CONTENTS

APS MEMBERSHIP STATUS

SEPTEMBER 1966

Regular members	2615
Associate members	196
Retired members	137
Honorary members	18
	2966

SUSTAINING ASSOCIATES

Abbott Laboratories
Ayerst Laboratories
Burroughs Wellcome & Co.
CIBA Pharmaceutical Products
E and M Instrument Co.
Gilford Instrument Laboratories
Gilson Medical Electronics
Grass Instrument Co.
Harvard Apparatus Co.
Hoffman-La Roche Laboratories
Lakeside Laboratories
Eli Lilly and Co.
Merck Sharp and Dohme Research Laboratories
The Norwich Pharmacal Co.
Chas. Pfizer & Co., Inc.
Phipps and Bird
A. H. Robins Co.
Smith Kline and French Laboratories
The Upjohn Co.
Warner-Lambert Research Institute
Wyeth Laboratories

DECEASED MEMBERS

The following deaths were reported since the 1966 Spring Meeting.

W. H. Chambers - April 16, 1966
R. T. Clark, Jr. - July 7, 1966
F. E. D'Amour (R) - January 31, 1966
T. S. Githens (R) - April 10, 1966
W. T. Goodale - May 26, 1966
I. S. Kleiner - June 15, 1966
A. E. Livingston (R) - May 8, 1966
T. L. Patterson (R) - May 14, 1966
J. F. Perkins, Jr. - August 7, 1966
J. M. Rogoff (R) - June 26, 1966
E. L. Scott (R) - January 19, 1966
F. N. Sudak - February 3, 1966

NEWLY ELECTED MEMBERS

The following, nominated by the Council, were elected to membership in the American Physiological Society at the Fall Meeting, 1966.

FULL MEMBERS

ALONSO-de FLORIDA, Francisco: Prof. Med., Univ. of Mexico
ALTURA, Burton M.: Asst. Prof. Exptl. Anesthesiol., N.Y. Med. Ctr.
ANTHONISEN, Nicholas R.: Dem. in Med., McGill Univ., CV Service
ARIMURA, Akira: Asst. Prof. Med., Tulane Univ. Med. Sch.
ASANUMA, Hiroshi D. : Asst. Prof. Physiol., New York Med. Coll.
BANK, Norman.: Asst. Prof. Med., New York Univ. Med. Ctr.
BENCHIMOL, Alberto: Res. Assoc., Scripps Cl. & Res. Fndn.
BERNARD, Rudy A.: Asst. Prof. Physiol., State Univ. of New York
BLYTHE, William B.: Assoc. Prof. Med., Univ. of North Carolina
BURGER, Ray E.: Assoc. Prof. Poultry Husb., Univ. of California
CARPENTER, David O.: Staff Assoc., Lab. Neurophysiol., NIH
CARRIER, Oliver, Jr.: Asst. Prof. Pharmacol., Univ. of Mississippi
CARVALHO, Arselio P.: Res. Assoc. Physiol., Inst. Muscle Disease
CHIEN, Shu: Assoc. Prof. Physiol., Columbia Univ.
COBURN, Ronald F.: Asst. Prof. Physiol., Div. Grad. Med., Univ. of Pennsylvania
CONRAD, John T.: Asst. Prof. Physiol. & Biophys., Univ. of Washington
COOK, John S.: Assoc. Prof. Physiol., New York Univ. Sch. Med.
CORBIN, Alan: Sr. Invest. Endocrinol., Abbott Labs., North Chicago
CRAMER, Carl F.: Assoc. Prof. Physiol., Univ. of British Columbia
CRANE, Robert K.: Prof., Chmn. Physiol., Rutgers Med. Sch.
DAGGETT, Willard M.: Vis. Instr. Surg., Massachusetts Gen. Hosp.
DeVOE, Robert D.: Asst. Prof. Physiol., Johns Hopkins Sch. Med.
DEWSON, James H. III: Asst. Prof. Psychol., Stanford Univ. Sch. Med.
DOUGLAS, Ben H.: Asst. Prof. Med., Univ. of Mississippi Med. Ctr.
ELY, Charles A.: Assoc. Prof. Anat., Columbia Univ.
ENSON, Yale: Asst. Prof. Med., Cardiopul. Lab., Bellevue Hosp, N.Y.
ESPEY, Lawrence L.: NIH Postdoctoral Fellow, Univ. of Michigan
FAND, Sally B.: Asst. Res. Prof. Med., State Univ. of New York
FARLEY, Belmont G.: Assoc. Prof. Biophys., Johnson Res. Fndn., Univ. of Pennsylvania
FELDMAN, Daniel S.: Asst. Prof. Neurol., Downstate Med. Ctr., State Univ. of New York
GARCIA, Joseph: Asst. Res. Physiologist, Univ. of California, Berkeley
GERSTEIN, George L.: Asst. Prof. Biophys. & Physiol., Univ. of Pennsylvania
GILBERT, Robert: Asst. Prof. Med., State Univ. of New York
GOLDMAN, Ralph F.: Res. Physiol., U.S. Army Res. Inst. Env. Med.
HALAS, Edward S.: Assoc. Prof. Psychol., Univ. of North Dakota
HALL, H. David: Assoc. Prof., Chmn. Oral Surg., Univ. of Alabama
HENKIN, Robert I.: Sr. Invest., Cl. Endocrinol., NIH
HOAK, John C.: Asst. Prof. Med., Univ. of Iowa Coll. Med.
HOCKMAN, Charles H.: Assoc. Prof. Pharmacol., Univ. of Toronto
HRACHOVEC, Josef P.: Assoc. Res., Univ. of California, L.A.
HULTGREN, Herbert N.: Assoc. Prof. Med., Stanford Univ. Sch. Med.

HYDE, Richard W.: Asst. Prof. Physiol., Div. Grad. Med., Univ. of Pennsylvania
IZZO, Joseph L.: Asst. Prof. Med., Univ. of Rochester
JACOBSON, Marcus: Assoc. Prof. Biol. Sci., Purdue Univ.
KASTIN, Abba J.: Instr. Med., Tulane Univ.
KAYNE, Herbert L.: Asst. Prof. Physiol., Boston Univ. Sch. Med.
KLOCKE, Francis J.: Asst. Prof. Med., Buffalo Gen. Hosp.
KOENIG, Edward: Asst. Prof. Physiol., State Univ. of New York
KOIKE, Thomas I.: Asst. Prof. Physiol., Univ. of Arkansas
KRNJEVIC, Kresimir: Dir., Wellcome Dept. Res. Anesthesia, McGill Univ.
LAMB, Thomas W.: Res. Assoc. Physiol., Dartmouth Med. Sch.
LANGE, Gertrude: Asst. Prof. Physiol., State Univ. of New York
LANGFITT, Thomas W.: Head, Dept. Neurosurg., Pennsylvania Hosp.
LARIMER, James L.: Assoc. Prof. Zool., Univ. of Texas
LINDLEY, Barry D.: Asst. Prof. Physiol., Western Reserve Univ.
LIPSCOMB, Harry S.: Assoc. Prof. Physiol. & Med., Baylor Univ.
LONG, David M., Jr.: Asst. Prof. Surg., Cook County Hosp., Chicago
LOSSOW, Walter J.: Assoc., Res. Physiol., Univ. California, Berkeley
MAGEE, Joseph H.: Asst. Prof. Med., Jefferson Med. Coll.
MARKLEY, Kehl III: Staff Member, Biochem. Pharm., NIH
MARSH, Donald J.: Asst. Prof. Physiol., New York Univ.
MASON, Dean T.: Sr. Invest., Cardiology Br., NIH
McCRADY, James D.: Assoc. Prof., Texas A & M Univ., Vet. Physiol.
MURRAY, Raymond H.: Assoc. Prof. Med., Indiana Univ.
NARAHASHI, Toshio: Asst. Prof. Physiol., Duke Univ. Med. Ctr.
NASH, Franklin D.: Asst. Prof. Physiol., Indiana Univ. Sch. Med.
PAGANELLI, Charles V.: Assoc. Prof. Physiol., State Univ. New York
PANUSKA, Joseph A.: Asst. Prof. Biol., Georgetown Univ.
PENHOS, Juan C. J.: Assoc. Prof. Med., New York Med. Coll.
PEREZ-CRUET, Jorge: Asst. Prof. Psychol., Johns Hopkins Univ.
PORTER, George A.: Asst. Prof. Med., Univ. of Oregon Med. Sch.
RACKOW, Herbert: Assoc. Prof. Anesthesiol., Columbia Univ.
RAPOPORT, Stanley I.: Surgeon, Sect. Membrane Physiol., NIH
REDMOND, James R.: Assoc. Prof. Zool. & Entomol., Iowa State Univ.
ROBERTSON, William G.: Av. Physiologist, USAF Sch. Aerospace Med.
ROCHESTER, Dudley F.: Asst. Prof. Med., Columbia Univ.
ROTHMAN, Stephen S.: Instr. Physiol., Harvard Sch. Dent. Med.
SAMBHI, Mohinder P.: Res. Assoc. Med., Univ. Southern California
SEALANDER, John A., Jr.: Prof. Zoology, Univ. of Arkansas
SECHZER, Jeri A.: Asst. Prof. Anat., Baylor Univ. Coll. Med.
SHIPP, Joseph C.: Assoc. Prof. Med., Univ. of Florida Coll. Med.
SINGER, Donald H.: Asst. Prof. Pharmacol., Columbia Univ.
SKINNER, Dorothy M.: Asst. Prof. Physiol. & Biophys., New York Univ.
SMEBY, Robert R.: Res. Staff, Res. Div., Cleveland Clinic
SMITH, Thomas G.: Res. Med. Officer, Neurophysiology, NIH
SOLOMON, Neil: Assoc. Prof. Physiol., Univ. of Maryland
SPROULE, Brian J.: Assoc. Prof. Med., Univ. of Alberta
STONE, H. Lowell: Res. Physiol., USAF Sch. Aerospace Med.
SYMMES, David: Asst. Prof. Physiol., Yale Sch. Med.
TAYLOR, Robert E. Jr.: Asst. Prof. Physiol-Biophys., Univ. Alabama
TESCHAN, Paul E.: Chief, Dept. Surg. Physiol., Walter Reed Army Inst. Res.

THOMAS, Duncan P.: Asst. Prof. Med., Tufts Univ.
TIPTON, Charles M.: Asst. Prof. Physiol., Univ. of Iowa
TOBIN, Richard B.: Asst. Prof. Physiol. & Med., Univ. of Rochester
TOMBES, Averett S.: NIH Spec. Post-doctoral Fellow, Univ. Virginia
ULLRICK, William C.: Prof. Physiol., Boston Univ. Sch. Med.
VOGEL, James A.: Res. Physiol., Fitzsimmons Gen. Hosp.
WALDHAUSEN, John A.: Asst. Prof. Surg., Indiana Univ. Med. Ctr.
WOLLMAN, Harry: Asst. Prof. Anesthesia, Hosp. Univ. Pennsylvania
WRIGHT, Peter H.: Assoc. Prof. Pharmacol., Indiana Univ.
WRUTZ, Robert H.: Res. Psychologist, Lab. Neurophysiol., NIH
WURZEL, Menachem: Assoc. Prof. Physiol., Univ. Ottawa
YALOW, Rosalyn S.: Asst. Chief, Radioisotope Serv., Bronx VA Hosp.
ZUMOFF, Barnett: Asst. Prog. Dir., Gen. Cl., Res. Ctr., Montefiore Hosp.

ASSOCIATE MEMBERS

ADKINS, Ronald J.: Instr. Physiol., New York Med. Coll.
BALKISSOON, Basdeo: Asst. Prof. Physiol. & Surg., Howard Univ.
BLOOR, Colin M.: Res. Internist, Walter Reed Inst. of Res.
BLOUNT, Robert W.: Asst. Prof. Physiol., Howard Med. Coll.
BOND, Robert F.: Instr. Physiol., Bowman Gray Sch. Med.
BULLOCK, John: Asst. Prof. Physiol., New Jersey Coll. Med. & Dent.
BURGESS, John H.: Res. Fellow, CV Inst., Univ. of California, S. F.
BURKE, Robert E.: Res. Assoc., Sect. Spinal Cord, Lab. Neurophysiol., NIH
CLANCY, Richard L.: Staff Fellow, Lab. of CV, NHI, NIH
CSERR, Helen: Fellow in Physiol., Harvard Univ.
DeVILLEZ, Edward J.: Asst. Prof. Zool. & Physiol., Miami Univ.
ENGEN, Richard L.: Asst. Prof. Physiol. & Pharmacol., Iowa State Univ.
FERRANTE, Frank L.: Asst. Prof. Physiol., New Jersey Coll. Med.
FRANK, Morton H.: Asst. Prof. Physiol., New York Med. Coll.
GILLESPIE, Jerry R.: Postdoctoral Fellow, CV Res. Inst., Univ. of California, S. F.
HOFFMAN, Joan C.: Asst. Prof. Physiol. & Nursing, Univ. Rochester
LUCIANO, Dorothy S.: Grad. Student, Physiol., Univ. of Michigan
MAUDE, David L.: Asst. Prof. Physiol., New York Med. Coll.
PETERSEN, Walter A.: NIH Spec. Fellow, Physiol., Univ. of Oregon
ROBBINS, Norman: Res. Assoc., Lab. of Neurophysiol., NIH
ROSAS, Oscar M.: Chrmn, Dept. Physiol., Univ. de Guanajuato, Mexico
SANDBERG, Russell L.: Asst. Prof. Physiol., Univ. of Arkansas
SIECK, Michael H.: Doctoral Candidate, Physiol., UCLA
WOODWARD, Donald J.: Grad. Student, Physiol., Univ. of Michigan
WOODY, Charles D.: Staff Assoc., Lab. Neurophysiol., NIH

THE AMERICAN PHYSIOLOGICAL SOCIETY

Founded December 30, 1887; Incorporated June 2, 1923

OFFICERS, 1966-1967

President - Robert E. Forster, University of Pennsylvania, Graduate School of Medicine, Philadelphia, Pennsylvania 19104
President-Elect - Robert W. Berliner, Intramural Research, National Heart Institute, NIH, Bethesda, Maryland 20014
Past President - John M. Brookhart, University of Oregon School of Medicine, Portland, Oregon 97201
Council - R. E. Forster (1968), R. W. Berliner (1969), J. M. Brookhart (1967), H. D. Patton (1967), J. D. Hardy (1968), C. L. Prosser (1969), L. D. Carlson (1970)
Executive Secretary-Treasurer - R. G. Daggs, 9650 Rockville Pike, Bethesda, Maryland 20014

STANDING COMMITTEES

Publications - A. C. Barger (1969), Chairman; J. Mead (1968), D. S. Fredrickson (1969); Ex-officio - R. G. Daggs, Exec. Secy-Treas., Sara F. Leslie, Publications Manager and Executive Editor
Finance - H. S. Mayerson (1967), Chairman; S. M. Tenney (1967), E. B. Brown, Jr. (1968); Ex-officio - R. G. Daggs, Exec. Secy-Treas., W. A. Sonnenberg, Business Manager
Membership Advisory - H. D. Patton (1967), Chairman; W. F. Ganong (1967), D. E. Goldman (1967), W. B. Kinter (1968), A. B. Otis (1968), D. F. Bohr (1969)
Education - R. B. Tschirgi (1967), Chairman; W. C. Randall (1967), C. G. Wilber (1967), D. K. Detweiler (1968), L. H. Marshall (1969); Representatives of Society of General Physiologists - R. R. Ronkin (1967), D. C. Tosteson (1967); Representatives of Comparative Physiology Division of American Society of Zoologists - I. J. Deyrup-Olsen (1968), G. C. Stephens (1969); Ex-officio - R. G. Daggs, Exec. Director Education Programs
Use and Care of Animals - B. J. Cohen (1967), Chairman; E. Knobil (1968), R. W. Doty (1969)
Placement of Senior Physiologists - D. B. Dill (1968), Chairman; E. M. Landis (1968), H. Davis (1969), H. E. Essex (1969)
Porter Fellowship Award - J. K. Hampton (1967), Chairman; C. C. Hunt (1968), W. F. H. M. Mommaerts (1969)
Program Advisory - A. P. Fishman (1968), Chairman; W. L. Nastuk (1967), S. B. Barker (1969)
International Physiology - M. B. Visscher (1971), Chairman; J. M. Brookhart (1969), R. E. Forster (1972)

REPRESENTATIVES TO OTHER ORGANIZATIONS

American Association for the Advancement of Science - R. E. Smith (1967), R. G. Daggs
American Institute of Biological Sciences - A. W. Martin (1969)
A. P. S. Members of the U. S. National Committee for the International

Union of Physiological Sciences -M. B. Visscher (1971), J. M. Brookhart (1969), R. E. Forster (1972)
National Research Council - Division of Biology and Agriculture - A. F. Sellers (1967); Division of Medical Sciences - R. W. Gerard (1967)
Federation Public Information Committee - C. S. Tidball (1967)
Federation Proceedings Editorial Committee - C. McC. Brooks (1967)
American Documentation Institute - M. O. Lee (1967)
Council on Medical Education and Hospitals of the A. M. A. - J. R. Brobeck (1967)

PUBLICATIONS

Publications Committee - A. C. Barger (1969), Chairman; J. Mead (1968), D. S. Fredrickson (1969)
Publications Manager and Executive Editor - Sara F. Leslie
Journal of Neurophysiology - J. M. Brookhart, Chief Editor
Physiological Reviews - J. R. Brobeck, Chairman Editorial Board; R. G. Daggs, Associate Editor
The Physiologist - R. G. Daggs, Editor

EDITORIAL BOARDS

American Journal of Physiology and Journal of Applied Physiology - Section Editors - Philip Dow, B. F. Hoffman (Circulation), Jere Mead, J. W. Severinghaus (Respiration), Jack Orloff (Renal and Electrolyte Physiology), O. D. Ratnoff (Hematology), Eugene Grim (Gastrointestinal Physiology), Jane A. Russell, R. K. Meyer (Endocrinology and Metabolism), J. D. Hardy (Environmental Physiology), A. W. Martin (Comparative and General Physiology), D. P. Purpura (Neurophysiology)

Editors - Mary E. Avery (1969), W. H. Bachrach (1967), C. H. Baker (1969), Bruno Balke (1967), D. F. Bohr (1969), Arend Bouhuys (1969), Eugene Braunwald (1967), F. P. Brooks (1968), R. W. Bullard (1967), C. R. Collier (1968), P. F. Cranefield (1967), P. F. Curran (1969), A. B. DuBois (1968), R. P. Durbin (1967), J. O. Davis (1969), R. W. Eckstein (1969), Sydney Ellis (1969), L. E. Farhi (1967), D. L. Fry (1967), Gerhard Giebisch (1968), J. P. Gilmore (1969), C. W. Gottschalk (1967), Eugene Grim (1968), F. J. Haddy (1969), E. W. Hawthorne (1969), R. E. Hyatt (1968), P. C. Johnson (1967), A. M. Katz (1968), Frederic Kavaler (1967), R. H. Kellogg (1969), D. L. Kline (1967), E. H. Lambert (1968), B. R. Landau (1968), F. N. LeBaron (1968), D. H. K. Lee (1967), N. G. Levinsky (1968), M. N. Levy (1967), Nathan Lifson (1969), L. S. Lillienfield (1968), P. T. Macklem (1969), R. L. Malvin (1969), M. B. McIlroy (1969), W. R. Milnor (1969), R. A. Mitchell (1969), Q. R. Murphy, Jr. (1969), J. A. Nadel (1969), Solbert Permutt (1968), E. P. Radford (1967), D. W. Rennie (1967), Aser Rothstein (1968), R. F. Rushmer (1969), G. C. Salmoiraghi (1968), G. M. Schoepfle (1968), R. O. Scow (1968), J. T. Shepherd (1969), William Sleator, Jr. (1969), R. E. Smith (1968), N. C. Staub (1967), S. M. Tenney (1968), Jay Tepperman (1968), D. F. Tierney (1969), D. C. Van Dyke (1967), H. D. Van Liew (1969), A. G. Wallace (1969), L. G. Welt (1967). Consultant Editors - A. F.

Cournand, H. W. Davenport, Hermann Rahn, E. E. Selkurt.
Physiological Reviews - J. R. Brobeck (1969), Chairman; V. E. Amassian (1969), C. W. Gottschalk (1969), R. C. Swan (1969), J. A. Clements (1968), E. Henneman (1968), A. P. Fishman (1967). Appointed from the Society of General Physiologists - A. G. Szent-Gyorgyi (1968), H. Eisen (1969). Appointed from the American Society of Biological Chemists - R. H. Burris (1969), J. M. Buchanan (1968). Appointed from the American Institute of Nutrition - Grace A. Goldsmith (1967). European Editorial Committee - Eric Neil, Chairman; N. Emmelin, R. B. Fisher, J. N. Hunt, Y. Laporte, Hans Schaefer.
Journal of Neurophysiology - J. M. Brookhart, Chief Editor; T. H. Bullock, Elwood Henneman, D. H. Hubel, C. C. Hunt, Bernard Katz, Anders Lundberg, V. B. Mountcastle, W. D. Neff, J. E. Rose.
Handbook of Physiology, Editorial Committee - M. B. Visscher, Chairman; A. B. Hastings, J. R. Pappenheimer, Hermann Rahn.

PAST OFFICERS

Presidents - 1888 H. P. Bowditch, 1889-1890 S. W. Mitchell, 1891-95 H. P. Bowditch, 1896-1904 R. H. Chittenden, 1905-10 W. H. Howell, 1911-13 S. J. Meltzer, 1914-16 W. B. Cannon, 1917-18 F. S. Lee, 1919-20 W. P. Lombard, 1921-22 J. J. R. MacLeod, 1923-25 A. J. Carlson, 1926-29 Joseph Erlanger, 1930-32 W. J. Meek, 1933-34 A. B. Luckhardt, 1935 C. W. Greene, 1936-37 F. C. Mann, 1938-39 W. E. Garrey, 1938 W. T. Porter, Honorary President, 1940-41 A. C. Ivy, 1942-45 Philip Bard, 1946-47 W. O. Fenn, 1948 M. B. Visscher, 1949 C. J. Wiggers, 1950 H. C. Bazett (April to July); D. B. Dill, 1951 R. W. Gerard, 1952 E. M. Landis, 1953 E. F. Adolph, 1954 H. E. Essex, 1955 W. F. Hamilton, 1956 A. C. Burton, 1957 L. N. Katz, 1958 Hallowell Davis, 1959 R. F. Pitts, 1960 J. H. Comroe, Jr., 1961 H. W. Davenport, 1962 H. S. Mayerson, 1963 Hermann Rahn, 1964 J. R. Pappenheimer, 1965 J. M. Brookhart.
Secretaries - 1888-92 H. N. Martin, 1893-94 W. P. Lombard, 1895-1903 F. S. Lee, 1904 W. T. Porter, 1905-07 L. B. Mendel, 1908-09 Reid Hunt, 1910-14 A. J. Carlson, 1915-23 C. W. Greene, 1924-29 W. J. Meek, 1930 A. C. Redfield, 1931-32 A. B. Luckhardt, 1933-35 F. C. Mann, 1936-39 A. C. Ivy, 1940-41 Philip Bard, 1942 C. J. Wiggers, 1943-46 W. O. Fenn, 1947 M. B. Visscher.
Treasurers - 1888-92 H. N. Martin, 1893-94 W. P. Lombard, 1895-1903 F. S. Lee, 1904 W. T. Porter, 1905-12 W. B. Cannon, 1913-23 Joseph Erlanger, 1924-26 C. K. Drinker, 1927-36 Alexander Forbes, 1937-40 W. O. Fenn, 1941 C. J. Wiggers, 1942-46 Hallowell Davis, 1947 D. B. Dill.
Executive Secretary-Treasurer - 1948-56 M. O. Lee, 1956 - R. G. Daggs.

CONSTITUTION AND BYLAWS

CONSTITUTION

(Adopted at the 1953 Spring Meeting)

ARTICLE I. Name

The name of this organization is THE AMERICAN PHYSIOLOGICAL SOCIETY.

ARTICLE II. Purpose

The purpose of the Society is to promote the increase of physiological knowledge and its utilization.

BYLAWS

(Adopted at the 1966 Spring Meeting)

ARTICLE I. Principal Office

Section 1. The Society shall have its principal place of business at 9650 Rockville Pike, Bethesda, Maryland 20014. The Central Office shall house all activities delegated to the employees of the Society.

ARTICLE II. Corporate Seal

Section 1. The corporate seal of the Society shall be a circle surrounded by the words, THE AMERICAN PHYSIOLOGICAL SOCIETY. The seal shall also show the founding date and the date and place of incorporation.

Section 2. The Executive Secretary-Treasurer shall have custody of the seal. It shall be used on all official documents requiring it, and shall be placed on the documents by the Executive Secretary-Treasurer upon approval by Council.

ARTICLE III. Membership

Section 1. The Society shall consist of regular members, honorary members, associate members, retired members and sustaining associates.

Section 2. Regular Members. Any person who has conducted and published meritorious original research in physiology, who is presently engaged in physiological work, and who is a resident of North America shall be eligible for proposal for regular membership in the Society.

Section 3. Honorary Members. Distinguished scientists of any country who have contributed to the advance of physiology shall be eligible for proposal as honorary members of the Society.

Section 4. Associate Members. Advanced graduate students in physiology at a predoctoral level, teachers of physiology, and investigators who have not yet had the opportunity or time to satisfy the requirements for regular membership shall eligible for proposal for associate mem-

bership in the Society provided they are residents of North America. Associate members may later be proposed for regular membership.

Section 5. Retired Members. A regular or associate member who has reached the age of 65 years and/or is retired from regular employment may, upon application to Council be granted retired member status.

Section 6. Sustaining Associates. Individuals and organizations who have an interest in the advancement of biological investigation may be invited by the President, with approval of Council, to become sustaining associates.

Section 7. Nominations for Membership. Two regular members of the Society must join in proposing a person for regular membership, honorary membership or associate membership, in writing and on forms provided by the Executive Secretary-Treasurer. The Membership Committee shall investigate their qualifications and recommend nominations to Council. Council shall nominate members for election at the Spring and Fall meetings of the Society. A list of nominees shall be sent to each regular member at least one month before the Spring and Fall meetings.

Section 8. Election of Members. Election of regular members, honorary members and associate members shall be by secret ballot at Spring and Fall business meetings of the Society. A two-thirds majority vote of the members present and voting shall be necessary for election.

Section 9. Voting. Only regular members shall be voting members. Honorary, retired and associate members shall have the privilege of attending business meetings of the Society but shall have no vote.

ARTICLE IV. Officers

Section 1. Council. The management of the Society shall be vested in a Council consisting of the President, the President-Elect, the immediate Past-President, and four other regular members. The terms of the President and of President-Elect shall be one year. The terms of the four additional Councilors shall be four years each and they shall not be eligible for immediate reelection except those who have served for two years or less in filling interim vacancies.

A quorum for conducting official business of the Society shall be five of the seven elected members of Council.

The Chairman of the Publications Committee; the Chairman of the Finance Committee; and the Executive Secretary-Treasurer are ex-officio members of the Council without vote. The Council may fill any interim vacancies in its membership. Council shall appoint members to all committees.

Section 2. President. A person shall serve only one term as President, except that if the President-Elect becomes President after September 30 he shall continue as President for the year beginning the next July 1. The President shall chair all sessions of the Council and business meetings of the Society and shall be an ex-officio member of all committees without vote.

Section 3. President-Elect. The President-Elect shall serve as

Vice-President of the Society and as official secretary of the Council. Should he have to function as President prematurely, the Council shall select from among its own members an official secretary.

Section 4. Election of Officers. Nominations and election of a President-Elect and Councilor(s) shall be by secret ballot at the Spring business meeting of the Society. They shall assume office on July 1 following their election.

Section 5. Executive Secretary-Treasurer. The Council shall be empowered to appoint and compensate an Executive Secretary-Treasurer who shall assist it in carrying on the functions of the Society including the receipt and disbursement of funds under the direction of the Council. He shall be responsible for management of the Central Office of the Society under general supervision of the Council.

ARTICLE V. Standing Committees

Section 1. Publications Committee. A Publications Committee composed of three regular members of the Society appointed by Council shall be responsible for the management of all of the publications of the Society. The term of each member of the Publication Committee shall be three years; a member may not serve more than two consecutive terms. The Council shall designate the Chairman of the Committee who shall be an ex-officio member of the Council, without vote. Council is empowered to appoint and compensate a Publications Manager who shall assist in carrying out the functions of the Publications Committee under the supervision of the Executive Secretary-Treasurer. The President, Executive Secretary-Treasurer and the Publications Manager shall be ex-officio members of the Publications Committee without vote. The Committee shall have the power to appoint editorial boards for the Society's publications. The Committee shall present an annual report on publications and policies to the Council for approval and present an annual budget coordinated through the Executive Secretary-Treasurer, to the Finance Committee for its approval and recommendation to Council.

Section 2. Finance Committee. A Finance Committee, composed of three regular members of the Society appointed by Council, shall receive the total coordinated budget proposals annually from the Executive Secretary-Treasurer and shall determine the annual budgets, reserve funds and investments of the Society, subject to approval by the Council. The term of each member of the Finance Committee shall be three years; a member may not serve more than two consecutive terms. The Council shall designate the Chairman of the Committee who shall be an ex-officio member of the Council, without vote. Council is empowered to appoint and compensate a Business Manager who shall assist in carrying out the functions of the Finance Committee under the supervision of the Executive Secretary-Treasurer. The President-Elect, Executive Secretary-Treasurer and the Business Manager shall be ex-officio members of the Finance Committee, without vote.

Section 3. Membership Committee. A Membership Committee, composed of six or more regular members of the Society appointed by the Council, shall receive and review processed applications for membership and make recommendations for nomination to the Council. The term of each member of the Membership Committee shall be three years;

a member shall not be eligible for immediate reappointment. The Chairman of the Committee shall be designated by the Council.

Section 4. Education Committee. An Education Committee, composed of five or more regular members of the Society and representatives of such other societies as may be designated by the Council, appointed by the Council, shall conduct such educational, teaching and recruitment programs as may be required or deemed advisable. The term of each member of the Education Committee shall be three years. The Chairman of the Committee shall be designated by the Council. The Executive Secretary-Treasurer may act as Executive Director of the educational programs with approval of the Council. The Committee shall present an annual report to the Council and an annual budget through the Executive Secretary-Treasurer to the Finance Committee for its approval.

Section 5. The Council may appoint such special and other standing committees as it deems necessary or that are voted by the Society. The Council may name regular members of the Society as representatives to other organizations whenever it deems such action desirable.

ARTICLE VI. Dues

Section 1. Annual Dues. The annual dues for regular members and associate members shall be determined by the Council and shall be paid in advance of July 1. Honorary members and retired members shall pay no membership dues.

Section 2. Non-payment of Dues. A regular or associate member whose dues are two years in arrears shall cease to be a member of the Society, unless after payment of his dues in arrears and application to the Council, he shall be reinstated at the next meeting by vote of the Council. It shall be the duty of the President-Elect to notify the delinquent of his right to request reinstatement.

Section 3. Retirement. A regular or associate member who has been granted retired membership status is relieved from the payment of dues but retains the other privileges of his former membership status, except voting privileges.

ARTICLE VII. Financial

Section 1. Society Operating Fund. The Society Operating Fund shall consist of all funds, other than Publication Operating Funds and Publication Contingency and Reserve Funds, restricted or unrestricted, uninvested or invested, short or long-term. The Executive Secretary-Treasurer shall be the responsible agent to the Council with signatory powers. Signatory powers may be delegated to the Business Manager by the Executive Secretary-Treasurer.

Section 2. Publications Operating Fund. The Publications Operating Fund shall consist of all funds that involve receipts, expenses, short-term investments relating to the annual receipts, disbursements and continuing operation of the Society's publications. The Executive Secretary-Treasurer shall be the responsible agent to the Council with signatory powers. Signatory powers may be delegated to the Publications Manager and/or the Business Manager by the Executive Secretary-Treasurer.

Section 3. Publications Contingency and Reserve Fund. The Publications Contingency and Reserve Fund shall consist of the long-term capital investments of publication earnings. The Executive Secretary-Treasurer, with advice from the Finance Committee, shall have discretionary and signatory powers, except for withdrawals. Authority for any withdrawal from this fund, shall require the following five signatures: 1) the Chairman of the Publications Committee (alternate, the senior member of the Committee); 2) the President of the Society (alternate, the President-Elect); 3) the Executive Secretary-Treasurer (alternate, the Publications Manager); 4) and 5) any two members of Council. The Finance Committee shall not recommend to Council the expenditures of any of this capital fund for non-publication purpose without the consent of the Publications Committee. The Finance Committee shall be responsible for the separate investment of the reserve fund for publications; any capital gains from such investment shall accrue to the fund (capital losses will, however, reduce its value). Any dividends, interest or income, other than capital gains, from this invested fund may be used for emergency support of any of the activities of the Society, including publications, as determined annually by the Council but the primary goal shall be to increase the investment capital.

Section 4. Fiscal Year. The official fiscal year shall be from January 1 through December 31.

Section 5. Audit. All statements of net assets and related statements of income, expenditures and fund capital shall be audited annually by an independent auditing firm.

Section 6. Bonding. All persons having signatory powers for the funds of the Society shall be bonded.

ARTICLE VIII. Publications

Section 1. The official organs of the Society shall be the American Journal of Physiology, the Journal of Applied Physiology, Physiological Reviews, the Journal of Neurophysiology, The Physiologist, and such other publications as the Society may own. All publications shall be under the jurisdiction and management of the Publications Committee unless otherwise designated by the Council. The names of the journals and publications may be changed by the Council on recommendation from the Publications Committee and any publication may be dropped by Council on recommendation from the Publications Committee.

ARTICLE IX. Meetings

Section 1. Spring Meeting. A meeting of the Society for transacting business, electing officers and members, presenting communications, and related activities, shall ordinarily be held in the Spring of each year.

Section 2. Fall Meeting. A Fall meeting of the Society shall be held at a time and place determined by the Council for presenting communications, electing members, and for transacting business except for the election of officers and adoption of amendments to the Bylaws. Under exceptional circumstances Council may cancel such a meeting.

Section 3. Special Meetings. Special meetings of the Society or of the Council may be held at such times and places as the Council may

determine.

Section 4. Quorum. At all business meetings of the Society fifty regular members shall constitute a quorum.

Section 5. Parliamentary Authority. The rules contained in Roberts Rules of Order, Revised shall govern the conduct of the business meetings of the Society in all cases to which they are applicable and in which they are not inconsistent with the Bylaws or special rules of order of the Society.

ARTICLE X. Society Affiliations

Section 1. The Society shall maintain membership in such organizations as determined by Council.

ARTICLE XI. Regulations

Section 1. General Prohibitions. Notwithstanding any provision of the Constitution or Bylaws which might be susceptible to contrary interpretation:

a. The Society is organized and operated exclusively for scientific and educational purposes.
b. No part of the net earnings of the Society shall or may under any circumstances inure to the benefit of any member or individuals.
c. No substantial part of the activities of the Society shall consist of carrying on propaganda, or otherwise attempt to influence local, state or national legislation. (All activities of the Society shall be determined by Council). The Society shall not participate in, or intervene in (including the publishing or distributing of statements) any campaign on behalf of any candidate for public office.
d. The Society shall not be organized or operated for profit.

Section 2. Distribution on Dissolution. Upon lawful dissolution of the Society and after payment of all just debts and obligations of the Society, Council shall distribute all remaining assets of the Society to one or more organizations selected by the Council which have been approved by the United States Internal Revenue Service as organizations formed and dedicated to exempt purposes.

ARTICLE XII. General

Section 1. Records. All official records, archives and historical material shall be held in the Central Office in the custody of the Executive Secretary-Treasurer.

Section 2. Procedures and Customs. The Society shall maintain a current Operational Guide detailing the procedures and current customs of the Society operations as well as the duties and responsibilities of officers, committees, and major employees. The Operational Guide shall be maintained current by the Executive Secretary-Treasurer as determined by the Council.

ARTICLE XIII. Amendments

Section 1. Presentation. Amendments to these Bylaws may be proposed in writing, by any regular member, to Council at any time up to three months in advance of the Spring meeting, or at a business meeting of the Society. Such proposed amendments must be presented in writing at the following Spring business meeting for action by the Society.

Section 2. Adoption. These Bylaws may be amended at any Spring business meeting of the Society by a two-thirds majority vote of the regular members present and voting.

APS
MEETING CALENDAR

1967 Spring - Chicago, Ill., April 16-21
1967 Fall - Howard Univ., Washington, D. C., August 23-26
1968 Spring - Atlantic City, N.J., April 15-20
1968 International Physiological Congress - Washington, D. C., August 25-30
1968 Fall - No Fall Meeting due to the Congress
1969 Spring - Atlantic City, N.J., April 13-18
1969 Fall - Oklahoma State Univ., Stillwater and Univ. of Oklahoma Medical Center, Oklahoma City
1970 Spring - Atlantic City, N.J., April 12-17

PAST-PRESIDENT'S ADDRESS

JOHN M. BROOKHART

THE APS AND MEDICAL EDUCATION

Before I begin this final effort to fulfill the obligations associated with my term as the President of the American Physiological Society, I should like to add my own personal expression of thanks to the members of the Local Committee who have worked so hard and successfully to make our meeting in Houston as delightful as it is proving to be. I cannot imagine that the facilities of the Great State of Texas, the City of Houston or of our host University have even come close to being strained by the presence of the American Physiological Society this week. I prefer to believe that the lack of any overt signs of strain is importantly dependent on the foresight and the skills of those responsible for the arrangements.

Facing up to the task of selecting a topic for the Past-President's address, one is plagued by an embarrassment of riches. Looking back on past events of this kind, I find that Hallowell Davis dwelt on the problems of bigness; Alan Burton waxed poetic on the meanings of life and science; Hy Mayerson took a retrospective look at the "American Journal of Physiology" 75 years earlier; Hermann Rahn searched for a distilling process which would separate knowledge from garbage; John Pappenheimer reminded us of our foundation footings in physiological literature; and Julius Comroe's remarks were unprintable. So - the tradition has already been established that the number of degrees of freedom in selection is almost limitless. In reacting to this permissive situation, I have decided to share with you some of my thoughts and my concern about the role of the physiologist as a teacher.

I am encouraged to dwell on this aspect of our lives by several observations. I am reminded that the Constitution of the Society specifies the purpose of the Society as the promotion of "the increase of physiological knowledge and its utilization." I am reminded that most of us bear academic titles which include the word "Doctor", - a word which comes rather directly from the Latin verb - docere - meaning to teach. The students of sociology look upon any social organization as an expression of a need for mutual assistance between individuals who have a common interest. As members of the American Physiological Society we have a common interest in teaching - whether this be in teaching ourselves through our investigative efforts, whether it be in teaching others who aspire to careers as professional physiologists, or whether it be in teaching others who require some physiological knowledge in order to aid them in their progress toward other career goals. And finally, the studies of membership indicate that the majority of us are identified as teachers by that simple but powerful economic tool called the pay check. From this perspective then, the American Physiological Society appears to be constituted of teachers who are expressing a need for the exchange of experience and opinion for their mutual assistance.

Having thus reduced the number of degrees of freedom rather sharply,

I propose to limit myself still further and concentrate on problems of teaching in medical schools which I think deserve serious attention on the part of the Society. I make no apologies to our non-medical colleagues for this further restriction. It seems to me appropriate that the Society concern itself with a problem of this particular but limited scope. Traditionally and historically the Society has been composed of physiologists having primarily an orientation toward mammalian or human or medical physiology. The founders of the Society were associated largely with schools of medicine. The presidents of the Society through the years have been overwhelmingly members of medical faculties. For the past five or six years this orientation of the Society has been recognized as a deficiency in our structure. Even though the Society has been making serious, and increasingly successful efforts over the past decade to assure them of welcome and interest, it is easy to understand why those physiologists with an orientation toward general or comparative studies still find it to their advantage to carry membership in other differently specialized societies. Consequently, we may say that the Society is making serious efforts to enlarge and expand its capability for service to a greater variety of physiologists. Nevertheless, I do not think that we can afford to ignore the possibility that the American Physiological Society may still have an important mission to fulfill with respect to the place of physiology in medical education.

I am sure you are aware that all is not serene and quiet on this sector of the educational front. The signs of turbulence and uncertainty come from several directions. For a number of years, Dr. Daggs has been calling the attention of Council to the disappearance of the word "Physiology" from course titles in college bulletins and catalogues. The Education Committee has done a magnificent job of attempting to counter this trend through several different mechanisms. Most of us are almost continuously engaged in staff debates at our home institutions about the proper balance of course content to be offered to medical students. And even more recently, the threat has begun to develop that physiology, as a distinct and separate discipline, may be encouraged to fade out of the medical curriculum. There is growing unrest and uncertainty with the manner in which medical students are being prepared in physiology. With increasing frequency, members of departments of medicine and surgery are publicly expressing the view that they are the ones who know best, through their experience, what aspects of human physiology should be presented to medical students. Both old and new medical schools are experimenting with new curricular arrangements - some of them quite bizarre departures from tradition - which challenge the role of physiology as a separate discipline pertinent to medicine and of the professional physiologist as a member of a medical faculty. On the one hand we hear of plans to create a new medical curriculum in which there is no specific physiology department, - that aspect of the students training being left to undergraduate years. On the other hand we hear of the retention of a department with the title of Physiology but newly staffed with brilliant young men who have had no previous contact with a department of physiology. Such departures from tradition may be temporary, - they may be interesting and perhaps successful plans for reorganization. I call attention to them not in any critical sense but simply as signs of general unrest and uneasiness.

The source of this discontent seems quite clear to me. Before the unbelievable explosion of scientific endeavor which has characterized the years since World War II, the "state of the art" in physiology forced our attention primarily to the description of the functional characteristics of organs, organ systems and organisms on high and complex levels of integration. As our technological capability has developed along with our understanding, our curiosity about functional mechanisms has led us deeper and deeper toward primary events at the cellular and subcellular levels. This is simply the expression of the incontrovertible fact that the properties of the organism can be understood only if the properties of the parts are clearly known. As a result, physiologists, molecular biologists, biochemists, biophysicists, and electron microscopists meet each other on common grounds and tend to lose their identity through confusion. It becomes difficult to determine who is a physiologist and what is physiology.

This new orientation toward interest in simpler levels of organization has had secondary repercussions which have spilled over into our teaching efforts. I am sure that it has changed the character and quality of our graduate training activities in a way which is absolutely essential to the generation of a professional physiologist, but in a way which detracts from his effectiveness as a teacher of medical students. I think it highly probable that in some cases our enthusiasm for exploration of primary events and our familiarity with our special field of interest colors our judgment about the wise and appropriate selection of material for presentation to students who need a background in physiology for their non-physiological careers. I know from personal experience that the examples which are selected in an attempt to convey the philosophy or the excitement of physiological ways of thinking are sometimes alien and meaningless to any but professional physiologists.

If this is, indeed, the source of the discontent, then it is obvious that the problem is self-terminating. Our present preoccupation with primary events will diminish as our knowledge of them grows and as we begin to put the pieces back together again. As the functions of the parts become clear, our curiosity will inevitably drive us to see how they interact, and ultimately, we will be looking again at organ systems and organisms with an eye to understanding the human being. My thoughts go in this direction because I fail to respond to the rationalization that the acquisition of knowledge is sufficient unto itself. I believe that our curiosity about basic processes stems from our recognition that knowledge is necessary to enable us to mold our human behavior and human environment to our human interests, and that in the long run all bits of information are valuable for their eventual applicability to human needs. When the capability for the resynthesis of complex systems develops, there will be no doubt about the role of physiology as a scientific discipline characterized by an interest in how things work at all levels of organization. When this time comes, the questions concerning the role of physiology in medical education will have disappeared. It is in this sense that these problems are self-terminating.

But this day has not yet come - and the threat to physiology as a discipline pertinent to the medical curriculum is now. My concern

stems from the possibility that the flood of discontent may have its effects before we can turn away from our attention to primary events. I think we must listen carefully and seriously to the critics of our role as teachers and consider whether we are, in fact, doing the job which must be done for medical students - or whether we are, in fact, following paths of least resistance and dealing with those aspects of physiology which are of greatest interest and familiarity to us. Occasionally, in discussions of this kind, one hears the scornful protestation that to consider the needs of medical students - as medical students - rather than as potential physiologists would contribute to converting a medical school into a trade school. I think this argument is completely without weight. To the extent that a trade school can be defined as a school wherein one learns special skills, medical schools cannot help but be trade schools. Medical schools are not trade schools to the extent that we can substitute rationality and understanding for empiricism. This same protester would never consider the training of a graduate student complete unless he had acquired useful skills in physiological techniques. Thus, I see no reason why we should consider it demeaning to give to the students entrusted to our care the instruction in those portions of physiology most relevant to their specific career goals. That their goal happens to be the best and most complete understanding of the human organism to the end that they can contribute to the maintenance of human life and productivity is certainly no cause for scorn. It does not bother me in the least that medical students are not interested in becoming completely skilled physiologists any more than it concerns me that engineering or law students show a similar lack of interest in physiology as a profession. I think the needs of the young physician for a foundation upon which he can build in the future can be defined better by us as physiologists looking ahead to the physiology of tomorrow than they can be by internists, surgeons and pediatricians looking back on the physiology of the past and relating this to their past experiences. Unless we give some recognition to the existence of this problem, I anticipate that this prerogative will be taken away from us and that physiology as a discipline will be diminished in importance.

To cry alarm about a problem is easy. To offer a concrete solution is another task - beyond my capability. This is the kind of problem which requires discussion and debate. It is in a certain way comforting, - but at the same time ominous - to recognize that Biochemistry is in a similar situation. The August 8th issue of the Journal of the American Medical Association carries an essay in which Samuel Bessman expresses the opinion that part of the problem stems from the orientation given to the training of young biochemists. Should we examine our own training activities to determine whether we are preparing our graduate students to accept the original meaning of the title of "Doctor" which our institutions will grant them? A number of questions, particularly pertinent to the place of physiology in medicine have been accepted as an assignment from the Council by the Education Committee for its attention. Many of you will undoubtedly hear from them in the near future. Perhaps the time has come for the American Physiological Society to turn more of its attention to the second of its two constitutional purposes - namely, the utilization of physiological knowledge. As things now stand, each one of us operates alone in his institution when he participates in the

activities of curriculum committees and planning committees. The opinions each one of us expresses are based on tradition colored by highly personal experiences. No one of us has any clear idea of what our physiological colleagues think about the ways in which we can best fulfill our roles as teachers of physiology in medicine. No one of us can ring our own variations on a generally accepted theme because there is no generally accepted theme. Do we need the support of an organized effort by the American Physiological Society to establish a general framework such as the Biological Sciences Curriculum Study recently completed by the AIBS? Certainly, the role of physiology in medical education is going to evolve in adaptation to increasing knowledge and understanding. Should we - as a professional society - concern ourselves with the direction of this evolution? The alternative is to allow this evolution to occur subject to the almost random forces that collectively operate in our medical schools and hope that the outcome will be satisfactory to us all. I don't know the answers to these questions; they can come only from you.

Well - I have been ponderous - I have been serious - I have not told a single funny story. Those of you who have been able to ignore the charms of Morpheus have probably been able to do so because of the discomfort associated with inhibition of your gastrointestinal secretory and motor activities. It is too late now to be light and frivolous. Consequently, in the same serious vein, - before I return the microphone to President Forster, - I am grateful for this opportunity to acknowledge publicly that I consider it a great honor to have been chosen to serve the Society as its President for the past year. It has been a most rewarding and instructive experience which could have been exceedingly difficult were it not for the wise guidance of the Council coupled with the very effective work of Dr. Ray Daggs. Their actions, in turn, are clearly an expression of the advice of a number of Standing Committees whose efforts deserve continuing solid recognition by the membership. The quality of being President of the American Physiological Society is transient; the quality of being a member of this Society is enduring. This is what I cherish. Thank you all.

TERMINOLOGY OF THERMOREGULATION

G. C. Whittow

In a recent issue of "The Physiologist" (May 1966), Dr. J. A. Miller drew attention to the unsatisfactory etymology of some of the terms used by physiologists interested in thermoregulation. Dr. Miller suggested alternative words and he attempted to define them. On all three counts he is to be commended. A minor criticism of his suggestions is that in introducing a numerical element into his definitions, he has apparently considered only man and some laboratory species. For example, "hyperthermia" is defined as "temperatures from 37.5°C to lethal heat" which would relegate most birds, which are, of course, homeotherms, to a permanent condition of moderate hyperthermia. "Coenothermia" is defined as "temperatures which are common to the species (from about 36.5°C - 37.5°C)". The quantitative limitations of this definition would exclude birds, primitive mammals, and many other animals which are unquestionably homeothermic.

However, the main purpose of the present note is to catalog further terms which, in the writer's opinion, are used with even greater ambiguity than are those listed by Dr. Miller. Notable among these is the word "panting". Panting is a specific type of respiratory activity which has never been adequately defined. Many investigators would require an animal to have its mouth open and its tongue protruding in order to qualify as a panting animal, although further investigations are needed in order to determine whether "open-mouthed panting" is more efficient from the standpoint of heat loss than "closed-mouth panting". Other factors such as airway resistance may be more important determinants of the open-mouthed condition. Synonyms that have been used for panting include "thermal polypnea", "heat-tachypnea", "thermally induced hyperpnea", and many other terms. Some investigators in the past have designated an arbitrary respiratory rate to denote the beginning of panting. This has led to the paradox that an animal could be losing, by respiratory evaporative cooling, most of the heat which it was producing, although, by definition, it was not panting. The terminology is complicated by the biphasic nature of the respiratory response to heat in some animals (8). In some species, e.g., reptiles, there is the problem of distinguishing between an increased respiratory activity resulting from an increased body temperature, per se, and an increased ventilation commensurate with an increased demand for oxygen secondary to the increased body temperature (7). Clearly, there is a need to define, in terms of thermoregulation value, the respiratory responses of an animal to heat.

"Hypothermia" and "hyperthermia" present problems of definition, mentioned above, and also of interpretation. As Macfarlane (5) has pointed out, hyperthermia is taken by some investigators to indicate a condition of impaired heat tolerance while others regard hyperthermia as an important thermoregulatory mechanism.

"Hibernation", "estivation" and "torpidity" are difficult to define because they are phenomena which are imperfectly understood (7).

However, there is an awareness of this, and further investigative work, it is hoped, will lead to less equivocal use of the terms (3).

"Acclimation", "acclimatization" and "adaptation" have enjoyed a greater uniformity of understanding than have many other terms used in the literature on thermoregulation. As far as can be ascertained, this is to be attributed to the prominence given to Hart's definitions of these terms by Burton and Edholm (1) in their book "Man in a Cold Environment."

"Poikilothermic" and "homeothermic" have long been used to differentiate between animals in which the body temperature fluctuates widely with environmental temperature and those with a relatively constant body temperature. These are terms which distinguish clearly between a man and a fish but they have serious limitations which do not take into account the low body temperature of a hibernating mammal or bird, or the relatively constant high body temperatures of many reptiles. Largely, it seems, as a result of the efforts of Dr. R. B. Cowles (2), "ectotherm" and "endotherm" have superseded "poikilotherm" and "homeotherm" in the usage of many investigators, particularly those interested in reptiles, amphibia and other lower vertebrates and invertebrates. The distinction between "endotherms" and "ectotherms" is an important one: the former can increase their body temperature, in the face of a decrease in environmental temperature, by increasing the amount of heat which they produce in their own tissues, while the latter, in similar circumstances, can only increase their body temperature by the acquisition of heat from outside their bodies, e.g., from solar radiation. It is questionable whether any term is generally applicable without exceptions and it seems that "ectotherm" might not encompass some of the changes in metabolic rate and temperature which occur in "poikilotherms" during "acclimation" (6). However, it is probably also true that "ectotherm" and "endotherm" are less subject to ambiguity than are "poikilotherm" and "homeotherm", although it is well to remember that the two sets of terms refer to different aspects of thermoregulation, viz., the means by which heat is produced or gained and the variability of body temperature, respectively.

"Heterotherm" has been used to classify those animals which, in certain circumstances, become torpid (2). It has also been used by Irving (4) to describe the variable temperature of the extremities and tissues in "homeotherms". Probably the term is best avoided.

The writer is aware that this contribution to the semantics of thermoregulation is largely negative. However, it will have served its purpose if, together with Dr. Miller's remarks, it has focussed attention on some of the equivocal terms now used by physiologists. Ideally, terminology should be philologically correct and generally used and understood. In the writer's opinion, usage and understanding should take precedence over etymological purity if this ideal situation cannot be achieved. The diverse meanings of words such as "panting" detract from a better understanding of the physiology of thermoregulation and make communication between investigators more difficult.

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BACK ISSUES OF APS JOURNALS

The May 1966 issue of The Physiologist carried an appeal for back issues of some of the Society's journals. The missing volumes are needed to complete our library now that we have consolidated central office space and a place to keep them.

We wish to acknowledge the very generous gift of Dr. L. G. Kiborn of Guelph, Ontario, Canada. As a result of our appeal he presented the Society with the volumes of Physiological Reviews that we needed to complete our set.

The Society still needs several back volumes of the American Journal of Physiology. They are Vols. 1 through 66 and Vols. 96 through 99. We would be pleased to receive any of these volumes. Perhaps some retired member like Dr. Kiborn, who may have little use for these back volumes, would be willing to assist the Society in completing its file set of the American Journal of Physiology.

CAPILLARY BLOOD FLOW AND TRANSCAPILLARY EXCHANGE*

EUGENE M. RENKIN

The function of the cardiovascular system is to exchange materials between blood and tissues. This is brought about by a combination of convection or flow of blood through the capillaries and diffusion and ultrafiltration across their walls. I shall review the physiological mechanisms which control circulation through peripheral vascular networks and then examine these mechanisms in terms of their influence on transcapillary exchange. In doing this I shall follow the terminology of Folkow and his associates which distinguishes the resistance, capacitance and exchange functions of the minute blood vessels (4).

The small arteries and arterioles are the precapillary resistance vessels. They present about 80 per cent of the total resistance to blood flow through the peripheral vascular network. They deliver the blood to the capillaries, and contraction and relaxation of the smooth muscle cells in their walls determines the total quantity of blood supplied. Arteriolar smooth muscle is subject to control by nervous, hormonal, physical and local chemical factors.

The exchange vessels are the capillaries and possibly also the smaller venules, since the wall structure of these vessels is closely similar to that of the capillaries, and their ratio of surface area to blood flow is comparably high. Through the delicate endothelial walls of these vessels interchange of materials between blood and tissue fluids takes place. The distribution of blood to the exchange vessel network is controlled by the precapillary sphincters. These are the last one or two smooth muscle cells located at the branch of a capillary from its parent arteriole. Contraction or relaxation of precapillary sphincters may simply determine the number of capillaries which are open to the flow of blood (3, 19) or may control the distribution of total blood flow between exchanging and non-exchanging pathways from arteriole to venule (6, 7). The precapillary sphincters are differentiated from arteriolar smooth muscle cells not only by their strategic location, but also to some extent in structure and in mechanisms of control. The permeability of the exchange vessels per unit effective surface area may also be subject to physiological control by chemical, hormonal or even possibly nervous influences on the endothelial cells themselves. But we know almost nothing about this.

The larger venules and small veins are the postcapillary resistance

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*Taken from the introductory remarks given at the session on Capillary Blood Flow at the 1966 Federation Meetings.

The author's research on this topic is supported by National Science Foundation Research Grant GB-1315.

and capacitance vessels. Postcapillary hydrodynamic resistance is a small fraction of total resistance, and thus has relatively little influence on total blood flow. However, the ratio of postcapillary to total resistance determines capillary hydrostatic pressure, which is an important variable controlling ultrafiltration exchange. The capacitance function of these vessels arises from the large volume of blood they contain. Contraction of capacitance vessel smooth muscle displaces part of this volume into the central circulation and thus is an important mechanism for controlling venous return. This action constitutes an important part of systemic circulatory control. The smooth muscle of the venules is subject principally to nervous and hormonal control. Local chemical factors have much less influence than on arterioles and precapillary sphincters. The extent to which capacitance and resistance functions of venules are separately controlled is not known.

In the capillary network, exchange of materials between blood and tissues takes place by two distinct processes: diffusion and ultrafiltration (14). Diffusion is a consequence of the incessant kinetic motion of individual molecules and ions. It is the process responsible for interchange of almost all the low molecular-weight solutes which are the substrates and products of cell metabolism. Ultrafiltration or osmosis are names for bulk flow of fluid into or out of capillaries in consequence of differences in hydrostatic and effective osmotic pressure across their walls. The quantities of material moved are small - negligible in terms of metabolic support - but close control is required for maintenance of the plasma and interstitial fluid volume.

According to Starling's Hypothesis (13, 23) the capillaries are freely permeable to water and low molecular-weight solutes, but impermeable to plasma colloids (plasma proteins). The tendency of capillary hydrostatic pressure (P_c) to force fluid out is balanced by the tendency of plasma protein osmotic pressure (π_p) to draw fluid in. When Pc and π_p are equal, there is no net fluid movement. If P_c is greater than π_p, fluid leaves the capillaries, diminishing plasma volume and increasing interstitial fluid volume. If π_p is less than P_c, fluid enters the circulation. The rate of fluid movement (ultrafiltration rate, F) is given by the following simplified relation:

$$F = K_F (P_c - \pi_p) \qquad (1)$$

K_F is the capillary filtration coefficient, proportional to capillary permeability to filtered fluid per unit surface area, and to capillary surface area.

The equation above is an over simplification, in that the influence of tissue hydrostatic pressure (P_T) and interstitial fluid osmotic pressure (π_T) is not taken into account. These quantities must be subtracted from capillary hydrostatic pressure and plasma colloid osmotic pressure, respectively. The full equation is

$$F = K_F \left[(P_c - P_T) - (\pi_p - \pi_T)\right] \qquad (2)$$

Ultrafiltration exchange is little influenced by capillary blood flow

per se, since the filtration fraction is ordinarily no more than a few percent of plasma flow. However, K_F is proportional to the number of capillaries open, and this is under control of the precapillary sphincters. K_F is also subject to any factors which can modify capillary permeability, but the extent to which these might participate in physiological control of ultrafiltration is unknown.

Of the other variables in equation 2, I can say little except with regard to P_C . π_p is determined largely by the concentration of serum albumin in the plasma, and this depends on the balance of its synthesis in the liver and its degradation. π_T and P_T are dependent on the rate of leakage of protein from capillaries and on the rate of its removal from the interstitial space by lymphatics, processes about which we know very little. But P_C is subject to control by well-known vasomotor mechanisms. Its magnitude depends on the relative values of pre- and postcapillary resistance (1, 17, 18).

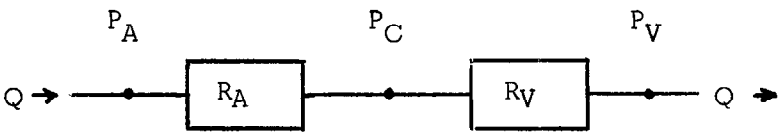

Fig. 1. Diagram illustrating control of capillary pressure. See text.

Figure 1 is a simplified diagram to illustrate the capillary hemodynamics responsible for this. Precapillary resistance (R_A) and postcapillary resistance are connected in series between terminations at arterial pressure (P_A) and venous pressure (P_V). Capillary pressure (P_C) is the pressure at their junction. The pressure drop across each or both is given by the hydrodynamic Ohm's law, $\Delta P = Q \cdot R$.

Thus

$$P_A - P_V = Q (R_A + R_V) \quad (3)$$

and

$$P_C - P_V = Q (R_V) \quad (4)$$

Since flow through R_A and R_V is the same,

$$\frac{P_C - P_V}{P_A - P_V} = \frac{R_V}{R_A + R_V} \quad (5)$$

If P_V is zero, then the ratio of capillary pressure to arterial pressure is equal to the ratio of postcapillary resistance to total vascular resistance. Normally in mammals, π_p = 25 mm, π_T in peripheral organs is about 5 mm Hg and P_T is close to zero. Thus P_C must be about 20 mm Hg for fluid balance. If mean P_A is 100 mm Hg, this requires that R_V be about 1/5 of total vascular resistance. Any vasomotor change must maintain this ratio if fluid balance is not to be disturbed. However R_A is variable over a much wider range than R_V. Thus vasoconstriction tends to increase R_A out of proportion to R_V, P_C falls and fluid moves into the capillaries. Vasodilatation usually has just the

opposite effect (15, 17).

There is reason to believe that specific local mechanisms exist in some vascular beds for controlling the relation of R_V to R_A. Increasing transmural pressure by raising either arterial or venous pressure, or both, has frequently been observed to produce precapillary vasoconstriction, decreasing transmural pressure precapillary vasodilatation (5). This is usually attributed to the direct effect of stretch on the smooth muscle cells of the arterioles. If the change in pressure is applied on the arterial side, the stretch response acts in the same direction as the response of these vessels to vasodilator metabolites, and reinforces metabolic autoregulation of blood flow. But if venous pressure is altered, the response to stretch is antagonistic to autoregulation of blood flow. In both cases, however, the stretch effect operates in the direction of stabilizing capillary pressure, and is consistent with a regulatory mechanism controlling either capillary pressure or ultrafiltration exchange. Stabilization of capillary pressure without regard to protein osmotic pressure appears incomplete as a useful control mechanism, but a receptor for transcapillary fluid movement is hard to imagine. It is known, however, that vascular resistance, presumable precapillary, is decreased if the blood is made hypertonic (16). We may be observing bits and pieces of a complex regulatory mechanism for controlling fluid balance.

The mechanism responsible for practically all exchange of metabolic substrates and products between blood and cells is diffusion. Diffusion is the result of thermal motions of individual molecules, and therefore different substances can diffuse independently and in opposite directions at the same site. Net transport of any substance by diffusion proceeds from regions where its concentration is high to regions where its concentration is low. According to Fick's Law, the rate of net transport is proportional to the concentration gradient and to the mobility of the diffusing particles in the medium in which diffusion takes place. Gradients of concentration in the tissue around capillaries are set up by consumption or production of materials by cellular metabolism. In the steady state, a field of stationary gradients is set up in which the concentration of various substances varies radially and longitudinally in relation to individual capillaries (2). For a substance consumed by the cells, like oxygen, glucose or free fatty acids, the concentration is highest in the capillary and falls as one moves radially outward. The lowest concentration is reached mid-way between adjacent open capillaries. Similarly, along the length of a capillary, concentration will fall as the blood moves from arteriole to venule, and substrate is lost by diffusion. The pattern of longitudinal and radial gradients is exactly the opposite for substances produced by cell metabolism and carried away by the blood.

Availability of metabolic substrate to the cells depends on supply relative to demand. If consumption of oxygen, glucose or free fatty acids is increased with no change in blood flow or intercapillary distance, the diffusion gradients for these substances must become steeper, both radially and longitudinally, and their concentration must fall at every point within the tissue. If the gradients are steep enough for a

given substrate, its concentration will reach zero some distance away from the capillary, thereby limiting its uptake by the cells. To some extent this limitation can be overcome by the local control mechanisms which produce metabolic vasodilatation. Contraction of resistance vessel smooth muscle is inhibited. This increases capillary blood flow and raises substrate concentration along the length of the capillaries. Contraction of precapillary sphincters is also inhibited, thereby opening capillaries previously closed or diverting blood from non-exchange to exchange vessels (10, 20). This action distributes the augmented flow over a larger area of capillary surface, diminishes the distance between open capillaries, and therefore diminishes the extent to which substrate concentration falls radially (2, 12, 21). Exactly the same considerations apply to diffusion of products of metabolism in the opposite direction. The adaptive mechanisms acting on resistance and exchange vessels constitute the two phases of metabolic vasodilatation. Both are believed to be produced by the direct action on the specific smooth muscle cells involved of the chemical changes produced within the tissue - either the increased concentration of certain metabolic products or the decreased concentration of certain substrates. What substance or substances are primarily responsible for this control process is not clear.

Another consequence of the negative-feedback control of resistance and exchange functions by chemical changes produced by local metabolism is what has been called metabolic autoregulation. By this is meant intrinsic stabilization of capillary blood flow in certain tissues and organs against variations in perfusion pressure and against extrinsic nervous or hormonal mechanisms which may conflict with local metabolic requirements (5, 9, 22). For example, stimulation of sympathetic vasoconstrictor nerves to contracting skeletal muscles has little or no influence on their blood flow (11). We say as a general rule that where systemic control mechanisms concerned with maintenance of arterial pressure and cardiac output oppose peripheral mechanisms concerned with support of local metabolism, the latter predominate. But this is not always the case - a notable exception is the diving reflex, in which blood flow in the exercising muscles is essentially zero (8). We need to know much more about the limits of metabolic autoregulation, and its specific interactions with other control mechanisms.

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TWO LECTURES ON RENAL PHYSIOLOGY

E. K. MARSHALL, JR.*

In the Spring of 1958, we had the good fortune to have Dr. E. K. Marshall, Jr. visit the department of physiology at the University of Florida College of Medicine. At that time he participated in our course in medical physiology and presented to the first year medical class two lectures on the historical development of renal physiology but interjected many interesting personal comments on a number of well known renal physiologists with whom he worked. Fortunately, we recorded the lectures and since then have played them back to other medical classes. Recently we transcribed and edited the lectures. Since our staff and students have always found these lectures both informative and entertaining, we thought that others might find them of value.

- S. Cassin and B. P. Vogh

LECTURE I

It was mighty fine of Dr. Otis to call me an expert on kidney. I must say, however, that I haven't done any active work on the kidney for 25 years. I have, however, been delighted to be a thorn in the side of the orthodox renal physiologists by going to seminars and meetings and bringing up disagreeable things. So my discussion this morning on the basic developments in renal physiology may be somewhat unorthodox compared to that presented by the orthodox renal physiologists.

The human kidney, as you know, contains small units called nephrons, and each human kidney contains about 1,000,000 of these nephrons. Histologically, one can divide the nephron into different parts. The human and mammalian nephron can be divided into the glomerulus, the so-called proximal tubule with high columnar epithelium and brush border, the thin segment with the so-called Loop of Henle, the distal convoluted tubule and the collecting ducts.

Since a good deal of our information about the mechanism of urine formation has been derived from a study of the amphibian kidney, we may describe the differences in the structure from that of the mammalian, or human kidney. The amphibian kidney consists of nephrons, just as the mammalian or human; but they are somewhat abbreviated in structure. They have a glomerulus, then usually a ciliated neck which does not occur in the mammal. The proximal convoluted tubule is similar in structure to that of the mammal and is followed by another short neck which leads directly to the distal tubule and collecting ducts; there is no Loop of Henle or thin segment.

The physiology of the kidney has been studied mainly from two view points: (1) an attempt to explain the mechanism of the formation of urine, or how blood plasma is converted into a fluid of an entirely

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*Deceased January 1966.

different quantitative composition; (2) a study of how changes in the composition of the plasma produce changes in the composition of the urine, and an attempt to put these changes on a quantitative basis. When we really have the true and final explanation of the mechanism of urine formation, these two points of view, I think, will fuse into one.

Now the physiology of the kidney really starts with the very classic paper of William Bowman, published in 1842 in "Philosophical Transaction," a paper which is almost entirely anatomical, but which did make certain suggestions about function. Bowman was the first to find out the real relation of the glomerulus to the tubule. He studied not only human kidneys, but was very broad in his outlook and studied the kidneys of all sorts of animals from fish to man. He did all of his dissections himself - there were no technicians around in those days - and he did all of his drawings himself, using a camera lucida, and most of them are beautiful. Bowman, after describing the anatomical relations of the glomerulus and tubule, proposed the first theory of urine formation. He asked, "Why should such a beautiful structure like the glomerulus be placed at the head of the tubule unless it were to put out the water and salts of the urine?" And then he suggested that the epithelium of the tubules might secrete "specific constituents of urine" just like the salivary glands, pancreas, or other secretory glands do. He simply made these suggestions in his paper and then left them there because, very shortly afterwards, William Bowman became a very successful ophthalmologist and practiced on Harley Street, and, unfortunately, did no more scientific work.

Now two years after William Bowman, came Carl Ludwig, the German physiologist, and probably the greatest physiologist of all times, one who probably has had more impact on physiology and trained a larger number of people than anyone before or since. Carl Ludwig proposed a somewhat different theory for the formation of urine. Ludwig was particularly anxious to get away from the so-called vitalism of the past and to attempt to explain physiological phenomena in terms of physics and chemistry. In brief, Ludwig proposed that the glomerulus was a filter and that it filtered, by means of the hydrostatic pressure of the blood, all of the constituents of the plasma except proteins, transferring the glomerular capsule, the so-called Bowman's capsule, a de-proteinized plasma. Of course, this was not urine, and Ludwig had to make the further assumption that during its passage down the tubule this filtrate was subjected to the reabsorption of water back into the blood and lymph. Since the differences in chemical composition, quantitatively, between plasma and urine were unknown at that time, Ludwig is not at all specific as to what constituents of urine, if any, went back by reabsorption into the blood and lymph. Ludwig's pupils carried out, in the subsequent years, a great many investigations in an attempt to test this theory of filtration, mainly from the standpoint of changes in blood pressure and blood flow through the kidney.

Several years later, in 1874, another German physiologist, Heidenhain, took up Bowman's suggestion and proposed the theory that came to be known as the Bowman-Heidenhain theory of renal secretion. Heidenhain believed that the glomerulus secreted, that is, it expended energy

in carrying out the work of filtration of water and salt just as Bowman said. "The specific constituents of urine" (just what's meant by that term I've never known) were secreted by the tubules.

Now for a period of about 40 years, really, nothing much occurred of importance in the physiology of the kidney except a wrangle between two opposing theories, the so-called Ludwig theory of filtration and reabsorption and the Bowman-Heidenhain theory of secretion. The workers seemed to be more interested during that period in making some experiments to support their own ideas than in finding out the truth. However, in the middle of that period, in 1896, an English physiologist made a real contribution to the subject. Parenthetically, I might say, that after Heidenhain, all of the individuals I mentioned have been personally known to me, and I've talked with all of them; with some of them I have talked many times upon the subject of renal physiology. Ernest H. Starling, who was a professor of physiology at that time, I think at King's College in London, was the first to recognize that the proteins of the plasma possessed an osmotic pressure. He measured this pressure roughly and then set down the conditions under which filtration could occur at the glomerulus. He said that since the proteins were not filtered (the membrane was impermeable to them) the pressure in the glomerular capillaries had to be higher than the osmotic pressure of the plasma protein in order for filtration to take place. Starling adduced by experiment a good deal of indirect evidence supporting the idea of glomerular filtration.

About 20 years later, in 1917, Arthur Cushny, who was at one time professor of pharmacology in this country, at the University of Michigan, later at the University College of London, and finally at Edinburgh, Scotland, was heard from. Cushny, who had been interested in the kidney and diuretics a large part of his scientific career, at Starling's suggestion prepared as one of a series of monographs on physiology then being edited by Starling, a monograph on the physiology of the kidney. And this marks a very important point in the history of renal physiology. In this monograph Cushny reviewed all of the evidence for the three processes about which there was argument - that is, glomerular filtration, tubular reabsorption, and tubular secretion. Finally, he proposed what he called the "modern theory", which, as he said, was taken from the work of many. But it was stated in such clear-cut and definite terms (the first time anything like that had been done), giving specific substances and what happened to them, that it could be very easily tested experimentally. Cushny also proposed that if people didn't like his theory, not to throw it overboard, but to modify it. However, he had one blind spot. He had absolutely no patience with tubular secretion. That spoke of vitalism. And I'm perfectly certain that if Cushny had lived a few years longer, and I'd been able to show him actual secretion of a dye stuff taking place in the tubule of the fish kidney, he'd have gone to an ophthalmologist and asked what was the matter with his eyes, because he hadn't been seeing so good. But, aside from that blind spot, he did a perfect job in this monograph and set the stage for the really modern developments.

Shortly after Cushny's monograph, prepared in 1920, Newton Richards,

professor at that time of pharmacology at the University of Pennsylvania, started on a series of kidney investigations which occupied his laboratory for about two decades. Richards utilized the amphibian kidney, the frog or the mudpuppy, Necturus, and after some preliminary work was able, by opening up the frogs, to visualize the glomeruli and part of the tubules under the microscope. He was also able, with a micropipette, to draw fluid out of the glomerular capsule, Bowman's capsule, and to subject this tiny drop of fluid (cubic millimeter or less) to quantitative analysis for various constituents of the plasma and urine. This was a perfectly magnificant technical accomplishment. Later this group was also able to puncture the middle or the end of the proximal tubule or the distal tubule. They could also collect urine from the ureter. They could inject substances into tubules, or into the glomerulus and see them go down the tubules if they were visible. With all this technique they got direct evidence supporting the theory of glomerular filtration and tubular reabsorption.

Now this evidence was obtained in the amphibian, and of course the argument immediately was raised that this was a frog and not a man, not even a mammal; so Arthur Walker and co-workers successfully were able to puncture and obtain the glomerular fluid from rats and opossum and also obtained fluid from certain parts of the tubules. These men did very few experiments, and their animals were under highly abnormal conditions (anesthetized, a violent mannitol or glucose diuresis was taking place, and when the respiration interfered with the movements of the open kidney, something was put on the chest to hold it down) and they didn't seem to worry. But, technically, Walker's group performed a superb job, because in the mammal - contrary to the amphibian - the glomeruli are not visible on the surface, and you've got to plunge down for them. But they were able, with the help of Oliver, a pathologist at Long Island College, to later identify exactly where the puncture had been made.

Taking into consideration all of the results obtained on both the amphibian and on the rat and opossum by Walker's group, I think it is fair to say that the following conclusions are justified. First, the capsular fluid (glomerular fluid) is an ultra filtrate of plasma and it agrees with plasma in its osmotic pressure, electrical conductivity, glucose content, pH, sodium content, potassium content, chloride content, phosphate content, urea content, uric acid (that was done in the snake), and creatinine. Now this is about as direct evidence as you can get that the capsular fluid is an ultra filtrate of plasma. Whether or not you call the production of capsular fluid a filtration, or as my friend Francis Chinard wants to do, call it a diffusion phenomena, I think is of no practical importance. The fluid is, essentially, in composition, an ultra filtrate of plasma. Secondly, from this work we can conclude that glucose and phosphate are reabsorbed by the proximal tubule. That's proven by puncturing the proximal tubule at its lower end, and finding that glucose and phosphate which were present in a capsular fluid at the same composition as plasma have disappeared, or nearly disappeared from the tubules. Thirdly, we can say that sodium and chloride are reabsorbed in the proximal tubule, but the tubular fluid remains isotonic with plasma. Apparently, the fluid leaving the end of the proximal tubule,

despite the fact that it has changed in composition from plasma, is still isotonic with plasma. Fourthly, by very beautiful experiments, it was shown that urine is modified in a sharply localized portion of the distal tubule. That was shown first by Richards, himself, by injecting an indicator, phenol red, into the glomerular capsule and watching it course down the tubule; it did not become an acid yellow color until it reached a certain definite segment of the distal tubule. Later on, Walker and Botts, I believe, were able with microelectrodes to actually measure pH at different points in the tubule. Lastly, it has been known for a considerable time that NH_3 was synthesized by the kidney (this had been proved by determining the ammonia content of renal arterial and venous blood). From the amphibian experiments it was shown that the NH_3 was synthesized and secreted by the distal tubule.

The next step that occurred in the physiology of the kidney, historically, was work done in my own laboratory, which, I think, is recognized now as the first conclusive proof that the tubule could secrete. Then, came a very important bit of work in 1932 by Robert Chambers which is still very interesting, and some of it unexplained, in which he was able to grow in tissue culture a portion of mesonphros of the chick, or the metanephros, the final kidney of a three and a half month old human embryo, and was able there to see the secretion of a dye, (I'll come to the details of proof of secretion a little later on). The next point in the story was an attempt to quantitatively measure glomerular filtrate and to have a quantitative tool for studying renal function. The work really started with the Dane, Rehberg in 1926. Although Rehberg published his first paper in 1926, I happened to see a great deal of him in August Krogh's laboratory the summer of 1923 when I was there; in fact, I used to eat lunch with him every day, the two of us alone in the laboratory. At that time, he had very definite theories about measuring glomerular filtrate, and about what the kidney did, but he had not yet done any experiments. His was a very curious approach. He read for a year and and evolved a theory and then set out perfectly fairly, I think, to test it out. Finally, comes one who has done a great deal on the kidney and also written largely upon it. I am talking about Homer Smith, who did his first research work with me during the First World War in the toxicology of gases. Smith came into kidney work just at the time I left it, about 1931. He has done more, I think, than anyone else to try to put everything on a quantitative basis.

We have taken up, in discussing Richard's work, the evidence that the glomerulus is a filter and that the glomerular fluid is an ultra filtrate. It is now quite obvious, if one compares the relative composition of urine and plasma, that if the glomerulus makes an ultra filtrate, the reabsorption of certain substances must take place through the tubule. In the first place, the reabsorption of water has got to occur in order to concentrate certain substances. The urine is usually free of glucose and frequently contains only traces of chloride, so that glucose and chloride must be reabsorbed through the tubule. In admitting glomerular filtration, we have to admit reabsorption. And in addition to this, as I pointed out, the micropuncture work by Richard's group at Pennsylvania provided direct evidence that reabsorption occurs and is localized to certain parts of the nephrons. The last problem that remained was to

decide whether filtration and reabsorption were sufficient to explain urine formation. Or did one, in addition, have to bring in this annoying process (to some people) of secretion by the renal tubule. I had started some work back in 1916 during World War I which I took up again after the war was over (Cushny's monograph had come out during the war). The results could not be explained, I believed, by filtration and reabsorption alone. However, I had no real conclusive proof and started in again in an attempt to find out whether one could obtain conclusive evidence of secretion of any substance by the renal tubule. During this early period of my work (in the early 1920's) both Cushny (who died in 1926) and Newton Richards, were violently opposed to secretion and would scarcely listen to any arguments in favor of it. In 1923, with Vickers, I carried out some experiments using phenol red, an indicator which already had been used for many years as a test for renal function. And, realizing that phenol red was very rapidly excreted by the kidney and that it was highly concentrated in urine as compared to the plasma, I thought that it was a likely substance to test the theory of tubular secretion. In addition, due to its brilliant color, it could be easily determined in the urine and blood and plasma. It had been known since Ludwig's time that if you cut the spinal cord of a dog and then pithed a good bit of the cord to reduce the blood pressure to about 38 to 40 millimeters of mercury, urine would not be formed. That was one of Ludwig's arguments for filtration - that the pressure at which urine was no longer made was about equal to the osmotic pressure of the plasma colloid. If we injected phenol red intravenously into such an animal, we would find that even though urine is not produced, the cells of the proximal convoluted tubules had accumulated the phenol red. It's interesting that everybody thought later that I was seeing things because in its ordinary secretion, phenol red is not accumulated. However, in the last few years, there's been plenty of evidence to show that I saw correctly. We were able to show also that a large part of the phenol red was adsorbed on the plasma proteins; about 75% of the total amount was being lost in this way. The phenol red attached to the proteins would not pass through collodion filter at normal blood pressure and, hence, would probably not pass through the glomerular capsule. I soon found by determining it in both the urine and plasma that the amount of phenol red in the urine was so great that, assuming a certain maximum blood flow for the kidney (we had no measurements at that time) it could not be accounted for even if all the plasma were filtered into the glomerulus. Now the argument for secretion of phenol red was concluded except for the weak point that there was no direct determination of the blood flow through the kidney; that came later.

A year later, Marion Crane and myself were able to put together another important piece of evidence which has been widely used by Smith and his colleagues in testing for secretion. We took and injected into dogs a mixture of phenol red and urea, and gradually gave further and further injections to increase the concentrations of both substances in plasma. If the concentration in plasma was plotted against the rate of excretion (milligrams per minute) for urea there was essentially a straight line, exactly what would be predicted if the substance were purely filtered. If it was purely filtered, no matter how much the concentration in the filtrate would increase, if it wasn't reabsorbed you'd get a straight line of proportionality. With phenol red, however, we

got a curve that went up to a maximum and then flattened off. We interpreted this, correctly as we know now, to mean that the tubules could handle only a certain amount of phenol red per minute and beyond this their secretory capacity was saturated. The slight rise represented the amount of phenol red being filtered by the glomerulus. These observations are really the basis of Smith's so-called Tm, or maximum secretory or reabsorptive capacity of the tubule.

Even after several years, Cushny still wouldn't believe anything about secretion; in fact, in the second edition of his book published posthumously he dismissed the evidence by saying we sort of didn't know what we were doing and our results were of no particular importance. Richards, who was still living, fortunately discussed things with me many times. I remember his spending a whole day with me one time (without any lunch) in Pennsylvania, when I dropped off on a trip north. We finally got to doing experiments on frogs. "Instead of talking," he said, "let's demonstrate." We spent the whole day, but he wasn't convinced at that time. The final experiment, I think, that convinced Richards was the following one. We injected phenol red into an anesthetized dog, and then obtained both arterial blood which would represent the renal artery blood (all arterial blood being the same composition) and renal venous blood. Now, to get blood from the renal veins I had known from some previous work you have to be careful because if you stimulate the renal artery you get certain reflexes occurring that makes a mess of the whole process of urine formation. So we adopted the idea of not exposing the renal artery, but instead threaded our needle through the spermatic ovarian vein which, on the left, empties into the renal vein and frequently is without a valve. Tying off below the point of insertion, we could draw blood from the renal vein if we pulled very slowly with a syringe so as not to draw any blood back from the vena cava. In this way we could compare the concentration of phenol red in renal artery blood and renal vein blood. We found that under these conditions, 70% of the phenol red might be removed in one passage through the kidney. That's what Smith came later to call the extraction ratio. Now, since only 25% of the phenol red could be filtered, if all the plasma were filtered (which is an absurdity), it was quite obvious that some of the phenol red had to be secreted by the tubule. A few years later, an English pathologist, Harold Sheehan, became interested in secretion and came to my laboratory where he confirmed all these experiments, using the Van Slyke-Allen technique of pulling the kidney out and transplanting it under the skin where you could get into the renal vein by puncturing it through the skin of unanesthetized dogs. He was quite an expert at it. And in addition, he also determined the excretion of phenol red so he could show that the phenol red had been removed from the arterial blood and, passing through the kidney, had been secreted in the urine. He obtained extraction ratios in the unanesthetized dog about the same as ours, around 65 - 70%. Richards never told me at the time, but one of his workers, Joe Hermann, a year later - two months after the paper describing these experiments was published - shook his head and said "you got the old chief; he doesn't talk to me on secretion anymore".

Before we leave the subject of phenol red secretion, I want to come back to Chambers' experiment with the tissue culture of the chick mes-

onephros, and the embryonic human kidney. When Chambers grew these fragments in tissue culture the proximal tubules would seal off at each end to form a sort of cyst. Then that cyst would get larger and larger as if fluid was going into it. If Chambers put some phenol red in the fluid in which these fragments were growing, very shortly you could see that it was highly concentrated in the lumen of these cysts formed by the proximal tubule although it was too dilute to see in the surrounding fluid. The distal tubules did not show this. Now that's a visible demonstration of the secretion of phenol red. You could watch it right under a microscope. The interesting thing to me there - and what I'm afraid annoys the orthodox physiologists - is the fact that while the phenol red is being secreted these cysts blow up. Certainly the fluid inside is increasing, and that certainly looks like the secretion of water. Of course, to say the tubules secrete water just wrecks everybody's beautiful quantitative calculations which are carried out to the second decimal place.

Roy Forster, biologist of Dartmouth College, who goes up to a little laboratory in Maine in the summer, where I also spent many years, found that if you took fish kidney of various fish (bony fish) and simply took out a piece and just shook it a little bit you could partly separate the tubules. If you put those tubules in a little dish of Ringer solution with a little phenol red around them, and oxygenated them, pretty soon you could see phenol red being taken from the fluid and secreted into the lumen of the tubules. Actually, by measurement, they found that the phenol red might be concentrated about 4000 times in the lumen over what it was in the fluid.

Another thing that interested me particularly was this: you may recall I stated that in our initial experiments with phenol red in the dog we found that when urine secretion was stopped by cutting and pithing the spinal cord getting the blood pressure between 30 and 40 mm Hg, the phenol red accumulated in the proximal tubule. Now, obviously during the normal process of secretion this does not happen because you can't see it either in Chambers' preparation or the tubules of the frog unless you lower the potassium content of the Ringer solution that the tubules are in, or lower the calcium and raise the potassium content. Then the phenol red doesn't go in the lumen at all, but goes and piles up in the cell. In other words, the process of secretion of phenol red is a two-step process. The first step is the entry into the renal cell, and the second step the extrusion from the renal cell into the tubular lumen. Ordinarily both processes take place so fast that you do not see any accumulation in renal cells. So much then, for the secretion of phenol red. We were able to show that, in the frog, urea had the same type of curve as phenol red when plasma concentration was plotted against rate of excretion. Crane and I were able to show that under certain conditions, urea was concentrated in the proximal convoluted tubule. We then suggested that urea is secreted by the frog proximal tubule. The suggestion that urea is secreted by the frog and by no other animal in the world horrified everyone, but that's pretty nearly true. It isn't even secreted by the mudpuppy, Necturus, another amphibian. Nevertheless, that has been followed up since my quantitative methods and Roy Forster again has done a lot of work on it. Now there is no question, for even Walker and

Richards' group finally obtained about 1937, by the puncture method, evidence that urea was secreted. That is important because urea is a normally occurring substance (of course it is in the frog). The work of two of Cushny's students, Mayrs and Gibbs, presented pretty conclusive evidence that uric acid was secreted in the chicken. You see, in the chicken and birds, uric acid is the end product of protein metabolism and not urea. Mayrs and Gibbs found that the amount of uric acid coming out in the chicken's urine under certain conditions was far more than could be accounted for if all the plasma were filtered. They actually measured the blood flow through the kidney by mechanical means.

Further evidence for secretion was obtained in 1926 or 1927 in our laboratory, or rather in the Marine Lab in Maine, that was pretty annoying to most of the opponents of secretion. I ran across, in the Spring of 1926, browsing over German handbooks on comparative physiology and histology, a description by a Frenchman named Huot (1902), claiming that fish had kidneys with no glomeruli. What he described was just a blind tubule. In 1910 or 1912 another Frenchman had described a lot of other fish, and stated that their kidneys didn't have an arterial supply of blood; this meant there could be no filtration pressure. Well, of course, this rather excited me. I knew nothing about fish, so I went around inquiring where I could get some of these fish. Some of them were small, like little pipe fish you've seen in the aquarium, or the little sea horse, which are a little bit too small to work with. But there was one atrocious looking brute called the goosefish (because it could swallow a whole goose). It even had a little wen on its head which hangs over and jiggles to attract fish and snap them up. But its very beautiful in that it does have an aglomerular kidney. Grafflin went up to Maine with me the second summer of the goosefish experiments, and we immediately got into trouble because, in serially sectioning the kidney, we found there were some structures which looked exactly like glomeruli. It was quite obvious that there might be in the whole kidney 25 glomeruli, although maybe 200,000 tubules, but that was awfully annoying until Grafflin did a very painstaking piece of work and showed that these glomeruli in the Lophius (the goosefish) kidney were disconnected from the tubule. They were just blind glomeruli. We worked with secretion in the goosefish tubule and then the joke came. After carrying apparatus and going up in the wilds of Maine to study this fish which was difficult to obtain, I found, by taking a suggestion of Homer Smith, that the aglomerular fish may come in one family or group, that there was a fish in my back yard in Baltimore which was completely aglomerular and much easier to work with, the ordinary toadfish. For a time I got toadfish and during the winter kept them in artificial sea water in the laboratory. The aglomerular fish kidney tubule starts blind, and, histologically, its epithelium resembles the proximal segment of other kidneys. There are collecting ducts, of course. Grafflin made wax reconstructions of the toadfish tubules, so we could have the whole structure well worked out. Now, what happens is that the aglomerular kidney can secrete water. It can secrete practically all of the constituents of the urine, and a large number of foreign substances. It concentrates phenol red. The only things known, so far, that are not secreted are glucose and other sugars (mainly known as non-metabolized sugars). Glucose would not be secreted under any conditions. Curiously enough, ferrocyanide is not secreted.

That was found by accident, as most of these discoveries are. One dye we worked with, called cyanol, was not secreted. Those were the only three substances we found that were not secreted.

Bieter (who is now Professor of Pharmacology at Minnesota; and worked a year or two with me) showed that in the toadfish the ureteral pressure could exceed the arterial blood pressure, which, of course, is Ludwig's old experiment proving secretion of the salivary glands. We frequently demonstrated it to students with the submaxillary gland. Bieter's work is particularly important because the toadfish kidney has no arterial supply - so that the ureteral pressure is way higher than the pressure of the blood in the kidney. That should eliminate any advocate of non-secretion, which it did. They saw that all the fluid was filtered across the top of the tubule and reabsorbed at the bottom. So that it was pretty clear evidence with the aglomerular kidney that secretion could occur; and also it tends very strongly, it seems to me, to localize secretion to the proximal convoluted tubule.

Now, again, I may say that taking the aglomerular kidney in connection with Chambers' experiments on transplants with human and chicken kidney, I would still like to ask the orthodox physiologist why can not the proximal tubule in the mammal or man secrete fluid? I don't know the answer. That is the picture up to a certain point. I think that all workers in renal physiology today would agree that there is strong evidence, to say the least, that the mechanism of urine formation consists initially in a ultrafilteration of de-proteinized plasma at the glomerulus; then water and certain constituents are reabsorbed in passing down the proximal tubule, but that the fluid in the proximal tubules remains isotonic. Further on, either in the Loop of Henle or the thin segment or the distal segment, other things occur. Secretion, aside from reabsorption, may occur in the proximal segment and there is some evidence that secretion of normally occurring substances occur in certain species. I think that part has been clearly recognized.

The rest of the story consists primarily in an attempt to put this qualitative conception that I presented on a more or less quantitative basis. Obviously if you can determine the magnitude of the glomerular filtrate, and if you know the composition of urine, you can make some beautiful calculations about secretion and about reabsorption, and put them in quantitative language, with only two assumptions. One assumption is that there's a one-way passage. Things are not, of course, secreted and reabsorbed. The second is that no water enters through the proximal tubule cells. We'll discuss that quantitation next time. In conclusion, I'd like to emphasize to you the importance of reading up carefully in your laboratory manual the experiment on renal function which you're going to carry out in the laboratory. It is highly important to read up a bit before you go in to carry out an experiment.

LECTURE II

After it had been established, as we pointed out in the last lecture, that qualitatively the formation of urine consisted of glomerular filtration, tubular reabsorption and tubular secretion, it became important

to try to quantitate these three parts of the urinary processes.

Now, the first stage in attempting to quantitate them was to find a method of measuring the quantity of the glomerular filtrate. Obviously if one found a substance which was completely filtered at the glomerulus, which was neither reabsorbed nor secreted by the tubule - one would have a means of measuring the glomerular filtrate. This is done by taking the so-called clearance of the substance. The clearance or renal clearance of the substance is simply the volume of plasma which must be completely freed of a clearance substance in one minute to produce the amount that is excreted in the urine in one minute. In other words, if we let U represent the concentration of any substance in the urine expressed as milligrams/cc. and let P represent the concentration of the same substance in the plasma expressed in milligrams/cc., and V represent the urine volume; the clearance of the substance in ml/min simply becomes $\frac{U}{P} \cdot V$. Now, with a substance which is completely filterable - yet neither reabsorbed nor secreted by the renal tubule - its clearance will measure the glomerular filtrate.

The first attempt to measure the glomerular filtrate quantitatively, was carried out in 1922 by Mayrs, the well-known Irishman I spoke to you about, at Cushny's laboratory in Edinburgh. Mayrs injected into rabbits creatinine, sodium sulfate, sodium phosphate and urea. He didn't inject all four into the same rabbit but did it more or less in pairs. And he found that the concentration ratio, or if you want to put it another way, the clearance of creatinine, sulfate and phosphate were essentially the same within the experimental error, whereas, that of urea was considerably less. Mayrs realized that if these three substances had the same concentration ratio or clearance that they were probably not reabsorbed; and hence he calculated from them the amount of glomerular filtrate. He showed a large amount of urea must have been reabsorbed. Mayrs went no further with the proposition and then some years later the Danish investigator Rehberg, simply on theoretical grounds, decided that since creatinine was the substance in plasma and urine which was concentrated to the greatest extent, that he would take creatinine as a measure of the glomerular filtrate.

On account of the small amount of endogenous creatinine in plasma, Rehberg loaded the animal or man with creatinine in order to raise the concentration in plasma. Now Rehberg carried out a number of experiments, using creatinine to measure the glomerular filtrate and made various calculations about the amounts of different substances that were reabsorbed. As we will see in a moment, this guess, if you want to put it that way, of Rehberg's was pretty good because creatinine to the best of our knowledge today does measure the glomerular filtrate in a large number of animals, but apparently does not in man, as a portion of the creatinine seems to be secreted by the tubules.

Now, the next step in quantitating the glomerular filtrate came from Homer Smith and Shannon, who, starting from the premise that if a substance was not secreted by the aglomerular fish kidney, it would not be secreted by the tubules of the higher mammals. Starting with the observation which we had made that the aglomerular kidney could not secrete

glucose, they tested the ability of the aglomerular kidney to secrete non-metabolized sugars such as xylose, sucrose and raffinose. Since the aglomerular kidneys did not excrete these sugars in fact, Smith and Shannon then attempted to measure the glomerular filtrate with xylose. They checked the question of whether the xylose (any of it) was reabsorbed by using three sugars of quite different molecular weights, xylose, sucrose, and raffinose. And the clearances of these three substances checked almost exactly in dog and man. About this time, both Shannon, in Smith's laboratory, and Newton Richards, in Philadelphia, decided that the most satisfactory substance for measuring the glomerular filtrate was a high molecular weight polysaccharide, inulin. Inulin is obtained from the root of the artichoke, and it can be dissolved in warm water, and doesn't crystallize when you cool it down. It is apparently quite inert pharmacologically. It is almost completely excreted by the kidney; you can recover way over 90% in urine, after an injection of inulin. Now the inulin clearance was definitely some 20% or so higher than the clearance of xylose. Consequently, since no inulin was secreted, the xylose did not measure the glomerular filtrate. Now in a very few years it was found that some of the xylose, sucrose, and raffinose was reabsorbed. And the reason the clearances checked is that the reabsorption of these three sugars takes place by the same mechanism as the reabsorption of glucose.

Inulin is now accepted, generally, as a measure of the glomerular filtrate in all species of animals. As I said before, and will mention later, creatinine will measure it in certain animals, and is much easier to determine analytically than is inulin. But so far we can say from the evidence that inulin does measure the glomerular filtrate. In the first place, in both the frog and Necturus, inulin in the glomerular fluid has the same concentration as is in plasma; so we can conclude that it is completely filterable at the glomerulus and that is the first criteria for measuring the formation of a glomerular filtrate. Secondly, it is not secreted by the aglomerular kidney which would suggest strongly that it is not secreted by the tubules of the higher animals. Richards gave a very beautiful demonstration that it was not secreted by the dog kidney. He prepared a dog, as I had done years before, and as I described to you before, by cutting the spinal cord and pithing it so that the dog was not making urine, and injecting a mixture of inulin and phenol red, (at that time he had accepted the fact that phenol red was secreted) and then after a few circulations of blood through the kidney, he perfused the kidney to raise the glomerular pressure and found in the fluid coming from the ureter there was considerable phenol red but no inulin. That, I think, with the evidence, on the aglomerular kidney, is pretty good evidence that inulin is not secreted by the tubules. Now, thirdly, we have to consider the question of whether any inulin is reabsorbed or back-diffuses. It has a molecular weight of about 5000 (so one wouldn't expect it to diffuse too readily), but since we do not know everything about the renal mechanism, there is some possibility that it might in some way be actively reabsorbed. That point has to be answered by more or less indirect evidence. It is best answered in this way: if we compare the inulin-creatinine clearance, that is, the ratio of inulin clearance over creatine clearance, we find that it is equal to 1 in the dog, in the rabbit, and in the frog. But the inulin-creatinine ratio is less than one in certain fish, chicken, and man. Now

that is taken to mean that in the dog, rabbit, and frog both inulin or creatine will measure glomerular filtrate; whereas in the other animals mentioned some creatinine is secreted. In addition to this, in the dog we find that other inert substances such as mannitol and sorbitol have approximately the same clearance as inulin. Also, in the dog, ferrocyanide (which you may recall is one of the three or four substances secreted by aglomerular fish) has a clearance identical with creatinine. This evidence of a large number of substances getting exactly the same clearance, makes it highly unlikely that they would all be reabsorbed to the same extent. Now, you may come back and say, "How about xylose, sucrose and raffinose?" But there we now know they are reabsorbed by an active mechanism present in the reabsorption of glucose, but when you have things as varied as inulin, creatinine and ferrocyanide - of markedly different chemical structure - it is highly unlikely that they should be reabsorbed.

Now there is one question on the use of inulin as a measure of glomerular filtrate which has never been answered. That is this: in general, if you have a substance which is secreted and whose clearance is greater than that of inulin, if you raise the plasma concentration of that substance, you come to a point where you have saturated the tubules, as with phenol red before, and then its clearance will come down practically to that of inulin. Now, if you do that in man, where creatinine is supposed to be secreted, you find that as you raise the creatinine in plasma sufficiently, the creatinine clearance comes very close to the inulin clearance. But the joker is when the creatinine concentration falls, the creatinine clearance doesn't rise as it should. Now that has been brushed aside. I give it to you for what it is worth. But for the moment we can accept inulin as measuring the glomerular filtrate in all animals and creatinine, a much easier substance to work with and determine, as measuring it in certain animals, but not man.

Now we can make various quantitative calculations, if we make the assumption that there is only a one-way passage of substances (in other words, a substance is either reabsorbed or secreted, but not both). We can easily then calculate the glomerular filtrate, and calculate the amount of substances reabsorbed, and tell whether at any point the substance is secreted. If the clearance of the substance exceeds that of inulin, then the substance must be secreted by the tubules.

In addition to measuring the glomerular filtrate, we can make another measurement which is of very great importance in renal hemodynamics, and that is we can measure the renal blood flow. That is done in this way: you recall that in the last lecture we pointed out that with phenol red 70% might be extracted in one passage through the kidney. Now there are other substances that are extracted in one passage to a greater extent. And there are several of these, certain organic iodine compounds that are used in roentgenography, diodrast and hippuran. About the simplest and easiest to determine is paraminohippuric acid, usually known as PAH. Paraminohippuric acid is extracted (as we know by determining the amount of arterial blood and renal vein blood) on an average of about 91% in one passage. Now, we consider the fact that blood flows through certain parts of the kidney which do not contain tubules, connecting

tissue and capsule. This means that actually the excretion of the amount of this substance that reaches the tubules is essentially complete. Now, all we have to do is to determine the clearance of PAH, and since PAH is present in plasma, that clearance will give us the renal plasma flow. If we can then determine the relative portion of red cells in plasma, we can divide the clearance by one minus the hematocrit ratio and obtain the renal blood flow.

These measurements lead us to the following rather startling (some of them, at least, I think) conclusions. In the first place, in man the average glomerular filtrate is about 127 ml/min; that is, the glomerular clearance. Now if we take this figure for glomerular filtration rate we find that it means 183 liters of fluid are filtered per day. But of this 183 liters, 181.5 of water, 1100 gm sodium chloride, 410 gm of sodium bicarbonate and 150 gm of glucose are reabsorbed by the tubules. Now, that looks astounding but let's just show it in another way. If we take this 181 liters, and find out how much is acted on by each nephron and divide by two million, you will find that each nephron reabsorbs only about 0.09 ml/day, and that certainly doesn't seem to be too much of a task. So, if you look at the two million units there, it makes a difference. Now, if we multiply the PAH clearance (assume the hemotocrit is 50) by 2 we get a renal blood flow of 1400 ml/min, or roughly, about one-quarter of the cardiac output. This shows the enormous blood flow that is going through the kidney.

Another thing of importance which is frequently determined is the maximal rate of tubular secretion. This gets right back to the old phenol red secretion. At certain concentrations the tubules become saturated and then the only additional phenol red from raising concentration is put out by the glomerulus. Now the maximal rate of tubular excretion has been designated as Tm. (The orthodox renal physiologist have a language all their own and when they write a paper they never explain anything and if you don't know it you had just better not even attempt to read it). So you have the Tm as they call it, and then if you want to identify the substance you put another little subscript there and have, for example, Tm_{PAH}. If you want to determine the Tm_{PAH} what you do is first determine the renal plasma flow using a fairly low concentration of PAH. Then you raise the concentration of PAH and determine several more clearances at higher levels, then calculate the amount that is secreted by the tubules, which is very simple. You simply take the amount of PAH filtered by the glomerulus which you can do very easily by knowing the glomerular filtration rate and the plasma concentration and subtract it from the amount coming out in the urine each minute. When the difference you find becomes constant, so many mg/min, that is the Tm. In other words, expressed in mg/min, it simply means that any particular kidneys will secrete so many mg of PAH/min, and that is their maximum secretory capacity. Now that has been, or was proposed when it was first put out as a measure of effective tubular mass. But it has been found that the injection of certain substances such as acetate may increase the Tm as much as 60 or 70%, and if the Tm is going to vary around like that, depending on the amount of acetate available to the tubules, it can't measure accurately the effective tubular mass.

Another thing which is important in tubular secretion is competition between various substances. There are a number of substances PAH, phenol red, penicillin, and quite a number more - diodrast, etc. - which are apparently secreted by the same mechanism. If you inject two of these substances together one will cut down the excretion of the other. In other words, they will compete for the mechanism. Now it is of some importance to realize that there is also, apparently, a second mechanism of secretion because quaternary bases and particularly n-methyl nicotinamide are not influenced competitively by these other substances and apparently are secreted by a different mechanism. We should also recall the fact we already have pointed out, that this secretory process in the tubules apparently takes place in two steps - the entrance of the subatance into the cell and the extrusion of the substance into the lumen.

There is still another interesting thing about tubular secretion. Certain substances which are not secreted by the tubules can block the secretion of substances which are. In some way they get in and jam the mechanism without going through themselves. There were a number of these substances sought for in the early days when penicillin was extremely scarce. I mentioned that penicillin is secreted very rapidly and is probably completely extracted in the kidney in one passage. If you could get a substance that would knock out its secretion, penicillin would stay in the blood much longer. A sulfonamide, the structure of which I'll not give you, which is known as Benemid ^{R} is probably the most potent of these substances, and this will block the secretion of all the substances that follow the phenol red PAH type of excretion. Now it is also interesting and I think very disturbing that Benemid ^{R} will also block secretion of urea in the frog. For many years now people have been working and claiming that they could tell just what structure was necessary to be secreted by the tubules - said it had to be a strong carboxylic acid or what not and this urea thing just disturbed them completely, and they just brushed it aside. Urea is an entirely different substance from these strong acids with certain definite pK's.

Now similar to having maximum rate of secretion by the tubule, we have a maximum rate of reabsorption which can be best illustrated by glucose. Here again, in the terminology of the renal physiologist, we shall call it the Tm of the glucose. It has been known for a long, long time that there is a so-called threshold in the excretion of glucose. In other words, normally, with the glucose concentration of the blood more or less normal, you obtain very little, if any, glucose in the urine. But if the concentration in the blood is raised by any means, by disease, or by injecting large amounts of glucose, or taking glucose, you find that the glucose appears in large quantities in the urine. One determines the amount of glucose in mg/min which is reabsorbed by simply determining the glomerular filtration rate, the concentration in the plasma and the amount of glucose that comes out in the urine. With these measurements and a few simple calculations, you find first that all of the glucose is reabsorbed, none appears in the urine if the amount in plasma is normal. But when you get up to the Tm, the maximum rate of reabsorption stays constant in the tubule even if you keep on raising the concentration in the plasma. This, of course, explains very beautifully the

so-called glucose threshold because it simply means that until the glucose concentration of the blood rises to such a point where the load, the amount in glomerular filtrate being presented to the tubules, is insifficient to saturate the reabsorptive mechanism - no glucose passes on into the urine and as soon as that load becomes greater than the maximum reabsorptive capacity the excess passes into the urine.

The Tm or maximum reabsorption of glucose has been used to settle one very much disputed question which dates back to Newton Richards' early work in the early 1920's on the frog's kidney. When he first visualized the frog's kidney under the microscope, before he made the capsule punctures which we described, he noticed that all the glomeruli were not active at the same time; some were completely quiescent and others were extremely active or were only partially active and the blood was apparently flowing in only certain capillaries of the loops. He studied the effects of various substances in increasing or decreasing the number of glomeruli that were active. Now this was believed to apply to mammals at first. Then there were some experiments made by injection of Janus green, a dye which stains the glomeruli (in Richards' laboratory in rabbits), and for some time quite a dispute went on as to whether this occurred in mammals. You can see obviously that if all nephrons are not functioning at the same time the calculations and interpretations are going to be very much complicated, much more than if they are all functioning at once. Now we can partly answer the thing by using the Tm or maximum reabsorptive capacity of glucose and assume that if we do something which would increase the blood flow to the kidney and which certainly should open up glomeruli if any are closed, then, the Tm or glucose ought to change. However, with hyperemia or increased blood flow to the kidney, the Tm of glucose remains the same. Or if we do something that will decrease active glomeruli like injecting a drug like epinephrine, the Tm ought to decrease, but we find it remains the same. There is other evidence that I can't go into which indicates that possibly all the glomeruli are active and all the nephrons are functioning in the mammalian kidney. Even if we go back to the frog's kidney it is possible or probable that in any normal frog that is properly hydrated all the glomeruli are working and it is by vasoconstriction or various things, shock or whatnot, that they close off. We may even say that although Richards' observations are quite accurate and can be easily confirmed (I've done it myself), it is probable that he was working under conditions that are quite different from that of a normal frog that is hopping around and jumping around.

Just as we can have competition in the secretion of substances that go by the same mechanism (phenol red, PAH, penicillin) we can have competition in reabsorption. Remember, I mentioned xylose and the trouble that Smith and Shannon got into in trying to measure the glomerular filtrate using xylose. Since xylose apparently is reabsorbed by the same mechanism as glucose, if you give large doses of glucose so as to produce quite a glycosuria you will find that you can abolish the reabsorption of xylose. In other words, if you are measuring glomerular filtrate, let's say with inulin in man or creatinine in the dog, and give you xylose, you find that the xylose clearance is about 80% roughly of the glomerular filtrate or creatinine or inulin clearance. But now if you give a large

amount of glucose so that it comes through in the urine then you find the xylose clearance approaches and may reach the inulin or creatinine clearance because of true competition with the reabsorption mechanism for xylose.

Urea is a very interesting substance - it is the end product of protein metabolism in the mammals, occurs in large amounts in the urine as the main nitrogenous constituent and has been studied for a long time. Urea was one of the first substances for which the term clearance or renal clearance was employed, and the clearance of urea is still used - clinically as a test of the efficiency of renal function. Rehberg, in his original work, saw that urea was reabsorbed, but proposed the idea which has been completely and widely accepted since (although not much credit is given to Rehberg) that urea is not reabsorbed by an active mechanism such as glucose and xylose, but that the reabsorption or urea is simply a back-diffusion. The simple facts that the tubular membranes are not completely impermeable to it and that it is such a small molecule and such a readily diffusable substance support the idea that it simply passes back by diffusion. There have been a great many studies giving indirect evidence tending to support this conception.

Quite recently Dr. Bodil Schmidt-Nielsen who was at Duke University has rather thrown a monkey wrench in the works. She went out to the Sahara Desert one time, and also went to the Western Desert of Arizona here and worked with two very interesting animals, the camel and the desert rat. Let's take the camel first which as you know, can go without water for days, and days and days. That was the main thing that she went to study, but she found that in the camel, if it was on a high-protein diet, the amount of urea in the urine was about 40% of that put out by the glomerular filtrate (it is 60% in man). But if you put this camel on a low-protein diet, then the amount of urea in the urine was only 1% of the amount in the glomerular filtrate. Now, by no stretch of the imagination, I think, can you consider that back diffusion could take care of all but 1%. There must be an active reabsorption of the urea in the camel. The desert rat, which is an animal that can live on completely dry food for days and days, puts out tremendously concentrated urine which may run up to 25% urea, just enormous. Dr. Schmidt-Nielsen has some evidence (I wouldn't accept it completely yet) that the clearance of urea in the desert rat is greater than the inulin clearance. That, of course, would mean that urea is secreted by the desert rat. She also finds the same phenomenon in ordinary laboratory rats if they are put for a long time on urea, or a high protein diet. Now this isn't so wild as it may sound because we must remember, looking back at the lower animals, that the elasmobranch fishes- those are the cartilagenous fishes, the ones without the bones, the sharks, dogfishes and that type, contain about 2% of urea in the blood instead of about 0.04% as in mammals. The urea is used to make the osmotic pressure of blood identical with sea-water. In the elasmobranch fishes a quantitative study of glomerular filtration rate and the urea excretion shows very clearly that there urea is actively reabsorbed. Then you can go back to the frog where there is perfectly good evidence now, as much evidence as in anything else, that urea is secreted. So, maybe the picture is not quite as simple as we thought.

I want to mention the excretion of one other substance because it, I think, is very annoying to certain people, that is, the excretion of potassium. Potassium usually occurs in the urine in amounts which would represent maybe 10 or 20% of that in the glomerular filtrate. This means that maybe 90% of the potassium that is filtered has been reabsorbed in the tubules. Now some few years ago two groups of investigators simultaneously and independently were able to show that under certain conditions in the dog, particularly where they had given large amounts of potassium or some of the mercurial diuretics, that the potassium clearance was greater than the creatinine clearance. In other words, the potassium clearance was greater than the clearance which measures the glomerular filtration rate. And sometimes it might be twice as great - way out of any slight errors. Now the only possible interpretation of that, if you are going to accept the general scheme which we have been talking about, is that potassium under certain conditions is secreted by the tubules. In other words, here now we have the double passage - it isn't a one-way traffic; it's going both ways. It is supposed now that practically all the potassium that is present in the glomerular filtrate is reabsorbed in the proximal tubules and that the potassium which comes out in the urine has been secreted by the distal tubules, apparently as an ion exchange of potassium for sodium in the urine or filtrate. This mechanism, if applied to other substances, would make a good deal of the calculations completely worthless. We wouldn't know quite what was happening. Whether or not it does apply to other substances we do not know, but I'd like to point out, in concluding, that renal physiology is not static. A good many people are calculating now to the first and second decimal place, which you do only when you think a thing is static, but there is still an opportunity for finding out things which don't check and for modifying theory. No theory is final, and these that I have given you will probably have to be modified, but I think we have presented a fairly good picture of what the general process is with pretty good evidence to support it.

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SOME DATA ON STATUS AND TRENDS IN PHYSIOLOGY

L. M. N. BACH

Some recent publications provide data of interest with respect to graduate training and salary levels among various departments of physiology and biology. These data have been extracted and consolidated in Table 1 which permits comparisons among institutions of opinions about quality of faculty, effectiveness of graduate training programs, numbers of enrolled graduate students and salary levels for each academic rank in institutions where training in physiology is provided. More specific details concerning trends in the number of graduate students are presented in Table 2 and for salary data in Tables 3A and 3B.

Data relevant to the quality of graduate faculty and the effectiveness of graduate training programs in physiology were taken from the recent report by Cartter on the quality of graduate education[1]. Physiology was one of 29 academic fields surveyed in this study and was rated in 71 of the 106 institutions responsible for more than 95% of the doctorates granted; 725 physiology doctorates were granted during the period 1953-1962. Questionnaires were sent to respondents who were selected from appropriate departments in these institutions in proportion to the doctorates awarded in physiology by these departments. Respondents included department chairmen, senior scholars and junior scholars. Questionnaires were distributed during the spring of 1964 and respondents were asked to rate the quality of graduate faculty in each of the listed institutions according to scholarly competence and achievement. Levels of quality judged and corresponding rankings in Table 1 include: "Distinguished" (1-2), "Strong" (3-27), "Good" (28, including 15 equally ranked departments), and "Adequate plus" (43, including 14 equally ranked departments). In addition, respondents were asked to rate the effectiveness of graduate programs in the listed institutions according to the following criteria with which are indicated rankings given in Table 1: "Extremely attractive" (1-6), "Attractive" (7-26), and "Acceptable plus" (27, including 29 equally ranked departments). Institutions were ranked according to scores assigned by an arbitrary scoring system (Distinguished - 5, Strong - 4, Good - 3, etc) and averaged by the number of respondents. A total of 117 usable responses was received from 177 physiologists. Among departments of physiology and biology, 13 departments were considered "marginal" and 2 were found to be "not sufficient" (sic); the effectiveness of graduate programs was considered "marginal" in 14 departments and unattractive in 2.

Information concerning the number of enrolled graduate students in physiology was secured from a periodic publication issued by the Resources Analysis Branch of the National Institutes of Health[2]. Data were obtained from questionnaires distributed among 100 institutions responsible for more than 90% of the science doctorates granted. These data reveal that physiology is one of the basic medical sciences experiencing the greatest increase in graduate enrollment (15%) over the previous (1963-1964) year; 1240 graduate students were enrolled in physiology and 530 in biophysics in 1964-1965 (data in Tables 1 and 2 do not

include students in biophysics). The totals indicated in Tables 1 and 2 are purely for purposes of ranking and actually indicate "student-years" since one or more students may be counted repeatedly for each year. The original article may be consulted concerning proportions of women and full-time students in physiology.

Annual reports on institutional levels of compensation for the academic profession are provided by the American Association of University Professors ("AAUP")[3]. The rankings indicated in Table 1 are based on data presented in Tables 3A and 3B. Rankings in Table 3B are based on the highest compensation for all ranks ("AA" for all academic positions) and in decreasing order according to decreasing compensation for each rank. It is a truism that the problem of adequate compensation is most often resolved for junior positions and delayed the longest for senior ranks. With the system of ranking employed here, scores are lower for those institutions which fail to provide optimum compensation for the junior academic ranks. The salary figures cited in Table 3A represent compensation adjusted for a nine months basis and include a number of fringe benefits such as social security, retirement programs, etc. Unfortunately, salary figures specifically relevant to departments of physiology or biology are not available. Varying methods of reporting by each institution may include or exclude medical school departments, may be limited to these or only to basic science or preclinical departments.

Only a few comments can be made concerning relationships among the data presented. It is not surprising to find that there is a rather high correlation between the rankings for "quality of faculty" and "effectiveness of graduate program". It is also not surprising to find that those departments with the most attractive graduate training programs are not necessarily among those which rank high in numbers of enrolled graduate students. Perhaps no more than 18% of all graduate students are to be found in those departments which rank among the first 10 for effectiveness of their program. For example, Harvard and Rockefeller, which lead in most categories, are ranked very low in the numbers of graduate students being trained. Yet some departments which are only slightly less distinguished, such as California at Berkeley, UCLA, Washington (Seattle), and Minnesota, account for a large proportion of the students in training. With a few exceptions, those departments which are not ranked among the highest in faculty distinction do not undertake the training of great quantities of graduate students. No account is taken here of post-doctoral training which often provides highly effective training at distinguished institutions for Fellows whose graduate training may have been in some lesser department.

It is also evident that salary levels bear no consistent relationship to judgments of faculty quality. While many departments of distinction provide salaries in the highest brackets, there are also many departments of lesser distinction which are able to offer similarly high salaries.

For the most part the tabular data afford the reader an opportunity to compare rankings of institutions of his acquaintance with perhaps some better opportunity to recommend departments for graduate study and to

secure graduates for postdoctoral and staff appointments.

TABLE 1

Consolidated Rankings of Institutions Providing Physiology Training

Institution	Quality of Faculty	Graduate Program Effectiveness	Total Grad. Students 1962-1965	Salary Scale
Alabama	--	--	65 [9]	--
Baylor	--	--	27	85
Boston	--	--	64	62 [8]
Brown [1, 2, 4]	28	27	--	38 [8]
Buffalo [2]	20	24	15 [9]	10
Calif.-Berkeley [1, 10]	5	3	2	27 [8]
Calif.-Davis [1, 2]	28	27	10	27 [8]
Calif.-Los Angeles [1]	13	9	5	38 [6, 8]
Calif.-San Francisco [10]	--	--	40 [9]	38 [6, 8]
Cal. Tech [1]	6	5	84	14 [8]
Catholic Univ.	--	--	30 [9]	59 [8]
Chicago [1, 2]	11	17	15 [9]	7 [8]
Cincinnati	43	--	67 [9]	68 [5, 8]
Colorado [1, 2]	43	27	76	1 [6, 8]
Colorado State	--	--	15 [9]	71 [8]
Columbia [1]	10	13	54	14 [5, 8]
Connecticut [1]	43	--	37 [9]	52 [8]
Cornell [1, 2]	14	14	7	11 [8]
Duke [1]	18	19	25 [9]	14 [5, 8]
Emory [1, 2]	28	27	32 [9]	56 [7]
Florida [1, 2, 4]	28	27	47 [9]	80 [5, 8]
Florida State [1, 2]	43	27	80	82

TABLE 1 (cont'd.)

Institution	Quality of Faculty	Graduate Program Effectiveness	Total Grad. Students 1962-1965	Salary Scale
Fordham	--	--	55 [9]	62 [8]
Georgetown	--	--	35 [9]	68 [5, 8]
George Washington	--	--	51 [9]	57 [8]
Hahneman	--	--	73 [9]	--
Harvard [1]	1	1	44 [9]	1 [6, 8]
Hawaii	--	--	51 [9]	38 [8]
Hopkins [1]	3	6	65 [9]	11 [7, 8]
Houston	--	--	61 [9]	57 [8]
Illinois	16	11	1	52 [8]
Ill. Inst. Tech.	--	--	77 [9]	38 [8]
Indiana [1, 2]	23	24	23	23 [8]
Iowa [1, 2]	28	27	24	6 [6]
Iowa State [1, 2]	43	27	59 [9]	27 [8]
Kansas [1, 2]	43	27	49 [9]	71 [5, 8]
Kansas State	--	--	67 [9]	68 [8]
Kentucky	--	--	40 [9]	37 [6]
Louisiana State	--	--	71 [9]	71 [5, 8]
Loyola	--	--	28 [9]	83
Maryland [1, 2]	43	27	9	62 [8]
Medical Coll. Va.	--	--	--	55 [7]
Michigan [1]	9	10	21	7 [6, 8]
Michigan State [2]	28	27	8	23 [8]
Minnesota	7	7	3	38 [5, 8]
Missouri	--	--	43	35 [7, 8]
Nebraska	--	--	44 [9]	62 [5, 8]

TABLE 1 (cont'd.)

Institution	Quality of Faculty	Graduate Program Effectiveness	Total Grad. Students 1962-1965	Salary Scale
N.Y.U. [1, 2]	28	27	37 [9]	38 [5, 8]
North Carolina [1, 2]	43	27	30 [9]	35 [7, 8]
North Carolina State	--	--	78 [9]	71 [8]
Northwestern [2]	26	27	67 [9]	7 [8]
Notre Dame	--	--	55 [9]	38 [8]
Ohio State [2]	43	27	4	38 [5, 8]
Oklahoma [1, 2]	43	27	22	84
Oklahoma State [2]	--	27	32 [9]	80 [8]
Oregon [1, 4]	27	23	49 [9]	52 [8]
Oregon State	--	--	44 [9]	71 [8]
Pennsylvania [1]	8	8	15 [9]	14 [5, 8]
Pittsburgh [1, 3]	28	26	78 [9]	--
Princeton [1, 2, 3]	15	14	--	14 [8]
Purdue	--	--	20	27 [8]
Rice [1, 2, 4]	28	27	--	23 [8]
Rochester [1]	12	12	15 [9]	11 [8]
Rockefeller [1]	2	2	71 [9]	--
Rutgers [1, 2]	28	27	18	59 [5, 8]
St. John's	--	--	53	62 [8]
St. Louis [1]	43	--	63	71 [5, 8]
Southern Calif. [2]	--	27	67 [9]	38 [5, 8]
Southern Illinois	--	--	32 [9]	71 [8]
Stanford [2]	24	21	28 [9]	14 [8]
SUNY Downstate	--	--	40 [9]	3 [6, 8]

TABLE 1 (cont'd.)

Institution	Quality of Faculty	Graduate Program Effectiveness	Total Grad. Students 1962-1965	Salary Scale
SUNY Upstate [1, 2]	28	27	58	3 [6, 8]
Temple	--	--	61 [9]	38 [5, 8]
Tennessee [1, 2]	43	27	13	3 [6, 8]
Texas [1, 2]	43	27	6	38 [5, 8]
Texas A & M	--	--	55 [9]	71 [8]
Tufts [2]	28	27	82	14 [7, 8]
Tulane [1, 2]	28	27	25 [9]	38 [6, 8]
Utah [1, 2]	28	27	81	59 [8]
Utah State	--	--	59 [9]	71 [8]
Vanderbilt [1, 2]	28	27	37 [9]	27 [5]
Vermont	--	--	--	67 [7]
Virginia	--	--	83	27 [8]
Washington (Seattle)[1]	4	4	11	22 [7]
Washington (St. Louis)[2]	25	27	73 [9]	38 [5, 8]
Wayne State [1, 2]	43	27	--	23 [7, 8]
Western Reserve [1]	19	20	35 [9]	--
Wisconsin [1, 2]	17	17	14	27 [7, 8]
Yale [1]	21	16	47 [9]	14 [5, 8]
Yeshiva [2, 4]	22	21	73 [9]	27 [8]

[1]Study offered in a department other than physiology or in more than one department.

[2]Rank of graduate program effectiveness shared with another institution.

[3]Doctorate reported in biology rather than in physiology.

[4]Less than 1/2 of respondents rated the institution; doctorates offered in biology or biological sciences rather than in physiology.

[5]Salary ranking excludes all departments in School of Medicine.

[6]Salary ranking limited to all departments in School of Medicine.
[7]Salary ranking limited to preclinical or basic science departments in School of Medicine.
[8]Salary ranking shared with one or more other institutions.
[9]Ranking of graduate student enrollment shared with one or more other institutions.
[10]The Cartter Report specifies California-Berkeley and does not refer to California-San Francisco. It is the author's personal opinion that these two campuses should, in fact, be consolidated for the graduate ratings assigned to Berkeley.
--Information not available because ranking or data not provided.

TABLE 2

Trends in Graduate Student Enrollment

Rank	Institution	Total Number of Students 1962-63	1963-64	1964-65	Total
1	Illinois	76	85	95	256
2	Calif.-Berkeley	40	45	52	137
3	Minnesota	34	47	49	130
4	Ohio State	33	41	36	110
5	Calif. - Los Angeles	23	32	37	92
6	Texas	23	30	34	87
7	Cornell	22	22	40	84
8	Michigan State	20	22	37	79
9	Maryland	16	15	41	72
10	California-Davis	18	21	32	71
11	Washington (Seattle)	18	25	23	66
12	Rutgers	16	22	26	64
13	Tennessee	22	18	21	61
14	Wisconsin	13	27	19	59
15	Buffalo	19	16	19	54
15	Chicago	18	19	17	54
15	Colorado State	7	15	32	54

TABLE 2 (cont'd.)

Rank	Institution	Total Number of Students 1962-63	1963-64	1964-65	Total
15	Pennsylvania	18	21	15	54
15	Rochester	14	20	20	54
20	Purdue	28	25	--	53
21	Michigan	16	17	17	50
22	Oklahoma	4	19	25	48
23	Indiana	14	17	15	46
24	Iowa	17	12	16	45
25	Duke	10	14	20	44
25	Tulane	16	15	13	44
27	Baylor	12	8	23	43
28	Loyola	15	9	15	39
28	Stanford	11	13	15	39
30	Catholic Univ.	15	15	8	38
30	North Carolina	13	13	12	38
32	Emory	11	13	13	37
32	Oklahoma State	8	15	14	37
32	Southern Illinois	11	13	13	37
35	Georgetown	9	14	13	36
35	Western Reserve	7	15	14	36
37	Connecticut	11	13	11	35
37	N.Y.U.	11	8	16	35
37	Vanderbilt	8	11	16	35
40	Calif.-San Francisco	8	14	12	34
40	Kentucky	9	14	11	34

TABLE 2 (cont'd.)

Rank	Institution	Total Number of Students 1962-63	1963-64	1964-65	Total
40	SUNY Downstate	10	11	13	34
43	Missouri	9	11	12	32
44	Harvard	9	9	13	31
44	Nebraska	11	10	10	31
44	Oregon State	13	9	9	31
47	Florida	15	7	8	30
47	Yale	3	13	14	30
49	Kansas	8	11	10	29
49	Oregon	9	9	11	29
51	George Washington	10	7	10	27
51	Hawaii	7	8	12	27
53	St. John's	13	6	6	25
54	Columbia	6	9	9	24
55	Fordham	11	2	9	22
55	Notre Dame	3	12	7	22
55	Texas A & M	10	7	5	22
58	SUNY Upstate	6	7	7	20
59	Iowa State	4	5	10	19
59	Utah State	6	5	8	19
61	Houston	1	3	14	18
61	Temple	5	8	5	18
63	St. Louis	4	6	7	17
64	Boston	4	5	5	14
65	Alabama	4	4	5	13

TABLE 2 (cont'd.)

Rank	Institution	Total Number of Students 1962-63	1963-64	1964-65	Total
65	Hopkins	3	4	6	13
67	Cincinnati	6	3	3	12
67	Kansas State	4	7	1	12
67	Northwestern	--	--	12	12
67	Southern Calif.	5	3	4	12
71	Louisiana State	4	6	1	11
71	Rockefeller	5	2	4	11
73	Hahneman	3	3	4	10
73	Washington (St. Louis)	3	3	4	10
73	Yeshiva	2	4	4	10
76	Colorado	4	4	1	9
77	Ill. Inst. Tech.	--	3	3	6
78	North Carolina State	--	2	3	5
78	Pittsburgh	1	--	4	5
80	Florida State	--	--	4	4
81	Utah	--	1	2	3
82	Tufts	--	--	2	2
83	Virginia	1	1	--	2
84	Cal. Tech.	--	--	1	1
	Totals	936	1079	1240	3255

TABLE 3A
(1965-1966)
Average Salary Scale and Rank Indicated in Table 3B

Scale	Rank			
	Professor 1	Associate Professor 2	Assistant Professor 3	Instructor 4
AA	23290	14440	10830	8130
A	18720	12440	9500	7190
B	14960	10820	8440	6550
C	12300	9330	7470	6010
D	10040	8120	6730	5610

TABLE 3B
(1965-1966)
Average Compensation by Rank Among Institutions Listed in Table 1

Rank	Institution	1	2	3	4
1	Colorado [2], Harvard [2]	AA	AA	AA	AA
3	SUNY Downstate [2], SUNY Upstate [2], Tennessee [2]	A	AA	AA	AA
6	Iowa [2]	A	AA	AA	A
7	Chicago, Michigan [2], Northwestern	A	A	AA	AA
10	Buffalo	A	A	AA	A
11	Cornell, Hopkins [3], Rochester	A	A	A	AA
14	Cal. Tech., Columbia [1], Duke [1], Pennsylvania [1], Princeton, Stanford, Tufts [3], Yale [1]	A	A	A	A
22	Washington (Seattle)[3]	B	A	AA	AA
23	Indiana, Michigan State, Rice, Wayne State [3]	B	A	A	AA
27	Calif.-Berkeley, -Davis, Iowa State, Purdue, Vanderbilt, [1] Virginia, Wis. [3], Yeshiva	B	A	A	A

TABLE 3B (cont'd.)

Rank	Institution	1	2	3	4
35	Missouri [3], North Carolina [3]	B	A	A	--
37	Kentucky [2]	B	B	A	AA
38	Brown, Calif.-L.A. [2], -S.F. [2], Hawaii, Ill. Inst. Tech., Minnesota [1], N.Y.U. [1], Notre Dame, Ohio State [1], So. Calif. [1], Temple [1], Texas [1], Tulane [2], Washington (St. Louis) [1]	B	B	A	A
52	Connecticut, Illinois, Oregon	B	B	B	A
55	Med. Coll. Virginia [3]	B	C	B	A
56	Emory [3]	C	A	A	A
57	George Washington, Houston	C	B	A	A
59	Catholic, Rutgers [1], Utah	C	B	B	A
62	Boston, Fordham, Maryland, Nebraska [1], St. John's	C	B	B	B
67	Vermont [3]	C	B	C	C
68	Cincinnati [1], Georgetown [1], Kansas State	C	C	B	A
71	Colorado State, Kansas [1], Louisiana State [1], North Carolina State, Oregon State, St. Louis [1], So. Illinois, Texas A & M, Utah State	C	C	B	B
80	Florida [1], Oklahoma State	C	C	B	C
82	Florida State	C	C	C	C
83	Loyola	C	C	--	--
84	Oklahoma	D	C	B	C
85	Baylor	D	D	D	D

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[1] Rating excludes all departments in School of Medicine.
[2] Rating limited to all departments in School of Medicine.
[3] Rating limited to all preclinical or basic science departments in School of Medicine.
-- Data not provided by institution.

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RADIATION RESEARCH SOCIETY MEETING

The 15th Annual Meeting of the Radiation Research Society will be held at the San Jeronimo Hilton Hotel, San Juan Puerto Rico, May 7-11, 1967.

In addition to contributed papers, there are planned symposia on either Radiation Chemistry or Physics, as well as Radiation Ecology and Biology. Specific plans will be announced at a later date.

Those desiring to report original research on radiation effects, or desiring attendance information may contact the Business Office of the Radiation Research Society:

Mrs. Charlene B. Gill
Administrative Assistant
Radiation Research Society
C/O The American University
Washington, D. C. 20016

WILLIAM HAROLD CHAMBERS

Those who had the good fortune of an association with William Harold Chambers became quickly aware of the unusual number of fine qualities that were his. Keen enthusiasm for the responsibilities of an academician in teaching, research, and university service, critical judgment in the assessment of standards of performance, sensitivity of conscience concerning human rights and behavior, tolerant understanding in dealings with co-workers, unselfish and innate dedication to the requirements of military service, and proficient competency in many and quite varied undertakings, these qualities and others were evident in the patterns of his life. Absent were egoism and conceit and any tendency to self-exploitation.

Born on March 2, 1894, he received the B.S. and M.S. degrees at the University of Illinois in 1915 and 1917. During this two-year period of graduate study he served as an assistant in dairy bacteriology at the University Experiment Station. He resumed predoctoral studies in 1919, after military service in World War I, at the Missouri Botanical Gardens where he was a graduate student of Washington University and earned the Ph.D. degree in 1920. For the next four years he served as a research assistant in the Barnard Free Skin and Cancer Hospital and in the Department of Surgery at the Washington University School of Medicine. In 1924 he was honored with a National Research Council Fellowship which he held for two years in the Department of Physiology of Cornell Medical College. An instructorship in Physiology at Yale University was accepted in 1926 but in 1928 he returned to Cornell where he served as a member of the staff until 1947. This period was interrupted by military service in World War II from 1942 to 1946.

On his return from military service, in 1946, he resumed his academic activities at Cornell. However, in 1947 he was persuaded to accept the position of Chief of the Physiology Division, Medical Laboratory, U.S. Army Chemical Corps, Edgewood, Maryland. He spent the next 14 years at Edgewood, directing with marked success the experimental studies of a large group of investigators working on basic physiological, biochemical, entomological, and pharmacological research of military significance. At the same time, he served as Assistant Scientific Director of the Laboratory and, as a Colonel in the U.S. Army Reserve and senior reserve officer in the Edgewood group, he encouraged his co-workers to maintain their interest in military problems. He directed the activities of the Medical Research Contract Administration Office at Edgewood during his final year. He retired in 1961 and that year he and Nona returned to Bar Harbor, Maine where he died on April 16, 1966 after an extended illness. The marriage of Will Chambers and Winona Hyland in 1919 was a happy union of like persons with similar fine ideals and enthusiasm for life. Nona and their daughter and son survive.

Will Chambers was productive in research from the beginning of his career. His first five publications reflected his early association with bacteriological problems in the handling of dairy products and, subsequently, with growth requirements of bacteria. Four of the five

papers appeared by 1920 and were a part of his predoctoral productivity. His postdoctoral research at Washington University also reflected the laboratory interests of his position and were concerned with the chemistry of blood in normal and in cancerous individuals. Interestingly, one of the papers resulted from his participation in an impressive study of the role of hemoglobin and of the chloride shift in the buffering of blood (E. A. Doisy, A. P. Briggs, E. P. Eaton, and W. H. Chambers, J. Biol. Chem. 54: 305-329, 1922), a paper that the undersigned has always considered of exceptional scientific merit.

It is not surprising that with Will's move to Cornell there was initiation of a continuing interest in many aspects of intermediary metabolism. The recent discovery of insulin had stimulated new approaches to the utilization of carbohydrate and fat in human and in experimental diabetes. Of particular significance at Cornell was the spirit and leadership of Graham Lusk who remained active in the department. Will plunged into important and independent studies of specific dynamic action, of energy metabolism, of fasting, and of the role of hormones in the utilization of dietary and tissue protein, fat, and carbohydrate, particularly as sources of energy for muscle activity. Collaborators during this 19-year period, less the World War II years, in addition to Graham Lusk included, among others, such distinguished investigators as H. J. Deuel, Jr., A. T. Milhorat, H. E. Himwich, L. H. Nahum, Margaret Dann, Herbert Pollack, J. P. Chandler, and S. B. Barker. Research papers published in the 16-year period from 1925 to 1941 totaled 28, 11 in the American Journal of Physiology and 17 in the Journal of Biological Chemistry, a distribution that illustrates pertinently the basic character of his research.

Will's graduate studies were interrupted by military service from 1917 to 1919. His duties in the Sanitary Corps, U. S. Army Medical Department took him to France and, with the Army of Occupation, to Germany. In 1942 he was encouraged to return to active duty as a nutrition officer with the rank of major. After several months of valuable service in the Office of The Surgeon General in Washington he was sent to the European Theater of Operations. Here his knowledge of food and nutrition proved especially important in the feeding of the sick and wounded military personnel in U. S. Army Hospitals. The concentration of hospitals in the United Kingdom made it desirable to assign responsibility for nutritional health in that area to him and he was placed in charge of this activity in the United Kingdom Base after the main headquarters moved to France and the continent. At the end of World War II he served at 12th Army Group Headquarters in Germany with responsibility for the determination of nutritional levels and food requirements of the German civilian population in the U. S. Zone. His knowledge and experience and his unusual ability to gain the confidence and cooperation of his associates were of a high order as is illustrated by his promotion and by the awarding of the Bronze Star Medal to him on two occasions.

In 1946, following the death of Dr. D. R. Hooker, greatly esteemed Secretary of the Federation of American Societies for Experimental Biology and Managing Editor of Federation Proceedings, Will succeeded Dr. Hooker in each of these responsible positions. At this time these

responsibilities were undertaken as extra activities in addition to the regular duties of full-time university staff member. Will served admirably during his last year at Cornell and during his first year at Edgewood. He was indispensable, in fact, during the period in 1947 when the combined offices of the Federation and of the American Physiological Society moved from Baltimore to the NAS-NRC building in Washington and when the expanding responsibilities of the Federation necessitated full-time service which was to be so excellently provided by the newly appointed Dr. Milton O. Lee. No more fitting description of Will Chambers' contribution can be given than the words of one who saw his response to the needs of that period:

> "As I remember Dr. Chambers that first year, he was our mentor and was always there when we needed him. The staff was new and we certainly needed guidance in every area - he never failed us. He was interested and knowledgeable in all phases of the operations; he told us how the job had been done in the past but never said that it should be handled in the same way in the future. He accepted the new regime, knowing that many changes were inevitable; with quiet enthusiasm he went along with them. We all leaned on him heavily those first months and on into the following year and were never disappointed when we called on him for assistance. He had all the time in the world for us, even though his own schedule was heavy. He was a 'good listener.' His devotion to APS and FASEB was contagious and an inspiration. His cheerfulness, calmness, and sense of humor - always with him - carried us through many crises."

Will became a member of the American Physiological Society in 1924 and of the American Institute of Nutrition in 1933, a charter member. He served as Assistant Editor of the Journal of Nutrition from 1939 to 1941 and later was a member of the Editorial Boards of Physiological Reviews and of the Journal of Applied Physiology.

Although I had known Will Chambers as a scientist acquaintance through our attendance at meetings of the Federation from the early 1920's, my personal and deep appreciation of his character and of his performance developed from his assignment to the European Theater in 1943. He was a loyal, friendly co-worker; calm and steady in emergencies. He accepted all assignments with eager interest and his reports and recommendations were noteworthy for their common sense. It was easy to have confidence in him. Recollections bring back many wartime happenings and one of these is remembered with great pleasure. It was desirable to bring Will from London to Belgium and France to increase his understanding of the feeding problems of active combat operations. By happy chance, the itinerary included a part of the Third Army in which his son served as the pilot of a small artillery observation plane. We spent a night with the son and his unit in a stable in a region surrounded by the carnage of war. Calm and gratifying was the meeting of father and son, a son who was soon to be honored for his

bravery and skill in helping to provide the air observations that made possible the unexpected crossing of the Rhine by General Patton's Third Army.

And so has passed a man whom it is a pleasure to remember, a true scientist, an unselfish citizen, a warm-hearted friend, a persevering supporter of all that he believed was good for his science and for his country. Many are his successors, some can follow the pattern and strides of his footsteps, too many can only falter because of their own self interest.

Wendell H. Griffith
August 9, 1966

JOSEPH DOUPE MEMORIAL SCHOLARSHIP FUND

Dr. Joseph Doupe, Chairman, Department of Physiology, Faculty of Medicine, University of Manitoba, died in Winnipeg at the age of 56, on August 27th, after a prolonged and disabling illness.

Professor Doupe's contributions to medicine were varied and of great importance. His reputation as a scientist and as a medical educator is international. However of all his interests his concern and careful cultivation of talents of students was paramount; and although he was not one to wear his heart on his sleeve, his interest extended to all aspects of his students' welfare, and not just to their medical or scientific skills.

It is particularly appropriate that a scholarship fund in medicine be named for him. The medical profession can show their appreciation of Professor Doupe by ensuring that this fund grows sufficiently to make a significant contribution to the education of medical scholars. The members of the profession in Manitoba - including so many of his former students and his colleagues - will feel a special affinity and an obligation to this cause.

Donations should be made payable to the University of Manitoba with mention of the Fund's name. They may be sent either to the Dean of the Faculty of Medicine or to the Comptroller of the University of Manitoba. Official receipts will be returned by mail for income tax purposes.