar creek widens abruptly from less than one-half mile to about a mile. Two miles below it narrows abruptly and flows for a mile through a gorge twenty to forty rods wide. The expansion and the narrows present each its own but related problem.

The expanded portion of the valley, about one mile by two, is bounded on the south by a boulder clay bluff sixty feet high; on the north by lower and gentler slopes and is invaded near the east end by a flat spur projecting like a dam from the north side nearly to the bluff on the south. Near the south end of the dam the stream has cut a narrow gap thirty feet deep. The lower ten feet exposed in the gap is shale with a thin cap of flinty limestone. The upper twenty feet of the dam is glacial clay with the exception of a little poorly stratified gravel and sand at the south end of the dam near the creek. These features present on their face the characteristics of a drift-dammed lake drained by the down cutting of its outlet. The supposed lake bottom is underlaid, so far as discoverable, by six feet of alluvium on top of the flinty limestone. The flat-topped dam appears to be a terrace and is accordant in level with other terrace fragments in the valley of West Little Sugar creek above. In the south bluff the boulder clay is interrupted for a few rods by ten feet of finely laminated lacustrine silts, the bottom of which is at about the same level as the terrace tops.

Various hypotheses may be entertained; (1) The expansion of the valley is due to the lateral corrasion and shifting of the preglacial stream over the surface of the resistent flinty limestone. The whole valley was filled with glacial clay. The interglacial stream had a temporary base level fifty feet higher than at present and cleaned out the filling down to the terrace level (510 feet above the sea). During this process a tributary stream from the south cut a valley out of the boulder clay down to the terrace level, which was afterward filled with silt. By a lowering of the base level in the Wabash the stream was enabled to cut down the dam and to clear out the valley to the present level, draining the lake and leaving fragments of the valley filling as terraces.

(2) The preglacial and interglacial course of the stream was northeastward into the valley of East Little Sugar Creek. A readvance of the ice left a till dam across the former course and the portion of the present valley through the narrows is entirely postglacial. The complete sequence of events is not so clear as could be wished.

(3) The resistance of the two limestone strata which outcrop along the Wabash bluff and over a belt about two miles wide, west of the bluff, may account for the southward course of East Little Sugar Creek, for the narrows of the lower end of the valley and for the single gap in the bluff through which the waters of the system escape to the Wabash plain.

Period of Ravine Cutting.—The valley slopes of the ultimate tributaries are so steep and the ravines are so narrow and sharp as to prevent cultivation and they are in most places forested. The frequent occurrence of large trees, one hundred to two hundred years old, in the bottom of the ravines, indicates that the present rate of downward corrasion is very slow, and that possibly the dissection of the region in the drift area was mostly accomplished during the period of ice melting and the succeeding period of bare ground, before the surface was covered with vegetation.

Culture.—The alluvial lands in the valleys are chiefly occupied by corn fields. The broken upland areas between the ravines are inconvenient for farming but many of the small fields produce good corn, oats and hay, especially hay. The only way by which cuts, fills, and bridges can be avoided in road building is to run the roads on the narrow divides between the heads of ravines. Coal mines are numerous. Along the front of the Wabash bluffs shale and coal are accessible near the surface and four large brick and tile factories have been established. The new industries have multiplied the population of West Terre Haute by five in ten years, and have caused three considerable villages to spring up from nothing in the same time.

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Collecting Area of the Waters of the Hot Springs, Hot Springs, Arkansas.[1]

By A. H. Purdue.

Introduction.—The conclusions in this paper were reached in the course of field work on the structure and stratigraphy of the area about Hot Springs during the summer of 1909. The paper is written with the as- tion is made partly because geologists in general have come to think of most of the ground water as having such origin, and partly because the sumption that the waters of the hot springs are meteoric. This assump- recent studies of Mr. Walter Harvey Weed upon the waters of these springs clearly indicate that they are meteoric.[2]

Topography of the Highlands of Arkansas.—The highlands of Arkansas and the eastern part of Oklahoma are divided into a northern and a southern part, separated by the valley of the Arkansas River. The northern division consists of the Boston Mountains, which are a dissected plateau, reaching the height of somewhat more than 2,200 feet above sea level, and a much lower area to the north of them. The southern division consists of the Ouachita Mountains, which cover an area about 60 miles in width. These mountains consist of ridges, the direction of which is in the main east and west and some of which surpass 2,000 feet in height.

Topography of the Area About the Hot Springs.—The topography in the vicinity of the hot springs is indicated by the accompanying relief map (Fig. 1). The springs (indicated by the cross) emerge from the western end of Hot Springs Mountain, which is known as Indian Mountain east of West Branch of Gulpha Creek. Immediately north of Hot Springs Mountain is North Mountain, which continues west of Hot Springs Creek as West Mountain. Three miles west of the springs West Mountain swings around in a horseshoe curve and extends northeastward, and is known as Sugarloaf Mountain. Hot Springs Creek, a considerable stream, flowing southward, carries off the overflow from the hot springs and the drainage of a portion of the valley just south of Sugarloaf Mountain.

[1] By permission of the Chief Geologist, U. S. Geol. Surv.

[2] The hot springs of Arkansas. Senate Document No. 282, p. 90, Washington, D. C., 1902. Prepared under the supervision of the Secretary of the Interior.

This valley is from a mile to a mile and a quarter in width. About two miles northeast of the hot springs, where West Branch of Gulpha Creek cuts through North Mountain, is a limited area with an elevation of about 620 feet. The greater part of the surface, however, stands above the 700-foot contour, and the highest hills exceed 800 feet. The highest elevation at which any of the springs emerge is about 640 feet.

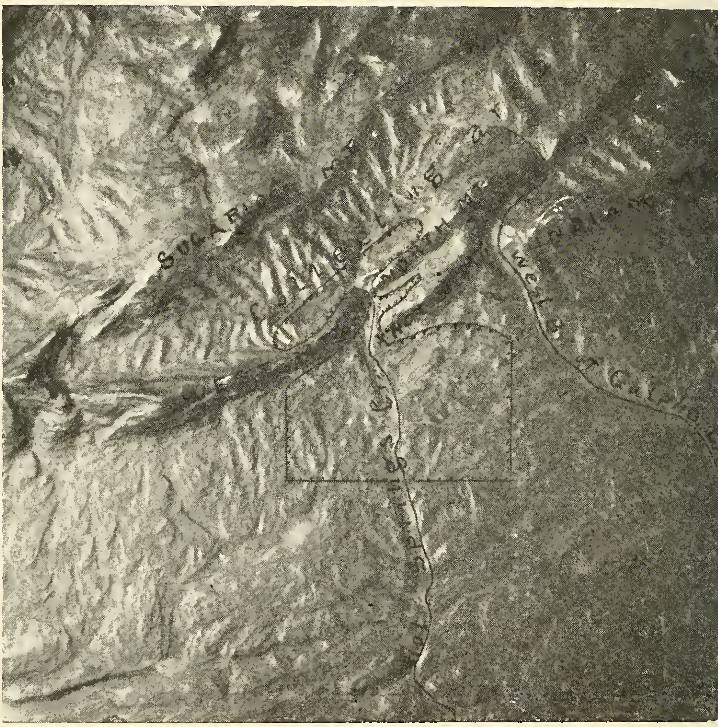

Fig. 1. Relief Map of the Hot Springs Area.

Stratigraphy of the Area About the Hot Springs.—The surface rocks about the hot springs are shown in the following section:[3]

[3] With the exception of the Stanley shale and the Hot Springs sandstone, these names were first applied to the formations as they appear in Montgomery County, Arkansas.

Carboniferous—		
Stanley shale	3,500	feet.
Hot Springs sandstone	100	"
Age unknown—		
Arkansas novaculite	380	"
Missouri Mountain slate	50	"
Ordovician—		
Polk Creek shale	210	"
Bigfork chert	570	"

The Bigfork chert is in layers from two to twelve inches thick. Throughout a good portion of the formation it consists almost entirely of chert, but in parts the layers are separated by thin beds of shale, and in other parts shale is the main constituent. The chert is very brittle and intensely fractured from the folding it has suffered.

The Polk Creek shale overlies the Bigfork chert, and is a very black, somewhat silicious shale, though soft enough from its graphitic nature to soil the fingers in handling. The upper part contains a few thin silicious beds, but the lower part is wholly shale.

The Missouri Mountain slate as it occurs in the vicinity of the hot springs is a red to brown or yellow shale, depending upon the stage of weathering. Further west in the Ouachita area it is a true slate.

The Arkansas novaculite as it is exposed in the city of the hot springs consists of three parts: A lower, massive one 275 feet thick, made up of heavy beds of much fractured novaculite. It is from this part of the formation that the Arkansas abrasives are secured. This is followed by fifty-five feet of very black clay shale, weathering in places to light gray; and this by fifty feet of what appears to be rotten, porous novaculite. The section of the novaculite formation over the Ouachita area varies greatly with the locality.

The Hot Springs sandstone[4] is a gray, quartzitic sandstone in beds from three to eight feet thick. The basal ten feet or more is conglomeratic. It is from this formation that most of the hot springs issue, which fact, however, is accidental and consequently not significant.

The Stanley shale is composed mainly of black to green clay shale, though a large per cent of it consists of rather soft, greenish sandstone. This shale skirts Hot Springs and West Mountains. While a large part

[4] This name has not been used before in Arkansas.

of the city of Hot Springs stands on this formation, only the waters of those springs that issue at the lowest levels move through it.

Structure and Rocks of the Highland Area.—The general structure of the highland area is that of a broad syncline with its trough in the Arkansas Valley. The rocks of the Boston Mountains and the area to their north lie for the most part horizontal, but in the south half of the Boston Mountains they dip perceptibly to the south, passing in the Arkansas Valley under several thousand feet of younger rocks.

The general structure of the Ouachita area is that of an anticlinorium dipping southward under the mesozoic and tertiary rocks, and northward beneath the Arkansas Valley. The rocks for the most part are intensely folded, to which, with erosion, is due the narrow valleys and parallel ridges of the area. The hot springs are located in the eastern part of this area.

The folds in the main have an east-west direction, but at Hot Springs and for some distance to the west their direction is northeast-southwest. The individual folds are as a rule not continuous for great distances, but are short and overlap each other laterally. Thrust faults, approximately parallel to the strike and of many hundred feet displacement, are common in the Ouachita Mountains and the Arkansas Valley.

Structure of the Area About the Hot Springs.—Like the remainder of the Ouachita region, the area about the hot springs is intensely folded. The folds are closely compressed and are all overturned to the south. As a result the dips are to the north. Some of these are as low as 15 degrees, and they seldom exceed 60 degrees. This means that at the points of greatest overturning the rock layers lie literally upside down, and in folding have described an arc of 165 degrees.

Possibilities of Ground-Water Flowage.—While the altitude of the Boston Mountains is sufficient to give the ground-water enough head for it to emerge at the height and distance of the hot springs, the intervening structure makes such impossible. The closely compressed folds, overlapping, and faulting of the Ouachita area are such as to prevent the uninterrupted movement of ground-water except for short distances. Likewise the stratigraphy, structure and topography to the south of the hot springs eliminate that area as a possible source of the water; and the same is true of the highlands of central and eastern Kentucky, Tennessee and Alabama and the intervening area.

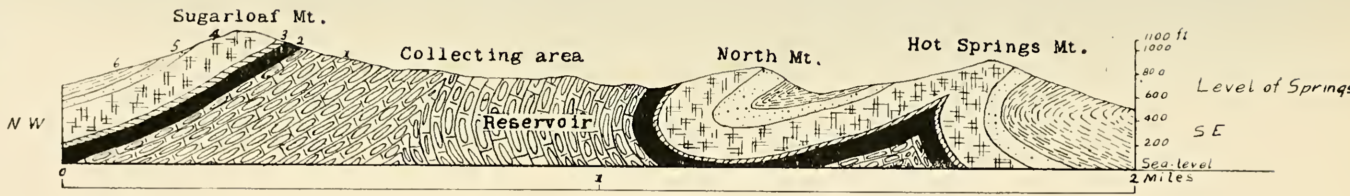

Fig. 2. Northwest-southeast section at Hot Springs.

1. Bigfork chert.
2. Polk Creek shale.
3. Missouri Mountain slate.
4. Arkansas novaculite.
5. Hot Springs sandstone.
6. Stanley shale.

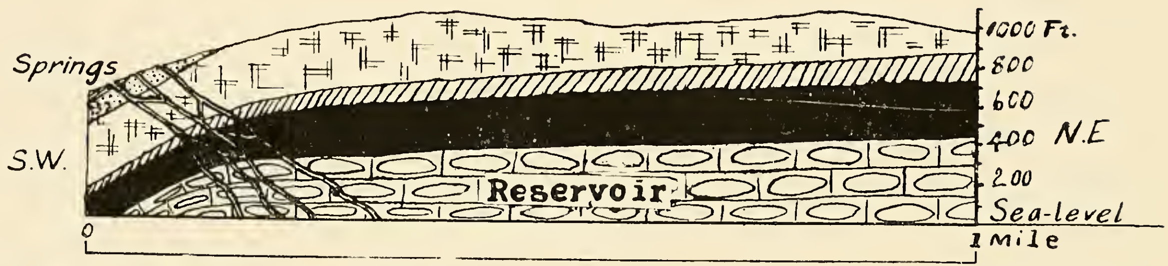

Fig. 3. Northeast-southwest (longitudinal) section of Hot Springs Mountain, showing the hypothetical water conduits at the plunging end of the anticline. Symbols same as in Fig. 2.

The Collecting Area.—It follows from the above that the collecting area must be in the near vicinity of the springs, and a study of the topography, stratigraphy and structure thereabout locates it with reasonable certainty. A glance at the section (Fig. 2) from Sugarloaf Mountain southeastward through Hot Springs Mountain will indicate the collecting area. The surface of the overturned, anticlinal valley between Sugarloaf and North Mountains is higher than the level of emergence of the springs. The rocks outcropping over the area are the Bigfork chert and the Polk Creek shale, the former occupying most of the area.

The considerable thickness of the Bigfork chert, its much fractured nature and the thin layers of which it is composed, all combine to make it a water-bearing formation of unusual importance. The greater number of the fine springs in the Ouachita area between Hot Springs and the western border of the state come from this horizon. In many places this formation occurs in anticlinal valleys with its highly inclinal beds truncated, affording the most favorable condition for the intake of water. A glance at figure 2 will show that these conditions obtain in the area between North Mountain and Sugarloaf Mountain. In addition to the favorable structure for the reception of water there is the stratigraphic condition for its retention brought about by the overlying Polk Creek shale. As a consequence of the topography, structure and stratigraphy the water is collected in the basin shown in the map (Fig. 1), conducted through the Bigfork chert beneath the North Mountain syncline, and rises in the Hot Springs anticline, at the western end of which it emerges in the hot springs. Including several of very weak flow, there are said to be seventy-two of these springs, and they are confined to a narrow strip about a quarter of a mile long.

The exact location of the springs is attributable to the southwestern plunge of the Hot Springs anticline, and as has been stated by Mr. Walter Harvey Weed[5] probably to fracturing and possible slight faulting in the process of folding, as shown in figure 3.

While not relevant to the title of the paper, it might be added that the considerable number of dikes in the immediate vicinity of the hot springs, the large number (eighty are known) only a few miles to the south on and near the Ouachita River, and the areas of igneous rock at Potash Sulphur Spring, Magnet Cove and other places, force the sugges-

[5] Loc. cit.

tion upon one that the waters of the springs owe their temperature to passing over hot rocks or the vapor from such in some part of their underground course.[6] The fact that these are practically[7] the only hot springs within the Ouachita area, though there are scores of cold springs issuing from the same formations and under practically the same geologic relations, gives this suggestion great weight; but inasmuch as some of the springs are said to be unusually radio-active, there is the alternative suggestion that atomic decomposition in igneous rocks (which may have lost their magmatic heat) is the source of the high temperature of the water.

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[6] Dr. J. C. Branner has already called attention to this as the probable source of the heat. See Geol. Surv. of Ark., Report on Mineral Waters, pp. 9 and 10.

[7] Recently a spring, said to have a temperature of 98° to 100° F., has been discovered issuing from the Arkansas novaculite in the bed of the Caddo River, at Caddo Gap, Montgomery County.

Where Do the Lance Creek ("Ceratops") Beds Belong, in the Cretaceous or in the Tertiary?

By Oliver P. Hay.

CONTENTS.

Historical Résumé.

Ever since the beginning of our knowledge of the geology of the Western plains and the Rocky Mountains there have existed contentions regarding the various deposits to which the names Laramie and Fort Union have been applied. These contentions have concerned the grouping of the various beds, the geological horizons to which the deposits of different basins and of different levels should be referred, and the members to which the names Laramie and Fort Union respectively should be restricted. Up to about the year 1896 certain deposits in the Judith River basin and others in Montana, Wyoming, Colorado and New Mexico were all regarded as the products of a single geological epoch and were all called Laramie. Although as early as 1860, or even earlier, some geologists, especially Dr. F. V. Hayden and Professor Leo Lesquereux, basing their opinion on the fossil plants, held that all or the greater part of the deposits in question belonged to the Tertiary, the prevailing opinion up to 1896 was that the Laramie, taking the term in its widest sense, was the uppermost portion of the Cretaceous. It may be said, however, that Professor Cope in his great work "The Vertebrata of the Tertiary Formations of the West" referred the Laramie, as well as the overlying Puerco (including what is now known as the Torrejon), to the "Post-Cretaceous," a group holding a position between the Cretaceous and the Tertiary. He had previously assigned the Puerco to the Eocene. However, in 1887 (Amer. Naturalist, xxi, pp. 446, 450) he transferred this "Post-Cretacic System" to what he called the Mesozoic realm. In the year 1896 Messrs. S. F. Emmons, Whitman Cross and G. H. Eldridge published their "Geology of the Denver Basin," in which the previously so-called Laramie in the region of Denver, Colorado, was shown to consist of three distinct formations. The name Laramie was by them restricted to the lowest member of these, the succeeding formations being called respectively the Arapahoe and the Denver. The last two had, however, already been recognized, named and published by Eldridge and Cross as early as 1888.

Although the authors of the Geology of the Denver Basin referred to the Upper Cretaceous the three formations mentioned, Whitman Cross (op. cit., p. 206, seq.) makes a strong argument in favor of including the Arapahoe and the Denver in the Tertiary. His plea was based especially on the existence of a great stratigraphical break between the lower and the middle of the three formations and on the evidence furnished by the fossil plants. Certain deposits in Middle Park, Colorado; others near Canyon, Colorado; others in the Huerfano basin; certain ones along the Animas River; still others in New Mexico, beneath the Puerco; and the so-called "Ceratops" beds of Wyoming were all provisionally correlated with one or other of the formations in the Denver basin.

It is interesting, therefore, to observe that about 1887 and 1888, while Cope was endeavoring to raise the boundary between the Mesozoic and the Cenozoic to a position above what is now known as the Torrejon, Cross was trying to depress it to the parting between the Arapahoe and the formation below it.

It was thought by Cross that the beds of the Judith River valley might be the equivalent of the Arapahoe beds; but it has since been conclusively shown by Stanton and Hatcher that, instead of being younger than the deposits called by Cross the Laramie, the Judith beds are older than the Fox Hills, older than the upper part of the Pierre.

Within the past two years the discussion of the subjects named above has again broken into flame and a number of papers have appeared, all presenting most instructive facts and suggestions, but very diverse conclusions.[1] Whether the name Laramie shall be restricted to the lower of the three divisions found in the Denver basin and its equivalents elsewhere, as proposed by Cross and Eldridge, Knowlton and Peale; or to one or both of the upper divisions, as advocated by Veatch; or retained to designate all the formations between the Fox Hills and the Fort Union; or wholly abandoned, is yet to be settled; and with this I have nothing to do. It is the purpose of the present paper to show that those deposits that lie above the Fox Hills and are known to contain remains of dinosaurs: more specifically the Laramie, as understood by Cross and Eldridge; the Arapahoe and the Denver of Colorado; the Lance Creek, or "Ceratops," beds of Converse County, Wyoming; the Hell Creek beds of Montana; and the beds underlying the Puerco in New Mexico, ought not to be referred to the Tertiary, but to be retained in the Upper Cretaceous.

2. Necessity for Accurate Correlation of the Primary Divisions of the Geological Column in the Different Continents.

It appears to the writer that it is a matter of great importance that the primary divisions of geological time, the ages and the periods, and the corresponding systems of rocks of all parts of the world should as far as possible coincide. By this is meant that geologists should not (to employ an illustration) include in the Lower Cretaceous any deposits of one continent that were being formed synchronously with Jurassic deposits of another continent. Nor ought they to include in the Tertiary of America any formations that are the time equivalents of European Cretaceous formations. It may be a matter of great difficulty to attain agreement in some cases, but it ought to be resolutely striven after. And in this connection the writer indorses fully the quotation made by Mr. Cross (Proc. Wash. Acad. Sci., xi, p. 46) from Dr. C. A. White's address. It may be added that modification of the primary divisions ought to be made by international bodies of geologists and paleontologists.

The reasons why the primary divisions of geological history should be fixed as accurately as possible, even though arbitrarily, seem to be simple enough. Geology is the history of the development of the earth and its

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[1] Veatch, A. C., Amer. Jour. Sci. (4), xxiv, 1907, pp. 18-22.
Cross, Whitman, Proc. Washington Acad. Sci., xi, 1909, pp. 27-45.
Knowlton, F. H., Washington Acad. Sci., xi, 1909, pp. 179-238.
Stanton, T. W., Washington Acad. Sci., xi, 1909, pp. 239-293.
Peale, A. C., Amer. Jour. Sci. (4), xxviii, pp. 45-58.

inhabitants. For sufficient reasons we divide this history into principal and subordinate portions, each having its own characteristics. Each continent has had its own course of development, physical and biological, this course sometimes agreeing only in a general way with that of other continents, being perhaps ahead of them or behind them, possibly sometimes only different. In order to compare and describe contemporary conditions in different lands there must be a few fixed dates from which to reckon the march of time and progress. These dates are found in the limits between the primary divisions, as that between the Silurian and the Devonian or that between the Cretaceous and the Tertiary. In a similar way we orient the history of even a savage people with reference to such dates as the founding of Rome and the birth of Christ.

3. The Primary Divisions of Geological Time Are Not Usually Indicated by Great Unconformities.

Inasmuch as those geologists and paleontologists who favor the reference of the Arapahoe and the Denver beds of Colorado, the Lance Creek beds of Wyoming and the Hell Creek beds of Montana to the Eocene, give as their principal reason therefor the existence of a great unconformity between the Arapahoe and the formation immediately below it, while there appears to be no similar unconformity below the Fort Union, it may be worth while to examine the adequacy of the reason. I believe that it is fallacious.

It is possible that, as Chamberlin and Salisbury suggest in their general work on geology (Geology, iii, p. 192), there is a natural basis for the larger divisions of geological history; that this basis is to be found in the profounder changes in the earth's crust; and that this basis is of world-wide application. This suggestion may be accepted as valuable without its arousing the expectation that a great stratigraphical break will be discovered everywhere between each great rock system and its predecessor and its successor. As a matter of fact, as geological history is now understood and now divided, such breaks are not commonly found. I will quote from Geikie's Text-book of Geology, ed. 4, 1903, p. 1081:

> Though no geologist now admits the abrupt lines of division which were at one time believed to mark off the limits of geological systems and to bear witness to the great terrestial revolutions by which these systems were supposed to have been terminated, nevertheless the influence of the ideas which gave life to these banished beliefs is by no means extinct.

On page 981 the author quoted, in speaking of the Old Red Sandstone, says:

> * * * in innumerable sections where the lowest strata of the system are found graduating downward into the top of the Ludlow group; and where its highest beds are seen to pass up into the base of the Carboniferous system.

On page 982 one reads as follows:

> The rocks termed Lower Devonian may partly represent some of the later phases of Silurian life. On the other hand, the upper parts of the Devonian system might in several respects be claimed as fairly belonging to the Carboniferous system above.

As to the relation of the Lower Carboniferous to the Devonian, Geikie (Text-book, p. 1014) says:

> Both in Europe and America it may be seen passing down conformably into the Devonian and the Old Red Sandstone. So insensible indeed is the gradation in many consecutive sections where the two systems join each other that no sharp line can be drawn between them. The stratigraphical passage is likewise frequently associated with a corresponding commingling of organic remains.

Chamberlin and Salisbury (Geology, ii, p. 499) tell us that the transition from the Devonian to the Mississippian seems to have been accomplished without notable deformative movement. Also (p. 518) it is stated that the Devonian fauna passed by graduation into the Mississippian.

There exists in many places the same doubt regarding the boundary line between the Carboniferous and the Permian. Geikie (Text-book, p. 1064) states that in the Midlands and the west of England no satisfactory line can be drawn between the two systems; (p. 1065) that the flora of the older Permian rocks presents many points of resemblance to that of the Carboniferous; (p. 1063) that in North America no good line of subdivision exists between Permian and Carboniferous; so certain deposits are called Permo-Carboniferous; (p. 1077) that in Russia the Permian attains an enormous development, the horizontal strata nearly lying conformably on the Carboniferous.

Of the Permian of North America Chamberlin and Salisbury write (Geology, ii, p. 620):

> The upper Barren Measures are commonly separated from the Pennsylvanian on the basis of the plant species rather than because of any stratigraphic break at their base.

The Artinskian of Russia is placed in the Permian by Lapparent and Geikie, but in the Carboniferous by Tschernyschew. a distinguished Russian geologist.

Similar difficulties are encountered in various parts of the world by geologists when they attempt to draw the line between the Paleozoic and the Mesozoic systems. Chamberlin and Salisbury (Geology, ii, p. 631) have this to say:

The Permian system of Europe seems to be more closely allied, stratigraphically, with the Trias than with the Carboniferous, and while the same is true of the western part of America, the opposite is true for the eastern part.

We have the statement of Geikie (Text-book. p. 1084) that in some regions, as in England, no very satisfactory line of demarcation can always be drawn between Permian and Triassic rocks.

Nor are geologists free from embarrassments when they endeavor to classify the Mesozoic and the Tertiary formations. The Rhætic is arranged by Geikie in the Triassic, by Lapparent in the Jurassic. Clark and Bibbins express doubt regarding the position of the two lower divisions of the Potomac formation of the eastern United States. They refer them provisionally to the Jurassic; the other two divisions are unhesitatingly placed in the Lower Cretaceous. According to Chamberlin and Salisbury, the fossils of the Trinity division of the Comanchean system have raised the question of its reference to the Jurassic. An indefinite number of similar cases could be cited.

The illustrations presented show that the great divisions of geological record are not even commonly separated by physical breaks, great or small. It would be quite as easy to show that important unconformities occur within the limits of systems of rocks. A few cases only need be cited. The following is quoted from Geikie (Text-book, p. 1007):

The Old Red Sandstone of Britain, according to the author's researches, consists of two subdivisions, the lower of which passes down conformably into the Upper Silurian deposits, the upper shading off in the same manner into the base of the Carboniferous system, while they are separated from each other by an unconformability. * * * [In Scotland] it consists of two well-marked groups of strata, separated from each other by a strong unconformability and a complete break in the succession of organic remains.

Geikie states further (p. 1146) that a considerable stratigraphical and paleontological break is to be remarked at the line between the Portlandian and the Purbeckian. Chamberlin and Salisbury (Geology, ii, p. 639) tell us that the close of the Paleozoic was marked by much more considerable geographic changes than the close of any period since the Algonkian. The statement is qualified by the remark that these changes may be said to have been in progress during the Permian rather than to have occurred at its close.

4. The Principal Divisions of Geological History Are Based on Fossil Organisms.

It may therefore be confidently affirmed that the primary divisions of geological history, as this history is now understood, are not based on unconformities and deformations, great or small, between the successive formations, but they are based on the history of the plants and animals whose remains have become entombed in the rocks. I will here quote from Lapparent (Traité de Géologie, ed. 5, p. 717):

> Il résulte de ces diverses considérations que les seules ressources de la stratigraphie, si précieuses et si indispensables qu'elles puissent être, sont insuffisantes pour l'établissement des grandes divisions de la géologie. Il faut donc recourir à quelqu'argument d'une portée plus générale. Cet argument, nous allons le trouver dans la considération des faunes et des flores fossiles.

It must not be supposed that the writer wishes to underestimate the value to the geologist of changes in the materials that constitute successive beds, of deformations of surfaces, or of unconformities, erosional and angular. All these indicate the physical changes that the earth was undergoing and mark the subordinate and more or less local divisions of geological history. Naturally the geologist in the field searches for such interruptions in the course of deposition and, following a bent very human, he may come to attach somewhat undue importance to them. In any case, however, final recourse must be had to the fossils enclosed in the rocks. Fossils are, to use a figure, the sands that, from the hourglass of the universe, have in an uninterrupted stream dropped into the successive strata to mark the passage of time. Local interruptions of sedimentation enable us to note the changes undergone by the organisms that then existed; but whether there were breaks in deposition or not, the evolution of the organisms went steadily on. The smaller divisions of time are marked by the less important changes that the animals and plants suffered; while the primary divisions are signalized by the profounder modifications of the living beings. These primary divisions are often indicated by such phrases as the age of mollusks, the age of fishes, and the age of mammals. As there were no universal cataclysms that characterized the terminations of the ages and the eras, so there were no sudden changes in the nature of the animals and the plants. The boundaries between the successive ages and the successive eras must therefore be more or less arbitrarily drawn. If one era is characterized by numerous powerful reptiles and a few inconspicuous mammals, while the next era presents mammals as the dominant animals, the reptiles as decadent, we must draw the line to suit our convenience and to express best the facts; but in the end it will be drawn more or less arbitrarily.

To appreciate the futility of seeking for great unconformities between the rock systems one has only to consider the relations of the Upper Cretaceous to the Tertiary in Europe. Lyell regarded the Thanet sands and certain equivalents in France and Belgium as the base of the Eocene. Between this and the Upper Cretaceous there appeared to be one of the profoundest breaks in geological history. Lyell says that the interval between the Upper Cretaceous and the Eocene must have been greater than that between the Eocene and the present. More recent investigations have shown that even in the north of Europe there are deposits of no great thickness that partly fill the gap between the two systems; while it is almost filled in the south of that country.

The conclusion applicable to the question being considered which I reach is that the magnitude of the break below the Arapahoe formation in the Denver basin has little or nothing to do with the determination of the boundary line between the Mesozoic and the Cenozoic. The position of this line is to be settled through the study of the organic remains found below and above the unconformity and the comparison of these with the fossils found at corresponding levels in regions geologically better understood. If the ensemble of the organisms found in the Arapahoe, the Denver, the Lance Creek and the Hell Creek beds, is essentially of Upper Cretaceous nature, on comparison with accepted standards, those beds belong to the Mesozoic, not to the Cenozoic, notwithstanding the great unconformity.

As has already been said and is well known, the base of the Eocene was established just below the Thanetian of England and its continental equivalents; and this line of separation of the Cenozoic from the Mesozoic has been recognized by practically all geologists since Lyell's time. Considering the great gap between the two systems, as known in Europe at that time, the separation did not appear to be at all an arbitrary one. In his "Text-book of Geology," edition of 1896, Geikie placed the Montian in the Eocene, but in the edition of 1903 this formation is restored to the Upper Cretaceous. Lapparent, too, draws the line above the Montian. Nor does this manner of division appear to arouse objections on the part of the paleontologists.

If, therefore, American geologists and paleontologists wish to have the boundary line between the Mesozoic and the Cenozoic of their country coincide with that of Europe, the type continent of the base of the Eocene, it will be necessary, unless there are compelling reasons for the contrary, to make the base of our Eocene the equivalent of the Thanetian of Europe. I believe that geologists and paleontologists generally will give assent to this proposition.

It is well understood that in the determination of the level of any geological formation not all kinds of fossils are of equal value; some are indeed of little value. It is agreed that the marine animals record most accurately the progress of geological time, because of their abundance, their wide distribution, the slow and steady changes which they undergo during geological periods, and the facility with which they become entombed in accumulating sediments. Furthermore, of marine species the pelagic forms are of greater value, because their remains are dropped indiscriminately into deposits of all kinds, thus enabling geologists to correlate formations widely separated and composed of very different materials. Terrestrial animals are of less value. They are subject to rapid and extreme changes in their environment through changes in climate and through sudden migrations. They suffer accordingly rapid modifications in their structure or sudden extinction. They are also less likely to be preserved in the rocks. Every shell in an oyster bed may be preserved, while from a million horses but a single tooth may escape destruction. In an interesting address at the meeting of the British Association at Montreal, in 1884, Blanford gave it as his opinion that determinations of geological age based on terrestrial and freshwater faunas and floras only are extremely likely to be incorrect.

Unfortunately for us, the deposits in which we are now especially interested contain few or no marine organisms, but abundant freshwater and terrestrial animals and numerous plants. We must therefore reach our conclusions by somewhat indirect methods and must be on our guard against errors. Still more unfortunately for us, the paleozoologists and the paleobotanists have not attained the same results from their studies.

5. The Value of Plants as Indices of Geological Dates.

I trust that the paleobotanists will not charge me with trying to disparage their science when I proceed to show that, in the present case at least, their results are less to be depended on than those obtained by the paleozoologists. Without doubt, the plants have as interesting, as trustworthy, and as valuable a story to tell, when rightly deciphered, as do the animals. It seems, however, that in some cases, other than the one before us, the significance of fossil plants has not been rightly comprehended. In Blanford's address, cited above, he mentions two important cases in which the determination of the age of certain formations have contradicted those made from the marine animals. One case is found in the Gondwana system of India, where, as Blanford says, "we have a Rhætic flora overlying a Jurassic flora and a Triassic fauna above both." Again he states that "in Australia we find a Jurassic flora associated with a Carboniferous marine fauna and overlain by a Permian freshwater fauna."

The following is quoted from Lapparent (Traité, p. 718):

> A plus d'une reprise, l'étude des flores terrestres a paru donner des indications contradictoires avec celles des faunes marines; et en dernière analyse la question a toujours été tranchée en faveur de ces dernières.

Geikie makes the following observation:

> Certainly a number of instances are known where an older type of marine fauna is associated with a younger type of flora.

One reason why plants, at least those of the northern hemisphere, which have existed since the beginning of the Upper Cretaceous, seem to be of only secondary value in correlating formations is found in their apparently extreme conservatism. While the species have changed, the genera have changed little. As an illustration of this, one may take the list of plants published by Doctor Knowlton (Wash. Acad. Sci., xi, 1909,

p. 219) as occurring in what he is pleased to call the Lower Fort Union, but which includes the Lance Creek and Hell Creek beds and their supposed equivalents. One might almost imagine it to be a list of plants found in a recently investigated corner of the world on the latitude of Louisiana. On page 225 it is stated that a number of species are yet living, while others are so obviously close to living species as to be separated with difficulty. Such inert organisms, subject also to all the vicissitudes of life on the land, can hardly be regarded as good indicators of the passage of time. Since that epoch the genera, families, and even orders of warm-blooded vertebrates have almost completely changed.

The opinion held by some distinguished geologists and paleontologists that the so-called Laramie beds, or all of these except the lowest, belong to the Tertiary appears to have rested until recently, at least, mostly on the statements of Professor Leo Lesquereux, the paleontologist of the Hayden Survey. He and Dr. Hayden at first regarded these deposits as belonging to the Miocene, but later as belonging to the lowermost Eocene. Passing over Lesquereux's earlier writings I refer to one of his latest utterances on the subject, found in the eighth volume of the monographs of the Geological Survey of the Territories, part three, published in 1883. On page 109 Lesquereux makes this statement:

> The flora of the Laramie group has a relation, remarkably defined, with that of Sézanne.

Now, the flora of Sézanne, a town in France, comes from beds that belong to the Thanetian, at the very base of the Lower Eocene. Lesquereux's statement is followed by a table of the species which he supposed had been found in the Laramie at various localities. The beds at some of these localities are now known to be somewhat older than any Laramie, those at one or two localities a little younger than Laramie. In the table is a column in which are checked off the species of Laramie plants that Lesquereux believed to be identical with or closely related to species found at Sézanne; in another column the species that he supposed were found also in the Oligocene of Europe; in a third column those that he believed to occur also in European Miocene deposits. Naturally, one would expect, in view of Lesquereux's statement quoted above, that the identical and closely related species of the Sézanne column would outnumber those of the Miocene column. On the contrary, only three species were regarded by him as identical with Sézanne species, while twenty-seven species are recorded as identical with European Miocene species. If we count in each case the plants that were supposed to be closely related to the European species. but not identical. we find twenty-five in the Sézanne column and thirty three in the Miocene column. Adding the identical and the related species in each case it is seen that there are in the Sézanne column twenty-eight species. sixty in the Miocene column. Therefore, it becomes difficult to understand how Professor Lesquereux derived his conclusion from his premises. What his table really proved was that the Laramie deposits belong to the Miocene. Had Cope and other paleontologists examined the table itself. instead of accepting the author's statement regarding it. they would either have distrusted the evidence from the plants more than they did or would have concluded that the dinosaurs ranged up into the Miocene.

It is not to be supposed that all paleobotanists accepted Lesquereux's views. These views were strongly opposed. especially by Newberry. as early as 1874 and as late as 1889. The following is quoted from Newberry (Trans. N. Y., Acad. Sci.. ix, 1889, p. 28) :

> If Prof. Cope had not accepted Mr. Lesquereux's conclusion in regard to the age of the deposit [at Black Buttes], and had recognized the fact that there are no Tertiary plants in the true Laramie, he would have seen that there is no discrepancy between the testimony of the plant and animal remains.

It is to be taken into consideration here that Newberry believed that the Laramie was directly overlain by the Fort Union. The latter beds have usually been regarded as belonging to the Eocene. However. the following may be quoted from Lester F. Ward. who had studied especially collections of plants from the Fort Union deposits (Bull. Geol. Soc. Amer.. i, 1890, p. 531) :

> In fact. the material from the Fort Union formation which is still in my hands inclines me to believe that there would really be. as I then stated. no inconsistency in assigning to the Fort Union an age as ancient as the closing period of the Cretaceous system.

6. The Completeness of Record of Animal Life as Compared With That of Plant Life.

There is, in the present state of knowledge. a great contrast between the incompleteness of the plant record above the Fox Hills formation and the fullness of the animal record. Plants are abundant throughout the series that has been called Laramie and in the Fort Union. Again, they are found in the Green River beds, in the White River beds, and in the deposits at Florissant, Colorado. Otherwise, the record is mostly missing. On the other hand, the history of the vertebrates is quite full. Between the Fox Hills and the present time there are known probably nearly twenty distinct faunas and it has been found possible to correlate these in most cases closely with European faunas. With such a series at command, the extremes of which differ enormously, while the mean terms sometimes grade into their successors, at other times differ greatly from the next comers, the paleontologist need not go far astray in determining the proper level of each fossil-bearing deposit. It may be remarked that when the paleobotanist refers the Green River beds to the Oligocene, while the vertebrate paleontologists put them at the bottom of the middle Eocene, a serious dislocation of views is indicated.

7. The Beginning of the Eocene in Europe and America.

When one comes to correlate formations in America with those of distant countries great difficulties are likely to be experienced. Interruptions in stratification are not likely to occur at the same time in America and Europe and Asia. On account of differences in the character of the deposited materials, the climate, the interposition of barriers, and other features of environment, the contained organisms must differ to a greater or less extent. In the case of the beds about which exists our dispute, they are neither of marine origin nor in contact with strata of purely marine origin. Hence they cannot be compared directly with either the typical uppermost Cretaceous deposits of Europe, the Danian, nor with the Thanetian, the lowermost European Eocene. The Lance Creek beds, the Hell Creek beds, and others related to them have been produced mostly through the action of fresh waters and they contain remains of land plants, freshwater mollusks and fishes, reptiles inhabiting the water and the land, and a few terrestrial mammals. In such a situation we must have recourse to indirect means of correlation.

In the vicinity of Rheims, France, in deposits belonging to the Thanetian, there has been found a considerable number of genera and species of extinct mammals, together with some birds, reptiles, and fishes. The mammals have been studied and described by Lemoine. On the strength of this fauna these Cernaysian beds were correlated with the Puerco at a time when this term was applied to beds now separated and known as Puerco and Torrejon. There is thus furnished a means of beginning a correlation of our land and freshwater Tertiary deposits with those of Europe; but we need ever to keep in mind the possibilities of error.

I believe that any one who may carefully compare the Cernaysian fauna with the faunas of our Puerco and Torrejon must conclude that the Cernaysian corresponds more closely with that of our Torrejon than with that of the older Puerco. I find that Osborn had reached this conclusion in 1900 (Ann. N. Y. Acad. Sci., xiii, pp. 9, 10); and in his latest matter on the subject he correlates the Torrejon with the Thanetian, or Cernaysian (Bull. 361, U. S. Geol. Surv., p. 34). Indeed, it seems not improbable that the Cernaysian is a little more recent even than our Torrejon.

It has been demonstrated that at least a part of the Fort Union formation is the equivalent of the Torrejon. Hence, wherever the latter is put the Fort Union or some part of it must go. The base of the Tertiary being drawn in Europe at the bottom of the Thanetian, there appears to be no good reason why in our country it should not be drawn above the Puerco, possibly above the Torrejon and the Fort Union. Certainly, when geologists and vertebrate paleontologists have consented to include the Puerco and the Torrejon in the Eocene they have lowered the base of the latter formation to its extreme level. To include now in the Eocene the "Ceratops" beds, the Hell Creek beds, the Arapahoe and the Denver, would be to add to it some hundreds of feet of deposits which, in the opinion of vertebrate paleontologists, contains a considerably older fauna than that occurring in the Cernaysian beds, and which with equal confidence the invertebrate paleontologists refer to the Cretaceous.

8. Relationship of Fauna of Lance Creek Epoch to Those of Puerco and Torrejon.

Inasmuch as those geologists and paleobotanists who favor the transference of a large part of the Laramie (as formerly understood) to the Tertiary insist that the fauna of the Lance Creek and the Hell Creek beds is more closely related to that of the Puerco and that of the Torrejon than to any Cretaceous fauna, this question must be considered. With regard to the relationships of the mammals of the Lance Creek beds to those of the Puerco and Torrejon extremely diverse views have been expressed. Marsh (Amer. Jour. Sci., xliii, 1892, pp. 250, 251) says that the mammals of the Lance Creek deposits are not transitional between the Mesozoic and Tertiary forms, but their affinities are with the former beyond a doubt; thus indicating a great faunal break. * * * and the great break is between this horizon [the Peurco] and the Ceratops beds of the Laramie. * * * It is safe to say that the faunal break as now known between the Laramie and the lower Wasatch [Puerco] is far more profound than would be the case if the entire Jurassic and the Cretaceous below the Laramie were wanting.

Cope (Amer. Naturalist, xxvi, 1892, p. 762), quoting from Marsh the words "the more the two [Laramie and Puerco] are compared the stronger the contrast between", adds:

It is true that no Ungulata have yet been found in the Laramie, while they abound in the Puerco, but we cannot be sure that they will not yet be found; the probabilities are that they existed during the Laramie and that it is due to accident that they have not been obtained. But the Multituberculata of the two faunæ are much alike.

Osborn (Bull. Amer. Mus. Nat. Hist., v., 1893, p. 311) writes:

This Laramie fauna is widely separated from the Upper Jurassic, and is more nearly parallel with the basal Eocene forms of the Puerco and the Cernaysian of France. * * * These conclusions are directly the reverse of those expressed by Marsh in his three papers upon this fauna.

Cross (Geology of the Denver Basin, p. 220) concludes that this difference of opinion deprives the mammalian remains of much of their value in the present discussion.

To the present writer Marsh's opinion seems to be erroneous. Geologically, of course, the Jurassic mammals are much farther removed from those of the Lance Creek beds than the latter are from those of the Puerco, Torrejon, and Fort Union. The same remark may justly be made regarding the stage of development attained by the Jurassic mammals. Systematically considered, the case is different; and the solution of the problem depends on the systematic relationships of the Jurassic mammals to those of the Lance Creek beds and of the latter to the mammals of the Puerco and Torrejon. If it shall result that all, or nearly all, of the Lance Creek mammals belonged to the Marsupialia and the Monotremata, then Marsh's opinion will be in great measure justified. If, on the other hand, it shall be shown hereafter that a large number of the Lance Creek mammals were placentals and the near-by ancestors of the Puerco and Torrejon faunas the break between the former and the latter will not be a profound one; nevertheless more important than formerly supposed by Osborn.

It must be understood that our knowledge of the mammals of the Lance Creek and related formations is of a very unsatisfactory kind. With few exceptions, all that is known of these animals has been derived from their teeth, not found in place in the jaws, but scattered singly through the rocks. Better known are the Jurassic mammals, for of these many jaws have been secured. Recently considerable light has been thrown on the marsupials of the Lance Creek and Fort Union formations through the discovery of the skull and some parts of the skeleton of *Ptilodus* (Gidley, Proc. U. S. Nat. Mus., xxxvi, p. 611). The other genera await elucidation. Osborn's statement of the situation may be accepted (Evolution of the mammalian molars, 1907, p. 95):

> It is possible that, besides Marsupials, we find here Insectivores, primitive Carnivores, and the ancestors of ancient Ungulates; but it is obvious that the determination of relationships from such isolated materials is a very difficult and hazardous matter.

Notwithstanding this appreciation of the situation, Professor Osborn has ventured (op. cit., pp. 12, 22, 115) to refer his Trituberculata, Marsh's Pantotheria, to the infraclass Placentalia. No adverse criticism can be made on this procedure, in case its tentative character is understood.

Now, while this uncertainty reigns regarding the systematic relationships of the mammals of the Lance Creek and related deposits, the case is different as soon as attention is given to the mammals of the Puerco, Torrejon, and Fort Union. Some of them betray by their tooth succession and other characters that they are true placentals. Many of them may be referred with confidence to orders and families that continued long afterwards, some of them probably to the present day.

That a considerable gap existed between the mammals of the Lance Creek formation and those of the Puerco and Torrejon is evident from the state of development of the teeth. Osborn, speaking of the teeth of the Upper Cretaceous mammals [Lance Creek] says (Bull. Amer. Mus. Nat. Hist., v., 1893, p. 321) that in none of the molars hitherto described and in none of his collection of about 400 teeth and some jaws was there any trace of the hypocone, or posterior internal tubercle. Nor was any hypocone recognized in the genera described by him in 1898 (Bull. Amer. Mus., Nat. Hist., x, p. 171). Undoubtedly, however, the hypocone is sometimes present in a rather rudimentary condition, as I have observed in teeth shown me by Mr. Gidley, of the U. S. National Museum. Nevertheless, the teeth of all the mammals of the Lance Creek stage, except those of the Allotheria, are triangular, showing that the possessors were either insectivorous or flesh-eating in their habits.

On the other hand, there are several genera of Puerco mammals that possess a well developed hypocone and internal cingulum. In some cases, where the hypocone had no great development, the hinder internal part of the tooth had swollen so as to reduce much the gap between the successive teeth and produce a broad triturating surface. In *Polymastodon*, which must have been a vegetarian, an extensive triturating surface was secured in another way. It presents a great advance over the teeth of any of the Lance Creek Allotheria. If it is considered how slowly changes in tooth structure had advanced during the Mesozoic era we must conclude either that a considerable interval had elapsed between the Lance Creek epoch and that of the Puerco or that the animals of the latter were not descendants of the former.

There are important differences between the mammals of the Lance Creek beds and those of the Puerco as regards the size attained. Most of the former are of insignificant proportions, resembling in this respect those of the Jurassic; while many of those of the Puerco are large. Furthermore, there was in the mammals of the Puerco a far greater variety of form, structure, and systematic relationships than among those of the Lance Creek mammals. Of the latter, there have been described about twenty-five genera and about forty-five species, most of them by Marsh. Osborn has regarded himself as justified in reducing these to about ten genera, these representing a very few families. From the Puerco Matthew (Bull. 361, U. S. Geol. Surv., 1909, p. 91) recognizes twenty-nine species, belonging to eighteen genera and nine families. To what extent this increased diversification of the mammalian life of the Puerco is due to immigration we can not now tell; but it does not seem to be necessary to assume that it was due to invasion of mammals from some other region. For, in view of the interval between the two formations that is indicated by the plants and reptiles, it is possible that the Puerco mammals are the direct descendants of those of the Lance Creek epoch.

In case there was no serious interruption in deposition between the Lance Creek beds and the Puerco and Fort Union, one might expect to find close relationships between the reptiles of the two levels. Crocodiles are not abundant in either and, so far as known, no species passes from the one formation to the other. *Champsosaurus*, belonging to another order, is found in the beds of the Lance Creek region and at Hell Creek and also in the Puerco; but probably no species is common to the lower and the upper levels. This genus, like *Ptilodus*, serves to show that, though there may have been a considerable interval between the Lance Creek and the Puerco, it was not an enormous one. The dinosaurs, which were such a conspicuous feature of the Lance Creek epoch, appear to have disappeared completely before the time of the Puerco and Fort Union. Of turtles, some families passed from the one formation to the other, but probably no species. A pleurodire, representing a large group of turtles found now mostly south of the equator, was present in the "Laramie" of New Mexico; but no member of the group is known to have existed in North America after that time. Certain other genera of turtles (*Adocus*, *Eubaëna*, *Thescelus*, *Basilemys*, *Helopanoplia*) are not known to have passed from the Lance Creek level into that of the Puerco and Fort Union; and other genera *(Alamosemys, Hoplochelys, Conchochelys, Amyda?)* appear to have had their beginning in the Puerco. It may further be said that, while turtles were very abundant in the Lance Creek epoch, they appear to have been very rare in the Fort Union, though of more frequent occurrence in the Puerco.

As regards the mollusks I find this statement made by Doctor Stanton (Wash. Acad. Sci., xi, p. 264), where he is speaking of a Fort Union locality in Montana:

> The Unios are all of simple type and do not include any of the peculiarly sculptured forms like those of Hell Creek, Converse County, and Black Buttes.

The plants, conservative as they are, testify even more strongly than do the animals to a considerable interval between the Lance Creek epoch and the Fort Union. According to Doctor Knowlton (Wash. Acad. Sci., xi, p. 221), out of 84 identified species found in the Lance Creek epoch ("Lower Fort Union") 68 occur in the Fort Union. Hence 16 species, nearly 20 per cent, appear to have failed to reach the higher beds. It is to be noted here that about 300 plants are known from the Fort Union and only about 200 from the Lance Creek beds. For a group of organisms that even then contained a considerable number of species yet existing, or very close to forms yet existing, the loss of a fifth of their forces, at a time when there appears to have been little change of climate, indicates the lapse of an important interval.

The base of the Eocene is usually regarded as containing a small per cent of the marine mollusks yet living; the beginning of the Miocene, about 17 per cent of yet existing species; and the beginning of the Pliocene about 36 per cent. If now plants have changed in species during the lapse of geological time with about the rapidity that marine mollusks have changed, the Fort Union beds ought to be arranged in the Lower Miocene. This would harmonize quite well with the idea that the Green River beds belong to the Oligocene.

9. Relationship of Lance Creek Fauna to That of the Judith River Epoch.

Having demonstrated, as I think I have, that there was, between the time of the deposition of the Lance Creek beds and those known as Puerco and Fort Union, a nearly complete change in the fauna and a considerable change in the flora, I will endeavor to show that the fauna of the former beds is closely related to that of the Judith River, a formation now recognized as being well down in the Upper Cretaceous and separated from the lowermost Laramie by about 1,000 feet of marine Cretaceous strata (Stanton, Wash. Acad. Sci., xi, p. 256). This close relationship of the two faunas has been recognized, it may be truthfully said, by all paleontologists who have given attention to the subject. For a long time it misled geologists and paleontologists into the conclusion that all the deposits in question belonged to a single epoch. Mr. J. B. Hatcher, who had collected extensively both in the Judith River region and in the Lance Creek beds, and who had studied closely the vertebrates of both regions, writes (Bull. U. S. Geol. Surv., 257, p. 101):

When considered in its entirety, the vertebrate fauna of these beds [Judith River] is remarkably similar to, though distinctly more primitive than, that of the Laramie [Lance Creek beds]. Almost or quite all of the types of vertebrates are present, though, as a rule, they are represented by smaller and more primitive forms.

Doctor T. W. Stanton, paleontologist of the U. S. Geological Survey, who examined in company with Professor Hatcher the Judith River basin, and who has given especial attention to the invertebrate fauna, records in the same bulletin (p. 121) his opinion:

When full collections are compared it will usually be easy to distinguish between Judith River and Laramie from the brackish-water fossils alone, but if the collections are meager and fragmentary it may not be practicable to do so. * * * Taken as a whole, the fresh-water faunas of the Judith River and the Laramie are somewhat more distinct than the brackish-water faunas of the same formations, and with fairly complete collections it should not be very difficult to distinguish them in the laboratory.

When we come to compare the vertebrates of the Judith River beds with those of the Lance Creek deposits it becomes necessary practically to ignore the mammals, inasmuch as only two species of these have up to this time been discovered in the Judith River. These are *Ptilodus primævus* and *Borodon matutinus*, both described by Lambe from the Belly River beds of British America. The former of these fossils is related to species of the same genus found in the Lance Creek beds and in the Torrejon, the latter genus is of undetermined relationship.

Fishes.—Beginning with the fishes, there have been described from the Judith River beds eight species. In the Lance Creek beds, Converse County, Wyoming, Professor Williston (Science, xvi, 1902, p. 952) found materials which he refers to two of these species (*Myledaphus bipartitus*, *Lepisosteus occidentalis*). One of these fishes, *Myledaphus bipartitus*, seems to be a ray. The rays are almost wholly inhabitants of salt water; hence the persistence of this Judith River freshwater form is somewhat remarkable. A supposed sturgeon, *Acipenser albertensis*, found by Lambe in the Belly River beds, occurs, according to Williston, in the Lance Creek beds. From the Belly River beds Mr. Lambe described a remarkable species of fish which he called *Diphyodus*. Hatcher states that similar jaws are common both in the Judith River beds of Montana and in the deposits of Converse County, Wyoming. From the Hell Creek beds of Wyoming Mr. Barnum Brown has reported the discovery of another species of the same genus.

Tailed Amphibians.—Of the tailed amphibians, at all times rare fossils, Cope described from the Judith River region four species, all members of the genus *Scapherpeton*. Lambe believes that he has found one of these in the Belly River beds, a fact that shows the somewhat extended distribution of the genus at that epoch. Williston found one of the species in the Lance Creek beds and Brown reported a species from the Hell Creek deposits. While it is true that these fishes and amphibians are mostly represented by fragmentary remains, these remains are usually characteristic and capable of accurate comparison. That *Myledaphus* should reappear after an interval allowing the deposition of 1,000 feet of marine strata and probably some hundreds of feet of freshwater strata, is remarkable enough; but that it should reappear in company with its old companions, the rare *Diphyodus* and *Scapherpeton*, not to mention the more highly developed fauna yet to be discussed, is very striking. Had there occurred at both levels only some pebbles of three peculiar forms or compositions, instead of the three genera, the conclusion would have been inevitable that there was some particular connection between the two formations.

Champsosaurus. Crocodiles.—Coming next to the reptiles, it may first be noted that species of *Champsosaurus* occur in the Judith River beds, in the Lance Creek beds, in those of the Hell Creek region, and in the Puerco. It is probable that the species vary from one formation to the other. The same statement can probably be made regarding the crocodiles. These genera, common to all three of the formations under discussion, may be left out of consideration; although it must not be overlooked that none the less they aid in binding together the formations in which they are found. As to the crocodiles, it may be mentioned that Williston recognized, in teeth and scutes found in the Lance Creek beds, Leidy's *Crocodylus humilis*, originally described from the Judith River region. From the Judith River beds of Alberta Lambe described *Leidyosuchus canadensis*. Mr. C. W. Gilmore will soon describe a second species of the genus, collected last summer in the Lance Creek beds of Converse County, Wyoming.

Turtles.—As regards the turtles, certain genera have already been mentioned as appearing not to pass the line between the Lance Creek formation and the Puerco and Fort Union. My study of the fossil turtles indicates that the species of these animals rarely pass from one epoch to another. If they have ever done so they passed from the Judith River into the Lance Creek epoch. There are five or six species of Judith River turtles which are represented in the Lance Creek and Hell Creek beds by turtles of identical or very closely related species. Most of these are marked by such peculiar sculpture that they are easily recognized and some of them likewise are represented by excellent materials. I shall take the pains to give some details.

Compsemys? obscura Leidy was originally described from beds probably belonging to the Lance Creek epoch and found at Long Lake, N. Dakota. Not much of it is known, but the sculpture is distinctive.

It was included by Cope in his list of Judith River vertebrates. Barnum Brown found what appears to be the same species in the Hell Creek beds.

Compsemys victa Leidy was described from the beds of Long Lake. Its sculpture is characteristic, resembling small, closely placed, pustules, that cover all parts of the shell, and appearing in no other turtles. It is fragmentary, but very common in the Lance Creek beds. Barnum Brown has collected it in the Hell Creek deposits. Cope included it in his list of Judith River vertebrates. He also found it in Colorado, in deposits that belong to either the Arapahoe or the Denver. I am able to say that the same genus is represented by an undescribed species in the Fort Union.

Aspideretes foveatus (Leidy) was described from the Judith River basin. Leidy had other specimens from Long Lake, N. Dakota. There are many fragments of the species in a collection made in the Judith Basin for Cope by Charles Sternberg. A nearly complete carapace was found in the Belly River beds by Lambe. Fragments indistinguishable from the type were secured by Barnum Brown in the Hell Creek region. The carapace is ornamented by a characteristic pitting.

Aspideretes beecheri Hay has for its type a specimen in Yale University which lacks little more than the head and a part of the neck. Mr. Hatcher collected in the Judith River beds two quite complete carapaces which I have examined, without being able to distinguish them from the type of *A. beecheri.*

Adocus lineolatus Cope is a turtle that is not well known, but fragments of what appear to be the same species are not uncommon. The sculpturing is peculiar. The type was found in Colorado, in probably the Arapahoe formation. Cope included it among the vertebrates of the Judith basin, and Lambe reported it from Belly River deposits in Alberta. Barnum Brown found in the Hell Creek beds what seems to be the same species.

The genus *Basilemys* is represented by turtles of large size and an extraordinary form of sculpture. The type *B. variolosa* (Cope) has as its type a large part of the plastron and considerable parts of the carapace. This type was found in the Judith River basin. Members of the Canadian Geological Survey found good specimens of the species in the Belly River beds in British America. A second species of the genus has been discovered in beds of the Lance Creek epoch, in Custer County, Montana. The type is a complete shell. Had only fragments been found that did not include distinctive parts, this specimen would have been regarded as belonging to *B. variolosa*. A species not certainly identified occurs in the Hell Creek beds. During the past season an undescribed, closely related species was discovered in the Lance Creek deposits in Converse County, Wyoming, by a member of the U. S. Geological Survey. Nothing resembling these turtles has ever been found in beds above those equivalent to the Lance Creek deposits. Indeed, all those turtles of the Upper Cretaceous which had the carapace and plastron sculptured in various ways, appear to have become extinct before the beginning of the Tertiary. Not long after the opening of the Tertiary, in the Wasatch, there came in the Emydidæ and the Testudinidæ, and these developed other styles of ornamentation of the shell.

Figures of all the species of turtles named above are to be found in the present writer's "Fossil Turtles of North America."

Dinosaurs.—Both in the Judith River beds and in those of the Lance Creek epoch the most abundant and the most conspicuous reptiles are the dinosaurs. Five families of these, belonging to four superfamilies and to two suborders, are represented in the Judith River epoch, and each of these families reappears in the Lance Creek epoch. Furthermore, many of the genera are common to the two formations and it is believed that the same is true of a considerable number of species. From the Judith River beds Cope described eight species of carnivorous dinosaurs that seem to come under the genus *Dryptosaurus*. Mr. Hatcher (Bull. U. S. Geol. Surv., 257, p. 86) mentions the occurrence of two of these, called by him *Deinodon explanatus* and *D. hazenianus*, in the Lance Creek beds. Another carnivorous dinosaur, *Deinodon horridus*, was originally described from the Judith River beds. Hatcher (loc. cit., p. 83, *Aublysodon mirandus*) believed that it was found likewise in the Lance Creek beds. Another, *Zapsalis abradens*, is thought (p. 84) to occur in both formations. The great carnivorous dinosaur described by Osborn, *Tyrannosaurus rex*, may be a descendant of Marsh's *Ornithomimus grandis*, of the Eagle formation, older still than the Judith beds.

In the herbivorous order Orthopoda are placed the remarkable reptiles called the Stegosauria. Two species, *Troodon formosus* and *Palæoscincus costatus*, are mentioned by Hatcher (loc. cit., pp. 83, 88) as being represented in the Lance Creek deposits by numerous teeth of size and pattern similar to the types, which were described from the Judith River formation. In addition to these. Barnum Brown has described from the Hell Creek beds a large stegosaur, *Ankylosaurus magniventris*, the type of a new family. We can not doubt that some day a closely related form will be discovered in the Judith River beds; and indeed, its immediate ancestor may be Lambe's *Stereocephalus tutus*. from the Belly River deposits.

The large herbivorous dinosaurs, the Hadrosauridæ. which were accustomed to walk about on their hinder limbs only, are, according to Cope's identifications, represented in the Judith River formation by about nine species. The Lance Creek and the Hell Creek beds furnish three or four species of the family, most of which are referred to the genus *Hadrosaurus*. or *Trachodon*. Whether or not there are species common to the two formations cannot now be definitely determined; but certainly their relationships are very close.

Of all the dinosaurs that are found in the formations in which our interest is now centered the Ceratopsia have received the most careful study. What the present state of knowledge is with regard to these remarkable reptiles, may be learned from Hatcher's monograph of the group, completed and edited by Dr. Lull (Mon. 49. U. S. Geol. Surv.). Unfortunately much needs yet to be learned about them, especially about those of the Judith River forms. Approximately nine species are known from the Judith River deposits of Montana and British America; and about fifteen species are credited to the Lance Creek beds, of Wyoming, and to the Arapahoe and the Denver, of Colorado. Hatcher and Lull conclude that those of the Judith epoch are somewhat more primitive than those of the beds higher up, being somewhat smaller, with a less completely developed nuchal frill, with the nasal horn relatively larger and the supraorbital horns relatively smaller than in the younger forms. It is, however, to be noted that the nasal horn of *Ceratops*, of the Judith River epoch, is not yet certainly known. For the most part the genera are based on the characters mentioned above. They may have the importance assigned to them, but they do not indicate radical differences. Such differences might easily have arisen during an interval of moderate duration. There can be no doubt that the Ceratopsia of the higher beds were derived directly from those of the lower.

The possibility may be fully granted that further investigations may prove that few or no species of vertebrates continued from the Judith

River epoch to that which witnessed the deposition of the Lance Creek and Hell Creek beds. Nevertheless, nothing can impair the force of the evidence that many species included among the fishes, the tailed amphibians, the turtles, the crocodiles, the champsosaurians, and the carnivorous and herbivorous dinosaurs are represented in both formations by closely related forms. The remarkable thing about the matter is that the faunas of the two formations, separated by so great a thickness of strata, should be so similar. We must conclude that deposition went on rapidly in that interval, so that it may not have been so long as otherwise might appear. There could hardly have been movements of the land in that region that produced any considerable changes of climate. During the Bearpaw epoch the sea probably quietly invaded a part of the territory that had previously been occupied by the Judith River animals; but around the border of this invading sea the turtles, the crocodiles, and the many genera of the dinosaurs continued their existence and their evolution undisturbed until that sea retired. And doubtless had all those animals in that region been destroyed there was an extensive territory, nearly the whole of North America as far as the Atlantic, that harbored similar forms, from which territory new recruits could swarm in. As far away as New Jersey there were living herbivorous and carnivorous dinosaurs not greatly different from those of the Judith River beds. This appears to be true, that whatever happened to the plants between the time of the Judith River and the Lance Creek beds, nothing of serious importance happened to the animals.

By those who insist on elevating the deposits of the Lance Creek epoch into the Tertiary, a persistent effort has been made to minimize or nullify the significance of the presence of dinosaurs. As long ago as 1880 Heer wrote thus (Arctic Flora, vol. 6, pt. 2, p. 7):

> Der *Agathaumas* von Black Buttes beweist daher keineswegs, dass dort eine Tertiär-Flora zu gleicher Zeit mit einer Kreide-Fauna gelebt habe, wie Prof. Cope dies behauptet, denn ein einzelnes Thier macht so wenig eine Fauna aus, als eine Pflanzenart eine Flora. Wir können daher Hrn. King nicht beistimmen, wenn er, mit Cope und Marsh, die Laramie-Gruppe zur Kreide bringt.

Mr. Cross and Dr. Knowlton have argued that the dinosaurs might have continued on into the Eocene, and in fact did so. As to the vertebrate paleontologists, it is not probable that any of them would have asserted that this was impossible and some of them have granted the possibility. In holding that the dinosaur beds belong to the Mesozoic, they have reasoned that, inasmuch as these animals are characteristic of the Mesozoic and are not known to occur in the Tertiary of any other region, they probably did not exist during any part of the Tertiary of this country. And certainly, there is a mass of confirmatory evidence for this conclusion. The plants have appeared to furnish evidence against it; but, in view of the discrepancy between Lesquereux's conclusion and his premises, it seems that the paleozoologists were justified in their conservatism.

Mr. Cross writes (Mon. U. S. Geol. Surv., xxvii, p. 251) :

> If the dinosaurs of the Ceratops fauna did actually live in the Laramie epoch of Colorado they survived a great orographic movement and its accompanying climatic changes, and continued through the Arapahoe and Denver epochs so little modified that Professor Marsh has not detected any changes corresponding to the stratigraphic time divisions.

Since this was written it has been found that the Judith River beds, which contain so many dinosaurs, were deposited long before the time of the Laramie. We thus have proof that these dinosaurs and many other forms of vertebrates survived, without important changes, the orographic movement mentioned by Mr. Cross. It seems probable, therefore, that this movement was not so widely extended and so long continued as has been supposed. Why the dinosaurs died out finally we do not know, any more than we know why numerous other vigorous races of animals have perished from the earth. That the causes were not local is shown by the fact that in Europe likewise they became extinct just before the appearance of the Cernaysian fauna. It may be regarded as very reprehensible in them that they thus permitted themselves to perish before the Eocene came on, but we are compelled to believe the record.

In the preceding pages I have endeavored to show that the deposits of the Lance Creek epoch are well separated from those of the Fort Union, as indicated by both the fauna and the flora. In case a biological break is required between the Cretaceous and the Tertiary such a break seems to be present here. The stratigraphical break appears to be less conspicuous; yet unconformities are not absent and the character of the deposits appears to be such that there is seldom difficulty in separating the one formation from the other. Nevertheless, it seems that accurate correlation demands that the line between the Mesozoic and the Cenozoic in that region ought to be drawn at least above the Puerco and probably through or above both the Torrejon and the Fort Union. The exact position of the parting must be settled after further investigations.

10. Conclusions.

1. The answer that the writer would give to the question at the head of this paper is that the Lance Creek beds belong to the Upper Cretaceous.

2. In the Upper Cretaceous ought to be included also the Puerco and not improbably also the Torrejon and the Fort Union.

3. In case of a conflict between the evidence furnished by the flora and the fauna of the Lance Creek beds and those of the Fort Union respectively, the evidence obtained from the faunas is to be preferred, as being part of a more complete and better understood history. Present knowledge regarding plants seems to indicate that they were precocious, having reached something like their present stage of development long before the mammals attained anything like their present stage of differentiation. There are also indications that the floras of the western world were, during the Cretaceous, considerably in advance of those of Europe.

4. Even if it were conceded that the Fort Union belongs to the Tertiary, and that the fauna and flora of the Lance Creek epoch are more closely related to those of the Fort Union than they are to those of the Judith River, it does not follow that the Lance Creek epoch must be included in the Tertiary. A quarter before midnight on Monday is much nearer to Tuesday than it is to the previous six o'clock; nevertheless, it is not yet Tuesday.

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Paleontology and the Recapitulation Theory.

By E. R. Cumings.

I.

In reply to a severe critique of the recapitulation theory, or biogenetic law, by Hurst (30), Bather remarks that "If the embryologists had not forestalled them, the paleontologists would have had to invent the theory of recapitulation." (1) This may be considered as a fair sample of the general attitude of paleontologists of the Hyatt school, to which Bather belongs, toward the recapitulation theory.

Even the more conservative paleontologists, while inclined to use the theory *cum grano salis*, recognize the weight of evidence that Hyatt and his coworkers in the realm of paleobiology, have brought together, as is evidenced by the following quotation from Zittel (65): "Nevertheless embryonic types are not entirely wanting among invertebrates. The Paleozoic Belinuridæ are bewilderingly like the larvae of the living Limulus. The pentacrinoid larva of *Antedon* is nearer many fossil crinoids than the full grown animal. . . . . Among pelecypods the stages of early youth of oysters and Pectinidæ may be compared with Paleozoic Aviculidæ. Among brachiopods, according to Beecher, the stages which living Terebratulidæ pass through in the development of their arm-skeleton correspond with a number of fossil genera. The beautiful researches of Hyatt, Würtenburger and Branco, have shown that all Ammonites and Ceratites pass through a goniatite stage, and that the inner whorls of an Ammonite constantly resemble, in form, ornament and suture line the adult condition of some previously existing genus or other."

In violent contrast with this full acceptance, or this guarded acceptance of the theory on the part of the paleontologists, is the position of a considerable school of embryologists and zoologists. Perhaps no one has put the case against the theory more baldly and forcibly than Montgomery in his recent book on "An Analysis of Racial Descent" (42). He says: "The method is wrong in principle, to compare an adult stage of one organism with an immature stage of another." And again: "Therefore we can only conclude that the embryogeny does not furnish any recapitulation of the phylogeny, not even a recapitulation marred at occasional points by secondary changes. . . . An analysis of the stages during the life of one individual can in no way present a knowledge of its ancestry, and the method of comparing non-correspondent stages of two species is wrong in principle." Equally sweeping is the statement of Hurst (30): "The ontogeny is not an epitome of the phylogeny, is not even a modified or 'falsified' epitome, is not a record, either perfect or imperfect of past history, is not a recapitulation of evolution."

It would seem as though two statements could not be more flatly contradictory than these of Hurst and Montgomery, and that of Bather quoted above. Nevertheless I venture to make the seemingly paradoxical assertion that both parties to the controversy may be right, for the simple reason that they are talking about quite different things. This has been nowhere better expressed than by Grabau (25). He says: "It has been the general custom to test the validity of the recapitulation theory by the embryological method; i. e., the comparableness of the changes which the individual undergoes during its embryonic period to the adults of more primitive types. Usually the comparison has been with the adults of existing types, since in most cases these alone were available for comparison. It is no wonder, then, that such comparisons have led to innumerable errors, if not absurdities, which have placed the recapitulation theory in an evil light and awakened in the minds of many serious investigators doubts as to the validity of the deductions based upon this doctrine. When, however, the *entire life history* of the individual is considered, instead of *only the embryonic period*, and when the successive stages of epembryonic development are compared with the adult characters of related types, in immediately preceding geologic periods, it will be found that the fundamental principle of recapitulation is sound, and that the individuals do repeat in their own *epembryonic* development the characters of their own immediate ancestors." (Italics mine.)

It is as a matter of fact true that the Hyatt school of paleontologists have based their phylogenies on epembryonic rather than embryonic stages —stages beginning with the nepionic or infantile—since in the nature of the case the true embryonic stages are scarcely ever accessible to the student of fossils. It is no less true that the severest critics of the theory of recapitulation have rested their case largely on the real or supposed lack of correspondence between the *embryonic* stages and the adult stages of assumed ancestors, or upon certain *a priori* considerations having to do with the laws of development and inheritance. To the former class belong such critics as Von Baer, and to the latter class such as Hatschek, His, Hurst, Montgomery and others.

In making this statement I am aware that paleontologists sometimes compare true embryonic stages with adult stages of pre-existing types. As examples of this we might cite the comparison of the larval stage of *Antedon* with adult Paleozoic crinoids, as mentioned by Zittel; and the classic attempt of Beecher to reconstruct the ancestor of the Brachiopoda by a comparison of the phylembryonic stages of a representative series of genera of recent and fossil brachiopods. Nevertheless by far the greater number of comparisons that have been instituted by paleontologists have been between epembryonic stages of individuals and adult stages of older forms. Such comparisons are those of Hyatt, Branco, Karpinsky, Würtenburger, Buckman, Neumayr, Smith, Beecher, Clarke and others among the Cephalopoda; of Beecher and Schuchert, Raymond, Greene and Cumings among the Brachiopoda; of Jackson among the Pelecypoda; of Grabau and Burnett Smith among the Gastropoda; of Lang and Cumings among the Bryozoa; of Ruedemann among the graptolites; and of Beecher, Girty, Lang and others among the corals. To many of these researches I shall refer later.

I am also not unmindful of the fact that many of those who are not primarily paleontologists recognize the fact that development does not terminate with the completion of the embryonic stages, and that recapitulation may be legitimately looked for in epembryonic as well as embryonic stages, or that it may be sought in epembryonic stages, even though masked or falsified in embryonic stages. It is true, of course, that some speak of a comparison of *ontogeny* and phylogeny when, judging by the context, they mean a comparison between *embryogeny* and phylogeny. There arises here a question of definition: does the biogenetic law mean that the *ontogeny* is a recapitulation of the phylogeny, or does it mean that the *embryogeny* is a recapitulation of the phylogeny? If we take the general consensus of opinion we shall find for the former definition, and if we take the words of Haeckel, whose statement of the law is the one usually quoted, we shall again find for the former definition. I believe that, as a matter of fact, no one would maintain that the second definition is correct, however much he might forget in his studies to take the epembryonic stages into consideration.

Nor would I create the impression that the embryologists and zoologists have utterly deserted the paleontologists in their support of the recapitulation theory. Several recent papers give considerable aid and comfort to those of us who still believe in recapitulation. I shall introduce here the conclusions of three of these workers, more particularly because it will afford me an opportunity to correct what I hold to be another error of those who oppose the theory.

One of the most interesting pieces of evidence that has recently been adduced in favor of the idea that ontogeny recapitulates phylogeny is to be found in a paper by Griggs on "Juvenile Kelps" (28). It is not my purpose, however, to discuss the very interesting evidence which he has recorded, but rather to quote his remarks on the views of His and Morgan, and his general conclusions. His maintains that the reason why ontogeny seems to recapitulate phylogeny is because the stages in development are, as Griggs paraphrases it, "only the physiologically necessary steps for the formation of the adult body from its earliest stage, which in most cases is the egg." With the ideas of Morgan as expressed in his valuable book on "Evolution and Adaptation" we are all familiar. He holds that organisms repeat in their development, not adult stages, but only embryonic stages of their ancestors. To this idea he has given the name of "repetition."

On this point of the recapitulation of embryonic conditions Griggs makes the following pertinent statements: "In the toothless animals, the whale and the bird, the development of teeth in the jaws is entirely unnecessary * * * it may even be said to hinder the attainment of the adult condition. The same is true of the mammalian gill slits and of most structures which have in the past attracted attention in connection with the recapitulation theory. As the ancestral period when such structures were fully developed in the adult becomes more and more remote, the tendency to inherit them becomes less and less, because of the cumulative impulses given to the heritage by the nearer ancestors. Consequently they are successively less and less developed. Any gradual loss of inheritance can, in the nature of the case, take place only from the mature condition backward toward the beginning of the life cycle; otherwise we should have adult structures with no ontogenetic history. Therefore we can understand why it is that in many cases only the embryonic stages of ancestral history persist in the ontogeny." In a foot note he says: "The cutting off of end stages in the development of organs has given rise to the idea that the adult stages are 'pushed back into the embryo.' Such a misconception easily arose from the loose language in which the facts have often been expressed. Thus the embryogeny will be gradually shortened by the omission of more and more of the superfluous ancestral stages; and it will tend finally to retain only such stages as are necessary to the attainment of the adult form." Morgan and His, he maintains, have confused morphology and physiology. "The recapitulation theory has nothing to do with physiology; it is purely a matter of morphology."

In conclusion Griggs says: "Taking all the evidence into consideration, it seems to the writer that we are bound to conclude that though organisms are subject to adaptations at any stage of their life cycles and may gradually cut out superfluous stages, yet, except as some such tendency has operated to change the heritage, the development of the individual does recapitulate the history of the race * * * recapitulation must take place if there is any force which tends to make offspring like parent, if heredity is of any importance in moulding the forms of organisms. On the other hand, if there is any variability of transmutation of individuals in stages other than the adult end stages of the life cycles, the recapitulation cannot be perfect, but must be marred at every stage where secondary change has taken place." I shall return later to some of the points raised by Griggs in the above statements.

Another eminent worker, Dr. Eigenmann, says at the close of a paper on the eyes of the blind vertebrates of North America (20): "We have seen in the preceding pages that the foundations of the eye [of *Amblyopsis*] are normally laid, but that the superstructure instead of continuing the plan with new material, completes it out of the material provided for the foundations, and that in fact not even all of this (lens) material enters into the structure of the adult eye. The development of the foundations of the eye is phylogenic, the stages beyond the foundations are direct."

The third writer, Dr. Zeleny (64), in his paper on "Compensatory Regulation," in a discussion of the development and regeneration of the opercula in serpulids, says that the morphologic series is so complete as to make sufficient ground for the conclusion that the opercula arose in the course of phylogeny as modified branchia. The ontogenetic series, he says, *corresponds very closely with the probable phylogenetic series.* Speaking of the regeneratory development he says: "The course of regeneratory development is characterized by great condensation and directness of the development. There is no trace of the branchial stage, and the development of the two rows of processes of the terminal cup does not follow the ontogenetic order."

His final conclusion is as follows: "The data furnished, therefore, by the opercula of the serpulids give a fairly close agreement between the ontogenetic stages and the probable phylogenetic ones as determined by the usual criteria. The regeneratory development, however, follows a course which may be modified by the character of the operation that leads to the regeneration." By the "usual criteria" he means morphology, etc., so that he cannot be accused of the *circulus vitiosus*.

Those who wish to review the detailed evidence given in the above papers, bearing on the theory of recapitulation, will, of course, consult the original papers. My main reason for quoting them is, as stated above, because of their bearing on what seem to me to be grave errors in the reasoning of His, Morgan and Montgomery and others who have adopted similar views. The error seems to me to be, as pointed out by Griggs, in the confusion of morphology and physiology. The adult characters that are supposed to be recapitulated in the ontogeny, as well as the characters in ontogeny that are supposed to represent them, are morphological characters solely. It matters not what new function they may have come to serve, nor by what physiologic process they have come to make their appearance in the recapitulating organism. The confusion arising from this source colors all the argument of Montgomery, in which he endeavors to prove that new specific characters must have some representation in the ovum—a view which we must certainly agree with—and that therefore "the whole row" of cells from the ovum to the adult must be different. We grant that "The whole row" *is* different in some way, physiologically different, different in its play of energies; but it may conceivably be morphologically identical up to the very point where the new character is added. It is just as easy to conceive that the energy, or whatever we choose to call it, that is at a certain stage in development to produce a certain rib or spine or color-band on the shell of a gastropod, may be handed through the row of cells reaching up to the given stage, without producing a single recognizable morphologic change in the row, as compared with the individual that is not to possess the new character, as it is to conceive the opposite. The argument for the one view is just as certainly *a priori* as the argument for the other view. It is also perfectly conceivable that the morphology of the *individual cells* in the row might differ after the acquisition of the new character (in so far as this assumption is required by recent cytological studies), and yet not a single organ or part of the organism be different up to the stage in ontogeny when the new character appears. Unless, therefore, a change in the energies of the cells *inevitably necessitates* a change in the morphology of all the cells or of all the organs which they compose, the argument of Montgomery proves nothing.

As to the argument of His and others, that the supposedly ancestral stages are merely the physiologically necessary stages in the development of the individual; it again, as Griggs points out, confuses morphology with physiology, and is open to the further objection that it is directly opposed to the facts. Why, for example, should the condition of perfect blindness, with complete loss of all the essential structures of the eye, be attainable only by the round-about way of first developing the foundations of a normal eye? Why should a serpulid be able to regenerate a perfect operculum in a manner entirely different from, and even opposed to the ontogenesis of the organ, if there is any physiologically necessary way in which that particular individual or that particular organ must develop? The thing that makes it necessary for development to take a certain course in a given individual is the fact that the development has taken that same course in the ancestors. This species of coercion may, to be sure, be relaxed, and the development take some other course, but it is usually relaxed with extreme slowness, and after many generations have passed.

If inheritance were perfect, the individual would take exactly the same course in development as its ancestors. That it does not do this in all cases is, as Griggs points out, a more remarkable fact than that in other cases it should follow the ancestral mode of development so closely. Griggs explains the loss of inheritance as due to a progressive condensation of the ontogeny by the "omission of more and more of the superfluous ancestral stages." This is the well-known law of acceleration or tachygenesis. Like most embryologists, however, he misconceives the law, as shown by the foot-note quoted above. Embryologists are especially prone to limit the law of acceleration in development to the skipping or omission of steps, and the consequent shortening of development. This is not in keeping with the views of Hyatt, who first definitely formulated the law; and, as all paleobiologists know, it is not in keeping with the facts. Hyatt (31) says: "All modifications and variations in progressive series tend to appear first in the adolescent or adult stages of growth, and then to be inherited in successive descendants at earlier stages according to the law of acceleration, until they either become embryonic or are crowded out of the organization and replaced in the development by characters of later origin." A more concise statement of the law is as follows: "The substages of development in ontogeny are the bearers of distal characters in inverse proportion and of proximal characters in direct proportion to their removal in time and position from the protoconch, or last embryonic stage" (31).

According to the definitions just quoted, acceleration involves not only the omission of characters, in some cases (and this is the only sort of acceleration that most embryologists seem to recognize), but it involves also condensation without omission, by crowding more into a given portion of the ontogeny, or again by "telescoping" of characters, as Grabau (25) calls it, so that characters that originally appeared in succession, come to appear simultaneously. In other words acceleration may be by *elimination*, by *condensation*, without change in the order of appearance of characters, or, third, by *telescoping*. The latter is condensation with change in the order of appearance, or as commonly expressed, unequal acceleration. It is probable that paleobiologists have erred in giving too much emphasis to the principle of earlier inheritance, involved in the law, just as embryologists and morphologists have erred in entirely neglecting this phase of inheritance. As conceived by the paleobiologist, the law of acceleration is an explanation of recapitulation, as well as an explanation of the failure to recapitulate.

Another factor in inheritance has been given the name of retardation by Cope (15). By the operation of this factor, characters that appear late in the ontogeny may disappear in descendants because development terminates before the given character is reached. In this way, it is conceived, the ontogeny may be shortened and simplified, and many ancestral characters lost entirely. The result of the continued operation of retardation would be retrogression. That is, the given form, if it continued to repeat the remoter ancestral stages in the early part of its ontogeny, and continued at the same time to drop off the later ancestral stages, by failing to proceed far enough in its development, would ultimately come to resemble the remote rather than the nearer ancestors. Manifestly the retarded forms do not recapitulate the lost characters, so that here, also, as in the omission of characters in the earlier stages of ontogeny, the heritage is incomplete.

Of the complications of inheritance that arise from larval adaptations, intra-uterine adaptations, and special adaptations arising in later life, I shall not speak. All of these have been repeatedly discussed (see for example Smith 57), and are well understood. Against all of these the paleobiologist must be on his guard. All of these factors tend to make the parallelism between ontogeny and phylogeny inexact, as long ago pointed out by Cope (15). Yet in spite of the operation of these factors, the cases in which there is clear evidence of recapitulation are so numerous, and so well known to the paleobiologist, that were it not for the continually reiterated statements of certain embryologists that there is no such thing as recapitulation, I should hesitate to again point them out. I shall now take up the evidence according to the groups of organisms in which it has been ascertained; and I once more remind the reader that most of this evidence applies to the epembryonic and not to the embryonic stages.

II.

Cephalopoda.—The only existing representative of the great group of Tetrabranchiata, the class to which nearly all of the fossil cephalopods belong, is the *Nautilus*. The genus *Nautilus* is a striking example of the persistence of a primitive type. It belongs to the more primitive branch of the tetrabranchs, from which, according to all the evidence, the marvelously complex ammonities, on the one hand, and the modern naked cephalopods are descended. Nautilus is the only tetrabranch of which the entire ontogeny, including the embryonic stages, is known.

This lack, however, in the case of the fossil genera is not as serious as might be supposed, for the reason that even in these ancient forms all of the growth stages from the latest embryonic (phylembryonic) stage to the adult are preserved in every complete individual shell. An inspection of the *Nautilus* shell makes this at once apparent, for the earlier stages of the shell are surrounded and protected by the later, and no part of the shell is lost or resorbed. In the straight and loosely coiled shells only, such for example as *Orthoceras, Cyrtoceras, etc.*, is the case different; and even here, barring injury, or the dehiscence of the earlier chambers, every post-embryonic stage is preserved. From a study, therefore, of a single shell, we are able to make out perfectly all of the epembryonic development in that part of the organism that was most vitally affected by the environment, and which must therefore indicate most perfectly the lines along which the evolution proceeded.

If the initial portion of the shell of *Nautilus* be examined, it will be found to be characterized by a scar or cicatrix. In the same region of the shells of ammonites and some Nautiloidea (*Orthoceras*), instead of this cicatrix, there is present a minute, bulbous or bag-like shell, attached to the apex of the shell proper. If in the case of *Orthoceras*, as shown by Hyatt (31), this bulb, or protoconch be broken away, there is exposed a scar (cicatrix) precisely similar to that of *Nautilus*. The initial shell or protoconch is therefore substantially the same in all of the Tetrabranchiata, and is supposed to point to a "septa-less and chamberless form similar to the protoconch" as the common ancestor of these two great divisions of the Tetrabranchiata; and possibly, as Hyatt suggests of the Cephalopoda, Pteropoda and Gastropoda (31). The protoconch represents the latest of the true embryonic stages, namely the phylembryo.

Succeeding this early stage are the stages of the shell proper.[1] In *Nautilus* the early nepionic portion of the shell, which includes the formation of the first three septa, is only slightly curved (cyrtoceraform). Up to the stage of the formation of the second septum, the shell is in fact nearly straight (orthorceraform). The first septum has an apically directed caecum, and the second septum an apically directed closed tube, the closed apical end of which fits into the caecum of the first septum. This tube is the beginning of the siphuncle. Since the tube fits closely into the caecum, the two together form a continuous tube, in which the apical end or bottom of the siphuncular tube forms a partition or septum, so that as Hyatt points out, the resemblance "of this early stage to the adult structures of *Diphragmoceras* becomes perfectly clear." (31)

In the later nepionic stages (i. e., after the formation of the third septum) the shell is rather sharply bent (the gyroceran curve), so that near the close of the first volution the whorl is brought back into contact with the apex of the conch. This manner of growth results in leaving an empty space or *umbilical perforation* between the two halves of the first volution. In the ancient coiled Nautiloidea there appears at the beginning of this (neanic) stage, when the whorls come into contact, a de-

[1] The stages from this point on are termed by Hyatt (31), and following him by practically all paleobiologists at the present time, the *nepionic, neanic, ephebic* and *gerontic* stages; meaning respectively, the infantile, youthful, mature and old age stages of growth.

pression or groove in the dorsum of the whorl, where it rests against the venter of the preceding whorl. This is the *impressed zone.* In the modern *Nautilus,* however, this furrow or impressed zone begins in the early nepionic stage, *before the whorls have come into contact.* This occurs also in the nautilian shells of the Carboniferous, Jurassic, Cretaceous and Tertiary.

Of this truly remarkable feature of cephalopod development, Hyatt says: "When one ascends in the same genetic series to the more specialized nautilian involved shells this purely acquired character becomes, through the action of tachygenesis, forced back, appearing as a rule in the nepionic stage before the whorls touch. It is therefore, in these forms entirely independent of the mechanical cause, the pressure of one whorl upon another, which first originated it. One need only add that this configuration of the dorsum is never found in the adults of any ancient and normally uncoiled shells, so far as I know, nor so far as have been figured." (31)

Without reviewing any of the further interesting details of the ontogeny of *Nautilus,* enough has been said to make it evident that if there is any truth in recapitulation, the development of *Nautilus* would indicate (disregarding the protoconchal characters) an ancestral line that contained, first straight or slightly arched, then loosely coiled, and finally closely coiled shells, and that the earliest of these possessed a septate siphuncle. That the geological series of shells indicates the same thing every paleontologist knows perfectly well. The development of *Nautilus* also affords one of the most perfect illustrations of the law of tachygenesis, in the earlier inheritance of the impressed zone, known in the whole animal kingdom.

One further illustration, from the Cephalopoda, of the parallelism of ontogeny and phylogeny must suffice. This illustration is drawn from the genus *Placenticeras,* one of the complex Ammonites of the Cretaceous. The development of this genus has been beautifully worked out by Professor J. P. Smith (58). The species *P. pacificum* comes from the Chico formation of the Upper Cretaceous. The following account applies to the development of this species and is drawn from the paper by Smith, cited above.

The earliest shelled stage was probably passed before the animal was hatched. This is the protoconch or phylembryo. It is a smooth, oval, bulbous body, similar to that of all the later ammonites. It probably represents an "adaptive form, due to life in the egg, and does not represent any ancient ancestral genus, for none of the early cephalopods were shaped like this."

"With the formation of the first septum, the young ammonite has taken its place among the chambered cephalopods, and has become, for the time being, a nautiloid, although it is not possible . . . . to correlate it with any special genus. . . . . The first septum . . . . is nautilian in character, but the siphuncle begins inside the protoconch with a siphonal knob, or caecum, and the protoconch itself is calcareous. These are two characters that the nautiloids even to this day, have never yet acquired. . . . . We have in this stage ammonite characters pushed back by unequal acceleration [telescoping], until they occur contemporaneously with more *remote* ancestral characters."

There is no sign of an umbilical perforation as in the *Nautilus*, described above, a fact which again shows the degree of acceleration of these ammonites.

With the second septum the ammonite characters are assumed. The shell at this stage is "distinctly goniatitic," but also possesses characters, introduced by acceleration, that belong to later genera. The evidence indicating the goniatitic as well as later stages to be mentioned, is mainly the character of the suture lines. "At about five-eighths of a coil . . . . the larva has reached a stage correlative with the goniatites of the Upper Carboniferous." This stage is quickly passed, and the goniatitic characters are lost and characters transitional to the ammonite stage make their appearance. "At one and one-twelfth coils the shell is transitional from the glyphioceran stage to what resembles closely the genus *Nannites* of the Trias." In regard to this stage Smith says: "If it had not been said that this was a minute shell taken out of an older individual, any paleontologist would refer it without hesitation to the Glyphioceratidæ, and probably to . . . . *Pronannites*, of the Lower Carboniferous." This stage lasts about one-half revolution.

In the neanic stage, at one and seven-twelfths coils, the shell resembles very strongly *Cymbites*, or some related genus of the Lower Jurassic. The first signs of shell sculpture occur in this stage. In the next stage the sculpture becomes stronger, and the shell assumes a decidedly aegoceran appearance. From two up to two and one-quarter coils, the shell resembles in most respects the stock to which *Perisphinctes* belongs, and this is accordingly called the perisphinctes stage. During this stage the sides of the shell become more flattened, and the abdominal shoulders squarer, the varices frequent, and strong intermediate ribs appear on the sides and abdomen.

In the next (*Cosmoceras*) stage "the ribs no longer cross the abdomen, but end in tubercles on the abdominal shoulders, forming well defined shoulder keels, with a furrow between them." Near the beginning of the fourth coil the ribs are reduced to mere faint undulations and fine sickle-shaped striae on the sides of the umbilicus, while the external tubercles become almost obsolete, forming mere notches on the continuous abdominal keels. Specific characters begin to appear here. This may be taken as the beginning of the *Hoplites* stage. The septa have not reached the complete development of the genus.

The umbilical knots begin at this stage, and growing stronger, become a characteristic feature of the adult *Placenticeras*. "*Placenticeras pacificum* at this stage is wholly unlike *P. californicum*, with which it is associated, being much more compressed and discoidal, with narrow abdomen, flatter sides, much less distinct sculpture, and narrower umbilicus, although in the earlier adolescent periods both species are very much alike." The shell passes from this stage by gradual changes into the adult *Placenticeras*.

Professor Smith's conclusions are of especial interest. He says: "The development of *Placenticeras* shows that it is possible, in spite of dogmatic assertions to the contrary, to decipher the race history of an animal in its individual ontogeny."[1]

[1] For further illustrations of recapitulation among the Cephalopoda, the student should consult the following papers: Branco, W., Beiträge zur Entwicklungsgeschichte der fossilen Cephalopoden, *Paleontographica*, vols. xxvi, xxvii, 1879, '80. Buckman, S. S., Monograph of the Inferior Oolite Ammonites, *Paleontographical Society*, 1887-'96. Hyatt, A., Paralellism of the individual and the order among tetrabranchiate Mollusks, *Mem. Bos. Soc. Nat. Hist.*, vol. i, 1866; Fossil cephalopods of the Museum of Comparative Zoology, *Bull. Mus. Comp. Zool.*, vol. iii, 1872; Genesis of the Arietidæ, *Smithsonian Contr. to Knowl.*, vol. xxvi, 1889; Phylogeny of an acquired characteristic, *Proc. Am. Phil. Soc.*, vol. xxxii; Cephalopoda, in *Text Book of Paleontology* by Zittel (Eastman trans.), 1899. Hyatt, A., and Smith, J. P., Triassic cephalopod genera of North America, *U. S. Geol. Surv. Prof. Paper* No. 40, 1905. Karpinsky, A., Ueber die Ammoneien der Artinsk-Stufe, *Mem. Acad. Sci. Imp. St. Petersburg*, vol. xxxvii, No. 2, 1889. Neumayr, M., Die Ammoniten der Kreide und die Systematik der Ammonitiden, *Zeitschr. der Deutch. Geol. Ges.*, 1875; Ueber unvermittelt auftretende Cephalopodentypen im Jura Mittel-Europas, *Jahrb. d. K. K. Geol. Reichs. Wien*, vol. xxviii, 1878. Smith, J. P., The development of Glyphioceras and the phylogeny of the Glyphioceratidæ, *Proc. Calif. Acad. Sci.*, (3) *Geol.*, vol. i, 1897; The Development of Lytoceras and Phylloceras, *Ibid.*, 1898; Larval stages of Schloenbachia, *Jour. Morphology*, vol. xvi, 1899; The Carboniferous Ammonoids of America, Monog. U. S. G. S., No. xlii, 1903. Würtenburger, R., Studien über die Stammgeschichte der Ammoniten, Leipzic, 1880.

Pelecypoda.—The classic memoir of Jackson (32) on the phylogeny of the Pelecypoda brings together numerous illustrations of recapitulation among the members of this class of animals. Jackson's conclusions are well-known, and I shall therefore review them very briefly.

From a study of a large number of genera representing widely divergent members of the Pelecypoda, Jackson concludes that there is present throughout the group an embryonic shell, which he calls the "prodissoconch" (a term correlative with the term protoconch of the Cephalopoda and Gastropoda), and which is a simple bivalved, equivalve shell. At this (phylembryonic) stage of development there are two adductor muscles, even in genera in which the adult have only one adductor. That is, the prodissoconch is dimyarian even though the adult animal may be monomyarian. In the Aviculidæ and their allies (*Ostrea*, *Avicula*, *Perna*, *Pecten*, *Plicatula*, *Anomia*) the prodissoconch very closely resembles in form the primitive genus *Nucula*. The anatomical characters of the prodissoconch also bear out this resemblance. It is therefore inferred that some such type as *Nucula* is the primitive ancestor of the Aviculidæ, and possibly of the Pelecypoda. The paleontological and anatomical evidence supports this conclusion.

We have here, then, in the Aviculidæ and their allies, a group of monomyarians, some of them, as *Ostrea*, *Plicatula*, and *Anomia*, of very aberrant form, the representation in the ontogeny of a dimyarian stage, which, from all the evidence, actually characterized the adults of the ancestors of the group. Whether or not *Nucula* is the actual ancestor of this group of pelecypoda, it is quite certain that the earliest pelecypods were of the same general form as the prodissoconch, and that they were dimyarian.

In the same paper Jackson has shown in a masterly manner that the ostreaform shape of the shell, which characterizes many more or less widely separated genera of pelecypods, is due to "the mechanical conditions of direct cemented fixation." These ostreaform shells are very variously derived, and should, if there is anything in the theory of recapitulation, each show in the young stages, before the valves have become fixed, the distinctive adult characters of its particular ancestor. In this case we are relieved from the danger of arguing in a circle by the fact that the genetic relations of most of the forms are fairly well known from lines of evidence other than the ontogeny. The following specific cases cited by Jackson are of especial interest.

Mulleria lobata, a member of the Unionidæ, "is so remarkably like an oyster (in the adult) that it has been called the fresh-water oyster. In the monomyarian adult . . . . the shell is rough and irregular with a deep attached and flattish free valve, and a specimen in the Museum of Comparative Zoology is indistinguishable in shape from forms commonly found in *Ostrea virginiana*. . . . . The young shell of *Mulleria* . . . . is Anodon-shaped, equivalvular and dimyarian as described by authors."

Hinnites is another genus which has the ostreaform adult. "In the young it is free and pectiniform, but in the adult . . . . so close is the likeness to an oyster that in the synonomy of the genus it has been named *Ostrea* and *Ostracites*." In *Hinnites cortesi* of the Tertiary, in the neanic stage, the right valve is purely pectiniform. "It has the well-developed ears, deep byssal sinus, and an evenly plicated shell which at this stage is nearly or quite equivalvular." With the period of attachment a most marked change in the valves takes place and the adult becomes deeply concave (in the right attached valve) and highly ostreaform. The byssal notch is filled up and "completely wiped out of existence."

In genera such as *Ostrea* and *Plicatula*, where fixation takes place at the close of the prodissoconch stage, the succeeding stages give very little indication of the ancestry, owing to the extensive modification of the shell as soon as fixation takes place. According to Dall *Ostrea* is derived from the Pteriidæ.

Spondylus is another genus in which cementation has caused extensive modification of the valves in the adult. Fixation takes place at the close of the nepionic period. Therefore this genus may be expected to afford some evidence of recapitulation. The first nepionic stage of *Spondylus* is decidedly pectiniform. It has a long hinge-line and a deep byssal sinus. After fixation, in the first stages of irregular growth, the byssal notch is soldered over, and eradicated in a manner similar to *Hinnites*.

Another illustration of recapitulation among the Pelecypoda is the case of *Pecten* itself. Of this genus Jackson says: "In the development of the modern *Pecten* we find in the first stages of dissoconch growth a form of shell . . . . presenting characters which make it referable in ancestral origin to *Rhombopteria*, a member of the true Aviculidæ, later succeeded by a growth . . . . bearing marked features referable in origin to an ancestral genus *Pterinopecten*. . . . . Still later a stage exists . . . . which is referable in its inherited form to *Aviculopecten*, and finally the true *Pecten* features characteristic of the adult . . . . are established. The geological sequence of these several groups is in the order indicated by the development of *Pecten*. We have, therefore, a clear case of the ontogeny of an individual illustrating the phylogeny of the group."

Gastropoda.—For studies of the Gastropoda in which growth stages have especially been taken into consideration we are indebted chiefly to Grabau (22, 23, 24, 25) and Burnett Smith (53, 54, 55, 56). My illustrations of recapitulation among the members of this class will be drawn, therefore, from the writings of these two authors.

It is commonly known that the apical whorl of the gastropod shell may differ materially from the succeeding portions of the shell (conch), being smooth and without ornament in cases where the conch is highly sculptured, or in some forms, as *Acmaea* and *Crepidula*, being coiled, although the adult shell is patelliform and non-coiled. To this apical whorl the name "protoconch" has come to be applied, a name which, as we have already seen, is also applied to the embryonic shell of the Cephalopoda. Grabau (22) has suggested the use of the name "protorteconch" in place of protoconch for the initial shell of the gastropods.

The protoconch of the existing Gastropoda is more variable than that of the Cephalopoda, as would be expected from the highly specialized nature of most of the extant representatives of the class. In most cases there is no definite line of demarkation between the protoconch and the conch, but in a few cases, as in *Fusus*, etc., the "end of the protoconch is strongly marked by the existence of a pronounced varix and an abrupt change of ornamentation." (22) "The early whorls of the protoconch . . . . are smooth rounded coils of the type found in adult *Natica*. . . . . In the majority of cases the initial whorl is minute, while the succeeding ones enlarge gradually and regularly. In some types the initial whorl is large and swollen. . . . . This type of protoconch has been termed 'bulbous' by Dall (19). The naticoid form of protoconch is in general umbilicated, and it is probable that at least the earlier portion of the protoconch is umbilicated in the majority of gastropods.

"From the characters of the initial whorls of the protoconch we may argue that the radicle of the coiled gatropods must have been a naticoid type with a well-marked umbilicus. Such a type is found in *Straparollina remota* Billings, one of the earliest coiled gastropods of the Etcheminian or Lower Cambrian of the Atlantic border province of North America. That it is not the most primitive type of gastropod is suggested by the consideration that the earliest stage . . . . of the protoconch is not coiled, but rather cap-shaped like modern *Patella*. Such primitive types are found in Lower Cambrian species which have variously been referred to *Platyceras*, *Scenella*, or *Stenotheca*, owing to the want of sufficient characteristics to define their exact relations." (22.)

From the above it appears that the early protoconch stages indicate an ancestor of the simple, smooth shelled, umbilicated type exemplified by *Straparollina*, and that this is actually the only type of coiled gastropod characteristic of the basal Cambrian. It is also likely from paleontological evidence that the very earliest type of gastropod possessed a conical or cornucopia-shaped shell of the *Scenella* type.[1] Such an ancestry is, according to Grabau, suggested by the cap-shaped earliest stage of the protoconch.[2]

One of the most completely worked-out cases of recapitulation among Gastropoda that has come to my knowledge is that of the races of *Athleta petrosa* Con. and its allies. The phylogeny of this group of gastropods has been very fully studied by Burnett Smith (54), from whose paper the following account is drawn.

[1] Sardeson (50) suggests that the gastropod ancestor was an "asymmetrical long conical shell" of the pteropod type. He may be right, but even so, I do not see that his conclusion would in the least invalidate the conclusions of Grabau in regard to the phylogenetic significance of the protoconch, although Sardeson seems to think so. Grabau says very plainly that the coiled shell is probably not the most primitive type of shell, and he points out the fact (quoted above) that the initial portion of the protoconch is cap-shaped and may indicate some such remote ancestor as the Cambrian forms referred to the genera *Platyceras*, *Stenotheca*, and *Scenella*. Whether this patelliform ancestor was in turn derived from a long conical shell, or whether on the other hand the coiled type of shell was derived directly from the "long conical" shell without the mediation of a patelliform ancestor, does not materially affect the conclusions that at a very remote time a coiled gastropod radicle was established from which practically all modern gastropods were derived. To my mind the conclusion that the ultimate ancestor of the Gastropoda was a "long conical" shell is by no means established.

[2] Burnett Smith (55) concludes from a study of the Tertiary species of the genus *Athleta* that "we can say for this restricted normal group at least that the apex is not only a variable feature, but the most variable feature which the shells furnish." In a footnote he says "The author is thoroughly convinced that the features of the apex must be used in classification with great caution." The variations which he cites in this and other papers (54, 55, 56) seem to be chiefly in the size of the protoconch, and the degree to which acceleration has caused conchal characters to appear in the later protoconchal stages. His caution, however, in regard to the classificatory value of the protoconch, should put students of the gastropods on their guard against a too free use of this portion of the shell in the establishing of genera.

The species under consideration occur in the Gulf Eocene, extending nearly throughout it. They have heretofore been referred to the genus *Volutilithes*, but are placed by Smith (55) in the genus *Athleta*. Smith states that the material at his disposal was very complete, and enabled him to study large series of individuals, very carefully collected with reference to horizon. The stratigraphy of the formations from which they came is also well understood. These favorable conditions of study, it may be remarked, are especially important in the present connection, because they enabled Smith to trace out the evolution of the forms practically continuously from zone to zone, without being chiefly dependent on ontogeny for indications of their relationships. Another fortunate circumstance is the fact that this author is disposed to use the evidence from ontogeny with the utmost discretion, everywhere checking it by an appeal to the morphological and geological series.

In the forms under consideration, the first two or three whorls are smooth and rounded, constituting the smooth or protoconchal stage. "The first ornamental feature to appear on the smooth, rounded whorl is the transverse rib, that is, a slight elevation of the whorl which runs across it from suture to suture. These early ribs are invariably curved slightly, and each one is simple and uniform from suture to suture. The curved ribs persist as a rule for about a quarter or a half of a whorl, or even for a much less space. . . . . The curved rib stage . . . . has been found in every species and race dealt with in this paper. The curved ribs, after about one-third of a whorl, change abruptly into the straight ribs of what has been designated the cancellated stage."

"The cancellated condition is found more or less well developed in all the different races. In the primitive races it may persist as a constant feature to the end of the individual's life; but in most forms it covers only a few whorls and is more variable than the preceding curved rib stage." The end of the cancellated stage is much less definite than the beginning. It is followed by the "spiny stage." In this stage the shoulder tubercle is sharp and spine-like. Other tubercles have disappeared, and this portion of the shell is therefore no longer cancellated. Succeeding the spiny stage, there may be a senile stage.

In the base of the Eocene at Matthew's Landing, Alabama, occurs a species, *Athleta limopsis*, which from its primitive characters, and its position at the base of the Eocene, Smith regards as the ancestor of the races and species which he deals with in his paper. This form presents no stages later than the cancellated stage. There is also very little individual variation. Associated with *A. limopsis* is the species *A. rugatus*. In its earlier stages this species very closely resembles *A. limopsis*, but "differs radically . . . . from that form with the progress of its ontogeny." In its later whorls it presents evidence, though not extreme, of senility. It has no spiny stage.

The next species *A. petrosa*, represents an assemblage of races connected by many intergrading forms. These races range upward from the Nanfalia beds to the Jackson beds of the Eocene. Several of them are senile races, and in the adult strikingly different from the ancestral form, *A. limopsis*. Smith says, however, that the young of all the races "are remarkably uniform and constant. The early whorls indicate clearly that they are all descended from a cancelated ancestor, and bear a strong resemblance . . . . to the characters of *A. limopsis*." Some of the senile races of *petrosa* are profoundly modified in the adult, as for example, the Hatchetigbee race, derived from the main stock through the Bell's Landing and Wood's Bluff races. Yet their derivation from the main stock is shown by intermediate forms, and the young of the terminal races greatly resemble the ancestral form. In the Jackson race, which is the terminal member of the main stock, the last two whorls are spiny, and the last whorl shows some senile characters at its close. "This race shows a regular and even ontogeny." Acceleration has carried the curved rib stage back to the beginning of the third whorl, whereas in the ancestral *A. limopsis* this stage begins near the close of the fourth whorl.

Smith has graphically expressed the main developmental and phylogenetic changes in the following diagram:

	1	2	3	4	5	6	7	8	9	10	11	12
A. limopsis. Matthew's Landing race. .	. . .	. . .	A	B	. . .	. . .	. . .	C	. . . .	. . . .	. . . .	Lower Eocene.
A. petrosa. Gregg's Landing race. . . .	. . .	A	. . .	B	. . .	C	. . .	. . .	D and I	D and I	. . . .	E Slight.
A. petrosa. Jackson race	A	. . .	B	. . .	C	. . .	. . .	. . .	D	D and E	. . . .	E never extreme. Upper Eocene.

In the above diagram the figures across the top stand for the number of the whorl of the shell, and the letters indicate the different ontogenetic stages as follows:

A—Smooth stage.
B—Curved rib stage.
C—Cancellated stage.
D—Spiny stage.
E—Senile stage.
I—Individual variation.

The acceleration of the Jackson race is beautifully brought out in this diagram, and as its correlative, the recapitulation in the earlier ontogeny of the later races, of the adult characters of the ancestral race. The individual variations may occur on any part of the shell, but usually follow stage C.[1]

Brachiopoda.—Among the members of this class there is a wealth of illustrations of recapitulation. I can only select a few cases that have been worked out in such a way that the relationships of the forms are indicated by the morphological and geological series as well as by the ontogeny. The pioneer student of the correlation of ontogeny and phylogeny among the brachiopods was Beecher, whose refined researches in paleobiology have never been excelled and rarely equaled.

The developing brachiopod, in the later embryonic stages, secretes in the mantle on opposite sides of the body two shell plates, which by peripheral growth ultimately meet at the edges and form the initial shelly investment of the animal. This initial shell to which Beecher has given the name "protegulum" (6) is of very simple form, consisting substantially of two convex plates of semicircular plan, gaping at the posterior straight edges. Through this gap between the two valves the pedicle (organ of attachment) projects. At first the pedicle occupies the full width of the valves, but subsequent peripheral growth of the shell with-

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[1] For additional studies of the gastropoda from the developmental standpoint see the following: Koken, E., Ueber der Gastropoden vom Cambrium bis zur Trias., *Jahrb. für Mineral. Geol. u. Pal.*, 1889. Beil. Bd. vi. Linden, Gräfen M. von, Die Entwicklung der Skulptur und der Zeichnung bei den Gehausschnecken des Meeres, *Zeitschr. Wiss. Zool.*, vol. lxi. 1896. Grabau. A. W.. Studies of Gastropoda II, Fulgur and Sycotypus, *Am. Nat.*, vol. xxxvii, 1903; Phylogeny of Fusus and its allies, *Smithsonian Miscell. Coll.*, vol. xliv, 1904; Studies of Gastropoda III on Orthogenetic variation, *Am. Nat.*, vol. xli, 1907. Smith, Burnett, Phylogeny of the species of Fulgur with remarks on an abnormal specimen of Fulgur canaliculatum and sexual dimorphism in Fulgur carica, *Proc. Acad. Nat. Sci. Phila.*, vol. liv, 1902; Senility among Gastropods, *Proc. Acad. Nat. Sci. Phila.*, vol. lvii, 1905; Phylogeny of the races of Volutilithes petrosus, *Proc. Acad. Nat. Sci. Phila.*, March, 1906; A new species of Athleta and a note on the morphology of Athleta petrosa, *Proc. Acad. Nat. Sci. Phila.*, May, 1907; A contribution to the morphology of Pyrula, *Proc. Acad. Nat. Sci. Phila.*, May, 1907.

out corresponding enlargement of the pedicle, leaves the latter restricted to a notch (delthyrium) in the posterior margins of the valves, providing the peripheral growth is about equal on all anterior and lateral radii. If the shell growth is greater in the anterior direction, the shell becomes pointed, the pedicle (posterior) end remaining of about the original width. If the shell growth is mainly in the lateral directions, the shell becomes wide, with a long straight hinge, of which the pedicle opening forms a very small proportion. Whatever may be the later growth of the shell, all the earlier stages are preserved, except in cases where the beaks are injured or resorbed by the encroachment of the pedicle in adult and senile stages. The growth of the shell is entirely by additions at the margins or on the inner surface. It follows that the protegulum may in exceptionally well preserved material be seen intact at the beaks of the adult shell. It is often seen at the apices of young shells.

Searching for the phylogenetic significance of the protegulum, Beecher (6) ascertained that certain of the earliest known brachiopods approximate very closely in form to the protegulum, and he selected the genus *Paterina* (*Iphidea*) as the radicle of the class. It has since been shown that *Paterina* is not the most primitive known brachiopod.[1] It is still true, however, that the most primitive brachiopods known are of the same general form and type as *Paterina*, in fact they approximate more closely, if anything, than that genus, to the form of the protegulum. It may be very safely concluded, therefore, from the geological evidence, that the primitive brachiopod was actually of the type indicated by the protegulum.

Beecher says of *Paterina:* "In mature specimens, all lines of growth, from the nucleal shell to the margin, are unvaryingly parallel and concentric, terminating abruptly at the cardinal line. In other words, no changes occur in the outlines or proportions of the shell during growth, through the nepionic and neanic stages up to and including the completed ephebic condition. The resemblance of this form to the protegulum of other brachiopods is very marked and significant, as it represents a mature type having only the common embryonal features of other genera."

Among the Brachiopoda, as among the Pelecypoda there are a number of forms in which the condition of very close fixation or of burrowing has

[1] Walcott (62) seems to reserve this distinction for his genus *Rustella*. Paterina is by him made a subgenus of the genus *Micromitra*. These forms are all placed in the superfamily *Rustellacea*.

given rise to extremely aberrant types. One of the most extreme of these types is the genus *Proboscidella*. The adults of this genus bear a very marked resemblance to the Pelecypod genus *Aspergillum*. In the early neanic stages *Proboscidella* resembles an ordinary *Productus*, from which genus the type is known to have descended. *Orbiculoidea* is a genus originating in the Ordovician, and extending through the Mesozoic. The first stage is paterina-like, the second resembles *Obolella*, the third is like *Schizocrania*, and adult growth brings in the characters of *Orbiculoidea*. The geological order of these genera is the same as the ontogenetic order of *Orbiculoidea*.

Of *Orbiculoidea* and its allies Beecher (7) says: "The early stages of Paleozoic *Orbiculoidea* have straight hinge-lines and marginal beaks, and in the adult stages of the shell the beaks are usually subcentral and the growth holoperipheral. This adult discinoid form, which originated and was acquired, through the conditions of fixation of the animals, has been accelerated in the recent *Discinisca* so that it appears in a free-swimming larval stage. Thus a character acquired in adolescent and adult stages in a Paleozoic species, through the mechanical conditions of growth, appears by acceleration in the larval stages of later forms before the assumption of the condition of fixation which first produced this character."

In the higher genera of the Terebratellidæ, the ontogeny recapitulates the phylogeny with remarkable fidelity, as pointed out by Beecher (7). This example has become classic, so that it is scarcely necessary to repeat the details. I shall give Beecher's conclusions in his own words. He says: "In each line of progression [the austral and boreal subfamilies] in the Terebratellidæ, the acceleration of the period of reproduction, by the influence of environment, threw off genera which do not go through the complete series of metamorphoses, but are otherwise fully adult and even may show reversional tendencies due to old age; so that nearly every stage passed through by the higher genera has a fixed representative in a lower genus. Moreover the lower genera are not merely equivalent to or in exact parallelism with, the early stages of the higher, but they express a permanent type of structure, as far as these genera are concerned, and after reaching maturity do not show a tendency to attain higher phases of development, but thicken the shell and cardinal process, absorb the deltidial plates, and exhibit all the evidences of senility."

Raymond (46) has pointed out a number of interesting cases of recapitulation. The very common and well-known Devonian Spirifer, *S. mucronatus*, has the cardinal extremities in the adult very acute (mucronate), sometimes, indeed, drawn out into needle-like points; while the number of plications may be thirty or more. In the neanic stage these transversely elongated spirifers pass through forms corresponding to the adults of certain Niagara species. The adult of *S. crispus*, corresponds very closely in shape, number of plications, and shell index with these young specimens of *S. mucronatus*.

Shimer and Grabau (51) have shown that in the upper part of the Hamilton series of Thedford, Ontario, there occurs a variety of *Spirifer mucronatus*, which though not mucronate at all in the adult, is "extremely mucronate" in the neanic stage. At this stage also there is evidence of the median plication of the sinus, another charactertistic of the adult of the normal *S. mucronatus*. In the adult of the Thedford variety this median plication has disappeared. The geological and morphological evidence of the derivation of this form of *S. mucronatus* is complete.

I have pointed out an exactly similar case in the variety *senex* of *Platystrophia acutilirata* (16). This variety occurs in the upper part of the Whitewater division of the Richmond series of Indiana and Ohio. *Platystrophia acutilirata*, as is well known, is very mucronate in the adult, resembling in its general outline, *Spirifer mucronatus*. It was in fact at first referred to the genus *Delthyris* (*Spirifer*). The normal form is shown by an unusually closely graded series of intermediate forms to be descended from *P. laticosta*, and it repeats the adult characters of the latter very faithfully in its late neanic stage, becoming always more mucronate as development proceeds. The upper Whitewater form, var. *senex*, frequently has entirely lost, in the adult stages, the acute angulation of the cardinal extremities, so that the lateral and cardinal edges make a right, or nearly a right angle. In the young (neanic) stages of *P. senex*, however, the shell is decidedly mucronate, so that these young shells exactly resemble the normal *Platystrophia acutilirata* of the lower Whitewater and Liberty formations. *P. senex*, it may be remarked, is a well defined form, and its derivation from *P. acutilirata* is beyond question, since it is connected with the latter by every gradation.

Another interesting case of recapitulation among the brachiopods has been worked out with great care by Mr. F. C. Greene (27). In this case also no pains was spared to ascertain the relationships of the various forms by tracing them continuously from zone to zone, and by a comparison of the morphological characters of the adults. The group studied by Greene is that of *Chonetes granulifer*, from the Upper Carboniferous rocks of Kansas. Here the forms from the higher zones repeat in their ontogeny the characters of forms from the lower zones with great fidelity. The very young stages also recall very forcibly the species of *Chonetes* from the Devonian. *Chonetes granulifer* is also very interesting from the fact that the first hinge-spines appear very much earlier in the ontogeny than is the case in the Devonian species studied by Raymond (46), therefore showing a considerable degree of acceleration of this character during the interval from the Devonian to the Upper Carboniferous.

Other interesting cases of recapitulation among brachiopods have been pointed out by Beecher and Schuchert (12) in the development of the brachial apparatus in *Dielasma* and *Zygospira*.[1]

Trilobita.—Studies of the early stages of the development of trilobites have been published by Barrande (3, 4), Walcott (59, 60, 61), Beecher (8, 9), Matthew (39, 40, 41) and others, but for indication of the correlation of the ontogeny and the phylogeny in this class we are almost entirely indebted to Beecher. In his papers on "Larval Stages of Trilobites" (8), and a "Natural Classification of the Trilobites" (9), he has not only pointed out the remarkable way in which characters are recapitulated in this class, but has also proposed what is probably to be regarded as the most perfect example of a phylogenetic classification of a group of organisms, in existence.

The earliest developmental stage of trilobites that has ever been found (barring supposed trilobite eggs) is the larval stage or "protaspis," as it is called by Beecher (8). The protaspis is a minute body of ovate or discoid shape, and about a millimeter in length. This larval stage has

[1] For additional examples of recapitulation among the brachiopods see the following: Beecher, C. E., Studies in Evolution (a series of collected papers), Scribners, 1901. Beecher, C. E., and Clarke, J. M., The Development of some Silurian Brachiopoda, *Mem. N. Y. State Mus.*, No. I, 1889. Beecher, C. E., and Schuchert, C., Development of the shell and brachial supports in Dielasma and Zygospira, *Proc. Biol. Soc. Washington*, vol. viii, 1893. Cumings, E. R., The morphogenesis of Platystrophia; A study of the Evolution of a Paleozoic Brachiopod, *Am. Jour. Sci.*, vol. xv, 1903. Raymond, P. E., The developmental change in some common Devonian brachiopods, *Am. Jour. Sci.*, vol. xvii, 1904. Greene, F. C., The development of the Carboniferous brachiopod Chonetes granulifer. Owen, *Jour. Geol.*, vol. xvi, 1908. Buckman, S. S., Homeomorphy among Jurassic Brachiopoda, *Proc. Cotteswold Nat. Field Club*, vol. xii, 1901.

been seen in a sufficiently representative series of genera to make it reasonably certain that it is the common larval type among the trilobites.

It is pretty well established that the eye of crustaceans has migrated from the ventral to the dorsal surface of the cephalon. At an intermediate stage in this process the eyes would appear on the margins of the cephalon. If this has been the history of the eye, the most primitive larvae should show no evidence of eyes on the dorsal surface, and since the eye is on the inner margin of the free cheek, there should be no evidence of the free cheek. This is exactly the case in the youngest larvae of *Ptychoparia*, *Solenopleura* and *Liostracus*, "which are the most primitive genera whose protaspis is known. The eye-line is present in the later larval and adolescent stages of these genera, and persists to the adult condition. In *Sao* it has been pushed forward to the earliest protaspis, and is also found in the two known larval stages of *Triarthrus*. *Sao* retains the eye-line throughout life, but in *Triarthrus* the adult has no traces of it, and none of the higher and later genera studied has an eye-line at any stage of development." This character according to Matthews, is characteristic of the Cambrian trilobites. In its phylogenesis in later trilobites it disappears first from the adult stages, and is finally lost from the entire ontogeny. The eyes appear on the margin of the cephalon in the last larval stage of *Ptychoparia*, *Solenopleura*, *Liostracus*, *Sao*, and *Triarthrus*. In the later genera the eyes are present "in all the protaspis stages, and persist to the mature, or ephebic condition, moving in from the margin to near the sides of the glabella."

According to Beecher (8) "A number of genera present adult characters which agree closely with some of the larval features [of later genera]. The main features of the cephalon in the simple protaspis forms of *Solenopleura*, *Liostracus*, and *Ptychoparia* are retained to maturity in such genera as *Carausia* and *Acontheus*, which have the glabella expanded in front, joining and forming the anterior margin. They are also without eyes or eye-line. *Ctenocephalus* retains the archaic glabella to maturity, and likewise shows eye-lines and the beginnings of the free cheeks (larval *Sao*). *Conocoryphe* and *Ptychoparia* are still further advanced in having the glabella rounded in front, and terminated within the margin (larva of *Triarthrus*). These facts and others of a similar nature show that there are characters appearing in the adults of later and higher genera, which successively make their appearance in the protaspis stage, sometimes to the exclusion or modification of structures present in the most primitive larvae. Thus the larvae of *Dalmanites* and *Proetus*, with their prominent eyes, and glabella distinctly terminated and rounded in front, have characters which do not appear in the larval stages of ancient genera, but which may appear in their adult stages. Evidently such modifications have been acquired by the action of the law of earlier inheritance or tachygenesis."

Bryozoa.—My studies (17, 18) were the first to show that there is in the bryozoan colony a definite recapitulation of ancestral characters, and that in this particular the colony behaves as an individual. This same fact was very clearly pointed out by Ruedemann (47) two years earlier in the Graptolites, and I take pleasure in quoting his very explicit statement. He says: "Furthermore the fact that the thecæ within the same colony show a gradation from phylogenetically older to younger forms, and therefore analogous to the organ of a growing individual, pass through ancestral stages, as, e. g., do the septa of a cephalopod shell, demonstrates how closely the zooids of this colony were united into one organism, and that practically they were more the organs of an individual than the component of a colony. . . . . If the graptolites so closely approached the morphologic value of an individual, it may be expected that, like an individual, the whole colony has its ontogeny and repassed ancestral stages."

My studies, referred to above, brought out the fact that the bryozoan colony begins as a minute hemispherical body, the "protœcium" which is the earliest exoskeletal stage of the first individual of the colony. This protœcium (basal disc) is very conspicuous in the Cyclostomata, and also in the ancient Cryptostomata (as shown in *Fenestella*).[1] It can not be definitely asserted that the protœcium corresponds to any ancestral bryozoan, but the marked resemblance of the zoœcia of some of the ancient *Stomatopora* of the Ordovician to the protœcium is at least very suggestive.

The ancestrula, or first complete individual of the colony, has long been known to present characters more similar to those of ancestral forms

[1] I first used the term protœcium as the designation of the first individual of the colony, and in this sense it would be exactly equivalent to the term ancestrula of Jullien. In a later paper (18) I restricted the term to the basal disc (of Barrois) which is the calicified wall of the metamorphosed and histolyzed embryo in its earliest sedentary stage. Out of this basal disc the first normal individual arises by a process stirctly analogous to budding. In this sense, therefore, the term protœcium is exactly correlative with the terms protegulum, protoconch, prodissoconch, etc.

than the characters of the ephebastic zoœcia (see Nitsche 44, and Pergens 45). I have succeeded in finding evidence (18) that this is true to a notable extent in the ancient *Fenestella*, where the tubular ancestrula bears a striking resemblance to the simple tubular ephebastic zoœcia of the Cyclostomata, from which group there is every reason to believe the Cryptostomata are descended.

It is also pointed out by Nitsche and Pergens *(loc. cit.)* that the earlier budding habit of the colony is similar to ancestral types. In my own studies I was able to show that the early budding habit is very uniform in the most diverse types of Bryozoa, and that it corresponds to the budding habit that prevails throughout the astogeny of the reptant stomatoporas.

In *Fenestella* my studies indicate that the earlier individuals (nepiastic) of the colony are very different from the adult (ephebastic) individuals and are strikingly similar to the ephebastic individuals of certain Cyclostomata that are on morphological grounds, as pointed out by Ulrich (63), probably ancestral. And again, the early neanastic zoœcia of the Devonian fenestellas studied are almost exactly like to the ephebastic zoœcia of the fenestellas of the Niagara series. Unpublished studies indicate that in the Fenestellas of the Upper Carboniferous the neanastic stage is more abbreviated, and that the adult type of zoœcia follows more closely upon the nepionic type.

Dr. Lang of the British Museum has published very interesting studies of the Stomatoporas and Eleids of the Mesozoic (35, 36, 37), and has come independently to exactly the same conclusions as the writer in regard to the development of the colony, and the relations of astogeny and phylogeny among the Bryozoa. He says (35), "The development of the colony is comparable with and follows the same laws as the development of the individual." And again: "Among Jurassic forms of *Stomatopora* and *Proboscina* it has been found that when any given character, such, for instance, as the ratio of the length of the zoœcium to its breadth, is followed from the first zoœcium to the last, that it has a progressive development, or anagenesis, reaches a maximum, or acme, and often may be seen to have a retrogressive development, or katagenesis, in the ultimate branches of the zoarium."

Lang has paid especial attention to the manner of branching in Jurassic stomatoporas. The nearly universal method of branching in the Jurassic members of this group is by dichotomy. This according to Lang may be by one or other of three types as follows: In type I the two zoœcia are separate throughout their entire length, only touching at their bases. In type II they are contiguous throughout their length, and in the intermediate type they are contiguous for part of their length. To a large extent correlated with these types of dichotomy is the angle of divergence of the branches.

In all the Jurassic stomatoporas and in a few proboscinas the first dichotomy is according to type I, and at a very wide angle (180°). The second dichotomy, in the majority of cases, is also according to type I, with an angle of 120°. The next is commonly only 90°, the next 60°, and the next 45°, all according to type I. "In primitive [Jurassic] forms the branching never gets beyond type I with a small angle. In the majority of forms, however, sooner or later the intermediate type of branching comes in, and in a great many forms this type is the final one. In a few cases of *Stomatopora*, and in all *Proboscina*, type II is at some time or other reached, and remains the ultimate form of branching of the zoarium. This sequence namely, Type I—Intermediate type—Type II, is invariably followed." (35).

In primitive *Proboscina* (a genus derived from *Stomatopora*) the first dichotomies are according to type I. "In the typical forms of *Proboscina* the early stages have been so condensed according to the law of acceleration (Tachygenesis), that the first dichotomy is formed on type II. . . . . In the more advanced types of *Proboscina* . . . . the arrangement of peristomes is irregular from the first." This is the typical arrangement for *Bernicea*, a derived genus of which *Stomatopora* and *Proboscina* are the first two terms. It is worthy of notice that while in the Jurassic forms of *Stomatopora* type II is not very common, it is extremely common in the Cretaceous forms.[1]

Graptolites.—The beautiful researches of Ruedemann in this group have shown us, as pointed out above, that the graptolite colony closely approaches the morphologic value of an individual, and that, like the individual, it presents definite ontogenetic (astogenetic) stages. Ruedemann (47) applies to the colonial development the terminology proposed by

[1] For studies in the zoarial development of Bryozoa see Cumings, E. R., The development of some Paleozoic Bryozoa, *Am. Jour. Sci.*, vol. xvii, 1904; Development of Fenestella, *Am. Jour. Sci.*, vol xx, 1905. Lang, W. D., The Jurassic forms of the 'genera' Stomatopora and Proboscina, *Geol. Mag.*, Dec. v, vol. i, 1904; The Reptant Eleid Polyzoa, *Geol. Mag.* Dec. v, vol. iii, 1906; Stomatopora antiqua, Haime, and its related Liassic forms, *Geol. Mag.*, Dec. v, vol. ii, 1905.

Hyatt (31). In a later paper, however, he approves the terminology introduced by me, and proposes to call the development of the colony the astogeny (48).

The embryonic stage of the graptolites is represented by the initial portion of the sicula (first zooid), according to Ruedemann; and Holm (29) asserts that the more pointed end of the sicula "corresponds to the original chitinous covering of the free zooid germ or embryo." This initial part of the sicula, according to Ruedemann, holds a position similar to the protoconch of the cephalopod shell.

In part I of his splendid monograph of the Graptolites (48) of New York, at page 530, Ruedemann says: "It has been pointed out in a former publication that not only did there exist in the graptolites ontogenetic growth stages in the development of the individual zooids, ... *but the rhabdosomes in toto and in their parts, the branches, seem also to pass through stages which suggest phylogenetically preceding forms.*"

Of the various ways in which these astogenetic stages express themselves, Ruedemann mentions the following: "The original direction of growth of the branches of the Dichograptidæ has been in the approximate continuation of the sicula, i. e., an ascending erect position as long as the rhabdosomes were sessile, on the ground. These became pendant when the graptolites attached themselves in a suspended position to seaweeds, as numerous hydroids do today. To restore to the zooids their original . . . . erect position, the branches began now to recurve . . . . [becoming progressively horizontal, reflexed, reclined and recumbent] . . . . We find now in the majority of the Dichograptidæ with the above cited growth directions of the branches, that the latter still retain their original dependent direction, in the proximal parts in some species . . . . while in others by the law of acceleration, the dependent proximal direction has already changed into a horizontal one . . . . the change in direction becoming progressively more abrupt as the final direction of the branches becomes reclined . . . . or recumbent. . . . . The branches pass hence, in their development, through different directions representing ontogenetic stages that repeat stations in their phylogenetic development." (48.)

An analogous fact is found in the character of the thecæ. "A comparison of the form of the thecæ of the youngest dichograptid genera . . . . with that of the older and presumably phylogenetically preceding genera . . . . shows that in general the older genera have the more tubular, simpler thecæ, with the less protected apertural margins. It is, hence, apparent that the stolonal or earlier thecæ of the rhabdosomes represent indeed the older types of thecal form." (48.)

Other Classes.—The case of the larva of *Antedon* has already been referred to. As pointed out by Bather (1), the stem ossicles of the larval *Antedon* are of a complex and specialized type, and in a general way resemble the stem ossicles of the Bourgueticrinidæ of the Upper Cretaceous. It is held by Bather that the structures of the adult ancestors have been pushed back by acceleration to the larval stages of the existing *Antedon.*

Recapitulation is also shown in the anal plate of *Antedon.* The anal plate appears between two of the radials and on the same level with them. Subsequently it is lifted out from between the radials, and the latter close beneath it. Still later the anal plate is resorbed entirely. That this is the recapitulation of an adult character and not of a larval character, as contended by Hurst, is shown by the fact that the oldest crinoids do not possess the anal plate at all. It appears from paleontological evidence that this plate first appeared above the level of the radials, that it gradually sank down between the two posterior radials, and that at a far later period (at about the close of the Paleozoic) it gradually passed upward again as it does in *Antedon*, and eventually disappeared.

Jackson has shown that there is good evidence of recapitulation among the fossil echinoids (33). In most regions of the echinoid the development is obscured by the more or less extensive resorption, but the plates of the corona may show by their position and number, the course of development. Jackson holds that the introduction of columns of plates, both interambulacral, and ambulacral, in *Melonites*, etc., indicates the stages of growth through which the individual has passed in its development. He shows that two columns of ambulacral plates "may be accepted as the usual characteristic of the whole class, which finds its representative in the majority of the adults, in nearly all young, and in the adult of the simplest and oldest known type, *Bothriocidaris.*"

Interambulacral areas originate ventrally in a single plate. Only one genus is known, however, that has a single row of plates in the adult, namely *Bothriocidaris.* This is the simplest known and "perhaps the simplest conceivable echinoid."

In *Goniocidaris* the interambulacral plates of the adult are approximately hexagonal in form instead of pentagonal. "The relative form of the plates in young *Goniocidaris* is almost exactly the same as in the primitive type, *Bothriocidaris*."

"The early stage in which we find a single interambulacral plate, together with two ambulacral plates, in each area is so important that it is desirable to give it a name, the protechinus stage. The protechinus is an early stage in developing Echini, belonging to the phylembryonic period, in which the essential features of the echinoid structure are first evinced. . . . . This protechinoid stage of Echinoderms is comparable as a stage in growth to a similar stage which is expressed in the protegulum of brachiopods, the protoconch of cephalous mollusks, the prodissoconch of pelecypods, and the protaspis of trilobites." (33.)

Miss Smith (Mrs. Alexander Shannon) has shown very conclusively the exact resemblance of the form of the young *Pentremites conoideus* to the adult *Codaster* (52). In *Codaster* the conical form, narrowest at the base and enlarging upward, is maintained throughout life. In *Pentremites* only the early stages of growth have this form, while the adult is broadest at the base and narrowest at the top.

This evidence from development would, according to the theory of recapitulation, indicate that *Codaster* stands in an ancestral relation to *Pentremites*, and it is therefore of importance to the theory that Bather (2) from other evidence has independently reached the same conclusion as Miss Smith in regard to the relationship of the two forms.[1]

Among corals Beecher (5) has worked out the development of *Pleurodictyum lenticulare* and concludes that the first neanic stage, in the manner of growth and the structure of the corallum, is very suggestive of *Aulopora*, and should be given considerable significance." Girty (21) comes to the same conclusion from a study of *Favosites forbesi*, etc.

Bernard (14) has shown that the coral colony in similar fashion to the bryozoan colony and the graptolite colony behaves as an individual. In another paper (13) he has recognized as the first growth stage of the

[1] Bather's conclusion was published in 1900, and Miss Smith's paper in 1906. The latter, however, was not aware of Bather's views as to the relationships of these two forms, so that the conclusions of the two workers, arrived at independently and from different lines of evidence are all the more important and convincing. Bather says in a review of Miss Smith's paper that he considers Pentremites as the "extreme link in the series *Codaster—Phaenoschisma—Cryptoschisma—Orophocrinus—Pentremitidea—Pentremites.*"

coral skeleton the "prototheka," or basal cup of the first individual of the colony.[1]

Lang (38) has written a very suggestive paper on growth stages of British species of corals, in which he points out the fact that the ontogenetic stages are repeated in each rejuvenescence (branching?), and suggests that we have here an example of localized stages in development (see Jackson 34). It may be remarked at this point that Ruedemann has also detected localized stages in graptolites (47, 48), and Lang in Bryozoa (36). Lang also, in the paper on corals, concludes that there is recapitulation in the coral genera studied by him, of ancestral characters, and he gives a table illustrating this.[2]

Summary.—Paleontologists almost universally accept the theory of recapitulation. Its chief critics have been embryologists. The reason for the difference in attitude is probably to be sought in the fact that the former ordinarily compare epembryonic stages with adult characters of geologically older species, while the latter too often compare embryonic stages with the adult stages of existing species. It is also to be noted that in recapitulation we have to do with morphological and not with physiological characters, and that the row of cells from the egg to the adult may be morphologically the same in two organisms, while being at the same time physiologically different. Until it can be shown that two organisms morphologically different in the adult must of necessity be morphologically different at all stages, the argument of Montgomery, Hurst and others proves nothing.

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[1] The term *prototheka* was proposed simultaneously (January, 1904) by Bernard and myself for the earliest skeletal structure of the coral colony. We have used it, however, in a slightly different sense. Bernard applies it not only to the first individual of the colony, but also to the basal plates or cups of later individuals. I intended to restrict it to the basal cup of the first individual. The references are as follows: Bernard, H. M., The prototheka of the Madreporaria, with special reference to the genera Calostylis, Linds., and Mosleya, Quelch. *Ann. Mag. Nat. Hist.*, Ser. 7, vol. xiii, Jan. 1904. Cumings, E. R., The development of some Paleozoic Bryozoa. *Am. Jour. Sci.*, vol. xvii, Jan., 1904 (footnote, p. 74).

[2] This so-called rejuvenescence in corals appears to be a species of budding, in which the bud is directly superimposed upon the parent. It is fission occurring in a horizontal plane, as suggested by Bernard (14), and the new skeleton is in direct continuity with the old. This is the same idea exactly as that advanced by Ulrich some years ago (63) to account for the diaphragms of the Bryozoa Trepostomata. In the case of the Trepostomata the zoœcium is frequently operculate (ex. *Callopora*), and there is good evidence that the bud grows up through the operculum hence leaving it behind as the floor of the new individual.

In the Cephalopoda, Pelecypoda, Gastropoda, Brachiopoda, Trilobita, Bryozoa, Graptolites, Echinoderms and Corals, examples are pointed out in which there is clear and unmistakable evidence of recapitulation. In most of these cases it is the epembryonic and not the embryonic stages that are the basis of comparison.

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The Tippecanoe an Infantile Drainage System.

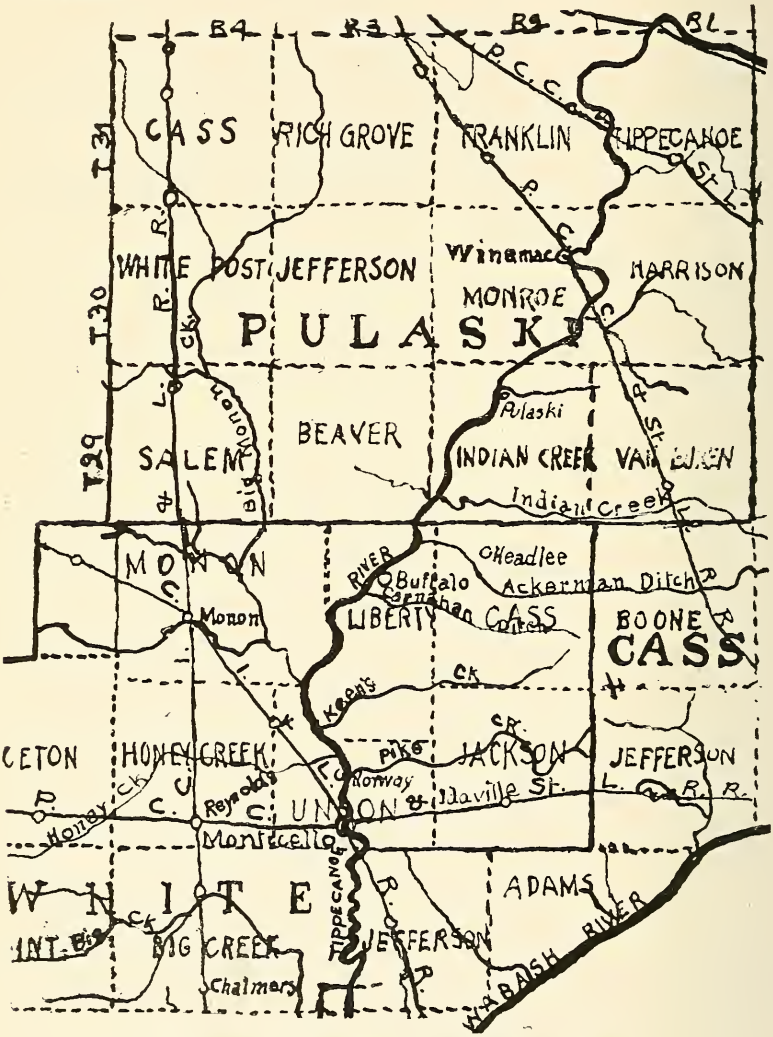
By Wm. A. McBeth.

Streams first come into existence on a recently emerged or uncovered land surface, with enough rainfall to leave a surplus for runoff after the requirements for soil saturation and evaporation have been met. An uplift of part of the sea bottom, the drainage of a lake or the melting of an ice sheet may produce the new surface on which the streams begin their cycle of existence and work.

Most of the streams of northern Indiana are in the youthful stage. They came into being with the recession of the North American Ice Sheet from that part of the State. If parts of the region retained areas of marsh, pond or lake, the location of streams in such areas would be delayed until outlets could be made by the intrenchment of channels by outflowing waters to such depth that the impounded waters would be drained off, when stream lines would be laid out on such newly uncovered lands.

The Tippecanoe river between the abrupt bend on the northeast corner of Pulaski County and Monticello in White County, with its tributaries, furnishes a fine example of extremely young drainage. This section of the river evidently traverses the bed of a former temporary lake which was held in by a moraine at Monticello. Evidence of this lake remains in the sand ridges, some of which seem to be beaches and others dunes numerous in the region. The sudden change in the width and depth of the river valley above Monticello also is significant of such a condition. The valley at Monticello is almost exactly 100 feet deep, and from one-fourth to one-half mile wide, and at Buffalo ten miles north of Monticello the channel is about 25 feet deep and is without floodplain or bluffs. In brief, the channel is just cut deep and wide enough barely to carry the flood waters. The trusses of the highway bridges crossing the river in Pulaski County can be seen miles away across the level prairie. The bridge floor at Winamac is level with the streets of the town. The river has a steep slope through this part of its course, the fall from Winamac to Monticello being not less than 100 feet in thirty miles.

The tributaries to the main stream in this region are examples of still younger drainage. In following the road from Monticello to Buffalo the way is over level country, except that where streams making their way west to the river are crossed, the road descends ten or fifteen feet to the bridge crossing the stream, then rises by the same distance to the level plain again.



The Drainage of Pulaski and White Counties, Indiana.

Persons going to Headlee, Pulaski or Winamac (see map) often turn east a short distance north of Monticello and going about three miles they turn north again, following the range line road. (Between R. 2 and R. 3 W.) This road is much more level. The streams crossed in small valleys on the west or river road have no valleys where crossed by the range line road. Formerly they existed as broad sloughs or strips of marsh land, and where crossed stretches of corduroy road were used to enable teams and vehicles to cross without miring. Scarcely the beginning of a channel could be discovered threading its way through the lowest part of the marsh or slough. The drainage was by over wash or sheet streams spreading out many rods in width and slowly creeping away to the river. Within the last mile or two of their course the channel became gradually deeper and wider and the stream sped freely down a steeper slope into the river. These sheet streams are good examples of the primary drainage or runoff. They were interrupted frequently by ponds and broader widths of marsh, keeping large areas so wet as to make cultivation impossible—the land furnishing a poor quality of pasturage.

Within the last few years man has done by machinery what nature has not done and could not do in thousands of years. Starting at the head of the sloughs fifteen to twenty miles from the river steam dredges have been used to dig channels for these over wash waters and practically every slough on both sides of the river in all this region has been furnished a channel ample for its drainage.

Pike Creek, Keen's Creek, the Carnahan Ditch, the Ackerman Ditch and Indian Creek on the east side of the river, and the Monon Creeks, Honey Creek and others on the west side, furnish examples of infant drainage aged by the aid of man in pushing forward the work the waters were so tardily doing.

A Paired Entoplastron in Trionyx and its Significance.*

By H. H. Lane.

There is no order of reptiles more distinctly circumscribed than the Testudinata. Even the fossil remains cast little if any light upon their affinities. That they are a highly specialized group need not be argued. Any point, therefore, which gives an indication of what may be considered to have been a primitive condition in the order, is of extreme interest and value.

Moreover, there has been much discussion as to the relative rank of the various suborders and families comprised in this order. A group concerning which there is much diversity of opinion is that now generally regarded as constituting a suborder, the Trionychia. Some have seen in their so-called "soft-shelled" condition, evidence of extreme specialization, and have therefore assigned them to a very high position in the order. Thus, Gadow (Cam. Nat. Hist., vol. viii, p. 406) asserts that "It is not open to much doubt that the characteristic features of the Trionychoidea are not primitive but secondary. This is indicated by the whole structure and behavior of the carapace and plastron. The softening of the whole shell, the loss of the horny shields, the reduction of the claws, are the direct and almost unavoidable results of life in muddy waters." Other authorities take exactly the opposite view, and from the same facts reach the conclusion that "the Trionychidæ stand nearest to the general structural plan of the Reptilia" (Adolph Th. Stoffert, Structure and Development of the Shell of Emyda ceylonensis, Gray).

On account of this difference of opinion the writer has undertaken a study of the embryonic development of *Trionyx* with the view, *first*, of determining, if possible, the relative position of the Trionychia among the Testudinata, and, *second*, if it should prove to be a comparatively generalized type, to secure some hint as to the reptilian form from which the chelonian ancestry may have been derived. I present in this paper only one phase of the evidence furnished by the plastron, relative to the first of these two problems, although my material sheds some light upon both.

* (Contribution No. 5, from the Department of Zoology and Embryology, State University of Oklahoma.)

No other terrestrial or freshwater tortoises possess so simple and perhaps so primitive a type of plastron as that found in the Trionychia. In the adult *Trionyx (Aspidonectes) spinifer*, the plastron (Fig. 1) is composed of nine elements, four paired and one unpaired, separated to a greater or less extent at first by three, and later sometimes by only two, large fontanelles. Different authors have proposed different theories relative to the homologies of these plastral bones, and along with these theories there has arisen a complex terminology. Each author has sought to give permanency to his own hypothesis by assigning to the plastral elements names indicative of his view. Thus the unpaired element is designated by G. St. Hilaire, Owen, Ruetimeyer, and others, who regard the plastron as the homologue of the amniote sternum, as the "ento-sternal"; Parker calls it the "inter-thoracic plate"; while Huxley gives it the noncommittal name of "ento-plastron," in which he is followed by most later writers. The four paired elements of the plastron have not fared any better. Thus, G. St. Hilaire, Owen and Ruetimeyer designate them as "episternal," "hyosternal," "hyposternal," and "xiphisternal," respectively; Parker, as usual, has his own set of terms, and calls them "praethoracic," "post-thoracic," "praeabdominal," and "abdominal" plates; while Huxley gives them the names of "epiplastron," "hyoplastron," "hypolastron," and "xiphiplastron." In the present state of our knowledge it is best, perhaps, to use Huxley's terms, since they commit one to no special theory regarding the homologies of the elements to which they apply.

Among the various attempts that have been made to homologize the plastral plates with certain skeletal elements of other amniotes, one of the earliest was that of Cuvier (Regne animal, Les Reptiles, p. 10), who identifies them with the sternum of the Lacertilia and higher vertebrates. G. St. Hilaire (Philosophie anatomique, vol. i, p. 106) makes a detailed comparison between the several parts of the plastron and the osseous pieces of the avian sternum. Carus (Von den Ur-Teilen des Knochen- und Schalengeruestes, 1828), and Peters (Observationes ad Anatomiam Cheloniorum, Berolini, 1838), maintain that it is only partially equivalent to the sternum. Owen (On the development and homologies of the carapace and plastron of the Chelonian Reptiles, Phil. Trans. London, 1849), advances the idea that the paired plates correspond to haemapophyses of the ribs. Rathke (Ueber die Entwickelung der Schildkröten, Braunschweig, 1848), holds the plastron to be wholly dermal in origin and hence a structure not to be homologized with the endoskeletal elements of other groups. Many of the more recent authorities, beginning with W. K. Parker (Structure and development of the shoulder girdle and sternum in the vertebrata, London, 1868), and Huxley (The Elements of Comparative Anatomy, London, 1864), consider the epiplastra and the entoplastron to be the homologues of the clavicles and interclavicle respectively, of other reptiles.

In form the entoplastron is quite as variable among the Testudinata generally, as are the paired elements associated with it. It is perhaps most frequently T-shaped or roughly triangular, with the apex of the triangle directed caudad. In *Trionyx*, however, it has an entirely different configuration, being in the form of a wide V with the apex or point directed cephalad (Fig. 1).

The other elements of the plastron have outlines and relationships characteristic of the family and can be easily identified by reference to the figure (Fig. 1), wherein the epiplastra (epi) are shown immediately cephalad of the entoplastron (ento), while the hyoplastra (hyo), hypoplastra (hypo), and xiphiplastra (xiph), lie caudad to that element in the order given.

In a Trionyx embryo with a carapace length of 14 mm., the elements of the plastron are all definitely laid down (Fig. 2). The nuchal plate of the carapace is a well marked and clearly defined dermal bone having as yet no connection with a vertebra. The ribs are fully laid down in cartilage, but there are no traces of costal plates, and neurals, likewise, are not present. The plastral elements are not only all present but they are also *all paired*. They are not preformed in cartilage but consist entirely of ossifications within the dermis. In shape and size they are clearly defined. As shown in the figure (Fig. 2) they form a series of five pairs of more or less rod-like structures, which are not in contact with one another, as is the case in the adult (Fig. 1), but on the contrary they are separated by comparatively large spaces in which the tissue of the dermis is clearly mesenchymatous and shows no trace of ossification. The position of the five pairs in two longitudinal rows and their absolutely similar origin as entirely dermal ossifications make it certain that, whatever their homology to structures in other forms may be, they must all be interpreted as serial homologues of each other. While it is agreed that the hyoplastra, hypoplastra, and xiphiplastra are the homologues of the abdominal ribs found in the Crocodile and Rhynchocephalia, the epiplastra and entoplastron are pretty generally regarded as representing the clavicles and interclavicle of other reptiles.

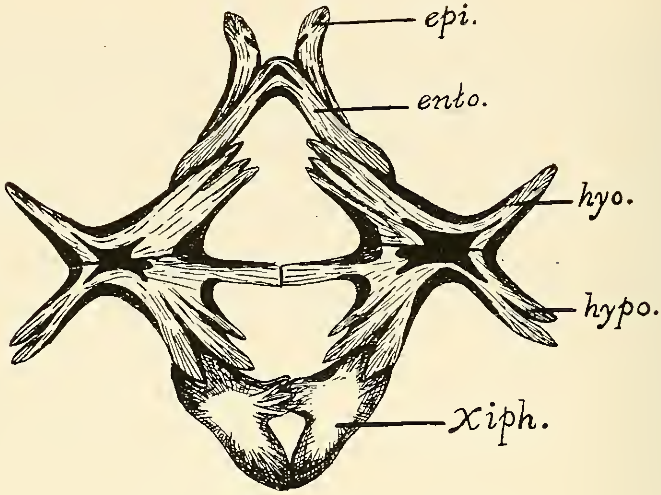


Fig. I

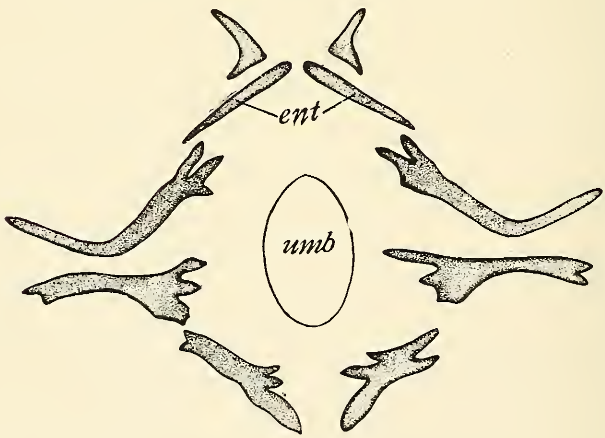


Fig. 2

EXPLANATION OF THE FIGURES.

Figure 1 shows the form and arrangement of the plastral elements (reduced in size) of an adult *Trionyx spinifer*, epi., *epiplastron;* ento., *entoplastron;* hyo., *hyoplastron;* hypo., *hypoplastron;* xiph., *xiphiplastron.*

Figure 2 is a graphic reproduction, magnified ten times, of the plastral elements in embryo *Trionyx spinifer* with a carapace length of 14 mm. umb., *umbilicus;* ent., *paired entoplastra.*

Accepting merely for the moment the correctness of this homology, it is interesting to note how very rarely a paired interclavicle has been found in reptiles. So far as I have been able to discover Parker is the only one heretofore to report such an observation, and in his monograph on the structure and development of the shoulder girdle and sternum, cited above, describes the interclavicle of *Anguis* as developing from paired elements and says:

"Above the Ganoid Fishes, this is the only instance I can give at present of the primordial symmetry of the interclavicle; but a careful study of the development of this bone in embryo lizards would, very probably, show it to be not at all rare" (p. 99).

Examining the question of the homologies of the plastral elements a little more closely, however, one is led to doubt Huxley and Parker's identification of the epiplastra and entoplastron as clavicles and interclavicle respectively. In all other rept s so far as known the clavicle is laid down, at least partially, in cartilage and in close connection with the other elements of the shoulder girdle. Even in the mammals, while its origin is still a matter for further investigation, it is definitely established that a portion at least of the clavicle is preformed in cartilage. In *Trionyx*, as in other of the Testudinata, the epiplastra, on the contrary, develop entirely without connection with the shoulder girdle, entirely outside the muscular layer of the body wall and within the much thickened dermis. They, in company with all the other plastral elements, are wholly without a cartilaginous preformation, and develop as direct ossifications in the dermal mesenchyme. Without further evidence it is very difficult to accept the view that the epiplastra are the testudinate homologues of the clavicles and the same arguments hold in regard to the identification of the entoplastron with the interclavicle. As is shown in this paper, the entoplastron in *Trionyx* is at first a pair of elements, so that there is nothing to prevent the interpretation of the entire series of plastral bones as the homologues of the so-called abdominal ribs so well known in Sphenodon and the Crocodilia.

Recurring to the question of the relative rank of the Trionychia among the Testudinata, the paired condition of the entoplastron, as it exists in this embryo (Fig. 2, ent) is especially important and instructive. As Rathke first pointed out, the entoplastron is wanting in *Sphargis*, perhaps on the whole the most specialized of all the Testudinata. It is reported by Stannius (Handbuch der Anatomie der Wirbeltiere, 1854) as absent also in Staurotypus, while L. Agassiz (Contributions to the Nat. Hist. of the U. S. A., vol. I, p. 267) states that it disappears in old specimens of other Cinosternidæ. With these exceptions the entoplastron occurs as a single median bone in all known species of turtles and tortoises both living and fossil, save where in some of the latter the fragments are too meagre to permit its presence or absence being positively determined. It is therefore phylogenetically a very old element in the testudinate skeleton, and was probably, in some form or other, a direct inheritance from the more generalized reptilian stock from which this order arose.

It follows, therefore, that we have in the paired entoplastron of the embryo *Trionyx*, a very primitive character, so primitive, indeed, that it occurs nowhere in the adult of any known species of Testudinata either living or fossil. It is therefore an indication that *Trionyx* is to be regarded as more primitive than any other known genus of the order. Were this the only evidence of primitiveness known to occur in *Trionyx*, one would not, perhaps, be justified in making so broad an assertion. But a considerable amount of corroborative evidence is also at hand. Thus in *Trionyx*, the atlas is temnospondylous, i. e., its three constituent parts, the neural arch, the centrum, and the intercentrum, are not ankylosed but remain loosely connected, there is no odontoid process on the second vertebra, the first centrum being freely movable on the second; the pubic and ischiadic symphyses are broad and are connected with each other by a longitudinal cartilaginous band, which is replaced in other testudinates, except *Chelone*, by a broad completely ossified plate (Gadow). In the young of all tortoises, but in the adult only in the Chelonidæ and Trionychidæ, the plastral plates are separated by large fontanelles (Fig. 1, f). And finally, as reported by Wiedersheim (Vergl. Anat. der Wirbelthiere, 5. Auflage, 1902) teeth rudiments also occur in the embryo of *Trionyx* and *nowhere else among the Testudinata*. I have not been able so far to corroborate this observation, but it is certainly, if correct, a most important argument in favor of the view herein set forth.

This conclusion regarding the Trionychia is not invalidated by certain secondary specializations, such as the flatness of the body and the webbed feet, all clearly adaptations to an aquatic habitat. However, these adaptations do show that *Trionyx* is in no sense directly ancestral to the other Testudinata; the Trionychia are to be regarded as an early offshoot of the main stem, which has retained certain of its primitive characters.

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Norman, Oklahoma.

Notes on Parasites Found in Frogs in the Vicinity of St. Paul, Minn.

By H. L. Osborn.

(Abstract.)

Our knowledge of the parasites of even the commonest animals is very incomplete. Examinations of all the organs and at all seasons of the year and extended over a period of several years have never been made except, possibly, for a few of the domesticated animals where the information possessed an evident and immediate utilitarian bearing. Such studies of a number of common and abundant animals are much to be desired. If a body of such information were available it would be of great service to students of the trematodes and very likely make it possible to complete many life histories, only fragments of which are known at the present time. The present paper is a first step in an attempt to do this with reference to the common frogs in the neighborhood of St. Paul. Twenty-one frogs were examined in June, seven in September and nine in November. These numbers are found to be too small for anything but a preliminary survey of the ground and larger numbers will be examined next year. The walls of the cœlom, particularly in the dorsal and anterior regions, are infected by nearly mature encysted individuals of *Clinostomum marginatum*, Rud. This form has been reported hitherto only from fish and fish-eating birds. The pericardial cavity, especially in frogs during June, was found to contain oval cysts, sometimes grouped in masses, each cyst containing a distome so immature that its generic affinities cannot be determined from the data furnished by a study of its structure. It may turn out to be a missing early stage of some trematode whose later stages are already known. The urinary bladder in a considerable fraction of the frogs examined harbors a species much like, if it is not identical with, the *Gorgodeda attenuata* which Stafford has described from a similar location in the frogs of Canada. A member of the Amphistomidæ occurs occasionally in the urinary bladder but is more characteristically a parasite of the rectum, where it is found at all seasons. In one instance *Cephalogonimus* was found in the rectum and small intestines. In a few cases a

cestode was found in the small intestine. also in the cœlomic cavity beside the small intestines and in cysts on the surface of the liver. The lungs contain *Distomum lanceolatum* in a large percentage of cases and a nematode also in many instances.

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St. Paul, Minn.

The Mocking-bird at Moores Hill, Ind.

By A. J. Bigney.

The purpose of this brief article is to show how this bird acts on entering a new community, and to give evidence of its enlarging field of activity.

In Mr. Butler's catalogue of the Birds of Indiana [1] in 1897, they were reported in twelve counties in small numbers. In recent years they are migrating in large numbers into the counties of southern Indiana. In 1905, about April 1st, the first mocking-bird was seen in the outskirts of Moores Hill. It was rather shy, but made its whereabouts known by its incessant singing, not only in the daytime, but also during most of the night. Such singing had never been heard by our citizens. It continued this behavior for about ten days, then left the community. The next season a pair returned to the same place and the air was again filled with their music. Their usual imitation of the notes of other birds was a marked characteristic. This season they nested in the honeysuckle vine alongside a neighbor's house. They remained until late in the fall and then migrated southward. During this season one other pair was seen about two miles from town.

The following season several pairs were seen in and about town. The last two seasons the numbers have gradually increased, so that now they constitute one of our regular bird inhabitants.

The question naturally comes up, why have they begun their rapid advance into the north during the past few years? I can not answer this question. I have heard that a kind of ant is troubling them in their nesting and so they migrate to get rid of them. If any positive information can be given, I should be glad to know of it.

Moores Hill College,
Moores Hill, Ind.

[1] Amos W. Butler. The Birds of Indiana. Twenty-second Annual Report of the Department of Geology and Natural Resources of Indiana, 1897.

Observations on Woodpeckers.

By John T. Campbell.

In May, 1883, I was surveying to build a levee along the east side of the Wabash River in Parke County, Indiana, from the mouth of Big Raccoon Creek southward to within a mile of the south boundary of the county—twelve miles long. Near the south end of this levee was a wide bottom, in which I had surveyed before it was cleared. Joseph J. Daniels, of Rockville, Indiana, bought this land, cut out the saw timber and deadened the remainder. In the spring of 1882, these deadened trees had decayed enough for the woodpeckers to bore holes for their nests. There were easily one thousand such trees on this seven hundred acres. Each tree had from three to twenty woodpecker holes. The marks of the great flood of 1883, in February, were very plain and could be recognized several years later. Of all those, probably ten thousand holes, not one was below the flood mark of the water of 1883. On the east side of the bottom the ground was very low, which made the flood marks about twenty feet above ground. The flood was twenty-eight feet above summer low water. Out west, near the river, the bottom was high, and the flood marks only about eight feet above the ground. Some of the holes were within two feet, but above the flood mark. The next year many holes were made below the flood mark, but whether they were kept above the top of the next and smaller flood, I did not think to notice. I ran the level over the land to grade it for assessment, and had a good opportunity to observe the holes. What is the explanation?

Lafayette, Ind.

Observations on Cerebral Localization.

By James Rollin Slonaker.

Ever since Hitzig[1] in 1870 sent a voltaic current through the brain of a wounded soldier and noticed a certain movement of the eyes, numerous investigators have been busy furthering our knowledge of cerebral localization.

Fritsch and Hitzig followed this discovery with many experiments on the cerebral hemispheres of the dog and noticed that stimulation of certain areas produced definite muscular movements on the opposite side of the body.

These experiments started many other investigators, among whom may be mentioned Ferrier,[2] Munk,[3] Horsley and Schafer,[4] Heidenhain,[5] and Beevor and Horsley.[6] The results of these and many later investigations have formed the basis of an exact cortical localization in the brain of man.

Numerous surgical operations and pathological observations have added to our fund of knowledge, so that now the cortical areas governing certain movements in man are quite definitely known. However, each new case will further prove and assist in making the localized areas in man more definite. With this in view I present the following data which I have gathered from the subject:

Mr. Ralph R. Laxton of Atlanta, Ga., met with an accident which fractured the skull near the median line in the Rolandic region. A portion of the bone was removed to relieve the pressure on the brain. As life was despaired of no metal plate was introduced, but the scalp simply closed over. The wound healed and the subject finally recovered. The external condition of the wound after recovery is that there is a more or less circular depression about one and a half inches across, due to the

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[1] Hitzig, Reichert u. Du Bois-Reymond's Archiv., 1870.

[2] Ferrier, The Functions of the Brain, London, 1886.

[3] Munk, Die Functionen der Grosshirnrinde, Berlin, 1877-1880.

[4] Horsley and Schafer, On the Functions of the Marginal Convolution, Proceedings of the Royal Society, No. 231, March, 1884. Horsley, British Medical Journal, Vol. II, 1884.

[5] Heidenhain, Pflüger's Archiv f. Physiologie, 1881.

[6] Beevor and Horsley, A Record of the Results Obtained by Electrical Excitation of the so-called Motor Cortex and Internal Capsule in an Orang-Outang (Simia satyrus), Phil. Trans. Royal Soc., Vol. 181, B, 1890.

absence of bone. This depression lies as shown in Figures 1 and 2. These figures are shadowgraphs representing the side and back views respectively.

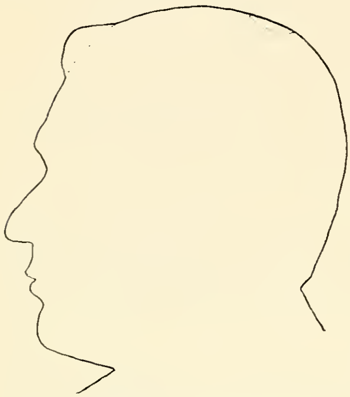

Fig. 1. Shadowgraph showing location of depression as seen from side.

Various muscular troubles arose, indicating a disturbance of the motor region of the brain. A line drawn outward, downward and forward at an angle of 71.5 degrees with the median line and starting from a point one-half inch, or about one centimeter, behind a point midway between the glabella and the inion, will approximately follow the central fissure[7], [8]. With such a line constructed one can quite accurately sketch in the outline of the brain and its principal fissures. Such a sketch is shown in Figure 3.

From this it is readily seen that the depression mostly affects the anterior central gyrus. Also by consulting Fig. 2 it is observed that the depression is situated almost wholly on the left side, passing over only about a quarter of an inch onto the right side.

The schemes representing the localized areas in man are based on the results of observations on the monkey, on human pathological data and on experiments on man.

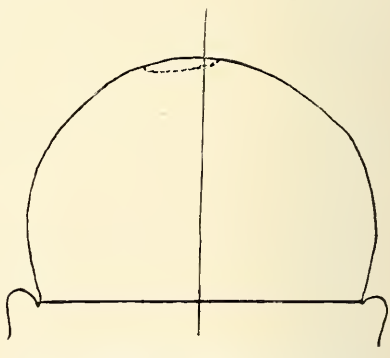

Fig. 2. Shadowgraph showing position of depression as seen from behind.

How Mr. Laxton's injury confirms our present knowledge of cerebral localization in man is shown in the following history of the case, a part of which I give in his own words:

"At the time of the injury, Nov. 25, 1892, I was 22½ years of age and weighed about 145 pounds. My height was about 5 feet 9 inches. At present I weigh 160 pounds and measure 5 feet 10 inches while standing on my left leg, and 5 feet 9 inches on my right.

[7] Deaver's Anatomy, Vol. II, p. 508.

[8] Reid, The Principal Fissures and Convolutions of the Cerebrum, Lancet, 1884.

"In perhaps sixty seconds from the time of the blow I was conscious again, but I do not remember any sensation in my right leg at the time, except that it was very cold. I did, however, observe the progress of paralysis in the right arm. This began in the fingers and extended gradually up the arm. For some time after I was operated upon I was unable to find the way to my mouth with a glass of water. This paralysis was, I think, due to extravasation of blood, which was gradually absorbed later, as I have for more than twelve years been doing a good deal of work with the pen and some with the telegraph key. I think I may safely say that I have entirely recovered the use of the arm. At times, however, I feel the characteristic dull sensation in the muscles of the right side of the body up to the shoulder, and even in the upper arm itself. Then, again, the sensation is hardly apparent above the waist line, all of which tends to show that the area of depression is not sharply defined."

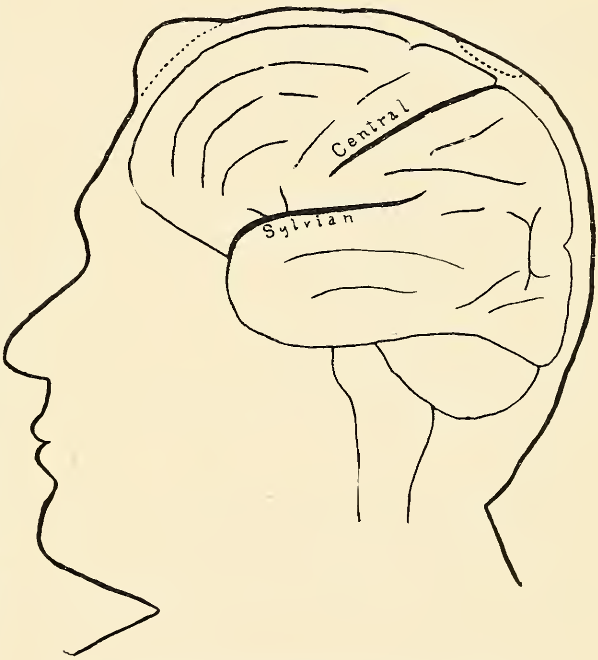

Fig. 3. Showing depression in relation to fissures of the brain.

The left arm was broken by the accident so he was unable to use it, but he states that it was not affected by the paralysis.

It would be interesting in this connection to know if the change in barometric pressure has any influence on the location of this dull sensation. Accurate observations in this respect are lacking. The only information Mr. Laxton can give on this point is as follows: "As regards barometric effects, I have not been able to form any definite idea, though I have lived for ten months of the past year in southern Mississippi, where my office was just seven feet above the level of the Gulf of Mexico. I believe, however, that if the humidity of the atmosphere and the general condition of my system were exactly the same in both localities, I would find a difference between the sea level and a point three or four thousand feet above it. I have not had an opportunity to make observations in higher altitudes, but know that I am capable of more physical exertion in the mountains of western North Carolina than in the low country. I was on Lookout Mountain a few weeks ago, making the trip up the incline railway, but was not able to notice any change in feeling due to the rapid rise, of something like one thousand feet, from the city of Chattanooga to the top of the mountain. Just prior to a sudden change from dry to wet weather, I am apt to suffer from pains in the right leg, which I suppose are akin to rheumatism. As soon as precipitation begins the pains cease. This pain is most marked in the right hip joint."

In regard to stature, as has already been stated, he stands one inch higher on the left foot than on the right. The right leg also measures one inch less in circumference than the left, both in the thigh and the calf region. The muscles of the right leg, especially in the region of the calf, are less firm than those of the left. These conditions did not prevail before the accident. There is also a difference in the development of the two sides of the chest, which condition existed to a certain extent before the accident.

Concerning the resulting disturbances, Mr. Laxton says:

"There is a certain deficiency of sensation in the right leg and abnormal reflex action occurs. There is also an apparent deficiency of synovial fluid. There is almost an entire lack of control of the toes of the right foot, particularly the big toe (see Figs. 4 and 5). There is consequently a lack of balance in walking somewhat related to that observed in people who have lost one leg and use an artificial one. There are times when I feel for a few minutes as if the paralysis were entirely gone, but I have to be extremely careful not to feel too sure of myself and to follow the plan of not attempting a full length step with the right foot. The sensory paralysis extends very slightly to the bottom of the left foot." (Fig. 4.)

"I am just now experiencing considerable local irritation, the scalp even becoming, at times, sore on the outside. There are times when the

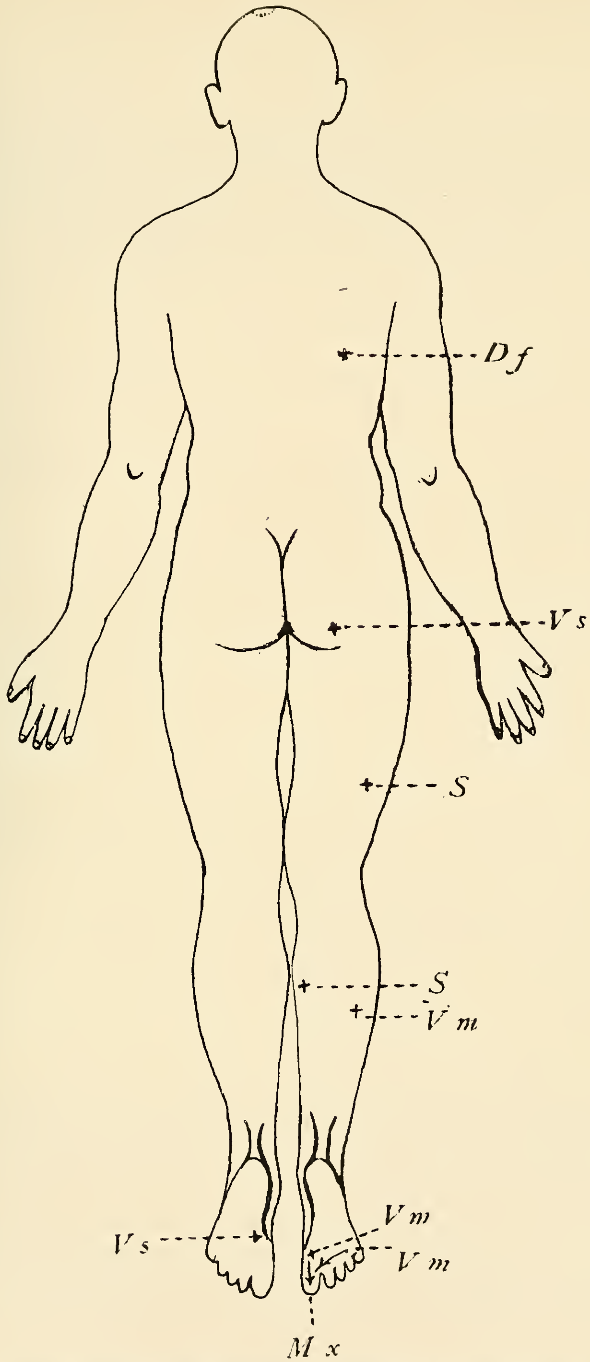

Fig. 4. Chart illustrating the positions of affected areas. Df, dull feeling; Vs very slight; S, slight; Vm, very marked; Mx, maximum.

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under side of the scalp, the point of adhesion, has a feeling very similar to that of a vaccination scar just before the scab is ready to come off. Sometimes when I run my hand through my hair, I feel a slight tremor in the nerves of the calf of the right leg. The most sensitive part which gives rise to the tremor is the anterior edge of the depression."

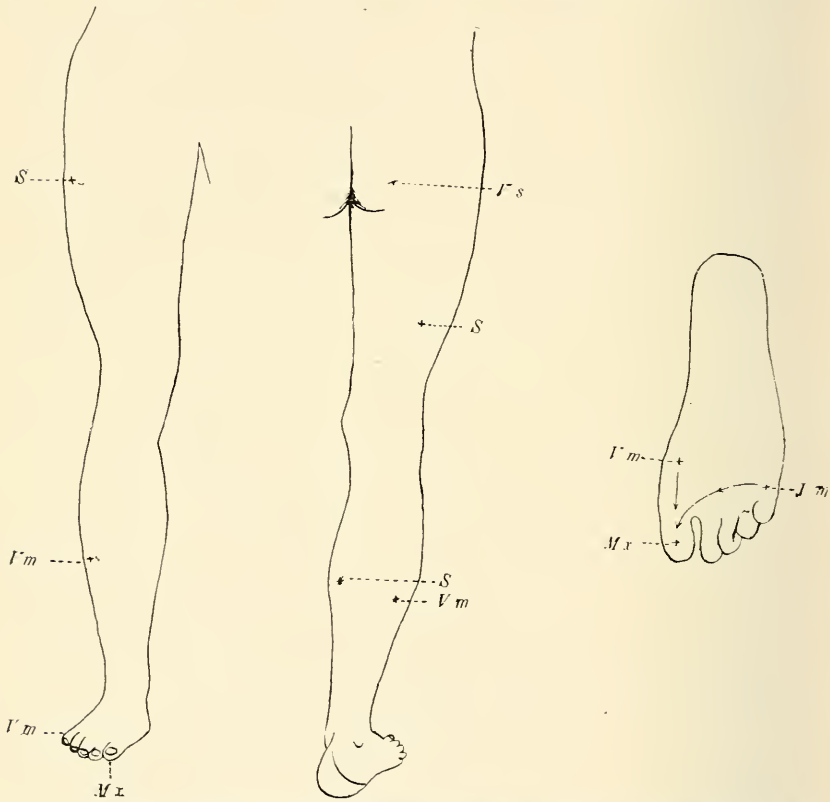

Fig. 5. Chart showing the position of affected areas in the right leg and foot Vs, very slight; S, slight; Vm, very marked; Mx, maximum. The arrows on the bottom of the foot indicate a continued but increasing disturbance.

In regard to the mental effect, Mr. Laxton says:

"There is no doubt that such an injury, so long as mechanical pressure continues, has at least a reflex bearing upon the mind itself, so that one suffering from it does not always feel like applying what he knows directly to the work in hand. If you will wear a brick fastened to your head continually for a term of years, or undertake a journey of indefinite length on foot through a tunnel not quite high enough to stand upright in, you will get an idea of the feeling."

In Figs. 4 and 5 I have indicated the location of the areas affected as described to me by Mr. Laxton. These areas range from a "dull sensation" to "very marked" and "maximum." It is interesting to note that the disturbance becomes more and more marked toward the feet. That while there is a great disturbance in all the toes of the right foot, this disturbance increases from the little toe and the center of the bottom of the foot to the hallux, where it is maximum.

One observes, also, that with the exception of a small area on the bottom of the left foot, which is very slightly affected, the disturbed areas lie wholly on the right side of the body. This we would naturally expect, as the greater part of the depression is on the left side of the median line of the skull. Since the depression extends slightly across to the right of the median line, we would expect some disturbance on the left side of the body. The slight disturbance on the bottom of the left foot would indicate that the portion of the brain close to the median line controls the center of the foot. We would expect a greater disturbance in the corresponding part of the right foot, because the corresponding area of the brain lies more nearly under the center of the depression.

We may, I think, reasonably infer that the region of greatest disturbance is controlled by that part of the brain lying under the center of the depression. Therefore the motor area controlling the movements of the great toe would lie about the center of the depression, and that of the small toes and the center of the bottom of the foot, in close proximity. As we have already concluded that the cortical area controlling the center of the bottom of the foot lies adjacent to the median longitudinal fissure, that for the small toes would be farther removed from this region than the center for the great toe. I think we may also conclude that the parts less and less affected are controlled by portions of the brain lying nearer and nearer the margin of the depression. The movement of the hair near the anterior margin producing a tremor in the calf of the right leg, would indicate that the motor center for this region is at this point.

Since all the muscles of a given region, i. e., thigh or calf of leg, are not equally affected, one may infer that different muscles of the same region may have somewhat widely separated centers of control in the cortex.

or that some of these centers may be more deeply seated than others, and for this reason less affected.

From the foregoing, I think the following conclusions can be drawn:

1. If we have made no mistake in locating the central fissure with reference to the area of depression, this area lies mainly over the anterior central gyrus of the left side and extends very slightly across the median longitudinal fissure to the corresponding gyrus of the right side.

2. The area controlling the center of the sole of each foot lies in the anterior central gyrus at the margin of the median longitudinal fissure.

3. The area controlling the hallux lies a little more lateral, perhaps one-half inch, from the margin of the median longitudinal fissure.

4. The area controlling the other toes is in close proximity to that of the great toe. It may be anterior, posterior or more lateral from that of the great toe. Since the region controlling the muscles of the calf lies anterior, it is very probable that it is more laterally situated. This accords with the results of Beever and Horsley.

5. The areas controlling the muscles of the calf on the outside and on the inside of the leg, the thigh, rump and scapular regions are located in the order named at greater and greater distances from the center of depression. I have no doubt that the scapular region (possibly some others, also) is only indirectly affected.

6. Though the data are not quite sufficient to indicate accurately the position of the motor centers involved, it is very probable that they are arranged laterally along the anterior central gyrus from the median longitudinal fissure in the following order: a. Center of sole of foot. b. Center for great toe. c. Small toes. d. Calf muscles on lateral surface of leg. e. Calf muscles on mesial side of leg. f. Thigh muscles. g. Rump muscles. h. Scapular muscles. With the exception of the first-named area this arrangement agrees with the results of other investigations.

Leland Stanford Junior University,
California.

The Development of Insect Galls as Illustrated by the Genus Amphibolips.

Mel T. Cook.

The study of the development of insect galls involves more complicating factors than most problems of evolution, since the host plant is forced to give both nourishment and protection to its enemy. The result of this enforced action is the formation of a structure which is normal for the parasite and pathological for the host. The histology of these gall structures presents some very interesting questions involving the point of stimulation, the character of the stimulation and the evolutionary lines along which the various species of galls have developed. For some time we have recognized that the point of stimulation is in the meristomatic tissues, and that in most cases the stimulation is not due to a glandular secretion from the parent insect.[1] However, there appears to be abundant evidence that in most cases the stimulation comes from the larva, but whether mechanical or chemical, or both, or the former in some species and the latter in others, is a practically untouched problem.

In 1902 the writer[2] advanced the opinion that "the morphological character of the gall depends upon the genus of the insect producing it, rather than upon the plant upon which it is produced, i. e., galls produced by insects of a particular genus show great similarity of structure, even though on plants widely separated; while galls on a particular genus of plants and produced by insects of different genera show great difference." Further studies along this line have convinced the writer of the correctness of this view, and have also led to efforts to work out a system of classification based on the histological character of the galls which would be correlated with the classification of the insects. However, the completion of such a series of studies is largely dependent upon a more satisfactory knowledge of the taxonomic relations.

While it is true that the histological characters of the galls depend upon the insects rather than upon the host plants, it is also true that we find certain characters common to all groups. The first step in the forma-

[1] Adler & Straton. Oak Galls and Gall Flies. 1894.

[2] Galls and Insects Producing Them. Ohio Naturalist, II:7, p. 270. 1907.

tion of a gall is (1) the excitation of growth and cell division, (2) the failure of the cells of the affected part to differentiate into the characteristic tissues of that part, and (3) the differentiation into characteristic tissues of the gall. We also recognize certain similar lines of development in what we now consider well-defined genera. The explanation of the similarities and differences in these lines of development will depend largely upon future work in both taxonomy and histology.

It is the purpose of this paper to call attention to certain points above referred to in connection with the genus *Amphibolips*. The taxonomy of the insects of this genera have been very thoroughly studied and carefully described and arranged by Mr. Wm. Beutenmuller.[3] The writer has also studied the histology of several of the galls.

The genus *Amphibolips* belongs to the family Cynipideæ, is quite distinct, and stands high in the line of development. As previously stated, the galls originate as a result of stimulation of meristomatic tissue, resulting in growth and cell division. This is followed by a differentiation of this mass of cells into the tissues characteristic of the galls. In the cynipidous galls we have the four distinct tissue zones which have been referred to by many writers, viz: (1) the epidermal zone, or outside layer of cells, (2) the parenchyma zone, which may be quite thick, either dense or loose, and in which may be found fibrous tissue radiating from the center of the gall, (3) the protective zone, composed of sclerenchyma tissue and varying in thickness in different species of galls, (4) the nutritive zone of parenchyma cells, rich in protoplasm and immediately surrounding the larval chamber. The galls belonging to this genus have the four well-defined zones, but with variation in the parenchyma and protective zones by which they may be subdivided into the following groups:

GROUP A.

Amphibolips confluens, Harris.
" *caroliniensis*, Bassett.
" *longicornis*. "
" *acuminata*, Ashmead.

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[3] The Species of Amphibolips and their Galls. Bulletin of the American Museum of Natural History, Vol. XXVI, Art. VI, pp. 47-66. 1909.

Group B.

Amphibolips inanis, O. S.
" *ilicifoliæ*, Bassett.
" *Coelebs*, O. S.
" *citriformis*, Ashmead.
" *melanocera*, "
" *cinerea*, "
" *cooki*, Gillette.
" *tinctoria*, Ashmead.

Group C.

Division a.

Amphibolips spinosa, Ashmead.
" *globulus*, Beutenmüller.

Division b.

Amphibolips nubilipennis, Harris.
" *racemaria*, Ashmead.

Division c.

Amphibolips prunus, Walsh.
" *gainesi*, Bassett.
" *fuliginosa*, Ashmead.
" *palmeri*, Bassett.
" *trizonata*, Ashmead.

The writer has previously made studies of the histology of *A. confluens*. *A. inanis*, *A. ilicifoliæ*. *A. nubilipennis*, and *A. prunus*. Taking *A. confluens* as a type of the group A, we find the parenchyma zone very thick and composed of cells which when mature have the character of a mass of colored cotton, and among which may be found fibro-vascular bundles. The parenchyma cells, when examined under the microscope, are found to be unicellular, long and threadlike. The protective zone is comparatively thin. The nutritive zone is prominent only in the young galls. The writer has not had an opportunity to examine the other three species of this group, but from the taxonomic discussion, they appear to coincide very closely with *A. confluens*.

In group B the writer has studied *A. inanis*, *A. ilicifoliæ* and *A. coelebs*, which, judging from Beutenmuller's description, are quite typical of the group. In these galls the parenchyma zone is characterized by large intercellular spaces. A part of the parenchyma cells remain attached to the epidermal zone, another part to the protective zone and some to the well-defined fibro-vascular bundles which radiate from the central body to the outer part of the gall. These fibro-vascular bundles are in general much better developed than in the galls of group A. The protective zone is subject to considerable variation in the different species; it is quite prominent in *A. inanis* and practically absent in *A. coelebs*. The nutritive zone, as in the first group, is prominent only when the gall is young.

In group C the writer has studied *A. nubilipennis* and *A. prunus*. This group may be readily divided into three sub-groups as indicated above. The species of sub-group (a) because of the inner radiating and spongy substance, appear to be intermediate between group B and the other species of group C. The species of sub-group (b) are more succulent than the species of sub-group (c).

My studies of *A. nubilipennis* demonstrate a thick parenchyma zone of large succulent cells and very small fibro-vascular bundles which were most numerous near the surface of the gall. The protective zone consisted of a few layers of thin-walled cells. The nutritive zone was prominent in the young galls and persisted quite late.

My studies of *A. prunus* demonstrated a very thick parenchyma zone, much firmer and drier than in *A. nubilipennis*, and in which were very few small, fibro-vascular bundles. The protective zone was entirely absent. The nutritive zone well developed in the young galls.

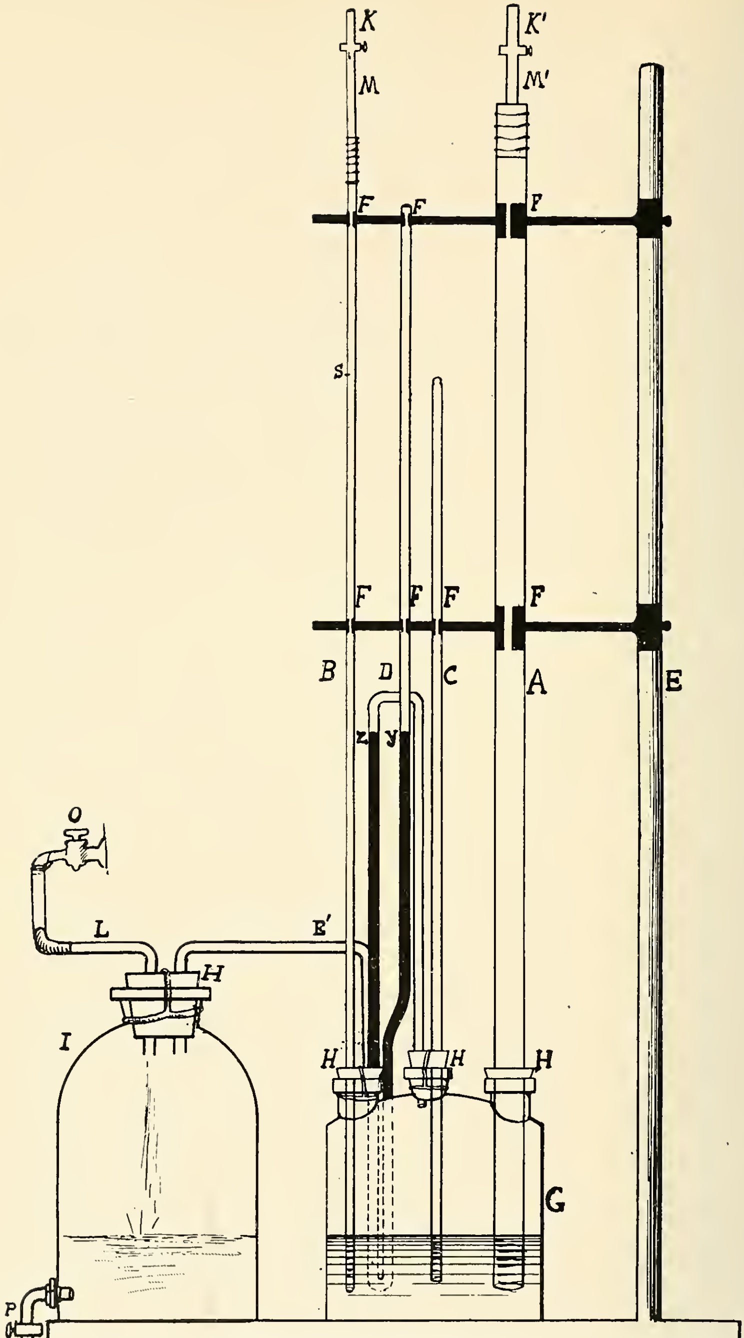
In general it will be noted that in this genus we have (1) the galls originating and developing in the normal manner which results in the formation of the four zones; (2) the variation in the parenchyma and protective zones, which enables the above division and sub-divisions; (3) that group A may be considered the most highly developed and sub-group c of group C the lowest. The significance of this line of development cannot be determined until we know more about other genera of gall-makers and their galls. However, a study of the known geographical distribution of the species of this genus is interesting in connection with this study. In group A, *Amphibolips confluens* is very widely distributed over Canada, the Eastern States south to Georgia, and west to Colorado, while the other three species have much more limited ranges, two and possibly all three within the range of the first. In group B we find that *A. inanis* ranges from Canada and the Eastern States west to Iowa and south to North Carolina; *A, cooki* has almost the same range; *A, ilicifoliæ*, *A,* *coelebs* and *A. tinctoria* are included within the above range; and *A. citriformis*, *A. melanocera* and *A. cinerea* are reported from Florida. In group C, we find *A. nubilipennis* very widely distributed from New York west to Illinois and south to Pennsylvania, *A. prunus* from New England west to Colorado and south to Georgia; *A. spinosa*, *A. racemaria* in Florida, *A. fuliginosa* in Florida and Georgia, *A. globulus* in New Jersey, *A. gainesi* in Texas, *A. palmeri* in Mexico, and *A. triazonata* in Arizona.

Delaware College,
Newark, Del.

Apparatus for Illustrating Boyle's Law.

By F. M. Andrews.

The apparatus shown in Figure 1 illustrates not only Boyle's or Mariotte's Law, but also a combination of attendant phenomena which I shall describe presently: Figure 1 is about one-fourth the true size of the apparatus. It consists of an ordinary iron ring-stand E, by means of which the various glass tubes A, B, C, and D, are held in the proper position by means of clamps at F. At the base is situated a Woulfe bottle G, with which A, B, C, D, and E′ communicate. The bottle G is about one-third filled with a concentrated aqueous solution of eosin. This solution is readily visible and on account of its intense red color is also seen at a considerable distance in the transparent glass tubes A, B, and C. Such an eosin solution has the additional advantage of being rather permanent in color, for in two years the solution I had used did not change perceptibly, and only a slight reddish brown precipitate was visible. It is also quite resistant in the presence of HCl, and even by the use of strong HCl a heavy precipitate results which is almost as red for a time as the original solution. The glass tubes A, B, and C extend below the surface of the eosin solution, while D merely projects through the rubber cork H. The connection of all the glass tubes A, B, C, D, E′, and L are made air-tight by means of the rubber corks H, and the latter are held firmly in place by copper wires to prevent their being blown out of the pressure generated in I and G. By means of the glass tube E′ the large glass bottle I is connected with G, and another glass tube connects I with the water-tap, air-pump or other contrivance for generating pressure. If the apparatus is connected as shown in the figure to water mains carrying a high pressure, and if then we open the valve O, the water will be forced into I. This will of course cause compression of the air in I, as well as pressure in proportion to the amount of water allowed to enter. Since G is connected with I by E′, the same pressure will be generated in G as in I. As A, B, and C project below the surface of the eosin solution, and if the valves K and K′ are closed and the water continues to enter I, in a few seconds the volume of air in the tube C, which is sealed at the top, will be compressed one-half its former volume by the eosin solution rising one-half the inside



Apparatus for Determining Boyle's Law.

length of the tube when the pressure in G equals one atmosphere. This illustrates Boyle's Law by showing that the volume of gas in C varied inversely as the pressure brought to bear upon it. The same principle would be shown in A and B under similar circumstances if K and K′ of the tubes M M′, which are fastened to A and B by means of rubber tubing held by copper fire and sealing-wax, remained closed.

Again, when the air in A, B, and C is compressed one-half its volume by a pressure of one atmosphere, this will be shown by the manometer which the tube D forms. This tube has each of its two arms filled to a height of forty centimeters with mercury. The total height of the two columns is therefore equivalent to more than an atmosphere. When the pressure in G is zero, then the two columns of mercury X and Y are equal in height. When, however, the pressure in G is equal to one atmosphere, then the column X will sink and column Y will rise till the difference of their heights is 76 cm. Since, in estimating accurately the height of a mercury column both pressure and temperature must be considered, this may be done by the usual formula.

When it is desired to again reduce the pressure in G to zero and allow the water in I to escape, this may be done by closing O, opening P, and either K or K′, or both. Unless I is interposed between O and G, water could not for obvious reasons be used. Air could, of course, be forced directly into G.

The apparatus can also be used to show that the height to which a liquid will rise in a tube is independent of its diameter. If we open O then, as mentioned above, the pressure developed in I and G will cause the eosin solution to rise with ease in A and B if K and K′ are left open. When the eosin solution has risen to S, or to any other height in B, whose internal diameter is three millimeters, then if we notice A, disregarding the small effect of capillarity in B, the column of liquid will stand at exactly the same height in A, whose internal diameter is one cm., as in B.

If, finally, both A and B are rapidly filled with the eosin solution by quickly and strongly generating pressure in G, then it will be seen by carefully timed observations that the liquid in A will rise to an equilibrium of the pressure in G somewhat more quickly than the same equilibrium will be attained by the liquid in B, due to the greater friction produced by the smaller tube B. For the same reason if the pressure is rapidly reduced to zero by opening P, the eosin solution in B will require a slightly longer time to fall from a polnt, as S, and reach the level of the liquid in G, than would be required by the same height of a column in A.

State University, Bloomington, Ind.

Some Monstrosities in Plants.

By F. M. Andrews.

In the proceedings of the Indiana Academy of Science for 1905, pages 187 and 188, I have mentioned some interesting variations which I noticed in *Trillium*. Since that time I have been favored by the announcement of some additional monstrosities shown in Trillium by Prof. John M. Holzinger[1] of Winona, Minnesota, in a paper which he has been good enough eo send me.

It occasionally happens that interesting monstrosities or variations occur in other plants. Such variations, although very common, are nevertheless often of great importance.

One of the most common folear variations occurs in clover, and these I have found more or less abundantly, especially in *Trifolium pratense*. De Vries[2] states that he rarely observed clover individuals with more than one quaternate leaf. I have observed from time to time some specimens of clover which had one leaf of four leaflets, and in one instance found two specimens of clover, each of which had in addition to ten regular leaves of three leaflets, seven (7) other leaves, each one of which had four (4) leaflets. One of these quaternate leaves was beginning to form a leaf having five (5) leaflets by the splitting process. Again another plant of clover near this one having seven quaternate leaves, had in addition to the ternate leaves, one with five leaflets. Another specimen of clover had ten leaves of five leaflets each, in addition to several ternate ones. One of these leaves with five leaflets shows the origin of the supernumerary leaflets by the splitting process, as De Vries describes on page 342 of his "Species and Varieties. Their Origin by Mutation," 1905.

Another specimen of clover had in addition to the usual ternate ones, one leaf having six leaflets, and another plant of clover, one leaf having seven leaflets. These plants all grew close together in a yard and were the only ones thereabouts which showed, in the many other specimens of clover present, any of the above mentioned deviations.

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[1] John M. Holzinger, Plant World. 4: July, 1901.

[2] Species and Varieties. Their origin by mutation, 1905, p. 340.

I have also noted deviations in the Buckeye tree where six and sometimes nine leaves occurred instead of five. Some plants, as the common blackberry, have at times flattened stems, and some have two periods of blooming in the same year, as the Weigelas and other plants.

Apparent deviations or monstrosities may sometimes be due to an injury, and therefore in deciding such points care is necessary.

State University,
Bloomington, Ind.

A List of Algæ.

(Chiefly from Monroe County, Indiana.)

By F. M. Andrews.

The list of Algæ given at the end of this paper includes about one hundred and seventy-five forms, most of which are from Monroe County, Indiana. Some few species of these Algæ are from the Eagle Lake and Turkey Lake in the northern part of Indiana, while a few others have been obtained from other sources. The collection of these forms has extended over a period of several years, for a continuous effort to obtain the forms here mentioned was not made except in the case of the Algæ found in the water works of this city in 1896. At this time some of the forms of Algæ then to be found in the city water works of Bloomington were collected by Dr. George J. Peirce, now of the Leland Stanford Junior University, Mr. A. C. Life, and myself. A title of the work done by us conjointly appeared in the proceedings of the Indiana Academy of Science for 1896, on page 208, entitled: "A Microscopic Examination of Certain Drinking Waters."

In this work not only the forms at the surface and edges of the reservoir were obtained, but also those to be found at different depths in the water. On account of the lack of more elaborate instruments and means for doing this, we hit upon a very simple but sufficiently effective plan. This was done by securing a bottle of the proper size and shape, fitted with a stopper, to a heavy cord. The stopper also was attached to a cord. After rowing out into the reservoir, the water of which varied from fifteen to thirty feet in depth, this weighted bottle was lowered to the desired depth by one string and the stopper partly removed by the other string. After the bottle had filled with water, as could be told by the rising of bubbles, the stopper was allowed to slide back in place, thus reclosing the bottle. To prevent the stopper from being pulled out of the bottle and thus rendering it impossible to replace it before raising the bottle from the water, a string of the proper length was tied around the neck of the bottle and to the stopper. To be sure that the glass stopper would settle back into the bottle after filling, I found a band of rubber fastened around the neck of the bottle and the stopper to be effective in accomplishing this end. It is always best to close the vessel used in such experiments to prevent the entrance of Algæ not at the depth at which it is desired to take the samples or to keep some in the bottle from being lost in raising the bottle to the surface. In this way it is easily possible to obtain specimens that are floating from any part of the body of water. By this method, too, it was shown that numerous forms of Algæ were distributed all through this body of water. The living ones were found in greater abundance at or near the surface, as would be expected, but they were also found in the deeper water as well. In some places the number of forms was often very small, but in order to make a study of the greater number from such a locality the following method was used: A suitable quantity of water was obtained in the above described way from the desired location, and this allowed to filter through a small surface. A funnel, the lower end of whose tube was closed with closely woven cloth, served quite well, and in this way enough forms would be obtained for a convenient study. Such concentration of forms, we may term it, also brings about a great saving of time in looking for forms that would otherwise be found only after much searching, and at the same time was more representative for any given depth.

The effectiveness with which various of the Algæ forms could be removed by means of sand was attempted. This will vary with the kind of sand employed. The kind of sand here employed was very fine, white sand, especially employed for the microscopic examination of water. The following are some of the results:

Twenty-five ccm. of water from the bottom of the reservoir, in five cm. of this sand, required seven minutes to filter.

One thousand ccm. of water from the surface of the reservoir required forty-three minutes to filter, through a closely woven cloth tied over the end of a very small glass tube. A considerable depth of very fine, clear sand is necessary to entirely remove all of the smaller Algæ forms from the water, for I found after only twenty-five ccm. of the water from this reservoir had been filtered through five ccm. of fine sand, a considerable number of some forms came through. In one instance, in tap water, coming from this same source, some small forms came through four cm. of this fine, white sand, which to filter twenty ccm. it required one hour and twenty-five minutes.

In another case only eighty-five minutes was required to filter twenty-five ccm. of this water through four cm. of sand, due in this case to the less quantity of sediment and forms than in the instance where one hour and twenty-five minutes was required for filtration. Some permanent slides were prepared in 1896 and part of these I still have, which show some of the various Algæ forms obtained by the method above referred to. These slides were prepared by making a mounting fluid of the following substances:

Alcohol, 95%	10 ccm.
Glycerin	30 "
Distilled water	30 "
Acetic acid, 5%........................	30 "

Specimens mounted in this mixture should be sealed with balsam. The slide should be thoroughly cleansed and dry before ringing the cover-glass with balsam. The only danger from the loss of specimens so mounted is from the liability of the balsam to crack and allow the liquid under the cover-glass to evaporate. For this reason they had better have the balsam covered with a layer of dammar-lac or shellac, and be noticed from time to time and not kept in too warm a place.

Dilute glycerin seems also to be a good medium for mounting Algæ. One specimen of Pandorina has been preserved and mounted in it fifteen years and is still apparently as green and as perfect as the day it was mounted. Camphor water[1] and glycerin also seem to give good results from the standpoint of preservation.

Other forms of the Algæ of this list not found in the water works reservoir have been observed at different times and recorded as found. It is not supposed that this list of Algæ here given is by any means complete, but gives an idea of a few out of an enormous number of forms that must be widely distributed. A good many of the forms here mentioned have been found by Mr. A. B. Williamson, one of the students in the Botany Department, and reported to me for the following list.

A list of the growing forms of plants in any locality is best made and more complete when extended over a series of years, so as to include those individuals which for various reasons or changes of conditions do not appear during one season.

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[1] Stokes—Analytical Keys to the Genera and Species of the Fresh-water Algæ, p. 20.

A LIST OF ALGÆ.

Glœocapsa polydermatica.
" aeruginosa.
" coracina.
" rupestris.
" sanguina.

Chrococcus turgidus.
" coherens.

Spirulina Jenneri.
" duplex.

Glœotrichia pisum.
" natans.

Calothrix gracilis.
Tolypothrix distorta.
" tenuis.

Rivularia Dura.
" echinulata.
Scytonema tolypothrichoides.
" myochrous.
" natans.

Sirosiphon pluvinatus.

Hapalosiphon tenuissimus.

Nostoc pruniforme.
" verrucosum.
" sphæricum.
" commune.

Anabæna inaequalis.

Nizschia sigmoidea.
" constricta.
" acicularis.

Cocconema lanceolatum.

Synedra Acus.
" pulchella.
Fragilaria capucina.
Achanthes Hungarica.

Cocconeis placentula.

Eunotia gracilis.
" pectinalis.

Amphora ovalis.

Epithemia turgida.
" gibba.

Gyrosigma attenuatum.

Spirogyra jugalis.
" nitida.
" crassa.
" decimina.
" setiformis.
" gracilis.
" fusco-atra.
" communis.
" quinina.
" longata.

Zygnema leiospermum.
" insigne.
" anomalum.

Zygogonium decussatum.

Mougeotia divarecata.

Mesocarpus nummuloides.
" recurvus.
" robustus.

Staurastrum arcticson.
" muticum.
" dejectum.
" incisum.
" alternans.
" crytocerum.
" arachne.
" gracile.
" vestitum.
" hirsutum.
" spongiosum.
" luteolum.

Pediastrum Boryanum.
" pertusum.
" tetras.

Sorastrum spinulosum.

Cœlastrum microporum.
" cambricum.

Scenedesmus obtusus.
" dimorphus.
" caudatus.
" acutus.

Pandorina morum.

Endorina stagnalis.

Volvox globator.
Sphærella (Chlamydococcus) pluvialis.

Ulothrix subtilis.
" muralis.

Gonium pectorale.
Cladophora glomerata.
" fracta.
" crispata.

Œdogonium crassum.
" sexangulare.
" obtruncatum.
" fonticola.

Bulbochæte intermedia.

Colochæte irregularis.
" soluta.
" scutata.

Draparnaldia glomerata.

Stigeoclonium nanum.

Cylindrospermum macrospermum.
Cylindrocapsa geminella.
Merismopædia glauca.
" convoluta.

Oscillaria chalybea.
" cruenta.
" tenuis.
" subfusca.
" natans.
" antliaria.
" limosa.
" percursa.
" princeps.
" Froelichii.
" brövis.

Navicula viridis.
" sphærophora.
" serians.
" alpina.

Cymbella lanceolata.

Meridion circulare.

Diatoma elongatum.

Melosira arenaria.
" varians.

Gomphonema geminatum.
" constrictum.

Licmorphora flabellata.

Tabellaria fenestrata.
" flocculosa.

Pleurocarpus mirabilis.

Cosmarium obsoletum.
" sexangulare.
" globosum.
" orbiculatum.
" cucumis.
" suborbiculare.
" benustum.
" quasillus.

Closterium acerosum.
" cucumis.
" Ehrenbergii.
" acutum.
" attenuatum.
" Leibleinii.

Hyalotheca dissiliens.

Desmidium Swartzii.

Mesotænium Braunii.

Spirotænia condensata.

Docidium crenulatum.
" connatum.
" nodosum.

Tetmemorus Brebissonii.

Xanthidium armatum.

Arthrodesmus convergens.

Euastrum crassum.
" cuneatrum.
" didelta.
" ansatum.

Micrasterias radiosa.
" papillifera.
" truncata.

Chætophora pisiformis.
" elegans.
" tuberulosa.

Pleurococcus viridis.

Dactylococcus bicaudatus.

Botryococcus Braunii.

Hydrodictyon utriculatum.

Conferva floccosa.
" fugacissima.
" affinis.
" vulgaris.

Chlamydomonas pluviale.
" tingens.

Dictyosphærium reniforme.

Tetraspora cylindrica.
" lubrica.

Raphidium polymorphum.
" convolutum.

Vaucheria sessilis.
" gemmata.
" terrestris.
" sericea.
" Dillwynii.

Botridium granulatum.

Batrachospermum moniliforme.

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Additions to Indiana State Flora, No. 4.

By Chas. C. Deam.

I offer the following as additions to the Indiana State flora. The determinations have been checked by recognized authorities, and specimens are deposited in my herbarium:

Andropogon scoparius, var. *littoralis* (Nash) Hitch.

Lake County, on sand dunes near Indiana Harbor.

Panicum tennesseense Ashe.

Madison County, on dry, wooded bank of White River, about two miles north of Anderson.

Bromus altissimus Pursh.

Allen County, on alluvial bank of the St. Mary's River, about one-quarter mile south of Ft. Wayne.

Bromus incanus (Shear) Hitchc.

Wells County, in Jackson Township.

Carex canescens, var. *disjuncta* Fernald.

Steuben County, on low border of Graveyard Lake.

Carex laxiculmis Schwein.

Johnson County, in dry woods about two miles southeast of Morgantown.

Carex siccata Dewey.

Steuben County, in dry soil in clearing one-quarter mile north of Clear Lake.

Carex stellulata, var. *angustata* Carey.

Steuben County, on low, sandy border on west side of Graveyard Lake.

Celtis occidentalis, var. *crassifolia* (Lam.) Gray.

Allen County, on bank of St. Mary's River one-half mile south of Ft. Wayne.

Thalictrum revolutum DC.

Fountain County, on wooded alluvial creek bank near Veedersburg.

Trifolium dubium Sibth.

Kosciusko County. Well established in yards of cottages on north bank of Lake Wawasee.

Sanicula canadensis L.

Allen, Blackford, Clark, Madison, Marion, Morgan, Wabash, and Wells counties.

Sanicula gregaria Bicknell.

Dekalb, Fountain, Madison and Noble counties.

Cephalanthus occidentalis, var. *pubescens* Raf.

Clark County, on State Reservation.

Vernonia illinoensis Gleason.

Steuben County, in prairie one-quarter mile east of Clear Lake. Clear Lake.

Solidago juncea, var. *scabrella* (T. & G.) Gray.

Wells County, in dry clay soil in clearing on east side of lakes in Jackson Township.

Indianapolis, Indiana.

Right and Wrong Conceptions of Plant Rusts.

By J. C. Arthur.

The plant rusts have been known both popularly and scientifically from the earliest times. Their study took the usual course of development of all cryptogamic plants up to the time that DeBary demonstrated that pleomorphism existed in many species in a more striking manner than known in other fungi. He showed that most if not all members of the genus *Æcidium* as recognized at the time were only stages in the life cycle of species of *Puccinia* and *Uromyces*, and other investigators soon followed with similar demonstrations for such genera as *Roestelia*, *Peridermium*, and *Cæoma*. It was in 1866 that he announced, with experimental proof, that one stage of a rust, as the *Æcidium*, often grows on a host wholly different from that on which the final stage grows, such rusts being called heterœcious.

Heterœcism, which was thus established by DeBary and confirmed by his contemporaries, was not generally accepted by mycologists for a score or more of years. That the *Æcidium poculiforme* of the barberry leaf, with its conspicuous cups filled with chains of verrucose spores, could not give rise to other similar cups on the barberry, but only to the powdery and echinulate spores of the red rust on wheat stems, as unlike the former as a caterpillar is unlike the pupa into which it is transformed, was such a strikingly new idea in botany, that when once it did find general credence, and was extended to many other species by culture work, it assumed undue prominence. This result was accelerated by the rather recent discovery of races, or so-called physiological species. When the well known *Puccinia graminis*, which has great economic importance by producing a destructive disease of cereals and grasses, became also one of the best illustrations of the division of a species into physiological strains or races, more or less well established, in some cases amounting to possible species, it assumed in the minds of many mycologists a typical position in reference to other rusts. It became common to speak of rusts as agreeing with *Puccinia graminis* in their life cycles and spore structures, or in showing a certain amount of deviation from it. This attitude has caused considerable distortion in the conception usually held of the rusts, even by the foremost students of the order. It affects systematic work adversely, keeps the terminology in an antiquated and ambiguous form, and makes it difficult to institute legitimate comparisons between different genera, species, or spore structures.

One of the wrong conceptions, wrong when viewed in the light of present knowledge, is to make the genus *Puccinia* include all species that possess a two-celled, pedicelled and free teliospore (excepting those with teliospore imbedded in gelatinous matrix, separated under *Gymnosporangium*), irrespective of the other morphological characters, or of the complexity of the life cycle, and furthermore, as part of the same conception, to make the genus *Uromyces* include all species that possess the same kind of teliospore, only one-celled instead of two-celled. The writer believes that the length and nature of the life cycle, which is a more unvarying character in the rusts than the one or two-celled teliospore (recall the *Uromyces-Puccinia* species on *Allium*, *Sida*, and some other hosts), should be accepted as a character for genera, as it is now quite generally accepted for species. Recognizing this as a valid generic character, and taken in connection with other characters, the genus *Puccinia* can be separated into four genera (i. e., *Dicæoma*, *Allodus*, *Bullaria*, *Dasyspora*), and the genus *Uromyces* also into four (i. e., *Nigredo*, *Uromycopsis*, *Klebahnia*, *Telospora*). If other characters, as well as the life cycle, mostly now generally ignored, are taken into account, *Puccinia Pruni-spinosæ* and its allies should form a genus (*Tranzschelia*) near to *Ravenelia*, on account of the adherent pedicels of the teliospores and peculiar structure of the urediniospores; *Uromyces rosicola*, on account of its evident spore structure, will go into a genus (*Ameris*) near to *Phragmidium*, but with a more limited life cycle; *Uromyces Terebinthi*, and its allies, on account of the remarkably distinctive characters of both urediniospores and teliospores, will form a genus somewhere between *Ravenelia* and *Tranzschelia*, while the similar *Uromyces effusus*, with a still more restricted life cycle, will go into another genus (*Discospora*). And in like manner quite a number of other species now commonly included under *Puccinia* and *Uromyces* could properly be separated and distributed to other genera, with much improvement in the nomenclature and great clarification of the systematic affinities. Other genera beside *Puccinia* and *Uromyces* could also be shown to be overburdened with species whose life cycle, or morphological structure, or both, entitle them to a different place in the systematic arrangement, if the extent of the life cycle and characters other than those pertaining to the teliospore were called into account.

The third epoch in the study of plant rusts (the second one being ushered in by DeBary's demonstration of heterœcism and the first epoch preceding that time), may be considered to have started with the study of the nucleus and its behavior. This was begun by the work of Sappin-Trouffy and of Poirault and Raciborski some fifteen years ago, and ably continued by Blackman, Christman, Holden and Harper, Olive and others. The nuclear history in the rusts is still in a very incomplete state, and part of what has been gone over needs further substantiation. Enough has been demonstrated, however, to modify profoundly our ideas of the significance of the different spore forms, the relation of the spore structures, and the possibility of sexuality.

While it may be interesting to review the present knowledge of nuclear changes in the rusts and show the bearing on taxonomy, it will suffice for the present purpose to bring up briefly a few points. It has been rather clearly shown that the rusts possess well marked antithetic alternation of generations. The gametophytic generation has uninucleated mycelium, and gives rise to two kinds of spores, basidiospores and pycniospores, both uninucleated, and these are the only truly asexual spores formed in the life cycle. The sporophytic generation begins shortly after the pycnia mature, being inaugurated by a sexual fusion of cells. This act introduces the binucleated condition. In many species of rusts only one spore form (teliospore) is produced in the sporophytic generation. In other species there is an initial spore form (æciospore), and usually a repeating form, in addition to the teliospore. All spores of this generation are binucleated. In the gametophytic generation all species behave essentially the same. It is in what follows during the sporophytic generation that the great diversity of the rusts is shown.

If the first binucleated spores arising after sexual cell fusion are teliospores, no other spore forms in this generation are produced, and the life cycle is a brief one. But if the first binucleated spores are formed in what has been called an æcidium, cæoma, or primary uredo, they are essentially of the same physiological nature, whatever form they may take. Any such sorus may be called an æcium, and the spores æciospores, this being an extension in the previous application of the terms to cover the primary uredo. Possibly new terms would be less liable to introduce ambiguity in subsequent discussions, but in this paper æcia will be understood to be the initial spore structures following the pycnia, when these structures are not telia. Such æcia are of varying complexity, the simplest being of the uredo-type with spores borne on pedicels and no peridium, intermediate forms being of the cæoma-type, with spores in chains and no peridium, and the most highly developed being of the æcidium-type with a well-formed peridium. There is a wide difference in complexity of structure between the lowest uredo-type of æcia (*e. g.*, those of the so-called *Chrysomyxa albida*) and the highest æcidium-type (*e. g.*, those of *Æcidium poculiforme* belonging to *Puccinia graminis*). But whatever the degree of complexity they are all strictly comparable in their relation to the life cycle of the different species to which they belong.

In most genera having species with initial æcia more rapid and extensive dissemination is brought about by means of repeating spores, often called summer spores. A few genera, like *Gymnosporangium* and *Calyptospora*, have no repeating spores in present known species. The repeating spore structures are either isomorphic with the æcia, and are known as secondary æcia and secondary uredinia, or they are heteromorphic, and are known simply as uredinia. In either case the repeating spores arise from an infection by initial æciospores, and are not immediately preceded by pycnia. Repeating spores are binucleated, but do not arise from fusing uninucleated hyphæ, as the initial æciospores do, for the mycelium on which they are seated is already binucleated, having been derived from a binucleated spore.

The accompanying chart enumerates the best understood genera of the rusts, arranged in such a way as to show the essential features in the life history of the species. It embraces about three-fourths of all genera of the Uredinales recognized at the present time. The chief value of the chart is to emphasize the need of taking into account the full life cycle in order to compare or to contrast genera. It will be seen that many genera, possibly a third of all known, have no æcia or repeating spores, but the formation of telia follows immediately upon the maturity of the pycnia. In the genera with æcia increasing complexity of development is shown by the presence of heteromorphic repeating spores, cupulate æcia with catenulate æciospores, and heterœcism while comparative simplicity of development is shown by isomorphic repeating spores, cæomate æcia with pedicelled æciospores, and autœcism.

DIAGRAM

Showing life history of 59 best known genera of rusts.

- **Pycnia**
  - **æcia**
    - **repeating spores heteromorphic**
      - **æcia cupulate—with catenulate spores**
        - heterœcious:
          - Coleosporium
          - Melampsoropsis
          - Pucciniastrum
          - Melampsoridium
          - Melampsorella
          - †Calyptospora
          - Cronartium
          - †Gymnosporangium
        - heter. and aut.:
          - Nigredo
          - Dicæoma
          - Tranzschelia
      - **æcia cæomate—with catenulate spores**
        - autœcious:
          - Eriosporangium
          - Neoravenelia
          - Phragmopyxis
          - Phragmidium
        - heter. and aut.:
          - Melampsora
    - **repeating spores isomorphic** (autœcious)
      - **æcia cupulate—with catenulate spores**
        - Cystingophora
        - *Lysospora
        - Uromycopsis
        - Allodus
      - **æcia cæomate**
        - **with catenulate spores**
          - Earlea
          - Gymnoconia
          - Xenodochus
          - *Pucciniostele
        - **with pedicellate spores**
          - Ravenelia
          - Pileolaria
          - Uropyxis
          - Prospodium
          - Triphragmium
          - Trachyspora
          - Spirechina
          - Kuehneola
          - Argotelium
          - Bullaria
          - Klebahnia
  - **telia** (autœcious)
    - Gallowaya
    - Necium
    - *Trichopsora
    - *Chrysomyxa
    - *Barclayella
    - Chaconia
    - Cionothrix
    - Alveolaria
    - Bæodromus
    - *Didymopsora
    - Dietelia
    - Endophyllum
    - Pucciniosira
    - Dendrœcia
    - Discospora
    - Macalpinia
    - Polythelis
    - Calliospora
    - Nephlyctis
    - Ameris
    - Nyssopsora
    - Polioma
    - Telospora
    - Dasyspora

*Not represented in North America.
†Repeating spores unknown.

It is evidently a right conception, in view of the foregoing statement, to regard *Puccinia graminis* (a better name is *Dicæoma poculiforme*) as a representative of the highest development of rusts. But to regard it as typical of all rusts, or even of all rusts having æcia, is clearly asking too much of an illustration, and likely to involve grave misconceptions of structure and relative values. If the most essential features of the rusts were to be illustrated by the smallest permissible number of examples of common and well known species, I should select *Polythelis Thalictri* (*Puccinia Thalictri*) for the forms without æcia, *Kuehneola albida* (often called *Chrysomyxa albida*) for the forms with æcia and isomorphic repeating spores, and *Dicæoma poculiforme* (*Puccinia graminis*) for the highly developed forms with æcia and heteromorphic repeating spores.

A wrong conception, which is doing much harm to the taxonomic study of the rusts, is the view that æciospores and urediniospores are of the nature of conidia, that is, asexual spores, comparable to the conidia so abundantly produced by many ascomycetous fungi. Cytological studies show, however, that in the rusts the only truly asexual spores, other than the basidiospores, are the pycniospores, and to these only can the term conidia be applied with approximate accuracy. The sexual process begins by the fusion of uninucleated hyphal cells, which immediately, or almost immediately, develop some kind of binucleated spore-structure. If only one kind of binucleated spore is produced by the species, it is properly called a teliospore. Such a teliospore has two nuclei in each cell, derived by a short succession of divisions from the two nuclei of the fusing cells. These two spore nuclei fuse into one nucleus prior to germination of the teliospore, thus completing the sexual process. If more than one kind of binucleated spore is produced, the initial kind may be called an æciospore, whatever the morphological structure in which it is formed. It has arisen as the consequence of sexual cell fusion, just as in the preceding case, and has the physiological character of greatly stimulated growth associated with sexuality. This initial æciospore gives rise to a binucleated mycelium, which in turn generally produces binucleated repeating spores of the same or of a different form, and so on, until finally a teliospore is produced in which nuclear fusion takes place, as in the first instance mentioned. The sexual process in this class of rusts extends from the cell fusion at the base of the æcia through all the succession of hyphal cells and repeating spores to the fusion of nuclei in the mature teliospore.

All rusts at present known fall into one of these two classes: the sporophytic generation gives rise either to a single spore-form, or else to initial and final spore-forms, with usually intermediate repeating forms. Whether one or more than one spore-form arises between the cell fusion and final nuclear fusion, constituting the sexual period, all such spores, of whatever morphological structure, are of a sexual nature, the initial form (whether of the æcidium-type, cæoma-type, primary uredo-type, or when none of these is produced, the teleuto-type) being the one which most clearly shows the stimulus of fertilization.

The above facts, especially when taken in connection with the highly differentiated structures associated with the initial and repeating spores, often being quite equal or superior to those of the teliospores, show every reason that may be based upon morphology and development for considering the initial and repeating spores as practically of equal taxonomic rank with the teliospores. To illustrate, a genus founded upon a repeating stage, like the genus of imperfectly known fern rusts, *Milesia*, should be as valid as if founded on the telia. This genus has recently been rechristened *Milesina* on the ground that the original name, given in 1870, is invalid because it was only applied to the uredinia and not to the telia. Again, now illustrating with a specific name, the heterœcious rust which was first specifically called *poculiforme* was described in its æcial stage under *Æcidium*, and according to the preceding argument on the importance of the initial spores, this name having priority, although not at the time made to include the telia, should be used, whatever genus name be considered the best, as *e. g.*, *Dicæoma poculiforme* or *Puccinia poculiformis*, not *Puccinia graminis*.

From the foregoing it will be seen that for purposes of taxonomy names applied to the pycnia (spermogonia) may properly be ignored, on the ground that they apply to asexual or conidial structures, but that names applied to æcia and uredinia (*Æcidium*, *Cæoma*, *Peridermium*, *Uredo*, and other such forms) should have the same standing as names applied to telia (teleutospore stage).

I have tried to show that the main features in the life cycle of all rusts exhibit essential uniformity, there being two large groups, one with a single form of spore (teliospore) in the sporophytic generation, and the other with additional initial and (usually) repeating spores, and that the great diversity lies in the details of their structural development. It is difficult to give a clear and concise account of the general features of the rusts on account of the inadequate and ambiguous terminology at present in use. It appears to be unquestionably established that the spore structures of the rusts are not to be homologized with those of the Ascomycetes, and that taxonomic practice in the rusts should not be influenced by what is correct or expedient in the Ascomycetes or other fungi with strongly marked conidial and sexual forms, but be based upon the unique characters of their own development.

Right conceptions of the rusts, according to the writer's position, are those based upon the full life histories of the species, taking into account all the present known facts, and wrong conceptions are based upon partial life histories, and on ideas derived from other fungi and formerly supposed to apply to the rusts but now known to be inapplicable and misleading.

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The Effect of Preservatives on the Development of Penicillium.

By Katherine Golden Bitting.

In examining ketchup for the organisms present, it was noted that the hyphæ of moulds in preserved ketchup were swollen and distorted. In many of the brands of ketchup, the mould present was the common blue mould, *Penicillium*. As this mould is apparently omnivorous in habit, thriving and fruiting on many media, has been used in many physiological investigations to determine the nutritive value of many compounds, grows normally in liquid media, and fruits normally in a saturated atmosphere, is regular in its germinative power, and, so far as known, constant in form, it was selected to determine the effect of sodium benzoate, used in varying quantities, on its structure and development. The media used in the experiments were tomato bouillon, tomato gelatin, and tomato pulp, and were selected because the tomato juice and pulp are present in ketchup, and also because they do not alter the toxic properties of the agents used toward the fungus. Afterwards the condiments used in ketchup were tested and also the ordinary food preservatives, though not so extensively as the sodium benzoate. In these latter experiments tomato bouillon was the only medium used.

The bouillon was made by adding to a can of tomatoes an equal volume of water, boiling for about half an hour, and then filtering. The filtrate is clear, and a good medium for growth. It has an acidity of approximately .2% calculated as citric acid. For the tomato gelatin, 10% of gelatin was added to the tomato bouillon, cleared with egg, and filtered. The tomato pulp was obtained from a factory, and was made from whole tomatoes. To these media the sodium benzoate was added in the various amounts used in factory practice. Before sterilizing the media, calendered paper was tied closely over the cotton plug to prevent the distillation of the benzoate. After sterilization and cooling, the media were inoculated with spores from a vigorously growing culture of the mould. During development, the cultures were kept at room temperature, unless otherwise stated. The method of culture was by moist chambers and flasks for the bouillon and gelatin, and Petri dishes for the pulp. The moist chambers had a few drops of the culture medium placed in the bottom, so as to keep the vapor tension unaltered.

The cultures were examined at regular intervals, as indicated in the tables, those in the flasks having specimens taken for examination with the miscroscope. The points noted were the swelling of the spores preceding germination, the length of hyphæ, and the earliest appearance of conidiophores, for the cultures in the moist chambers. For all other cultures a hand lens was used to determine the first appearance of germination. The appearance of the conidia was shown by the blue color, and the maturing by the change in color from the blue to green, and then to olive. The volume of mycelium and conidia was noted to determine the extent of development.

Penicillium Grown in Tomato Bouillon 500 cc. 70° F.

Per Cent Sod. Benz.	Time to Germ. Hours.	Development.
——	24	Spores germinated. 48 hours—surface covered. 72 hours—spores developed, surface blue. Hyphæ uniform in outline, protoplasm homogeneous, many vacuoles. 120 hours—fully matured.
1–12	48	Thin ring at edge, small colonies submerged. 120 hours—surface covered, blue. Hyphæ, uneven outlines, protoplasm granular, walls broken easily. 240 hours—fully matured.
1–10	48	Slightly less developed than in the preceding, otherwise alike. 240 hours—fully matured.
1–5	120	Thin interrupted ring at edge. 168 hours—spores swollen, irregular in outline, filled with coarsely granular protoplasm, walls broken by cover glass. 336 hours—surface dotted with colonies, showing blue spots. 348 hours—fully matured.
1–2	——	——————————

The effect of the sodium benzoate on the development is shown in a retarded and abnormal development, these being accentuated as the amount of the salt was increased, to a point where no development occurred.

Penicillium Grown in 10% Tomato Gelatin. 100 cc. 70° F.

Per Cent Sod. Benz.	Time to Germ. Hours.	Development.
——	24	White colonies dotting surface. 96 hours—surface covered, green, mature.
1–12	24	Same as control.
1–10	24	Same as control.
1–8	24	Same as control.
1–6	24	Less developed than control. 96 hours—surface covered, nearly all green. 432 hours—mycelium curled up round edges.
1–4	48	Small white colonies dotting surface. 96 hours—surface covered, nearly all green. 342 hours—mycelium curled from edges to center.
1–2	72	Small white colonies dotting surface. 96 hours—surface about two-thirds covered, center green. 342 hours—mycelium curled up so as to enclose the spores.

In this experiment in which a solid medium was used, the effect of the sodium benzoate on the development of the mould was not marked, except in the cultures containing the larger amounts. In these there was a slight retardation, and also a curling up by the mycelium from the substratum.

Penicillium Grown in Tomato Bouillon in Moist Chambers, 70° F.

Per Cent Sod. Benz.	Time to Germ. Hours.	Development.
——	24	Short tubes formed. 48 hours—well developed colonies formed.
1–12	24	Short tubes formed. 48 hours—colonies smaller than in the control.
1–10	24	Tubes just forming. 48 hours—less development than in the 1–12th solution.
1–8	24	Less than in the 1–10th solution. 48 hours—less than in the 1–10th solution.
1–6	48	Spores germinated, shorter tubes than in the 1–8th solution.
1–2	96	Spores germinated, short tubes.

Note.—In 120 hours the control was exhausted, having empty hyphæ; the other cultures, with the exception of the ½% solution, have hyphæ with many vacuoles in the protoplasm, the conidiophores formed are apparently normal.

The effect of the antiseptic on the development of the mould grown in the moist chambers was not so pronounced as when a larger quantity of solution was used. Neither was the effect always uniform; sometimes the spores in the ¼% and the ½% solutions merely swelled, but no development of hyphæ occurred; in others short tubes developed from some of the spores, while still other spores showed no changes whatever.

To test the effect of the larger quantity of solution, inoculations were made into flasks containing 100 and 500 cc., respectively, of the solutions. The results indicated that the effect of the antiseptic on the mould development was greater when grown in the larger quantity of the solution.

Penicillium Grown in Tomato Pulp, in Petri Dishes, 65° F.

Per Cent Sod. Benz.	Time to Germ. Hours.	Development.
---	72	White colonies dotting surface. 96 hours—spores formed. 192 hours—surface covered, green.
1–12	144	White colonies growing up on side. 192 hours—spores formed on one side, colonies starting in center.
1–10	192	Colonies started in center. 312 hours—spores formed.
1–8	144	White colonies growing up one side. 192 hours—spores formed.
1–6	—	—
1–4	—	—
1–2	—	—

The pulp used in the experiments was of fine quality, and without any added ingredients such as are used in ketchup, and was used so as to determine the action of the sodium benzoate alone in the pulp. During the early stages of development, the mould grows down into the pulp, so that the whole surface of the hyphæ acts as an absorbent and would thus be affected to a greater extent than where only a part of the surface was in contact. This may serve to explain the more pronounced action of the sodium benzoate when in the pulp, and also the fact that after the mould has developed sufficiently to grow out of the pulp the development becomes more nearly normal.

The experiments were repeated many times and show slight variations, but the results as shown in the tables given are fairly representative.

BENZOIC ACID IN CRANBERRIES.

The occurrence of benzoic acid in cranberries has been cited so often, and in a manner that is often misleading, figures obtained by Lafar[1] on the low-bush cranberry, *Vaccinium Vitis Idaea*, being given for the common cranberries, *Vaccinium macrocarpon* and *Vaccinium Oxycoccus*. *Vac. Vitis Idaea* is a common form in Europe, growing wild, and also in this country in Nova Scotia, and though it is imported into the United States, it is not the form which is used to any extent as compared with *Vac. macrocarpon*, the large cranberry and *Vac. Oxycoccus*, the small cranberry. The amount of benzoic acid in *V. Vitis-Idaea*, as quoted by Lafar, varies from .64–.86 grams per liter.

Testimony[2] given before the committee on interstate and foreign commerce of the House of Representatives on the pure food bills in February, 1906, gave the amount occurring in raw cranberries as ½%, and that half of this was volatilized in the cooking. It was not stated which of the two American species was used for the determination. These figures have not been verified, so far as known to the writer, though diligent search has been made in many chemical and food journals.

There is undoubtedly an antiseptic present in cranberries, a fact known to any one who has made either cranberry jelly or sauce, as these can be kept without spoiling for a long time, even when exposed to the germs in the air.

Experiments were made to determine the effect of growth in cranberry juice on the development of the organism used in the previous experiments.

The cranberries selected were the small oval ones, said to contain the largest amount of the antiseptic and were tested in three ways:

1. 200 grams were crushed in a mortar, then covered with 200 cc. water, and allowed to stand for 12 hours, after which the juice was filtered.

2. 200 grams placed in an open vessel in the sterilizer and steamed until the cranberries were soft, after which they were crushed in a mortar,

[1] Lafar, F., Technical Mycology, Vol. I, p. 117, 1898.
[2] The Canner and Dried Fruit Packer, Vol. XXVI, No. 8.

had 200 cc. water added, then stood for 12 hours, after which the juice was filtered.

3. This was similar to 2, but the vessel was covered closely during the steaming.

For the experiments, 50 cc. of the filtrate from each set were placed in flasks. They were inoculated with the mould without any previous sterilization. The following table shows the time required for, and the effect on, development:

PENICILLIUM GROWN IN CRANBERRY JUICE.

MEDIUM.	Days to Germinate.	Development.
Raw juice	4	Short tubes. 7 days—only small white colonies.
Juice cooked, open	2	Short tubes. 7 days—colony green.
Juice cooked, closed	2	Surface nearly covered, white. 7 days—surface green.
Raw juice + 10cc. water	3	Small white colony. 7 days—surface green.
Juice cooked, open + 10cc. water	2	Surface nearly covered. 7 days—surface green.
Juice cooked, closed + 10cc. water	2	Surface nearly covered. 7 days—surface green.

After two weeks' development, the color of the spores of Penicillium was a yellowish green, instead of the normal bluish green, and the mycelium was very scantily developed. The surface had a somewhat granular appearance, instead of the smooth, even appearance of a normal culture. The filaments, when seen with the microscope, were thin, shrunken, and clear, with distorted outlines. The cultures were kept for months, remaining scanty and granular looking, and a peculiar feature was that no development of bacteria occurred, even in the uninoculated ones, though no sterilization had been done, and the uninoculated were exposed to the air at times. Sometimes the cultures become infected with yeast, which will develop in a normal manner, seemingly not affected as is the mould.

The antiseptic in the cranberries was weakened by the cooking, and it made little difference whether the vessel in which they were cooked was open or closed, development occurring in the same time in both. It is probable that the contained acid would evaporate to a greater extent if the cooking had been done on a stove, as they are cooked ordinarily, instead of in the enclosed sterilizer. It is also probable that some of the antiseptic property is due to the astringent present, which is said to be destroyed in the cooking[1], and which gives the raw cranberry its unpleasant taste. This is further borne out by the fact that the effect produced on the mould is different from that produced by the benzoate, used either as a salt or acid.

In nearly all the experiments with other media, in which sodium benzoate was used, in the lesser amounts, the organisms though delayed in germination, and at first forming an abnormal development, apparently became accustomed to their environment, and later developed fairly normally, which is different from the result in the cranberry juice, in the latter the restrictive effect persisted.

CONDIMENTS.

The condiments used were those which are used in ketchup—salt, sugar, celery, cinnamon, cloves, garlic, ginger, mace, mustard, paprika, black, white, and red pepper, and vinegar. Along with these acetic acid and alcohol were also tested. With the exception of the cinnamon and cloves, the other spices showed slight antiseptic properties, so are not reported. They were tested in the form of infusions, made according to the method of the U. S. pharmacopoeia[2], also as acetic acid and oil extracts. The ordinary table salt and sugar were used. The quantities of the condiments used in the report were determined after a series of experiments had been made to locate their point of inhibition.

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[1] Willis, C. R., Practical Flora, p. 174, 1894.
[2] U. S. Dispensatory, 19th ed., p. 651.

EFFECT OF CONDIMENTS ON DEVELOPMENT OF PENICILLIUM.

Moist chamber cultures, capacity 1.23 cc.

Solutions.	20 Hours.		26 Hours.	48 Hours.		Description.
	Germination.	Hyphæ, Length in μ.	Hyphæ, Length in μ.	Germination.	Hyphæ, Length in μ.	
Tomato bouillon	100%	56.0	....	....	∝	
Tomato bouillon + salt, 5%	—	—	—	100%	∝	
" " " 10%	—	—	—	—	—	
" " " 25%	—	—	—	—	—	
Tomato bouillon + sugar, 25%	15%	30.7	....	75%	....	Hyphæ distorted, appear empty.
" " " 50%	5%	24.0	....	75%	....	" " " "
" " " 75%	—	—	—	—	—	
Tomato bouillon + cinnamon, 5%	100%	136.0	369.1	....	∝	Nearly normal, spores blue, vertical sterigmata.
" " " 10%	30%	40.0	230.7	....	∝	Like the 5% but less development.
" " " 25%	20%	32.0	200.0	....	∝	" 10% " " "
Tomato bouillon + cloves, 5%	100%	88.0	569.1	....	∝	Hyphæ like the 5% cinnamon, heads close to germinated spore, few conidia formed.
" " " 10%	25%	48.0	369.1	50%	∝	Hyphæ well developed, no conidiophores.
" " " 25%	—	—	—	10%	722.8	
Tomato bouillon + alcohol, 5%	—	—	—	100%	Swollen	
" " " 10%	—	—	—	10%	"	
" " " 15%	—	—	—	—	—	
" " " 20%	—	—	—	—	—	

EFFECT OF CONDIMENTS ON DEVELOPMENT OF PENICILLIUM.

Flask cultures, 50 cc. medium; 70° F.

Solutions.	48 Hours.	72 Hours.	96 Hours.	120 Hours.
Tomato bouillon	Ring, heavy colonies, blue spots, many submerged colonies.	Ring $\frac{1}{2}''$ wide, older part blue	Ring, $\frac{3}{4}''$, green, surface dotted.	Ring thick, surface covered olive.
Tomato bouillon + salt, 5 %	Few small, submerged colonies.	Many small, submerged colonies.	Few surface colonies	Surface colonies curled, light green.
" " " 10 %	—	—	—	—
" " " 15 %	—	—	—	—
Tomato bouillon + sugar, 25 %	Thin ring at edge	Ring $\frac{1}{4}''$ wide, edge blue	Small surface colonies	Ring thick, surface nearly covered, green.
" " " 50 %	—	—	Ring barely perceptible	Thin ring at edge, green.
" " " 75 %	—	—	—	—
Tomato bouillon + cinnamon, 5 %	Large colonies at edge	Colonies enlarged, blue center, many sub.	Colonies increased in number, older ones green.	Surface nearly covered, thick, wrinkled, olive.
" " " 10 %	—	One small colony	Colony enlarged, center green	More colonies formed, oldest one olive.
" " " 25 %	—	—	—	—
Tomato bouillon + cloves, 5 %	Thin ring at edge	Ring $\frac{3}{8}''$, edge blue, many sub. col.	Ring $\frac{1}{2}''$, edge green	Ring thick, wrinkled, olive. Surface colonies.
" " " 10 %	—	Tiny colonies at edge	No change	Tiny colonies curled.
" " " 25 %	—	—	—	—
Tomato bouillon + alcohol, 5 %	Tiny colonies on surface	Colonies slightly enlarged, many sub. col.	Few colonies $\frac{3}{8}''$, curled	Colonies thick, wrinkled, blu .
" " " 10 %	—	—	—	—
" " " 15 %	—	—	—	—
" " " 20 %	—	—	—	—
Tomato bouillon + acetic acid, $\frac{1}{2}$ %	—	—	Few surface colonies	Slight increase.
" " " " 1 %	—	—	—	—
" " " " 2 %	—	—	—	—

The tables show the germinative power and also the gross effect in development. The moist chamber cultures gave closer results on the germination and the earlier effects on growth, but were not as satisfactory as the flask cultures in showing the general effect on development. In the flasks the amount of development, the method of formation, and the color in maturing could be seen to better advantage.

The 5% salt had a retarding effect, and also induced an abnormal development, the growth being confined to a small amount of curled surface mycelium not spreading normally over the surface, and some submerged colonies. The sugar caused a delayed, stunted development, sometimes the growth in the 50% consisting of a scanty, submerged mycelium. In lesser amounts than 25% a thin surface mycelium forms, with a thick layer of spores. The cinnamon and cloves in the 5% solutions were stimulating, while stronger solutions retarded the development, the cloves being stronger in action than the cinnamon. In the 5% solution of alcohol in the moist chambers the conidia became swollen as they do previous to germination, but no further development took place. In the flask cultures the action of the alcohol was weaker, the conidia germinating and forming small colonies, which was probably due to the evaporation of the alcohol, causing the solution to become weaker on standing. The ½% acetic acid retarded growth, and caused the mycelium to wrinkle. In all the flask cultures with the exception of the alcohol the effect of the condiment of corresponding per cent. was stronger than in the moist chambers.

PRESERVATIVES.

The preservatives are those which have been used in foods, and used in approximately the same amounts. The results show that they have a retarding effect on the development of the mould, even when in small amounts, and that most of them become inhibitive when the amounts are increased, the increase not exceeding the amounts which have been used in foods.

EFFECT OF PRESERVATIVES ON DEVELOPMENT OF PENICILLIUM.

Moist chamber cultures, 80° F.

Solutions.	21 Hours.		28 Hours.	48 Hours.	Description.
	Germination.	Hyphæ, length in μ.	Hyphæ, length in μ.	Hyphæ, length in μ.	
Tomato bouillon	100%	307.6	999.7	∝	Conidiophores formed.
Tomato bouillon + sodium benzoate, 1-10%	100%	46.1	169.2	∝	Conidiophores formed.
" " " " 1-5%	—	—	30.8	∝	Conidiophores starting. Only about 50% spores have developed.
" " " " 1-2%	—	—	—	—	
Tomato bouillon + benzoic acid, 1-10%	25%	30.8	46.1	∝	Only about 35% spores have developed.
" " " " 1-5%	—	—	15.4	769.0	" " 50% " "
" " " " 1-2%	—	—	—	—	
Tomato bouillon + borax, 1-10%	100%	276.8	461.4	∝	Conidiophores formed.
" " " 1-5%	100%	15.4	30.8	....	No change after 28 hours.
" " " 1-2%	—	—	—	—	
Tomato bouillon + boric acid, 1-10%	50%	76.9	307.6	492.2	
" " " " 1-5%	50%	15.4	30.8	....	No change in hyphæ after 28 hours. 50% spores swollen.
" " " " 1-2%	—	—	—	—	About 10% of the spores swollen.
Tomato bouillon + sod. salicylate, 1-10%	100%	46.1	307.6	∝	Conidiophores formed, but have few sterigmata.
" " " " 1-5%	100%	123.0	169.2	∝	Thin mycelium, conidiophores formed close to germinated spore
" " " " 1-2%	10%	15.4	76.9	∝	
Tomato bouillon + salicylic acid, 1-10%	50%	30.8	123.0	∝	Like the 1-5% sod. salicylate.
" " " " 1-5%	—	—	—	—	
" " " " 1-2%	—	—	—	—	

EFFECT OF PRESERVATIVES ON DEVELOPMENT OF PENICILLIUM—Continued.

Solutions.	21 Hours.		28 Hours.	48 Hours.	Description.
	Germination.	Hyphæ, length in μ.	Hyphæ, length in μ.	Hyphæ, length in μ.	
Tomato bouillon + sod. sulphite, 1-10%	100%	30.8	92.3	769.0	Like the 1-5% sod. salicylate.
" " " " 1-5%	—	—	—	153.8	
" " " " 1-2%	—	—	—	—	
Tomato bouillon + saccharin 1-10%	10%	15.4	61.5	∝	Conidiophores formed.
" " " 1-5%	—	—	30.8	∝	Conidiophores small, few.
" " " 1-2%	—	—	15.4	507.5	
Tomato bouillon + cop. sulphate, 1-10%	100%	30.8	169.2	∝	
" " " " 1-5%	—	—	15.4	276.8	
" " " " 1-2%	—	—	—	—	
Tomato bouillon + sod. formate, 1-5%	10%	19.0	....	....	
" " " " 1-2%	2%	11.4	....	....	
Tomato bouillon + formic acid, 1-5%	2%	11.4	....	....	
" " " " 1-2%	50%	11.4	....	....	

EFFECT OF PRESERVATIVES ON DEVELOPMENT OF PENICILLIUM.

Flask cultures, 50cc. medium; 80°.

Solutions.	24 Hours.	48 Hours.	72 Hours.	18 Days.	*
Tomato bouillon	Tiny colonies at edge.	Edge colonies $\frac{3}{8}''$	Ring $\frac{1}{2}''$, center blue, many sub. col.	Surface covered, wrinkled, olive. Liquid nearly black.	1
Tomato bouillon + sod. benzoate, 1-10 %	—	Tiny colonies at edge, few submerged.	Slight increase	Surface covered, wrinkled, olive Liquid darkened.	2
" " " " 1-5 %	—	—	—	Surface nearly covered, green, mycelium still growing.	8
" " " " 1-2 %	—	—	—	—	
Tomato bouillon + benzoic acid, 1-10 %	—	—	—	Surface nearly covered, wrinkled, green, mycelium growing.	6
" " " " 1-5 %	—	—	—	—	—
" " " " 1-2 %	—	—	—	—	—
Tomato bouillon + borax, 1-10 %	Tiny colonies at edge	Edge colonies $\frac{1}{4}''$, many submerged.	Ring $\frac{3}{8}''$	Surface covered, wrinkled, drab, thin spore layer. Liq. dark.	1
" " " 1-5 %	—	—	Few tiny submerged colonies	Few surface col., curled, drab, thin spore layer, many sub.	3
" " " 1-2 %	—	—	—	—	—
Tomato bouillon + boric acid, 1-10 %	Tiny colonies at edge	Edge colonies $\frac{1}{4}''$, many submerged.	Few surface colonies curled.	Surface partly covered, curled, thin spore layer, many sub.	1
" " " " 1-5 %	—	—	Few submerged colonies	Same general appearance as 28, but less developed.	3
" " " " 1-2 %	—	—	—	Tiny submerged colonies.	14
Tomato bouillon + sod. salicylate, 1-10 %	—	—	Few colonies at edge	Surface nearly covered, wrinkled, olive, liquid darkened.	3

*No. days to germinate.

EFFECT OF PRESERVATIVES ON DEVELOPMENT OF PENICILLIUM—Continued.

Solutions.	24 Hours.	48 Hours.	72 Hours.	18 Days.	*
Tomato bouillon + sod. salicylate, 1–5 %	———	Few edge colonies, few submerged.	Larger than the 1–10 %....	Same general appearance as the 1–10 % but less devel. Liquid slightly darkened.	2 2
" " " " 1–2 %	———	——— ———	———	Colony on side, green, many sub. Liquid darkened slightly.	12
Tomato bouillon + salicylic acid, 1–10 %.	———	Few edge colonies, few submerged.	Colonies curled...........	Surface $\frac{2}{3}$ covered, dark olive. Liquid darkened.	2
" " " " 1–5 %.	———	———	———	Like the 1–10 %, but less developed, liquid lighter.	4
" " " " 1–2 %.	———	———	———	———	—
Tomato bouillon + sod. sulphite, 1–10 %.	———	———	———	Surface nearly covered, olive, many sub. col. Liq. darkened.	4
" " " " 1–5 %.	———	———	———	Ring at edge 1″, blue to olive, many sub. col. Liq. darkened.	10
" " " " 1–2 %.	———	———	———	Few edge colonies, green, few submerged colonies.	15
Tomato bouillon + saccharin, 1–10 %....	Tiny colonies at edge......	Edge colonies $\frac{1}{4}$″, many submerged.	Interrupted ring $\frac{1}{2}$″, center blue.	Surface nearly covered, wrinkled, olive. Liq. dark slightly.	1
" " " 1–5 %....	———	Edge colonies $\frac{1}{8}$″, many submerged.	Ring $\frac{1}{4}$″, blue spots.	Slightly less development than the 1–10 %.	2
" " " 1–2 %....	———	Tiny colonies at edge, many submerged.	Interrupted ring, surface colony curled.	Same as the 1–5 %.	2
Tomato bouillon + cop. sulphate, 1–10%.	———	Ring of colonies, many submerged.	Surface nearly covered, older parts blue.	Surface nearly covered wrinkled, olive, many sub.	2
" " " " 1–5 %.	———	Ring of colonies, many submerged.	Surface nearly covered, older parts blue.	Surface nearly covered, wrinkled, olive, many sub.	2

" " " " 1-2 %.	———	Thin ring.	Surface colonies curled.	Enlarged blue spots, ring olive.	2
Tomato bouillon + sod. formate, 1-5 %.	———	White ring, few submerged.	Thick ring, blue at edge, many sub.	Surface covered, olive, many-submerged.	2
" " " " 1-2 %.	———	Thin interrupted ring, few submerged.	Many surface colonies.	Surface covered, olive, many submerged.	2
Tomato bouillon + formic acid, 1-5 %.	———	Thin ring, few submerged colonies.	Ring enlarged, blue at edge.	Surface covered, olive, many submerged.	2
" " " " 1-2 %.	———	———	———	Surface nearly covered, green, many submerged.	4

*No. days to germinote.

The results indicate the acid to be stronger in its effect than the corresponding salt, though *Penicillium* is a plant which grows luxuriantly on acid fruits. The sodium sulphite bleached the solutions, ½% being a pale straw color. The copper sulphate solutions were also changed in color, the ½% solution was a decided green.

In all cases microscopic examination was made of material from the flask cultures, and indicated more conclusively than the gross appearance the effect on the development. Submerged colonies have been used for the reports in the table, as they are more uniform. The surface colonies have the characteristics of the submerged in their earlier growth, but as development proceeds and the hyphæ grow away from the medium, the characteristics may change, sometimes more nearly approaching the normal, or they may develop characters more pronounced than the submerged. In a few instances, only submerged colonies, and in the raw cranberry and cinnamon solutions only surface colonies, developed. In making measurements the germinated spores were used, and only the average sizes; the extreme in size was avoided, as not giving a fair estimate of the effect of the preservative. Where only one measurement is given, it indicates that the spores were fairly uniform; where two measurements are given, the spores showed such strong variation that an average was taken of the smaller and also of the larger instead of taking the average of the two sets. The hyphæ were measured but varied so much that it was thought a better estimate could be obtained from the photographs.

MICROSCOPIC APPEARANCE OF PENICILLIUM GROWN IN PRESERVATIVE SOLUTIONS.

PRESERVATIVE.	Size of Germinated Conidia in μ.	Characteristics of Development.
Control	8.5	Hyphæ somewhat irregular in outline near germinated conidia, tapering tips, homogeneous protoplasm, many large round vacuoles.
Salt, 5%	7.6	Hyphæ short, distorted, homogeneous protoplasm, no vacuoles, blunt tips.
Sugar, 50%	7.6	Hyphæ shrunken, distorted, homogeneous protoplasm, vacuoles show as pink spots, giving a beaded appearance.

MICROSCOPIC APPEARANCE OF PENICILLIUM—Continued.

PRESERVATIVE.	Size of Germinated Conidia in μ.	Characteristics of Development.
Cinnamon, 10%	15.2	Hyphæ swollen, blunt tips, protoplasm finely granular, without cohesion, walls break with weight of cover-glass. Few sepia in some, in others prominent. Few side branches. Hyphæ disorganized when placed in water.
Cloves, 10%	13.3	Hyphæ swollen, blunt tips thicker than older part, short thick side branches, finely granular protoplasm, not so badly disorganized as in cinnamon.
Cranberry, raw	7.6	Hyphæ shrunken, distorted, tendency to develop conidiophores close to germinated conidia.
Cranberry, cooked, open	6.7	Hyphæ thin, tapering, protoplasm finely granular.
Cranberry, cooked, covered	7.6	Hyphæ slender, tapering to threads, protoplasm reduced to lining of walls, coarse granules, many septa.
Alcohol, 5%	15.2	Hyphæ swollen, distorted, walls tough, protoplasm clear.
Acetic acid, 1–5%	11.4	Hyphæ enlarged, blunt tips, few septa, short side branches, proto plasm finely granular.
Sodium benz., 1–5%	15.2 38.0	Hyphæ and conidia swollen and distorted, no uniformity in formation of septa, some hyphæ, few, others many; protoplasm coarsely granular, filling tubes; walls break readily.
Benzoic acid, 1–10%	15.2 49.4	Hyphæ larger than in benzoate, more easily broken, distorted. Less swollen hyphæ have less distortion and less disorganization.
Borax, 1–5%	9.5 15.2	Hyphæ short, distorted or long and swollen, blunt ends, protoplasm clear, homogeneous or finely granular.
Boric acid, 1–5%	15.2 19.0	Hyphæ swollen, short thick side branches, blunt ends, protoplasm finely or coarsely granular.
Sodium salicylate, 1–5%	9.5 15.2	Hyphæ as wide as germinated conidia, few septa, granular protoplasm.
Salicylic acid, 1–5%	15.2 30.4	Hyphæ and conidia swollen, some of the conidia much elongated, hyphal ends blunt, few septa, protoplasm yellow, coarsely granular, protoplasm and walls disorganized.
Sodium sulphite, 1–5%	11.4	Hyphæ enlarged, few septa, protoplasm coarsely granular.
Saccharin, 1–5%	13.3	Hyphæ enlarged, some much swollen, slight distortion, clear, homogeneous protoplasm, thick, stunted conidiophores.
Copper sulphate, 1–5%	11.4	Hyphæ enlarged, slight distortion, protoplasm yellow, finely granular, dirty appearance.

Microscopic Appearance of Penicillium—Continued.

Preservative.	Size of Germinated Conidia in μ.	Characteristics of Development.
Sodium formate, 1–5%......	13.3	Hyphæ swollen, coarsely granular protoplasm, short side branches, blunt ends, disorganized, or normal size with fine granules and many vacuoles, some cells empty.
Sodium formate, 1–2%......	13.6	Hyphæ swollen, coarsely granular, short side branches which do not develop, blunt ends, disorganized, break easily.
Formic acid, 1–5%..........	14.0	Hyphæ swollen coarsely granular, blunt ends, many broken, or normal size, finely granular, many vacuoles.
Formic acid, 1–2%..........	11.4 41.8	Hyphæ swollen, coarsely granular, yellow, distorted, badly disorganized, break easily. Nearly all germinated conidia broken.

The sugar and salt caused the hyphæ to shrink and to assume distorted shapes when in sufficient amounts to cause a retardation. The cranberry juice, both raw and cooked, also caused shrinkage, and the raw juice a distortion. All of the others caused the conidia and hyphæ to swell and some of them also caused a distortion. The mould grown in the alcohol solution had tough walls in spite of the swelling, and a clear, sharp appearance. The borax and boric acid also produced a clear appearance. The sodium benzoate, benzoic acid, sodium salicylate, salicylic acid, sodium formate, formic acid, acetic acid, and cinnamon produced swelling, distortion, a disorganization of both the protoplasm and cell wall, and a yellowing of the protoplasm. The cell wall had no elasticity nor toughness, so that the placing of the cover-glass gently on a mount was sufficient to break the walls of the more distended hyphæ and to allow the protoplasm to flow out. The protoplasm appeared to be without coherence; when the wall gave way, it flowed in all directions, as if it were composed of loose particles having no cohesion. The sodium sulphite, saccharin, cloves, and copper sulphate growths had similar characteristics to those enumerated for the other preservatives, but not so strongly developed.

In summarizing the results, there seem to be two different actions induced by the action of the substances on the protoplasm, in one case a plasmolyzing effect causing a shrinkage and distortion, as in the salt and sugar, and in the other case a toxic effect producing a disorganization of both the protoplasm and wall, and a discoloration of the protoplasm, the substances showing varying degrees of toxic power.

GERMINATION FROM PRESERVATIVE MATERIAL.

To determine if there were a permanent deleterious effect produced on the plant through the toxic effect of the chemicals, inoculations were made from two weeks' old cultures into tomato bouillon. The result is shown in the table:

GERMINATION OF PENICILLIUM GROWN IN PRESERVATIVE SOLUTIONS 14 DAYS.

PRESERVATIVE.	Number Days to Germinate.	Stage of Development in 5 Days.
Control	1	Surface covered, green.
Sodium benz. 1-10%	2	Surface covered, green.
" " 1-5%	2	" " "
" " 1-2%	4	Thin ring, having blue dots.
Benzoic acid, 1-10%	2	Surface nearly covered, green.
" " 1-5%	4	Small surface colonies, blue.
" " 1-2%	-	———
Borax, 1-10%	2	Surface covered, green.
" 1-5%	2	One surface colony, green.
" 1-2%	-	———
Boric acid, 1-10%	1	Surface covered, green.
" " 1-5%	2	" nearly covered, green in center.
" " 1-2%	5	Few submerged colonies.
Sod. salicylate, 1-10%	1	Surface covered, green.
" " 1-5%	2	" " "
" " 1-2%	1	" " "
Salicylic acid, 1-10%	1	" " "
" " 1-5%	1	" "
" " 1-2%	-	———
Sod. sulphite, 1-10%	3	Colonies on surface, green.
" " 1-5%	2	Surface covered, "
" " 1-2%	-	———
Saccharin, 1-10%	2	Surface covered, green.
" 1-5%	2	" " "
" 1-2%	1	" " "
Copper sulphate, 1-10%	3	Colonies on surface, green.
" " 1-5%	2	Surface covered, "
" " 1-2%	2	" " "

The germination and subsequent development indicate that the preservative affected the conidia deleteriously, as some were retarded, while the conidia from the solutions showing the strongest effects on the previous development, did not germinate, except from the $\frac{1}{2}$% boric acid solution which formed a few submerged colonies, no surface development taking place. Lafar[1] states that the waterproof character of the conidial walls has a value in preventing the entrance of poisons to the protoplasm, but in the cases noted it is either dissolved by the chemicals or powerless to prevent their passage, for the results indicate that they exercised a decided toxic effect on the protoplasm.

SUMMARY.

Salt and sugar injure the plant by preventing normal action of the protoplasm through plasmolysis.

Alcohol hardens the protoplasm and walls and prevents development.

Cranberry juice, both raw and cooked, retards development and causes shrinkage, though not having the appearance of the shrinkage due to plasmolysis.

All of the other chemicals tested acted as poisons on the protoplasm, retarding development and causing abnormal swelling and disorganization of varying degrees of intensity on both the protoplasm and cell membrane.

Lafayette, Ind.

EXPLANATION OF PHOTOGRAPHS.

The photographs have the same magnification, ×395, so that comparisons may be made as to the effect of the preservatives. The specimens were submerged colonies in all cases except the raw cranberry and cinnamon, and no submerged colonies developed in these solutions. The endeavor was to have all of the same age, but this was impossible, as some developed much more rapidly than others, and in those which were slow in developing it was impossible to determine the changes which the conidia may have been undergoing before the development had attained the colony stage. The submerged colonies were used as soon as they made their appearance. In some of the specimens that show little or no swelling the disorganization can be seen in the collapsed ends of the hyphæ and the floating fragments of protoplasm.

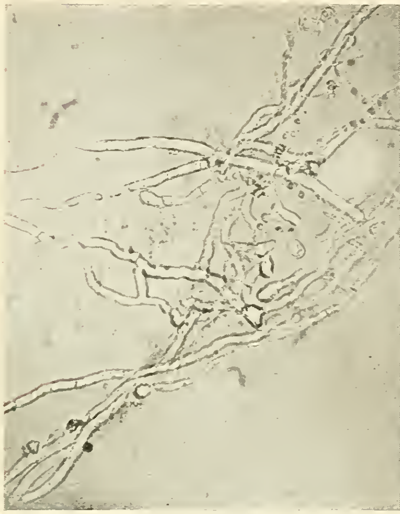
[1] Lafar, F., Technical Mycology, Vol. II, Part 1, p. 40.

1. Control - conidiophore and hyphæ.

2. Mycelium, grown in tomato pulp.

3. Hyphæ from ketchup preserved with sodium benzoate. The label gave amount as 1-10%.

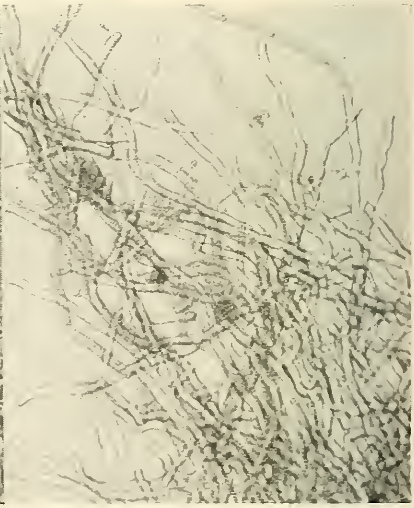
4. Mycelium from 5% salt solution.



5. Mycelium from 50% sugar solution.

6. Mycelium and conidiophores from 10% cinnamon solution.

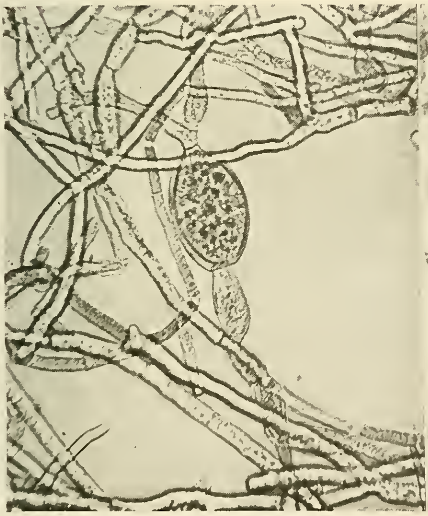
7. Mycelium showing disorganized hyphæ from 10% clove solution.



8. Mycelium from 10% vinegar (50 grain.)

9. Mycelium from 1·5% acetic acid solution.

10. Conidia from 9.6% alcohol solution.



11. Mycelium and enlarged conidium from 1·5% sodium benzoate.

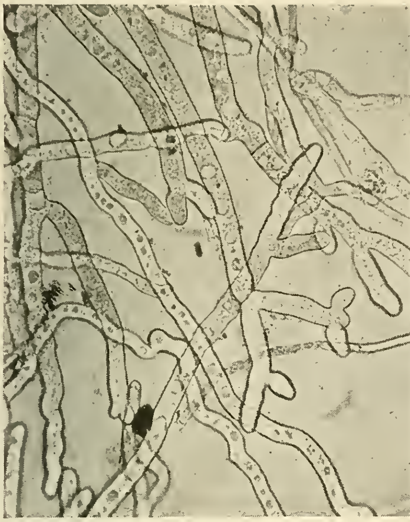
12. Mycelium swollen and disorganized from 1·5% benzoic acid solution.

13. Colony from raw cranberry solution.

14. Mycelium with disorganized hyphal ends from cooked (open) cranberry solution.

15. Mycelium with swollen conidia from cooked (closed) cranberry solution.

16. Mycelium from 1-5% borax solution.



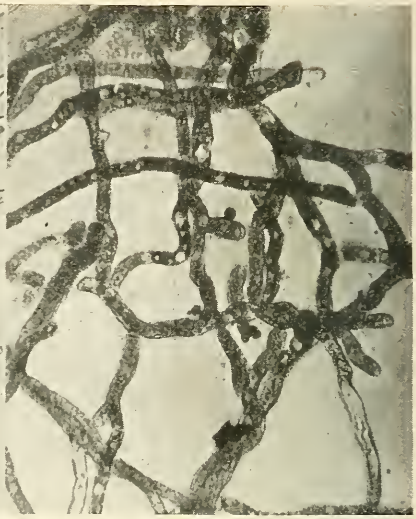
17. Mycelium from 1-5% boric acid solution.

18. Mycelium from 1-5% sodium salicylate solution.

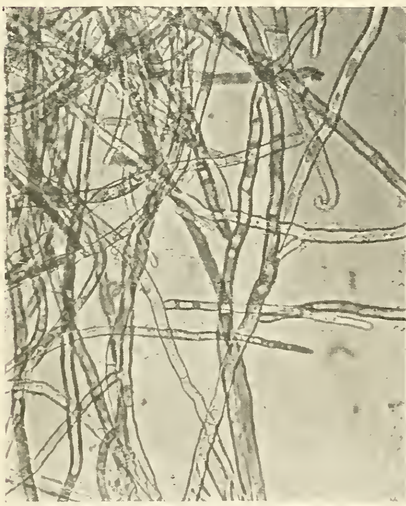
19. Germinated conidia and hyphæ from 1-5% salicylic acid solution.

20. Mycelium from 1-5% sodium sulphite solution.

21. Mycelium from 1-5% saccharin solution.



22. Mycelium from 1–5% copper sulphate solution.



23. Mycelium with disorganized hyphal ends from 1-5% sodium formate.

24. Mycelium with disorganized hyphæ from 1-5% formic acid.

Further Notes on Timothy Rust.

By Frank D. Kern.

At the last meeting of the Academy, November 27, 1908, the writer presented a short paper on "The Rust of Timothy,"[1] in which the history of its occurrence in this country was discussed and its distribution at that time was given. The remark was made, although not incorporated in the paper, that this rust had not yet been reported from Indiana but that it was becoming more general in its distribution and might be expected here sooner or later. Since this prediction has come true within the year it is considered worthy of mention at this time. A collection consisting only of summer spores (urediniospores) was made in October, 1909, near Columbus, Indiana.[2] Last year the fungus was known in states both east and west of Indiana, so that while this report does not extend the range geographically, it is nevertheless of especial interest since it is the first definite information we have of its advent into the State. A second collection made in November at the same locality shows also a few winter spores (teliospores). It is of further interest to note that where the rust was found it was low ground with unusually rich soil. The place was originally a wet swamp but is now tile-drained. None was found on the high land adjoining. Low regions furnish more moisture in the atmosphere surrounding the plants, especially at nights, and this means better conditions for the germination of the spores.

In the paper read last year it was said that this rust was seemingly increasing in its distribution. The season of 1909 has proved the correctness of this prediction. A specimen was collected in September, 1909, in Maine by Dr. J. C. Arthur. This is the first collection that the writer has seen from the New England states. Last year Wisconsin was the most western state which had reported the rust. This year it has been found as far west as Minnesota, according to a report recently received from an official of the U. S. Department of Agriculture.

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[1] Only an abstract of this paper appeared in the Proceedings of the Academy for 1908, p. 85, but it was published in full in Torreya, a Journal of the Torrey Botanical Club, Vol. 9, pp. 3-5, Jan., 1909.

[2] This collection was made by Mr. C. G. Hunter, on his farm near Columbus, and communicated by him to the writer.

The present known range is from Maine west to Minnesota, south to West Virginia and Indiana. Collections from the following states have been examined by the writer and are represented by specimens in the herbarium of Dr. J. C. Arthur at Purdue University, where the writer has carried on the major part of his studies. The collectors' names are included in parentheses.

Delaware (*Jackson*).
Indiana (*Hunter*).
Maine (*Arthur*).
Michigan (*Arthur, Kern*).
New York (*Webber, Reddick, Edgerton, Stone*).
Ontario (*Arthur, Dearness*).
Pennsylvania (*Sumstine*).
West Virginia (*Sheldon*).
Wisconsin (*Davis*).

During the year no additional facts have been brought out which throw any light on the specific standing of the timothy rust. The writer is still of the opinion that it is not entitled to specific rank and would include it under *Puccinia poculiformis* (Jacq.) Wettst. (*Puccinia graminis* Pers.) The statement made last year could, perhaps, be somewhat modified. Rather than calling it a race, physiological species or form species, it might be better to consider it a variety or subspecies since it does, as previously pointed out, possess some slight morphological differences from the typical form, particularly in the smaller æcial cups and the more delicate uredinial mycelium.

Purdue University,
Lafayette, Ind.

The Woodlot for Central Indiana.

By E. C. Pegg and M. B. Thomas.

INTRODUCTION.

The purpose of this paper is to show as accurately as possible with the information at hand the conditions of central Indiana woodlots and to make suggestions for their improvement and perpetuation.

A SHORT HISTORY OF INDIANA'S FORESTS.

Early explorers of Indiana found a wilderness of giant trees. Upon the tops of hills and higher ground were such trees as beech, hickory, oak, hard maple, walnut, ash and tulip; in the richer lowlands were the elms, buckeye, basswood and soft maples; and tall sycamores and overhanging willows lined the banks of the streams. It was not uncommon to find trees nearly two hundred feet in height and twenty to twenty-five feet in circumference. Everywhere smaller trees, shrubs and herbaceous plants struggled for their requisite amounts of sunlight. A spongy mass of forest litter made a floor that held rainfall and fed the innumerable springs, which in their turn supplied the streams and rivers with a constant and uniform volume. Such was the unbroken forest.

Clearing.—It was soon discovered that Indiana's soil was well adapted to agriculture. The early settlers began the work of forest destruction by clearing their homesteads for agricultural purposes. Regular log-rollings were held at which tree after tree was cut down, piled in log heaps and burned. Such work at that time was justifiable because timber was very plentiful and because the ground thus cleared was necessary to furnish a living for the ever increasing population.

Lumbering.—For this reason much of the land was cleared. Official records, which begin in 1870, show an acreage of 7,189,334 acres in timbered lands. In 1880 only 4,335,000 acres were left. As Indiana became more thickly settled, better houses, cities, roads, railroads and factories were being built, each requiring a certain amount of timber for construction. And in additional ways the consumption steadily increased. The towns and cities afforded market places, the roads and railroads a means of transportation for lumber. Thus began the other chief influence in forest destruction. By 1890 over 2,500,000 acres more were cleared, of which 75,000 acres became waste land. The timber supply of the East was falling, the demand increasing. Then Indiana ranked fifth with the states of the Union in the total output of lumber. In 1907 she ranked twenty-seventh.

At the present time there are probably less than a million acres of woodland in the State. This fact shows us the truth of the prophecy made twenty-five years ago that "At the present rate of consumption the forests of the State must soon cease to be commercially important." Very little now remains of the once seemingly inexhaustible supply of valuable timber, such as oak, walnut and yellow poplar.

Formation and Evolution of the Woodlot.—It is with this small remainder, especially that portion which lies in the central part of the State, that this paper deals. Formerly the farmer removed only the timber on the land he actually needed for agricultural pursuits. Gradually, as his needs increased, he extended the boundaries of his fields. The trees which he removed more than furnished him with firewood and other necessary timber. But when a market was opened up the owners began to cut the still vast forests for purely financial reasons. These became more and more exhausted until now very few acres of virgin timber, and comparatively few of any kind, remain. The farmer is at present apparently satitfied with his acreage of cultivated land, good timber is too scarce for extensive clearing or sale, and he is willing that a small portion of his farm should remain covered with a more or less depleted forest in order to provide wood for general purposes about the farm. These are the chief reasons for the presence of a woodlot today. Some timber was left because it was difficult to reach. Other tracts were left because of the pasturage they afforded in the grass which sprang up when the dense forest cover was partially removed. So, for one reason or another, or purely by accident, certainly not from choice, the woodlot of today occupies the position it does, oftentimes on the best land of the farm.

Present Conditions.—To get an idea of the present condition of these woodlots one need only travel a few miles in the country. In the distance he can see trees in a seemingly unbroken line. Closer examination, however, shows them to be in small, scattered patches ten to thirty acres in extent. After the best trees had been cut out and sold, the custom of cutting trees for special uses, such as handle stock and spoke material, led to the removal of the next best. All the most valuable species, black walnut, yellow poplar, white ash and the best oaks, have been cut away, leaving only a few maples, beech, ironwood, buckeye and the like. Many of these are crooked, defective and otherwise undesirable. At no time has any care been exercised to protect the undergrowth of young seedlings. The floor also presents a very different appearance from what it once did. A dense bluegrass sod has taken the place of the undergrowth and rich forest litter destroyed by constant pasturage. A heavy growth of grass is in itself an enemy of trees, for it not only makes reproduction harder but also smothers the roots of those already growing and robs the soil of moisture so essential to good tree development.

Some may ask what it matters if the conditions are thus. Are not the farmers in better circumstances now than they were forty years ago, yes, even ten years ago? Financially they are, but with wise and proper management of their woodlots they could realize still larger profits from their farms.

THE WOODLOT.

USES.

There are many reasons why woodlots are valuable. They furnish timber for all farm needs, protect buildings and crops, shelter live stock and materially help in preventing erosion and in ornamenting the country.

Firewood.—Firewood comes first in the list of timber used for farm purposes. The early methods of using wood in a fireplace were wasteful. The introduction of stoves resulted in a great saving of fuel. But fuel production was not the only purpose served by the forest. Now lack of timber and the cost of getting crooked and knotty trees cut into firewood have compelled the use of a substitute. Most farmers would be glad to have again a plentiful supply of cheap fuel.

Posts.—The setting of 1,000,000,000 (estimated) fence posts per year shows us another very important use for timber. According to the last census 8,715,661 of these posts were produced from the regular logging camps of the country. The use of these posts as supports for woven wire fence is very economical when compared to the former practice of building rail fences, many of which were of black walnut, the most valuable timber Indiana ever produced. Their gradual displacement by wire or picket fences is a great step towards forest preservation.

General Farm Uses.—Then there are other innumerable general uses about the farm for poles, boards and lumber. After all these needs are satisfied there should remain some timber (logs and railroad ties) for market.

Climatic Influences.—The influence of woodlots on the climate makes their presence desirable. A great deal has been written about forests as a factor in rainfall, but it has never been satisfactorily proved that they increase the total amount. It is known, however, that about twenty-eight per cent. less of the annual rainfall is evaporated within the woods than outside of them, and that the mean annual temperature of forest soil is about twenty-one degrees lower than that of cultivated fields. In summer this cool soil tempers the air above, and by starting currents from the adjoining fields lowers their temperature. Besides, woodlots, if situated in favorable positions, check strong winds, in this way protecting farm buildings and preventing fruit trees and crops from being blown down.

Shelter.—A woodlot is invaluable for the shelter it affords to live stock in both summer and winter. Less food is required to maintain the body warmth of animals when they are well protected from the cold winter winds. Therefore the use of grain in fattening stock is much economized. The cool shade offered by a small portion fenced off from the best part of the woodlot prevents fattening animals from losing flesh during the hot weather.

Aesthetic.—But these uses are not all. Every one knows that a good strip of timber greatly increases the value of a farm, for by this means not only the beauty of individual farms but also that of the entire community is increased as much, if not more, than by more expensive improvements. For no other reason than this each farmer should strive to maintain a well managed woodlot.

Water Supply.—Forests at the head waters of streams regulate their flow. As has been said before, the amount of evaporation within the forest is much less than that outside because the loose litter offers little capillarity to the water content of the soil and also permits of a more rapid absorption of heavy rainfall. The water is then given out to the springs and streams in an almost constant supply.

Erosion.—The problem of erosion is a very perplexing one, especially in a rolling country. The unlimited removal of forests has left but little resistance to the flowing away of rainfall, for everywhere the soil is more or less hard and compact. Water speedily runs over the surface, carrying soil and debris, which it deposits in the beds of streams. Places which wash badly are exceedingly common and cause the loss of much tillable land.

THE MODEL WOODLOT.

After a review of the reasons for maintaining woodlots it is well to consider the organization of a model woodlot.

Number of Trees.—It should contain the number of trees consistent with the most rapid development of the best timber. Trees should stand close enough in youth to stimulate growth in height and to produce long, clear trunks. As the stand approaches maturity more and more space is required for each tree until at last probably only one hundred and fifty to two hundred trees of the original three or four thousand remain per acre. Thinning is brought about naturally by the struggle for supremacy.

Distribution and Soil Cover.—Trees should be evenly distributed over the entire area, always close enough together to prevent many direct rays of the sun from reaching the ground in summer, since the large openings give grass, a very dangerous enemy of forests, a chance to grow. The ideal soil is loose, porous, rich in vegetable mould and is covered with a thick mat of leaves and leaf humus to the exclusion of all grass and light-demanding weeds.

Forest Cover.—The trees which should be found in a woodlot depend upon two factors—(1) the economic value and (2) silvical characteristics. Such trees as black walnut, black cherry, ash, oak, maple and poplar have the greatest economic values. The other factor has to do principally with the soil, moisture and light requirements. For example, sugar maple requires rich upland soil and very little sunlight for its best development, while sycamore will grow on any wet soil if it has plenty of light. Thus we shall find in a model woodlot the species best suited to the soil, water supply and the uses to which the timber is to be subjected. In no case should there be any worthless species.

Reproduction.—In order to maintain the desired acreage of our timber producing area some efficient method of reproduction is necessary. This is usually found in the presence of large and mature seed-bearing trees, which scatter their fruits over long distances until they find lodgment in places suitable for germination. Another method of reproduction is by stump sprouts or coppice growth. However, the size and quality of the timber produced in this way is much inferior to that formed from seedlings. For quick reproduction, advantage of this sprouting tendency should be taken in trees like the oak, basswood, catalpa and hickory.

HOW TO REACH THE MODEL.

The next point to demand our attention is how to bring the existing woodlots into model conditions. The examination of this problem may be conveniently considered under three heads: (1) Protection, (2) General Improvement Cuttings, and (3) Improvement of Type Stands.

PROTECTION.

The necessity for protection arises from the loss occasioned by grazing, fire, insects, fungi, wind and careless work in the woods.

Grazing.—Grazing injures a forest in two ways—by browsing and by trampling. Domestic animals browse sprouts and young seedlings, break off shoots and buds and gnaw the bark of trees. By the destruction of herbage the sharp hoofs of sheep cause loose soil to become looser and stiff soil to become more compact. Cattle and horses are much less harmful than sheep about trampling, although their hoofs frequently tear away small rootlets. This disturbance of the soil and soil cover seriously interferes with its water supply. In general the results of grazing make it imperative to exclude all stock from the woodlot.

Fire.—Fire is another great enemy of forests. The leaf litter and humus, young growth upon which the future supply depends, and mature trees are all affected. A single fire does not usually seriously injure older trees but a series of fires either burns them up completely or leaves them in such a weakened condition that they are blown down by wind or attacked by insects and fungi, and then furnish a source of infection for other trees. But in this thickly settled region fires are easily handled, for they can readily be seen and extinguished.

Insects.—The following conclusions regarding insect injury have been drawn from a careful investigation of the existing conditions throughout the state:*

(1) Insects causing the death of the tree:

(a) Found in extensive numbers and causing serious injury, as follows: Bark beetles on oaks, hickories and locust.

*Report of State Board of Foresty, 1907.

(b) Found in limited numbers and causing secondary injury as follows: Bark beetles on walnut, cherry, hackberry, elm, mulberry and ash; bark-boring grubs on oak and chestnut.

(2) Insects not causing the immediate death of the tree:

(a) Found doing serious damage to timber as follows: Carpenter worm on oak; wood borers on hickory; powder post borers on hickory.

(b) Injury to foliage: Nearly all species of trees found affected by one or more of the following forms, of which all except the cottony maple scale cause little damage: Leaf eaters, leaf miners, leaf rollers, saw flies, scale insects and gall flies.

The bark and wood borers can usually be detected by pits or deposits of fine sawdust around the holes. About the only remedy is to remove the infected trees at such times as will prevent the hatching of the larvae. Damage due to leaf insects is usually so slight that it may practically be disregarded.

Fungi.—Fungi attack trees in several ways. Some kill the roots, others grow upward from the ground into the trees and change the sound wood of the trunks to a useless, rotten mass or leave only a hollow shell. The spores of others come in contact with every part of the tree as they float about through the air. These spores find a very suitable place for germination if they fall on wounds. By removing infected trees and destroying old logs fungous diseases may be fairly well controlled.

Wind.—Wind-blown timber frequently exists in open or unprotected stands and in moist places where root systems are shallow. Trees weakened by fire, fungous and insect attacks are easily broken off. Of course the mature trees may be partially or wholly utilized. The greatest damage is done to those for which there is no immediate use.

Woodlots which have been unprotected from the time they were comparatively small usually have their own windbreaks made by the development of numerous side branches. A strip a few rods wide along exposed margins of woods should always be kept as dense as possible. The development of brush and undergrowth should be encouraged. Unless there are others to take their places no trees should be cut in this protective area

Should it be necessary to plant a windbreak it is best to employ two species, one a rapid grower to provide early protection, the other of slower growth to make a permanent and more efficient shield. Carolina poplar, black walnut and catalpa are types of the first class, and any of the evergreens types of the second class. The spacing should be about four feet in rows six feet apart. At least half of the trees should be removed when they begin to crowd badly. When a good protection has been well established trees may be removed anywhere within the grove with practically no danger of windfall.

Work in Woods.—Another important thing to keep in mind is care while working in the woods. The object of management is to have new trees of the most desirable species to replace as soon as possible those which are removed. Therefore it is necessary to protect young growth. Care should be taken in felling trees not to injure others nor crush young seedlings. Brush should be piled in places where danger to timber from fire is reduced to a minimum.

IMPROVEMENT.

The second part of our examination, general improvement cuttings, deals with defective and infected trees, tree weeds and a general plan for harvesting.

Defective and Infected Trees.—Many woodlots contain stag-headed or entirely dead trees which are rapidly decreasing in value. They spoil the beauty of a grove as well as furnish a convenient place for beetles and fungi to live and propagate. They should be removed immediately.

Tree Weeds.—Tree weeds are another waste of our resources. A tree weed occupies space in a timber stand but has comparatively little value. Ironwood, water beech, dogwood, scrub oak, pawpaw and sassafras are examples. It is advisable to remove these as well as the dead, dying and infected trees at once unless by so doing large spaces are opened up in the forest cover which will not close before grass has a chance to start.

Mature Cutting.—One more general rule of improvement is in regard to cutting. Usually only such trees as have passed their maturity or the point where the amount of wood formed each year begins to decrease should be cut. And no more wood should be removed than is actually grown. Thus, if a woodlot is producing five cords of wood annually, it is better to cut five or only four cords than six. If a method like this is used and care taken to keep the ground fully stocked with thrifty young trees the woodlot may be kept up indefinitely.

Coppice.—In cutting the following suggestions should be kept in mind: Stumps should be cut low in order that the sprouts may become independent of the old root system as soon as possible; they should be cut smooth and slanting or have the sharp edges removed so as to prevent water from collecting on them, for in such cases they are apt to rot and infect the sprouts; care should be taken not to tear the bark from the stump since this often prevents buds from developing at the root collar; the sprout should be cut when the sap is down, early spring or late fall, for when cut in midsummer frosts are apt to kill the new sprouts which start up, before their growth is completed and their wood hardened.

MATURE OPEN STANDS.

Character.—Most woodlots are remnants of the original hardwood forest. The valuable straight grained and easy splitting trees have been cut for lumber or firewood. Those which remain have received no attention. They are mature, crooked, knotty or badly diseased and grow in clumps or are scattered over the lot. Few are of any value. Almost all these timbered tracts have been used for pasture, and as a result of constant grazing the ground is covered with a thick, heavy bluegrass sod to the exclusion of desirable young growth. If any reproduction does occur it is very irregular and is composed mostly of weed species.

Treatment.—The treatment of such stands depends upon the degree to which it has deteriorated, its location and the owner's need for timber. If it is on land better suited for agriculture and the farmer is more in need of fields than timber, probably the best thing to do would be to remove the timber completely and cultivate.

But if the lot is to be rejuvenated, the first step to take is to exclude all live stock. Should it be necessary to keep some of the woodland for pasture the thriftiest portions should be fenced off and most of the trees removed from the remainder. More timber and more grass can be produced separately than together. The next step is to remove tree weeds and other trees whose value is decreasing. The remainder will furnish seed. In order that the seeds may have the best possible conditions for germination the sod should be broken up by means of a bull-tongue plow or disc harrow. A rank growth of briars and weeds will probably spring up as soon as the sod is removed, but these make a very good protection under which the young seedlings are to develop. Soon the new growth kills out the weeds and briars and rapidly establishes a good stand. Should other species than those present be desired it is necessary to plant them. As soon as reproduction is well under way the mature trees may be cut. Still it is a wise plan to leave some of them for seed and to furnish timber while the new crop is growing.

IRREGULAR, UNEVEN-AGED STANDS.

Character.—It is from the irregular, uneven-aged stands that we expect the earliest good results. These are parts of the original forest retained in almost virgin condition. Some are dense, others more or less open. In them the soil is almost ideal, but not so with the forest. Fungous and insect hosts, old logs in various stages of decay, are scattered over the ground. Many of the trees are mature but in very poor condition. Some, however, are large and have long, smooth trunks and compact crowns. Increase in height has practically ceased and diameter growth is very slow. A young growth of various species, many of which are undesirable, fills up small openings made by fallen trees. On the whole the forest capital is slowly but surely decreasing, for the amount of timber produced annually is more than offset by death and deterioration of the overmature trees.

Improvement.—The first requirement for the improvement of this type is the same as for mature open stands; that is, the removal of tree weeds and the species undesirable for other reasons. The next process, thinning, is brought about naturally by shading. Trees which are crowded while young try to get their crowns into sunlight, and consequently produce long, slender stems. If, after a sufficient height has been reached, space is given for increased root and foliage development, an increase in wood production occurs. This increase takes place in diameter growth, since there is no longer any incentive for height growth. The purpose of artificial thinning is, then, to accelerate diameter growth as much as possible, to substitute for nature's wasteful struggle a systematic removal of weaker and inferior trees, leaving as many of the good ones as can develop without retardation for a given period.

Thinning.—This process requires considerable judgment and experience, for special attention is given to the trees which are to remain rather than to those which are to be cut. Of course the most valuable and rapid growing species take precedence over others. The following list will serve as a guide, although it is by no means invariable:

Species specially favored: oak, hickory, ash, black walnut.

Species of less value: yellow poplar, butternut, basswood, maple, elm, beech.

Species usually removed: ironwood, cottonwood, sassafras, water beech, etc.

The character of the tree is more important than the species. Tall, straight trees with well developed, thrifty top are left in preference to those which are spindling, weak-topped, crooked or unsound. In a group of equally good trees it is often best to remove one or more, for by so doing the remaining trees will produce more wood than all of them had they been left undisturbed. Trees with their crowns entirely exposed to sunlight are seldom removed unless a number of thrifty ones will be assisted. Those completely overtopped by others have ceased to be a factor in the growth of the stand and may be cut whenever their wood will pay for their removal. Another class of trees are those which receive sunlight from above but which have their sides shaded. It is in this class, where the struggle for existence is most severe and where the greatest economy of energy can be brought about, that most thinning is done. It is better to make light thinnings, never more than a fifth of the stand at a time, than to remove too many at once, for this opens up large patches of ground which dry out on exposure to sun and wind and furnish an excellent opportunity for the growth of grass and undesirable brush. It is not safe to say that this species must be removed to make room for that or that three sprouts must be cut from a group of six. All the improvement thinnings must be made upon the judgment of the operator.

In the woods which contain large open spaces here and there trees should be planted as in mature open stands. In any case growth of young trees and shrubs should not be hindered but rather encouraged on a strip at least two rods wide. A windbreak should be planted if necessary.

YOUNG STANDS.

Character.—The third type of woodlots is the young stand. The ground, seeded by the trees left after all merchantable timber was cut, has become covered with second growth trees four to twelve inches in diameter and twenty to fifty feet in height. Many of them are straight and thrifty, but many more are gradually being suppressed and are dying. Trees in little groups here and there which started from seed the same year are so evenly matched in size that growth is temporarily arrested.

Care.—The seed trees which determined the composition of the young stand are becoming useless through decay and other defects. Often there are grape vines, old fire-scarred snags and other material with which the lot could well dispense. These should be removed. At the same time a thinning could profitably be made if the stand is too thick. The aims and results of thinning have already been discussed.

PLANTING.

Under this subject the main points of planting and growing woodlots are mentioned for the benefit of those who wish to have more timber than can grow on the land already forested.

Location.—In general not less than one-eighth of a farm's total area should be in woodland. Some have more than this amount, but many others have practically not a single acre in woods. As has been said before, the woodlots existing at present have little, if any, relation to farm buildings. A little corner cut off by a stream or railroad, or land otherwise unfit for agriculture because of steepness, rocks, etc., furnish a place to plant a woodlot. It would be well if these so-called waste portions were so situated that timber growing on them could form a windbreak. This idea of protection should always come into consideration when preparing to plant.

Species.—Whatever the opinions of individual nurserymen may be regarding the species to plant, there will never be found trees better suited to any region than those which are natural to its soil. For central Indiana we recognize white oak, red oak and burr oak, ash, walnut, hickory, sugar maple, black cherry and elm as types for lumber; and osage orange, black locust, coffee tree, catalpa, etc., as types best suited for the production of posts, poles and ties. Careful examination of the soil should also be made, and only such species which will develop best under the existing conditions should be planted. These two points were brought out fully under the topic "Forest Cover" in "The Model Woodlot."

Preparation of the Ground.—The ground should be plowed, harrowed and worked into as good condition as for any agricultural crop in order to secure the best results. However, it is not necessary to prepare it so carefully. Planting has frequently been done with good results on burned over woodland according to the third method described below under "Planting." But trees growing on well prepared ground have as much advantage over those on unprepared as has corn under the same conditions.

Where to Procure Seedlings.—The farmer may grow his own trees from seed, procure wild seedlings or purchase from a nursery. Wherever possible wild seedlings are much cheaper. They are weaker than nursery grown stock, and should be transplanted to a nursery for one or two years before being planted in the field.

Care of Trees Before Field Planting.—Trees should be planted with as little exposure of their roots as possible, for the root hairs, upon which the tree depends for taking in its food supply, will dry out and shrivel up when exposed to dry atmosphere for even a few minutes. Some of the broad-leaved species can withstand this drying out if they receive proper treatment afterwards. The best way to prevent this is by "puddling." A "puddle" is a mixture of earth and water about as thick as cream. It may be mixed in buckets, tubs or barrels and drawn along where trees are being dug up so they can be plunged into it immediately, or, if the seedlings have been received from a nursery, as soon as they are unpacked. If planting is to take place at once the trees may be carried to the field in the "puddle." But if some time is to elapse before planting they should be "heeled in" as they are "puddled." For "heeling in":

Dig a trench deep enough to bury the roots and part of the stem. The trench should run east and west, with its south bank at a slope of about thirty degrees to the surface of the ground. A layer of trees should be placed in the trench on its sloping side, the tops toward the south. The roots and stems should be covered with fresh earth dug from a second trench, in which a layer of trees is put and covered in the same way. The digging of parallel trenches is repeated and layers of trees put in until all have been "heeled in."

Time for Planting.—The best time for planting is just before growth begins in the spring. At such a time the seedlings are apt to receive the least injury. In general the frost should be out of the ground. Frost is one of the chief dangers of fall transplanting, for the young trees are often heaved out of the ground as it freezes. It is also best to choose a wet or cloudy day for transplanting.

Methods.—After everything has been made ready for planting the ground should be marked out in rows four, six or eight feet apart, depending on the species, character of the soil and length of time cultivation is to continue. The methods of planting are very simple. The best perhaps requires two men. One carries a bucket of "puddled" seedlings. The other carries a spade which he sets full length in the ground. He then pushes the handle forward, sticks a seedling, which the first man hands him, in behind the blade, withdraws the spade and then steps firmly with both feet on the ground around the tree. Another rapid method which often succeeds is to plow a furrow, lay the trees against the side of it, cover with a hoe and tramp firmly. The remainder of the furrow may be filled by means of a cultivator. A third way is to dig a hole with a grub hoe or mattock. This method is used only on unprepared ground. The size of the hole depends upon the size and character of the root system. Fine dirt is then thrown in next to the roots and the hole filled up, the earth being firmly tramped as before. All trees should be planted deep enough so that when the ground settles they are covered to the same depth they were before being transplanted.

Cultivation.—One of the great troubles with the plantings already made in central Indiana is that they have not received sufficient care. They have been plowed or hoed a few times and then left to take care of themselves. The methods and aims of cultivation in the state reservation are given in the following:

"The cultivation given the young trees growing in the regularly planted fields was of two forms, plowing in the same manner that corn is cultivated and by hoeing. In some fields the trees were plowed and hoed, while in others they were simply hoed without plowing. They were given two complete cultivations. One plan seems to be as successful as the other. The aim sought by the cultivation was to keep down weeds and other wild forms of growth that might overcome the young trees. In the fields where the soil around the young trees was kept loose and free from weeds for a short distance from the trees (eight to twelve inches) by hoeing, and the other forms of growth permitted to stand around them, the young trees seemed to do the best. The only reasonable opinion that can be given for this fact is that the other growth formed a mulch over the soil and prevented evaporation and also a forest condition of shade and protection which resulted in good to the trees, and by keeping a clear opening around them prevented them from any smothering out, as will occur where the weeds and other growths are permitted to grow up close around them. The young trees in such fields are larger and have better boles formed than those growing in the fields where more complete cultivation was performed. Those growing in the more open fields and where the most complete cultivation as to keeping the soil cleaned of all forms of outside growth seemed to grow more bushy and to cease growing earlier in the summer than the others. The only reasonable opinion to be given for this fact is that they were more exposed to the heat of the sun, nothing formed a covering to the soil to prevent evaporation and the trees were deprived of any sort of shade protection. No forest influence was thrown around them. . . . .

It must not be inferred from the discussion of the cultivation here given that no cultivation is needed. The young trees must be given cultivation necessary to protect them from weeds and other wild forms of growth immediately around them. . . . . The trees at the reservation are given the cultivation that can be performed with the means supplied, and no more. If more means were provided they would be cultivated more and better results might accrue."

It can be seen that the Board of Forestry recognizes the fact that they are not caring for the young trees in the best possible manner. A crop of weeds is not the best way to prevent evaporation from the soil. The maintenance of a dust mulch by cultivation will do this and will not use food material stored in the soil. A disc harrow or a five-toothed cultivator run through between the rows after each rain during the summer will keep up the dust mulch and keep down the weeds. In other words, a forest crop should be cared for just as a corn crop, except that the period of cultivation is longer, sometimes three or four years.

Thinning.—The maximum number of trees per acre at maturity is about two hundred. It has already been shown why thinning is beneficial, so only this remains to be said: a few years after the plantation has become well established the process of thinning should begin. The weakest and poorest trees and those crowding better ones should be removed here and there to make room for their more vigorous neighbors. Gradually this process should continue, the material being utilized, until at maturity the woodlot has the requisite number of good trees and also has provided for a permanent supply.

DOES A WOODLOT PAY?

The question naturally arises, Is a woodlot a paying proposition? If it is not, why are the most progressive farmers taking such an interest in forest planting and forest management? Timber is a necessity. In earlier times it was not so valuable, so the land was cleared. The remnants of the old forest may easily be improved at odd times. The cost is much reduced if the farmer does his own work. So it is with planted woodlots, especially if wild seedlings are used. Besides, the price of timber is advancing as the supply is diminishing. This alone encourages planting.

The following extract from a letter shows that with a little care a woodlot can be made to pay:

"I have logs enough cut now to make from forty to fifty thousand feet of lumber. These logs I cut from a ten-acre grove that was only a brush patch thirteen years ago. In addition to the logs the grove has supplied plenty of wood for from two to four stoves, and some for sale, besides posts and poles, all of which came from the thinnings. There are still enough trees on the land to make a good grove." The present generation may not reap the profits but the next one will.

SUMMARY.

The following conclusions have been drawn from this study:

1. The present woodlots, only the remnants of the early forests, are in very bad condition.

2. Well-ordered woodlots are valuable financially, climatically and aesthetically.

3. Old woodlots may be improved and new ones planted successfully.

4. Woodlots must be protected and well cared for in order to secure the best results.

5. A woodlot is a paying investment.

The one thing lacking is universal interest.

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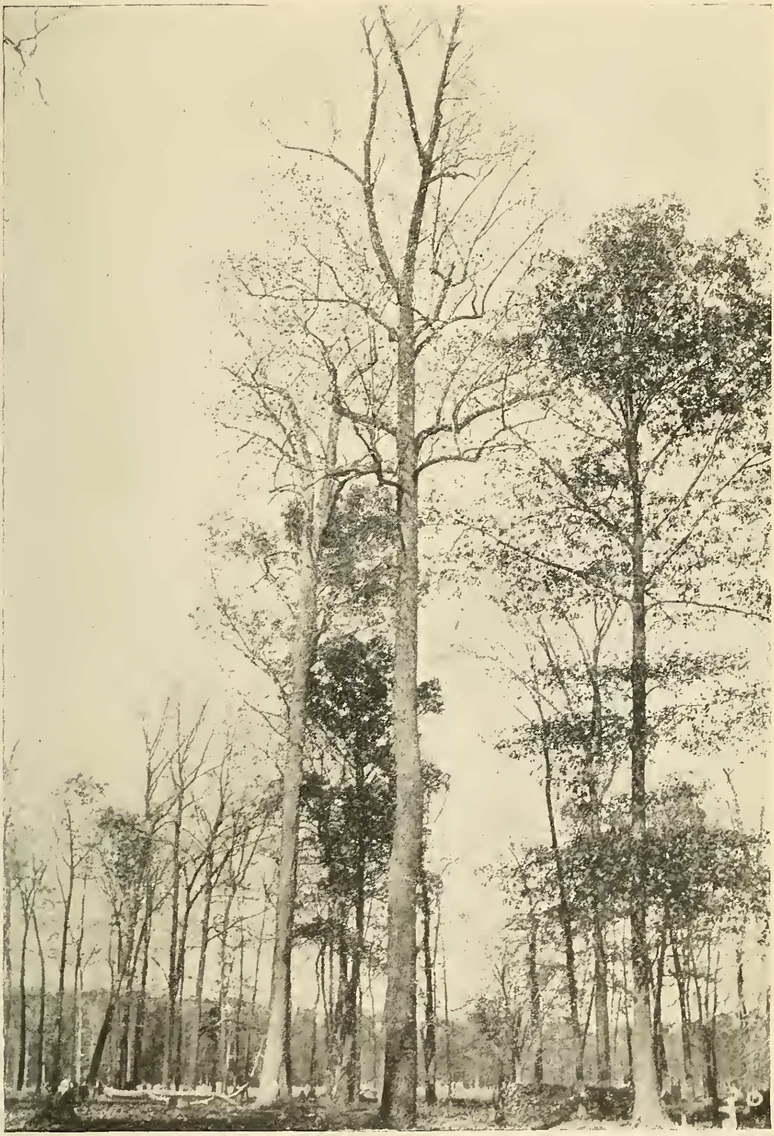
How to Cultivate and Care for Forest Plantations on the Semi-Arid Plains. Cir. 54.

How to Transplant Forest Trees. Cir. 61.

No. 1. An irregular stand in almost virgin condition. The owner will not allow a single tree to be cut. The fallen trees, decaying logs and accumulating underbrush indicate waste. This is not forestry, but neglect.

No. 2. A mature open stand. The ground is covered with a thick growth of grass, a great enemy of reproduction.

No. 3. A young stand of hickory about fifteen years old. All the grass has been tramped down and killed under the large black oak on the right. This would make a good grove if the cattle were excluded and the growth of seedlings encouraged.



No. 4. The usual evidence of neglect of the woodlot. Pasturing has destroyed the young growth. The mature trees not needed for seeding should be cut. The fallen timber should be taken out and saved. Unless the stand is thickened, the young growth will be low branched and worthless.

Recent Work in Wood Physics.

By William Kendrick Hatt.

(Abstract.)[1]

The new series timber tests of the Forest Service, which constitutes the most important recent series of experiments, was begun in 1902 under the direction of Mr. Gifford Pinchot, Forester, Forest Service, United States Department of Agriculture. About 44,000 test pieces have been tested.

These timber tests are divided into two parts: Class (a). Tests on market products of actual size, in which characteristic defects occur, such as stringers, vehicle parts, railroad ties, of interest and value to engineers and manufacturers. These correspond to tests on riveted joints or built-up structures in metal testing. Class (b) includes so-called "scientific" tests of small, perfect specimens with uniform moisture content, representing material collected from the forest, in which the strength is related to the physical structure and position in the tree. These tests are of especial value to the botanists and foresters and aid the solution of silvicultural problems.

A summary of results obtained to date will be presented.

INFLUENCE OF CONDITIONS OF TESTS UPON RESULTS.

(In these studies small, perfect specimens are used).

1. *Speed of Test.*—The strength of wood varies significantly with the speed at which stress is applied. increasing more rapidly as the speed increases. Tests are standardized for speed[2] on the basis of fiber strain per unit of time; and experimental factors obtained to adjust strength values from one speed to another. The adopted standards of fiber strain are as follows, expressed in inches per inch per minute:

Large beans	.0007
Small beams	.0015
Compression parallel to grain, small pieces	.003
Compression parallel to grain, large pieces	.0015

[1] Abstracted from paper by the author, read before the Copenhagen Congress of the International Society for Testing Materials.

[2] See Proceedings American Society for Testing Materials, Vol. 8, 1908, page 541. "The Effect of the Speed of Testing upon the Strength of Wood and the Standardization of Tests for Speed," by H. D. Tiemann.

Strength of wet or green wood is much more sensitive to changes of speed than is dry wood. At the speed adopted for official tests a change in speed of 50% may ordinarily be allowed without causing a variation in strength of over 2%.

2. *Temperature.*—Since wood is a more or less plastic substance it is sensitive to changes of temperature. Tiemann's[3] experiments show that soaking certain species in water at normal temperature does not affect their strength. It appears, however, that warm water has a marked weakening effect. The extreme condition is when wood is made pliable by boiling. Some woods are no doubt more sensitive than others to the effect of temperature of the water in which they are immersed. In recent tests made in winter weather on red oak (Quercus Rubra) ties at Purdue University, ties taken from the temperature of the storehouse (about 25° F.) were from 9 to 17 per cent stronger than those tested at the temperature of the laboratory (about 70° F.). Probably this marked difference in strength is to be found only in case of green or wet wood. The rupture work is not affected to the degree of the ultimate strength. Hickory seems specially sensitive to change of temperature. It is concluded that the ordinary temperature variations of the air of a laboratory are not important, but that the temperature of the storehouse may render it necessary to warm the wood. In fact, the effect of a given factor on the strength of timber, or difference of strength of two species, may at times be entirely masked by variations of temperature of timber at the time of test.

3. *Moisture.*—The effect of moisture on the strength of wood has been thoroughly investigated by Tiemann.[4] His material was small test pieces uniform in moisture content throughout the cross-section; and he determined the distinct "fiber saturation" point, above which increased moisture content did not affect the strength of timber and below which there was an increase of strength. Previous experiments, yielding a continuous moisture strength law, were apparently made with "case-hardened material."

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[3] Bulletin 70. Forest Service, 1906. "Effect of Moisture on the Strength and Stiffness of Wood," by H. D. Tiemann.

[4] Bulletin 70. Forest Service, 1906. "Effect of Moisture on the Strength and Stiffness fo Wood," by H. D. Tiemann.

Circular 108, Forest Service, 1907. "The Strength of Wood as Influenced by Moisture," by H. D. Tiemann.

RELATION OF TESTS.

The relation between the strength under various kinds of tests, such as shear, bending, etc., and compression parallel to the grain, have not been determined yet by an analysis of the data. It is doubtful if any one test can be used to predict the strength of the material under other forms of tests when conditions vary with respect to previous heat treatment, moisture, drying or preservative treatment. For instance, brittleness induced by overheating is evident in impact tests, but this will not necessarily be evident from the compression test parallel to the grain.

An investigation of the effect of speed of test is a part of the general study of behavior of wood under three conditions of loading:

(a) Dead or constant load.

(b) Ordinary static test with increasing load.

(c) Impact test.

(a) Dead load tests exhibit the plasticity of wood. Nearly all deformations increase with duration of load, but the deformed beams subsequently tested show no loss of ultimate strength. Deflection brought about by humid atmosphere is not recovered by subsequent drying. The question is often asked: "What per cent of the load, as determined by the ordinary static test, will break a beam if left on indefinitely?" This has no answer.

(c) Under impact loading, wood will submit to greater elastic deformation than under the ordinary static tests. Impact bending tests show elastic deformation largely in excess of those experienced under static load. The impact test is made under increasing height of drop.[5] The order of resistance of air dry woods at the ultimate failure strength, so far obtained is as follows:

Hickory, Longleaf Pine, Douglas Fir, Loblolly Pine, Chestnut, Spruce, Yellow Poplar, Western Yellow Pine, Western Hemlock, Sugar Pine, and Coast Redwood.

(d) *Abrasion Test.* The abrasion test is under study.[6] Wood is worn by sand-paper in the Dorrey Machine.

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[5] Circular 38, Revised, Forest Service. "Instructions to Engineers in Timber Tests," by W. K. Hatt.

[6] See American S. for T. M., Vol. 7, 1907. "P. U. Impact Testing Mach.," by W. K. Hatt.

INFLUENCE OF TREATMENT PREVIOUS TO TEST.

(a) Drying in Hot Air, Steam, Saturated Steam, etc. A research is under way to investigate the safe limits and the most advantageous conditions for the commercial processes of drying wood. The immediate strength after drying is of course usually greater because of the lessened moisture content. It is now apparent, however, that all processes of drying wood, even air-drying, are attended with weakening of structure, so that when the dried wood is resoaked there is a loss in strength of 10%, and generally more. The drying of white ash (Fraxinus americana,), for instance, at 145° F. in either dry air or exhausted steam, or in superheated steam at 312°, caused no significant loss in strength in the air dry condition, but the resoaked wood was considerably weaker than the green wood. Under 20 to 30 pounds of steam applied during 1 to 4 hours, pine and ash suffer but little loss in static strength after the moisture from the steam is removed by air drying. At higher steam pressures (above 50 lbs.) large and permanent losses result. An equal amount of dry heat is less injurious to wood than moist air or saturated vapor, whenever the temperature exceeds 212° F. The hygroscopicity of the wood in the air-dry condition is reduced by the process of drying in steam, dry-air or saturated steam. Microscopic study shows that the cell walls split open because of the shrinkage of these walls when they begin to dry out.

The results from the Drying-Strength Study are not sufficiently advanced to allow complete conclusions.

(b) *Treatment with Preservatives.* Tests at the Louisiana Purchase Exposition[7] established the safe limit of steaming for seasoned loblolly pine to be 30 lbs. applied for 4 hours, or 20 lbs. applied for 6 hours. Burnettized loblolly pine ties exhibited some degree of brittleness under impact test. Creosote appeared to act upon the strength in the same way as water. It retards the seasoning of timber, with beneficial results to its physical condition. Present evidence points to steaming, or effect of heat in preliminary seasoning, as the only dangerous element of the treating process. The proper limits of heat should be determined for different species of timber.

In the case of bridge timbers, of coniferous species, of large size, incomplete evidence indicates that the desired penetration of creosote can

[7] Circular 39, Forest Service. Experiments on the Strength of Treated Timber," by W. K. Hatt.

only be obtained by cylinder processes that reduce the strength of the timber. The unit stresses used in the design of creosote structures should, therefore, in these cases, be decreased below standards established for natural wood.

UNIT STRESSES FOR DESIGN.

The relation of strength of large sticks, involving defects, to small and perfect pieces, taken from the parent beam, is reported in Circular 115, Forest Service. The strength of large and small sizes is not a question of geometrical magnitude, but of the existence of defects in the large sticks such as knots, shakes, checks and the presence of inferior growth.

Study has been given to the failure of large beams under longitudinal shear. It is apparent that, in the case of large beams of seasoned timber, the failure is due to longitudinal shear rather than to bending. In green beams, also, this form of failure is frequent. Therefore, shearing stresses should be taken account of in the design. The result of later tests confirm the early results that the strength of large pieces is not increased by subsequent seasoning, except in case of select grades. In other words, unit stresses for design should usually be based upon strength of green timber.

NEW SPECIES AND SUBSTITUTES.

The eucalypts of California and the South have been tested. They are among the strongest of our woods. The quality of the various species differs greatly, varying in kiln dry state from 25,000 pounds per sq. in. to 13,000 pounds per sq. in. in modulus of rupture. Tests have been completed on tan-bark oak, which formerly was left stripped of its bark in the woods.

GENERAL STUDIES OF SPECIES.

Tests of red gum are completed.[8] Tests of various species of hickory collected from various site conditions have been made and the report completed. These latter tests established relations between rate of growth and strength, locality and strength, and species and strength. It appears that the most fundamental factor governing the strength of wood of any species is the specific gravity, or, in the conifers per cent of summer wood.

[8] Bulletin No. 58, Forest Service. "The Red Gum," by Alfred K. Chittenden.

Technical Problems.—The study of track fastenings, including common and screw spikes, and tie plates, and the relation of these to the strength of ties is in progress. Laboratory tests are supplemented by service tests in tracks of railroads under operation.

TECHNIQUE OF TESTS AND THE ORGANIZATION OF THE LABORATORY WORK.

The methods and records and organization are now well developed. The results of experience for the past six years are contained in Circular 38, (Revised), entitled "Instruction to Engineers in Timber Tests." Recently a department of microscopic examination of wood has been added to study manner of failures in the tissues, changes in structure resulting from heat treatment, location of preservative fluids and allied problems.

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Forest Conditions in Indiana.

By Stanley Coulter.

Certain economic statements may serve as a suggestive introduction to this study of Forest conditions in Indiana. Some of these will be more fully elaborated later in the paper, others need no comment since their mere statement is sufficient to call attention to existing conditions.

A reference to the Census report of 1880 will show that at that time Indiana ranked sixth in the list of lumber producing states. In 1908 it ranked twenty-seventh.[1] Not only had it fallen to this low position in the list of lumber producing states, but the cut of 1908 was very decidedly less than that of 1907. While some part of this latter loss may be attributed to the reduced demand for lumber in 1907, all of it cannot be so referred. As a matter of fact the cut made represented all of the high grade timber upon which lumbermen could lay their hands.

While certain regions of the state, notably in the southern counties, still appear to be heavily timbered, an examination shows that practically all forms of high value have been cut from them. They have been swept clean of their yellow poplar, white oak, black walnut, and cherry and are made up almost entirely of what may be regarded from an economic standpoint as second grade or inferior forms. It is these inferior forms that are furnishing the future forest, if indeed there is any promise of a future forest. The splendid forests of the past,[2] splendid not only in extent but in the quality of the timber they yielded, have disappeared and the forests that remain are infinitely inferior to them both in extent and quality. Present conditions indicate a still further deterioration unless prompt remedial measures are taken.

A rather careful examination of the existing areas, supplemented by the opinion of lumber buyers, leads to the conclusion that few extensive areas in the state will show a stumpage of desirable forms exceeding 2,500 feet board measure. My own judgment is that the average stumpage

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[1] Forest Products No. 2. Lumber, Lath and Shingles, 1908. Bureau of the Census, issued November 15, 1909, p. 8.

[2] Stanley Coulter. The Forest Trees of Indiana, Trans. Ind. Hort. Soc., 1891, p. 8. A. W. Butler. Indiana: A Century of Changes in the Aspects of Nature, Proc. Ind. Acad. of Sci., 1895, pp. 32, 33.

is below this figure. In order to reach this estimate it has been necessary to include beech, elm, and sycamore, species which for various reasons are not to be classed with white oak, yellow poplar and black walnut. Indeed the eager search for beech and elm is a fairly conclusive evidence of the paucity of forms of higher quality in the forests of the state. Of course there exists here and there throughout the state small tracts showing a heavy stumpage of high grade species, but such areas are the exceptions that prove the rule.

A constantly increasing number of wood-working plants are shutting down because of inability to secure the needed raw material. The radius marking the limit from which this raw material can be drawn is very definitely limited by freight charges. I have received a statement, which may be considered as official, that fifty per cent. of the veneer plants of the state are shut down because they are unable to secure logs suitable for their work. What is true of the veneer industry is true in varying degree of other wood-working industries. This means, unless checked, loss of employment to hundreds or even thousands of men, and either a removal of capital to other states or its absolute loss. The reduction in the number of wood-working plants in the state within the last decade has been startlingly large and can only be explained by the rapidly waning supply of suitable raw material.

While the data in my hands are not yet complete, I have records of over five hundred thousand (500,000) acres of waste land in the state. This waste land, located in a very great measure in the southern portion of the state, is the result in almost every instance of destructive lumbering. Concerning this conclusion there can be no doubt. We have knowledge of former forestal conditions, and in many cases the history of the cuttings of specific tracts. These waste lands lie open and are absolutely waste; they are not used in agriculture or horticulture and have wasted to such an extent that they are completely abandoned. They yield revenue neither to the owner nor the state. The indications are that the amount of deforested land abandoned by the owners is constantly increasing. The surest, indeed the absolutely unmistakable sign of a decadent state from an economical standpoint, is a constant increase in the area of abandoned lands.

To counteract the conditions indicated in the preceding paragraphs, tree planting has been undertaken in the state on a fairly large scale within the past few years. These plantings have been made by individuals and by corporations. The tree plantations run up into the hundreds and the number of trees into the hundreds of thousands. A careful inspection of sixty-nine of these plantings, embracing two hundred fourteen thousand (214,-000) trees was made in 1908-09 by Messrs. U. C. Allen and H. C. Kennedy. The plantations examined covered the state with the exception of the southeastern counties and represented practically every type of soil and drainage conditions. Supplementing their records by my own observations and those of Secretary C. C. Deam of the State Board of Forestry, I am led to the conclusion that afforestation operations in the state have been, in a large measure at least, unsatisfactory. While there are occasional instances of successful and apparently profitable tree culture in the state, it is very certain that, taken as a whole, the results are not of such character as to give promise of any relief from present conditions in the immediate future. The plantings have been chiefly catalpa and black locust. Only in exceptional cases, and then rather as the result of chance than a definite purpose have other species been tried. A very few small plantings of black walnut and white ash practically represent the attempt in growing trees of high grade.

The reasons for these unsatisfactory results are not far to seek. They may be grouped under three categories:

1. Ignorance of the silvical qualities of the species.
2. Poor seed or seedlings which were not of the species desired.
3. Ignorance of the cultural requirements for securing rapid and healthy development.

Apparently in many of the plantations no question as to the fitness of the soil, or drainage, or exposure entered. In another large series of catalpa plantings the larger number of the trees were not the hardy catalpa *(speciosa)* but *C. bignonioides* or some hybrid. In more than one-half of the cases absolutely no attention was given after the planting, to cultivation, to pruning or to coppicing. A study of the conditions in these plantations is sufficient proof that afforestation operations will not be successful in Indiana until a much fuller knowledge of the silvical qualities and requirements of the species selected becomes common property.

Bad as the existing conditions are, the case is far from hopeless. The aggregate timbered area of the state is still large and while the stumpage is not heavy nor the quality all that could be desired, yet these areas furnish not only the hope but the assurance of the future, if, *and only if*, they are intelligently managed. All of the timber lands in the state, with the exception of the State Forest Reservation, is held by private owners. As a rule these holdings are relatively small and our forests may be considered as made up of a large number of wood lots. It is a fact that cannot be too often repeated or over emphasized, that it is a much more certain and a much cheaper process to maintain and improve an existing stand of timber than to produce a new one by planting. Not only is it much surer and cheaper, but it is also much more rapid.

The problem of the future of the forests of Indiana is merely the problem of securing the proper handling and care of the wood lots and small timbered areas held by individual owners. If such areas are wisely handled and conservatively lumbered there is no reason why they should not for years yield a steady and increasing income and at the same time show a marked increase in quality and value. In other words the problem of the future timber supply in the state is very largely a problem of education. Owners of timbered tracts must be brought to a realization of the value of such holdings and trained in methods of management which will secure the results indicated. It must be shown also that such methods of management are profitable, for unless this can be done no method, however theoretically desirable it may be shown to be, will ever come into general use. The real peril lies in the fact that this process of education is a very slow one and that existing timber areas may be greatly reduced in value or completely destroyed before a knowledge of the better methods has become common property. An examination of a number of such tracts covering many counties of the state indicates fairly well what may be considered the average condition of the forests of Indiana today.

Almost without exception these timbered areas are used as pasture land, and have, in most instances, been so heavily overpastured as to practically destroy all prospects of the regeneration of the forest after the removal of the present trees. An examination of seventeen such wood lot pasture tracts during the past season which were distributed through twelve counties of the state, revealed the fact that in not a single one could any young seedlings or healthy, well formed saplings be found. Any system of management under such conditions is perfectly useless. Unless the condition of the wood lot areas is improved and the regeneration of the forests provided for by an abundant and vigorous growth of seedlings, the end of our forests is not far distant.

In most instances the withdrawal of the tract from pasturage will be sufficient to permit an immediate springing up of sufficient seedlings to care for the future of the tract. This withdrawal from pasturage should be absolute until such time as the young growth is beyond danger from browsing animals. After that time light grazing may not be injurious, although if grazing is permitted at all, there is the constant temptation to overgraze.

The effect of this overgrazing is very easily demonstrated by simply enclosing a tract which contains no seedlings, thus protecting it from cattle. Almost invariably a dense and abundant undergrowth representing many species of tree forms will spring up and in a few years will have provided a stand sufficiently dense to allow improvement cuttings and thinnings, leading to the formation of a new forest.

In the State Reserve a large acreage was burned over the year before the State took possession of the tract. At the present time, some eight years after the fire, the tract which was burned over is densely covered with a growth of vigorous and healthy young trees, with valuable species represented in such large numbers as to give certain promise of a fine even-aged stand after the cleaning and thinning cuttings have been made. The area was regenerated from adjoining seed trees. No treatment of any kind was given the tract; it was simply freed from pasturage.

In the hill regions of the southern counties, and especially in localities where the hills faced the Ohio river, the forests were removed many years ago. For years such tracts were left unfenced and during those years the land wasted through erosion and no seedlings obtained a foothold. At a later period when laws forbidding stock running at large were passed and when wire fencing came into general use, these denuded hills were quickly covered with a dense growth of vigorous young trees. No planting had been done, the soil had received no treatment, but the tract as in the former case was freed from pasturage. Such instances could be multiplied almost indefinitely and from them can be drawn a conclusion of high economic value, namely, that very many of the denuded areas of the state could be afforested by the simple process of relieving them from the burden of pasturage. It is safe to say that 90% or more of the timber areas of the state are so heavily over-pastured as to preclude any possibility of their future improvement or growth. Until the owners of these small forest tracts realize the utter destructiveness of over-pasturage but little can be done to improve forest conditions in the state. That these statements are not exaggerated is a matter of fairly easy demonstration by any person who will go through an average forest in his vicinity and make a close examination for the young trees which stand as a prophecy of the future forest. In almost every instance they will be found to occur in such small numbers as to indicate a constantly waning forest. Indeed, in very many cases not a single seedling or sapling of a desirable species can be found.

A further examination of these areas within our state shows that in by far too many cases they have suffered damage by fire. In very many instances these fires have spread into the timber tract from the right of way of railroads or from meadow fires which have been started for the purpose of cleaning and have escaped control. However they may have originated, their effect upon the forest has been two-fold. First, in a serious damage to the mature trees and second, in practically obliterating all the young growth which may have become established. As a result of the action of such fires, not only is the young growth killed but the soil is placed in such condition as to preclude a future growth for several years. The damage by forest fires in the state during the past year, which was by no means an exceptional one, amounted at a conservative estimate to at least $100,000. A very large part of this loss could have been avoided by exercising ordinary care. Very much more of it could have been prevented by the rigorous application of the laws fixing the responsibility for the occurrence and spread of forest fires.

The value of these wood lots as they stand might also be very greatly improved in many cases if improvement cuttings of various kinds were undertaken. Almost all of them need "cleanings" in order to remove from them various undesirable forms. It must be remembered, however, that such cleanings must not be too vigorously undertaken lest too great an amount of soil be exposed to the action of the sun and the wind. Sudden changes in ecologic conditions are particularly fatal to young tree growth. Where the undergrowth or undesirable forms are at all dense, probably not to exceed 25% should be removed at any one time and the ground should not be cut over again in less than four or five years. In these cleanings the object should be to remove all forms the absence of which would improve the forest and give the trees left standing an opportunity for a more perfect development. In this cleaning should ultimately be removed all trees, which, even if allowed to reach full maturity, would never have an economic value. It should also include all trees that are dead or dying, since such trees are not only deteriorating in value but also serve as centers from which various diseases destructive to the forest may spread, and because in addition they furnish natural breeding places for many species of harmful insects. When such dead or dying trees are infested with fungus diseases or injurious insects, they should be completely burned. The cleaning should also include all trees which are over-mature or for any cause are losing value. Trees which are undesirable in shape or from other causes do not promise to make a satisfactory growth should also be included in the cleaning. Special attention should be paid to seed bearing trees of undesirable species. These should be removed whenever found in order to prevent their seedlings from occupying the ground at the expense of the more desirable forms.

As has been suggested, these operations must not be carried on too vigorously since the young seedlings, which are to make the future forest, require shelter from the wind and from the sun during their earlier years and if the removal of these undesirable forms is made too completely at a single operation the object in view will be defeated. By the application of such methods not only may the condition of the wood lot be constantly improved so that in the end it will contain a vigorous and healthy growth of valuable forms, but at the same time much material which may be utilized for fuel and for other purposes will have been removed from the area. In almost every instance, if care is taken, these cleaning cuttings will more than pay for the expense required to make them. It is a conservative statement to say that over one-half of the existing wood lots in the state would be very greatly improved in value and in productive capacity by a series of judicious cleanings.

In addition to these cleaning cuttings, in certain regions "thinnings" seem to be required. Two trees of a valuable species may stand so close together that if both were allowed to remain, neither would develop into a good tree. One of them should be cut away. In almost every wood lot also, there are to be found clumps of trees which stand so close together that they have developed thin, weak stems instead of stout and sturdy trunks. Enough of these should be cut out to insure a healthy and vigorous growth on the part of the trees that remain. The thinnings differ from the cleanings in that, while the cleaning removes undesirable and injurious forms only, the process of thinning removes desirable forms where they are wrongly placed in order that the trees left standing may have a better chance. There is scarcely a wood lot in the state in which manifold instances of the value which would result from careful thinning cannot be found.

The existing wood lots can be still further maintained in good condition by a more careful use of the material which is cut from them. There is a constant tendency to cut such trees as will work up most easily, whatever may be the purpose for which they are to be used. Good straight white oak of sufficient size to have a high value for lumber is cut for fire wood, or rails, or posts, when a score of other species which have no lumber value might serve these purposes as well if not better. In the same way large numbers of vigorous and straight young saplings are cut down for hoops, for poles, or for other of the manifold uses of the farm. Such wastefulness under present conditions is little short of criminal. The woods of high value should be allowed to come to their full size and development and the ordinary uses of the farm supplied from inferior timbers which are of less value and of less general usefulness.

Great care should also be taken in working up the tops of the trees cut in such a way as to utilize them as far as possible. Not only does such utilization reduce the number of trees that are cut from the tract, but it at the same time protects it from damage by fire, since the dry tops of trees burn fiercely and are always a great peril in case of fire. An examination of an ordinary cutting whether for wood or lumber or clearing will show that scarcely 50% of the tree is utilized.

It is very difficult to form any estimate of the amount of the present timber stand of the state. As contrasted with the past the average amount per acre has been very largely reduced. As examination of the sources of supply of wood manufacturing plants will show that a large proportion of the more valuable timbers which they use in their work are secured from without the boundaries of the state. As an illustration, information derived from certain veneering companies of the State may be given.

The Indiana Veneer and Lumber Company uses in its operation oak and principally white oak. Most of this is derived from the states between Ohio and Missouri, but not above 25% of it is secured from Indiana.

The Evansville Veneer Company cuts gum, poplar, white oak, red oak, sycamore and beech. They purchase these woods in Tennessee, Kentucky, and Mississippi, getting none from Indiana.

The Goshen Veneer Company uses bass wood, maple, ash, elm, sycamore, beech, poplar, oak and walnut. The oak they buy in Illinois and Kentucky; the poplar south of the Ohio river. As nearly as they can estimate, 60% of the material which they use comes from Indiana.

The Hoosier Veneer Company uses white oak very largely, with some red oak. About 35% of this material comes from the south and about 65% from Indiana.

Showers Brothers Company, Bloomington, cut only those woods that are native to the southern part of the state. They include in their work the different varieties of oak, poplar, beech, maple, sycamore, elm, ash, and hard gum with occasional logs of walnut and cherry. The last two are taken from southern Indiana. A direct quotation from the letter of their secretary is extremely suggestive. "There is yet quite a quantity of timber in this section of Indiana. It is, however, becoming very much scattered. The visible supply of veneering timber in Indiana is rapidly diminishing. In my opinion *within four or five years it will be necessary for the larger mills to draw from out of the state a large part of their logs.* The quality of southern Indiana logs, principally the oak varieties, is the best in the country for veneering purposes. The texture of the grain and of the figure being far superior for cabinet purposes to the southern varieties. We use in our veneering mill alone about 35,000 feet log measure of timber per week. It is my opinion that further development of the veneering industry will do more to save the diminishing supply of timber in this state than any other one thing, as in working timber into veneer an enormous saving in waste is effected."

The Diamond Veneer Company uses only quartered oak in its operations, buying flitches from the saw mills and not buying logs. The company estimates that about 90% of its stock comes from Indiana mills, but has no knowledge as to the sources of supply of the mills.

The Putnam Oak Veneer Company uses practically any of the native woods of Indiana. The woods principally used are white, burr, and red oak, ash, hickory, bass wood, soft elm, poplar, walnut, black gum and beech. "Probably 20% of the wood, such as gum, cottonwood, poplar, red and white oak, comes from our native forests, the balance comes from the south, where the timber is better as to size and cheaper as to price than our own timber. In my judgment we do not furnish over 40% of the lumber consumed in the state, the balance comes from the south. As is a well known fact, Indiana oak is the finest grade of oak that was ever grown in this continent. It is beyond the power of any living man to produce the wonderful forests of oak, poplar, ash, and walnut that once covered this state of ours. We gather our supply from all over the state. Fifteen to twenty-five years ago we were able to buy bunches of oak timber in from 75,000 to 100,000 feet lots, but now we pick up a tree here and there where possible. The condition has been reached that the state is swept practically clean of all its native oak."

Mr. Howard I. Young, Secretary of the National Veneer Association, estimates that there is in the neighborhood of ninety million feet of oak veneer manufactured in Indiana annually. This output is classified broadly into two parts, quartered oak veneer amounting to about sixty-eight million feet, and rotary cut oak veneer, amounting to twenty-two million feet. While much of the oak material is secured from Indiana, Ohio, and Illinois, a very material quantity of oak logs are shipped from the southern states to fill the demand for this class of material.

These extracts indicate that for many years selective cutting has been practised and in fact has been increasing as the years have passed. Timber area after timber area has been swept clean of its black walnut, its yellow poplar, its white oak, its cherry, and other trees of high grade and large size. As a result the forests that are left are composed of less desirable forms, and it is these less desirable forms that are furnishing the forest of the future in so far as any such future is to be hoped for. It is very evident from this statement of facts that if the high reputation of Indiana timbers is to be maintained and that if Indiana continues to be able to provide material for its own wood manufacturing industries, some close attention is demanded along the lines of the regeneration of existing wood tracts with desirable species. This may mean planting in certain open places, but even in spite of the considerable expense involved in such a process, the results reached would far exceed in value the cost incurred. While the experimental period at the State Forest Reserve has as yet been too brief to furnish data for authoritative conclusions, the indications all point to the fact that high grade trees such as yellow poplar, black walnut, and ash will grow as rapidly as the catalpa and black locust.

Not only are the indications that they will grow as rapidly, but also that they will maintain themselves in a healthy state, in good form and be relatively free from insect attack and fungus disease. While it is true that the oaks which are at present in very high demand will not make such rapid growth, it has been found that they will make a sure and healthy growth and that in all probability a natural regeneration of the existing wood tracts with our native oaks and other high grade timbers would be easily within the range of possibility, were it not for over-pasturage, damage by fire and destructive lumbering.

All of this means that in the use of the wood lot or small timber tract the owner should have constantly in mind *its perpetuation in unimpaired value.* No tree should be cut unless there is evidence that its place will be quickly taken by another equally desirable form and this evidence is always at hand in the presence of an abundant young growth. If such a young growth is not present, cutting cannot be done without diminishing the value of the stand. In every case the owner should regard a stand of timber as an investment from which he should derive a constant revenue, while at the same time the investment remains unimpaired. The scarcity of high grade timber, the eagerness with which it is sought and the relatively high stumpage values all combine to tempt the owner to such an impairment of his investment, but a yielding to the temptation is an indication of poor business judgment.

It may be necessary in many instances to reinforce the relatively slow process of natural seed regeneration. This may be done cheaply and efficiently in many ways, which are self-suggestive, yet which will bear restatement. The weeds and brush may be cut away from the immediate neighborhood of the "mother seed tree" in order that the seeds may come in closer contact with the ground when they fall, thus greatly increasing their chances of successful germination. If the soil is hard and compact it may be broken with a hoe or plow so as to furnish a more satisfactory seed bed. In some cases where the litter of leaves is quite deep it may be advisable to rake it off in order to expose the mineral soil and even in extreme instances to burn it off, although burning over a tract to reinforce natural seed regeneration is an extremely doubtful process in unskilled hands. The methods suggested do not cover wide areas and are the ordinary methods used in the management of other crops. Whatever form they may take the result sought is the same, an increase in the number of seeds germinating by improving the character of the seed bed.

It is very obvious from this résumé of conditions that unless the owners of existing wood lots attack the problem in an intelligent way the time is not far removed when practically all of the material used in our wood manufacturing plants will have to be shipped in from other states.

The conclusion to be drawn from the statements in the above paragraphs are all but obvious. Practically all of our forests are in private hands and it is very evident that the timber problem in Indiana is to be solved by private forestry. The obstacles to private forestry are summarized by Treadwell Cleveland, Jr.,[3] as fire risk, ill-devised taxation and cheap stumpage. The first two of these he suggests are "artificial obstacles" which may be removed by suitable state legislation. Concerning the third, Mr. Cleveland says: "Cheap stumpage is the chief material obstacle to the wide extension of private forestry. Forestry involves an investment in growing timber. If the investment is to show a satisfactory profit, the product must not sell too cheap. As long as the product sells cheap, expenditures will not be made to produce it, and the lumberman will continue to be the nomad and the speculator which past conditions have inevitably made him. In order to hold out inducements to private enterprise, forestry must offer a reasonable margin of profit above the cost of growing the timber.

'This obstacle to forestry is being steadily removed by the depletion of the virgin forests and the consequent rise in stumpage prices. Already the scarcity of supplies has resulted in a number of cases in the holding of tracts for more than a single crop."

It is evident that if the timber supply of the state be maintained there must be cooperation between the state and private owners. Just what form state laws for the encouragement of forestry should take is not perfectly clear. It is evident, however, that legislation should develop out of state conditions and until the resources of cooperation have been exhausted, definite legislation should not be enacted. An examination of State laws encouraging forestry shows that they may be grouped under two general heads. First, those which seek to stimulate tree planting by bounties or tax exemptions; second, those establishing Forest Commissions and, in late years, State Foresters charged with duties suggested by the conditions in the state creating these offices. The laws under the first group have been, almost without exception, ineffective and in very many

[3] Status of Forestry in the United States. Forest Service Circular 167, pp. 23-24.

cases have been repealed and in a considerable number of other cases declared unconstitutional. Such laws "have had some slight educational value, but they have led neither to the planting nor to the preservation of forests."[4]

Laws falling under the second group, on the contrary, seem to have greatly advanced the cause of forestry. This has been done mainly by gathering information, cooperating with private land owners and giving advice concerning the care of private holdings and tree plantings. In many states, state forests have been established and these have in every instance proved of high value. To quote directly from Mr. Cleveland.[5] "These State forests represent a line of state action which has been preeminently successful. New York leads the list in State forest area (1,611,-817 acres), followed by Pennsylvania (863,000), and Wisconsin (253,573 acres.) The smaller attempts of Minnesota, Michigan, Connecticut, Massachusetts, New Jersey, Indiana, etc., are all important. The State forests speak for themselves. First, they furnish object lessons of great value; second, they form the nucleus of what some day must be the principal center of state forest work. It is a fundamentally sound policy for the State to own land, especially land which does not offer the conditions necessary for prosperous settlement."

Under existing conditions in our own state, the most important and immediate duty is an extension of knowledge concerning the significance of existing timbered areas in their relation to the future of the forests and of the wood working industries; of their value as investments; of methods of management and utilization which will secure the maximum revenue without deterioration of the stand; of the importance of reinforcing natural seed regeneration and of a more general practice of wisely considered afforestation methods. The most casual inspection of the present timbered areas would prove sufficient to convince the most skeptical of the importance of intelligent and persistent effort along the lines indicated. If, in addition, we consider the large area of land at present utterly unproductive, areas which are increasing in extent each year, some wisely planned and judiciously applied remedial measures seem absolutely imperative. The Academy of Science could do much as a body and through the efforts of its members to aid in this work. The problem is sufficiently acute to

[4 and 5] Status of Forestry in the United States. Forest Service Circular 167, September, 1909, p. 21.

indicate that the time for destructive criticisms of present attempts for its solution has passed. and that the time has arrived for cooperation in this work. If this cannot be given, the criticism should at least be constructive. In eight years of service on the State Board of Forestry, it has been my privilege to hear many sharp criticisms of its personnel and its work, but in all that time there has come neither to the board nor to any individual member of it a single suggestion as to how either might be improved.

It may be assumed without argument that a complete invoice of the present stand, as to amount, composition and distribution is absolutely necessary in order to secure results which are even approximately satisfactory. As a matter of fact, it has been demonstrated that with the present sources of information and with the present limitations as to the functions of the State Board of Forestry the collection of such data is absolutely impossible. Yet, it is evident that such a census of our forests and such knowledge of their composition and distribution are conditions precedent to any successful work looking to the maintenance of our timber supply. It is at this point that the state should cooperate with the National Forest Service. In many states, such a forest census has been or is being taken, the Forest Service detailing experts for the work and the state paying the expenses of the survey. Such cooperation gives the most complete, the most accurate and the most easily comparable results in the shortest time and at the least expense. If such cooperative work is impossible, then the Board of Forestry should as rapidly as its means will permit, collect and organize information covering these points. The slightest consideration of the future of the forests and of the wood-working industries of the state will show that the results of such a census would prove of the highest importance, not only in determining the policy of the state but in emphasizing the significance and value of existing timbered areas.

There is need also of much more exact and indeed of much additional knowledge in relation to the selection of species for planting in the different soil, drainage and exposure conditions of the state. There is need also of equally exact knowledge concerning the silvical qualities of these species, the most economical methods of propagation, their spacing in plantings, their cultivation and care and above all their rate of growth under variant conditions. The securing of such data is a matter of years of continuous experimentation and this work the state is properly undertaking on the State Reserve. There is necessity, however, that the fact should be kept in mind that results sufficiently definite to prove of general application can only be secured as the results of large series of experiments continued through many years. In order, however, that such work may reach its highest value there should be close cooperation with individual land owners throughout the state. Cooperative experimental plats should be found in every part of the state. The seedlings should be furnished from the state reservation and should be planted and cultivated under regulations prescribed by the State Board of Forestry. Regular reports should be made by the owners to the Board and regular inspections of such plantings should be made by its Secretary. The conclusions resulting from observations covering a wide range of conditions and involving varying degrees of care and attention would evidently be of much greater value than those possible under present methods.

There is cause for congratulation in the fact that the state realizing the gravity of the problem confronting it is taking steps to avert the disaster which our rapidly waning timber supply seems to indicate. Caution in such matters is of course wise, but it should not be forgotten that as a rule a Fabian policy is ineffective in acute cases. There is every reason for confidence, however, in believing that no backward steps will be taken and that as the years pass the development of a wise forest management on the part of individual land owners, will under the guidance of the state be far more rapid than in the past. There is reason for hope also in the general observance of Arbor Day for it gives assurance that the next generation will have a fuller knowledge and a truer appreciation of the value of our forests than their parents ever possessed.

Summarizing; the present forestry conditions in Indiana being as stated, three great lines of work suggest themselves as immediately necessary if the timber supply of the state is maintained:

1. An educational propaganda emphasizing the importance of correct forest methods, the value and potentiality of existing wood lots, and of the importance of reclaiming waste lands by tree planting.

2. A census of the present timber stand, its composition and its distribution.

3. Cooperative experimental work on the part of the state and individual land owners, for the determination of suitable species for afforestation, their silvical qualities and their rate of growth.

Quite apart from any sentiment, no more acute problem nor one which directly affects more business and individual interests confronts the state. Others may be of greater magnitude, but certainly no other one touches so intimately such wide and varied interests.

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