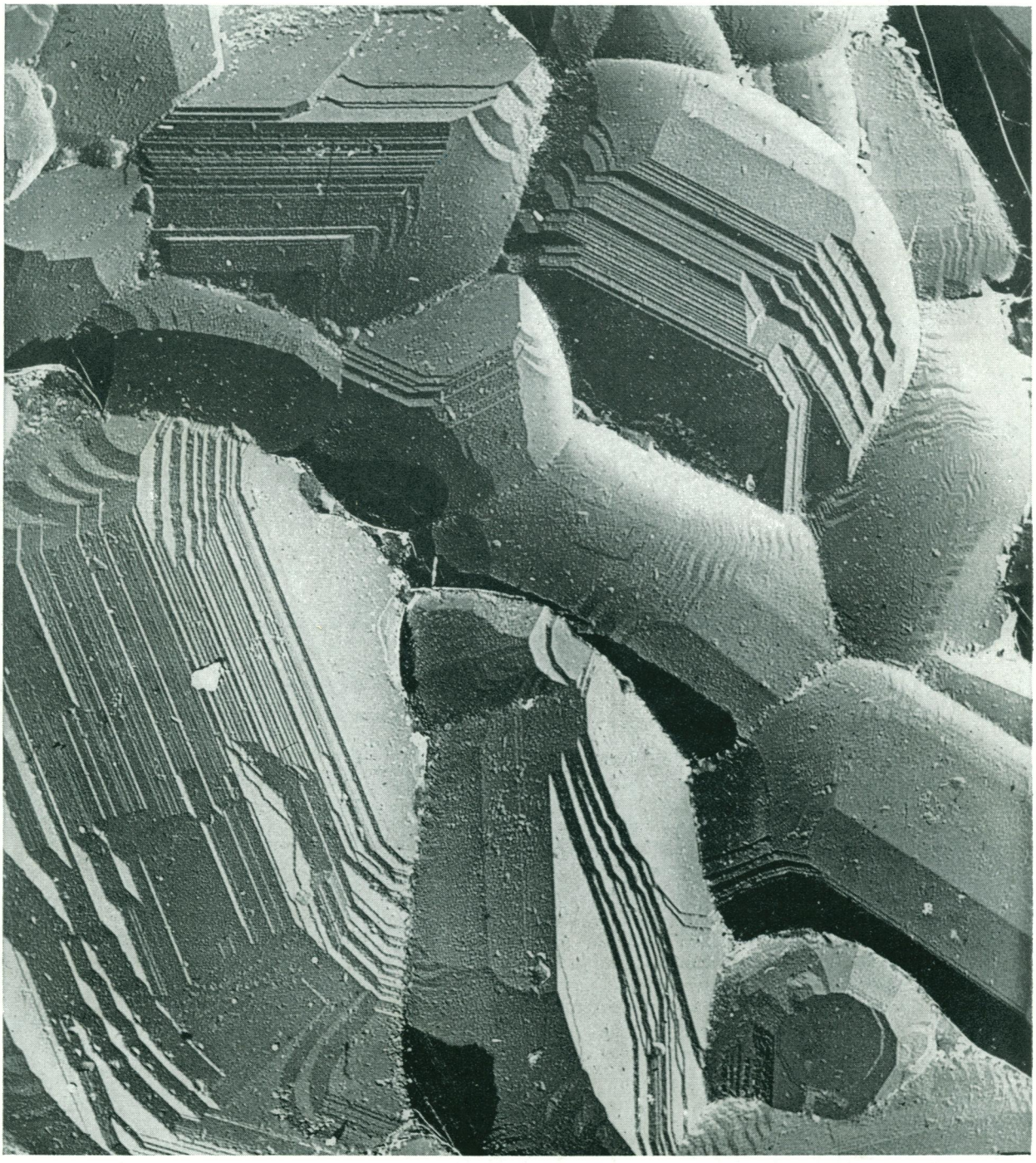
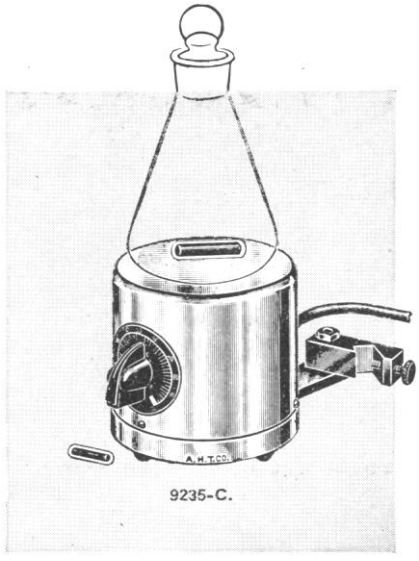
SCIENCE

4 December 1959

AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE



Preconvention Issue



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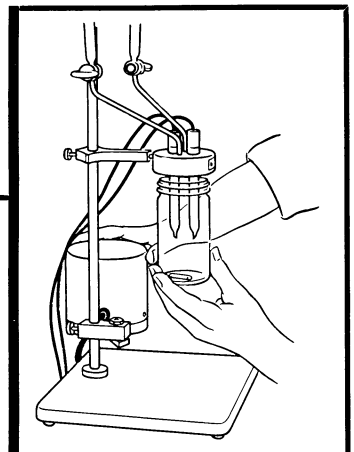
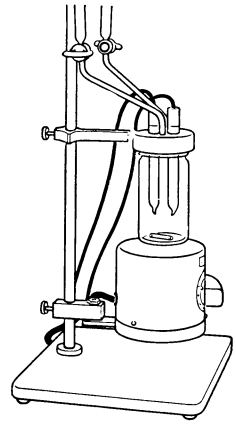


Stirrer swings aside to permit convenient and safe removal of vessel containing sample.



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4 December 1959, Volume 130, Number 3388

SCIENCE

Editorial

Headlines and Highlights 1533

Articles

Living Clocks: *F. A. Brown, Jr.* 1535

The clocks are accounted for as "open systems" depending upon subtle geophysical rhythms.

Environmental Radiation and Cancer: *H. F. Blum* 1545

No threshold is demonstrable, and incidence may have rising inflection with intensity.

AAAS Chicago Meeting: *R. L. Taylor* 1547

Science in the News

United States and Soviet Union Agree to Reciprocal Exchange in Peaceful Uses of Atomic Energy; Efforts To Control Locusts in Africa Described 1563

Book Reviews

Organizing Peace in the Nuclear Age, reviewed by *I. de Sola Pool;* other reviews 1568

Reports

Natural Selection and the Origin and Evolution of Weeping in Man: *A. Montagu* 1572

Parietal Eye Nerve in the Fence Lizard: *R. M. Eakin* and *R. C. Stebbins* 1573

Structural Similarities between Hemoglobins A and F: *A. M. Katz* and *A. I. Chernoff* 1574

Oxygen Isotope Paleotemperature Determinations of Australian Cainozoic Fossils: *F. H. Dorman* and *E. D. Gill* 1576

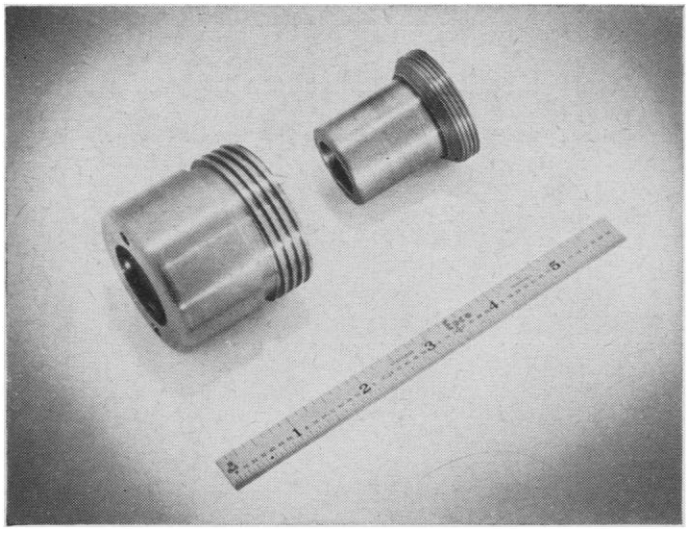
Departments

Letters 1530

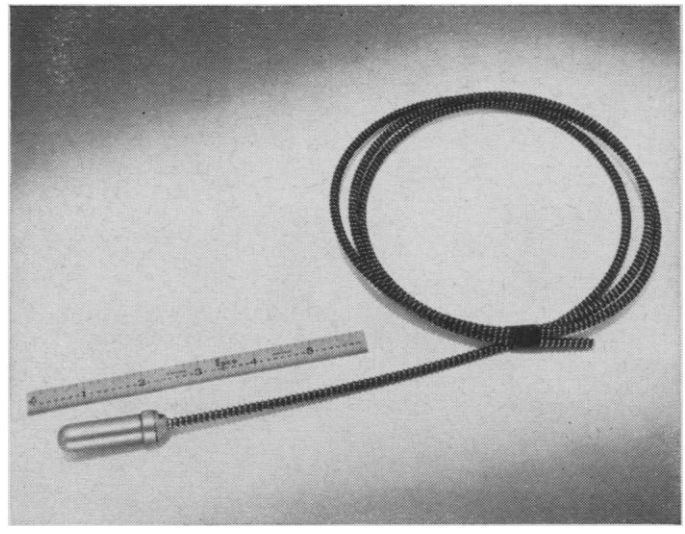
Speleology; Forthcoming Events; New Products 1580

Cover

Surface of a ceramic especially developed for magnets, showing a perfect geometrical pattern subjected to tremendous upheaval. (× 14,500) [Courtesy Phillips', Eindhoven, Holland, from *The Thirteen Steps to the Atom,* published by Franklin Watts, reviewed on page 1570]



Teletherapy



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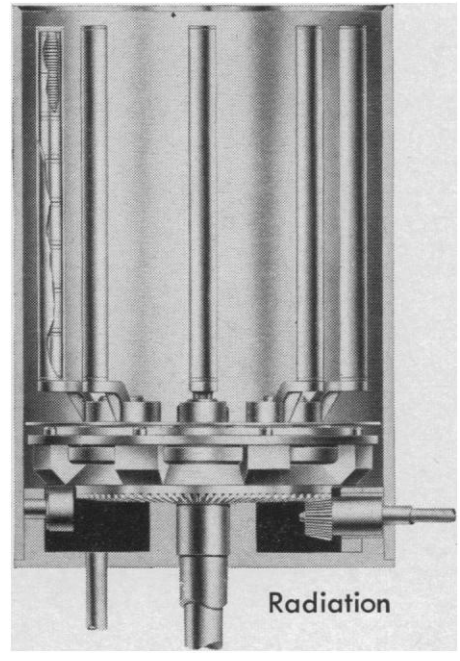

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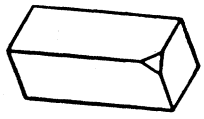

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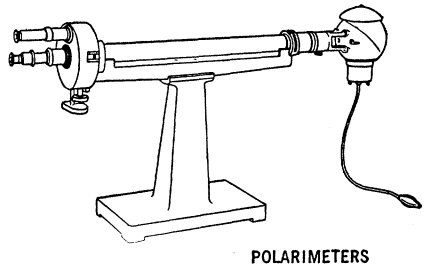

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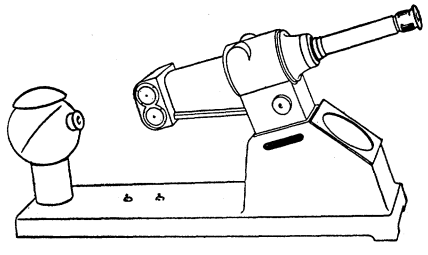
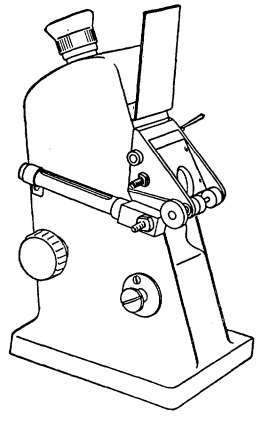


ANOMALOSCOPE



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Letters

Policy and the First Bombs

In a recent issue of *Science* [**130**, 32 (3 July 1959)] Edward Condon reviews Amrine's book *The Great Decision*, which discusses in part the making of high policy by the United States Government on how to deal with the first atomic bombs. Both author and reviewer emphasize the importance of studying and understanding this piece of history. With this view I am in deep agreement. From earlier experience of my own, and from conversations with contemporary historians who have looked into the problem, it seems clear that the available documentary record is meager. I should not be astonished if the existing record is itself quite meager. Just for this reason it seems to me important not to let serious errors or misrepresentations pass unnoticed.

On one such error, which has to do with the information made "available to our policy makers at the time policy decisions on how to use the bomb were being made," I feel competent to make a comment. Since Condon's review repeats and elaborates a mistake also to be found in Feis' authoritative *Churchill, Roosevelt, Stalin*, I shall do so at some length.

The paragraphs in Condon's review which I wish to discuss are the following:

> . . . Amrine tells us (page 132) that General Groves, in a memorandum to General George Marshall dated 30 December 1944, vastly underestimated the power of the bomb. He estimated the power of the bomb at only 500 tons of TNT, whereas it was actually 20,000 tons when used on Hiroshima. As Amrine says, our military planners "were only given reason to think it was a spectacular improvement in bombs, not another kind of warfare".
>
> Now I know that General Groves did not know enough physics to make his own estimate; and I do not believe that anyone at Los Alamos would have made such a low estimate. How then could Groves have erred by a factor of 40? Could it have been intentional, so that the top policy planners would not be aware of the horribly serious nature of the decision they were taking?

I have turned to the memorandum of 30 December 1944 from General Groves to General Marshall, which is published in the first volume of *Foreign Relations of the United States: The Conferences at Malta and Yalta, 1945*. The first paragraph reads:

> It is now reasonably certain that our operation plans should be based on the gun type bomb, which, it is estimated, will produce the equivalent of a ten thousand ton TNT explosion. The first bomb, without previous full scale test which we do not believe will be necessary, should be ready about 1 August 1945. The second one should be ready by the end of the year and succeeding ones at . . . intervals thereafter.

In the first days of August 1945 the final deliveries of components of the gun-type bomb reached the Pacific. This bomb was used against Hiroshima. Its yield is not known very accurately; it was not far from 17,000 tons. The calculation of the yield of nuclear weapons made a priori, and unverified, was not as difficult for this type of weapon as for the implosion bombs; it was nevertheless subject to some uncertainty. The discrepancy between General Groves' prediction of 10,000 tons and the actual value reflects some underestimate in our calculations of the yield, and a corresponding over-design.

General Groves' second paragraph reads:

> Our previous hopes that an implosion (compression) type of bomb might be developed in the late spring have now been dissipated by scientific difficulties which we have not as yet been able to solve. The present effects of these difficulties are that more material will be required and that the material will be less efficiently used. We should have sufficient material for the first implosion type bomb sometime in the latter part of July. This bomb would have an effect which would be equivalent to about 500 tons of TNT. During the remainder of 1945 it is estimated that we can produce . . . additional bombs. The effectiveness of these should increase towards 1000 tons each as development proceeds and, if some of our problems are solved, to as much as 2500 tons.

As I recall it, these estimates were based on extensive conversations between General Groves and senior members of the Los Alamos Laboratory, and reflected the then prevailing opinion, and the results of the most recent experiments and calculations. We had been convinced of our inability to make implosion bombs as we had originally designed them. I believe that it was not until about a decade later that such designs were tested.

By early July, however, we had solved, as General Groves suggested we might, "some of our problems"; and with a more conservative model were prepared to test a first bomb. Even at that time, however, our estimates of the yield were quite uncertain, and for the most part quite low. We established a pool in the laboratory, to record guesses as to the yield of the first bomb. An overwhelming majority made estimates under a few thousand tons; figures in the hundreds of tons were popular. Two visitors, Lee Dubridge and I. I. Rabi, picked the extreme

(Continued on page 1592)

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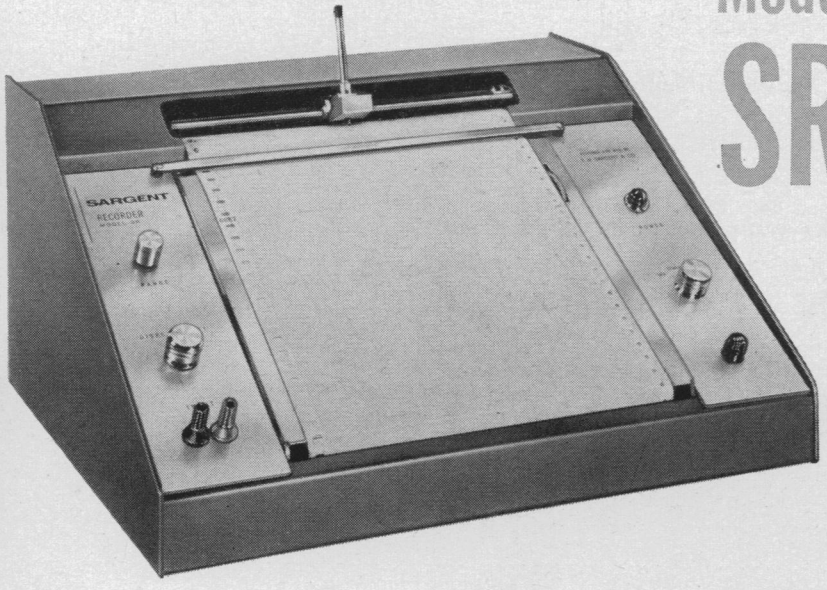

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Effective this year, the former General Program-Directory, which had become an unwieldy book of more than 400 pages, has been separated into *two* publications, namely:

a) The Directory of AAAS Officers and Activities, 96 pp., already published; and

b) The General Program of the Annual Meeting, c. 200 pp., which will appear early in December

Both of these, sold at cost, may be purchased separately—in advance (see coupon below), or at the meeting. Some of their *respective* contents are:

The General Program

1. The two-session general symposium "Moving Frontiers of Science IV," arranged by the Committee on AAAS Meetings.
2. Programs of the 18 AAAS sections (symposia and contributed papers).
3. Programs of the more than 80 participating societies.
4. Sessions of the Conference on Scientific Communication, Conference on Scientific Manpower, and the Academy Conference.
5. The Special Sessions: AAAS Address and Reception, National Geographic Society, Phi Beta Kappa, Sigma Xi, RESA, Tau Beta Pi Association.
6. Details of the Morrison Hotel—center of the Meeting—and of the other session sites.
7. Titles of the latest foreign and domestic scientific films to be shown in the AAAS Science Theatre.
8. Exhibitors in the 1959 Annual Exposition of Science and Industry and descriptions of their exhibits.

The Directory

1. AAAS officers, staff, committees, for 1959.
2. Section committees and other AAAS Council members.
3. The 285 affiliated organizations.
4. Historical sketch and organization of the Association.
5. Complete roll of AAAS presidents and their fields.
6. Publications of the Association, including all symposium volumes.
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8. Future Meetings of the AAAS through 1963.
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Chicago, 26-31 December 1959

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If desired, the hotels will add a cot at $3.00 per night—except that all children under 12 are free. Mail your application *now* to secure your first choice of desired accommodations. All requests for reservations must give a definite date and estimated hour of arrival, and also probable date of departure.

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**No sessions in public rooms.* For a list of the headquarters of each participating society and section, see page 228, *Science,* 24 July.

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Meetings

Speleology

The 16th annual convention of the National Speleological Society was held in Springfield, Mo., 1–4 April 1959. The convention brought together specialists in several fields, all of them having a common interest in caves. Speleological investigations include inquiry into the origin and development of limestone caves, examination of fossils and evidences of human activity preserved in them, and study of the specialized biota characterizing caves today. The constant temperature and the uniformity with respect to other physical variables has also made caves a natural laboratory for investigation of mineral formation under conditions known to have been stable for long periods of time. These same uniform conditions have been employed in studies of the physics of cave temperature distribution and air movement. As a common meeting ground, speleology might best be compared to oceanography, which also brings together biologists, geologists, physicists, and workers in related fields.

Several papers were given in Springfield on the geomorphology of limestone terrain. Kennedy Nicholson and W. B. White of Pennsylvania State University described investigations of 8 miles of underground drainage in a karst region in Virginia, and R. G. Deike of Columbia, Mo., presented data suggesting that strike joints are predominant in controlling cave and karst development. An investigation of an area of glaciated karst in northern New York state, by R. R. Anderson of Berkeley Heights, N.J., was contrasted with a reconnaissance of tropical karst in Puerto Rico, by Jeanne Gurnee of Closter, N.J. The geomorphic relations of large single-passage caves in central Missouri were discussed by G. H. Deike, III, of the University of Missouri, and W. B. White and his collaborators noted the effect of geologic structure on cave-passage cross section in Cueva del Guacharo, Venezuela. R. L. Curl of Berkeley, Calif., presented a detailed analysis of the application of solutional markings on limestone as quantitative indicators of water flow in the past.

In an important paper, W. S. Broecker of Lamont Geological Observatory described the first successful attempt at dating the calcium carbonate of cave travertine by the radiocarbon method. Using stalactites of known ages as standards, he found the C^{14} content of speleothems to be approximately 80 percent that of wood of comparable age. The method will find wide application in speleological research. M. G. Mehl of the University of Missouri discussed Pleistocene vertebrate remains in Missouri caves, and H. D. Weaver of Jefferson City, Mo., reported on the abandoned onyx mining industry of Missouri.

The field of cave meteorology was reviewed by R. F. Brown of Davenport, Iowa, who discussed results obtained from instruments stationed in caves in national parks during the International Geophysical Year.

The biologic papers began with a report on the movements and numbers of cave bats in Missouri, by R. F. Myers of the Missouri Cooperative Wildlife Research Unit. Cave salamanders were discussed in three papers: C. E. Mohr (Audubon Nature Center) described the life history of *Eurycea longicauda*; W. W. Varnedoe (Huntsville, Ala.) presented a study of the brooding of *Plethodon g. glutinosus*; and Joe Gorman (St. Louis University) discussed the evolution of cave salamanders. N. B. Causey of Fayetteville, Ark., spoke on speciation in the cave milliped *Scoterpes*, and Kenneth Christiansen of Grinnell College gave an outline of the genus *Sinella* in North American caves. The biologic session was concluded with a report on the cave beetles of Tennessee and Kentucky, by T. C. Barr, Jr., of Tennessee Polytechnic Institute.

The banquet speaker at the convention was Walter B. Jones, state geologist of Alabama, who is completing a monograph on the caves of Alabama.

Officers elected for the coming year are as follows: president, Brother G. Nicholas, Notre Dame University; vice president for administration, D. N. Cournoyer, Washington, D.C.; vice president for organization, D. F. Black, Chattanooga, Tenn.; vice president for publications, T. C. Barr, Jr., Tennessee Polytechnic Institute; vice president for research, G. W. Moore, U.S. Geological Survey; and treasurer, Barbara Munson, Chattanooga, Tenn.

W. E. Davies (U.S. Geological Survey, Washington, D.C.) will continue as editor of the *Bulletin* of the National Speleological Society. The current issue of the *Bulletin* contains a comprehensive index to its first 20 volumes.

Chairman of this convention, which in addition to the technical sessions offered field trips to caves in the Ozark Mountains, was Oscar Hawksley of Central Missouri State College. The 1960 convention of the society will be held in Carlsbad, N.M., and will include field trips to Carlsbad Caverns and other caves of the Guadalupe Mountains. Convention chairman will be J. K. Baker of the National Park Service.

GEORGE W. MOORE
U.S. Geological Survey, Menlo Park, California

Forthcoming Events

December

26–30. American Assoc. for the Advancement of Science, annual, Chicago, Ill. (R. L. Taylor, AAAS, 1515 Massachusetts Ave., NW, Washington 5.)

The following 46 meetings are being held in conjunction with the AAAS annual meeting.

AAAS Committee on Science and the Promotion of Human Welfare (B. Commoner, School of Botany, Washington Univ., St. Louis 5, Mo.). 27 Dec.

AAAS Cooperative Committee on the Teaching of Science and Mathematics (Brother G. Nicholas, Dept. of Biology, Univ. of Notre Dame, Notre Dame, Ind.). 27 Dec.

Academy Conference (A. M. Winchester, Stetson Univ., De Land, Fla.). 27–28 Dec.

Alpha Epsilon Delta (M. L. Moore, 7 Brookside Circle, Bronxville, N.Y.). 29 Dec.

American Assoc. of Clinical Chemists (A. Dubin, Cook County Hospital, Chicago 12, Ill.). 26–27 Dec.

American Geophysical Union (W. C. Krumbein, Dept. of Geology, Northwestern Univ., Evanston, Ill.). 28 Dec.

American Meteorological Soc. (K. Spengler, 3 Joy St., Boston, Mass.).

American Nature Study Soc. (E. L. Will, State Univ. Teachers College, Oneonta, N.Y.). 26–30 Dec.

American Physiological Assoc. (F. A. Hitchcock, Ohio State Univ., Columbus). 28 Dec.

American Political Science Assoc. (J. Robinson, Dept. of Political Science, Northwestern Univ., Evanston, Ill.). 28 Dec.

American Psychiatric Assoc. (E. L. Bliss, General Hospital, Salt Lake City, Utah). 28–29 Dec.

American Soc. of Criminology (D. E. J. MacNamara, New York Inst. of Criminology, Inc., New York 36). 28–29 Dec.

American Soc. of Naturalists (A. D. Hasler, Dept. of Zoology, Univ. of Wisconsin, Madison). 27–28 Dec.

American Soc. of Plant Taxonomists (L. R. Heckard, Dept. of Botany, Univ. of Illinois, Urbana). 28–30 Dec.

American Sociological Soc. (J. S. Coleman, Dept. of Sociology, Univ. of Chicago, Chicago 37, Ill.). 28–29 Dec.

American Statistical Assoc. (R. F. Winch, Dept. of Sociology, Northwestern Univ., Evanston, Ill.). 29–30 Dec.

Association of American Geographers (A. Cutshall, Univ. of Illinois, Navy Pier, Chicago 11). 29 Dec.

Association for Computing Machinery (W. F. Cahill, Goddard Space Flight Center, Silver Spring, Md.). 29 Dec.

Astronomical League (E. Halbach, 2971 S. 52 St., Milwaukee 19, Wisc.). 26 Dec.

Beta Beta Beta (Mrs. F. G. Brooks, P.O. Box 515, Ansonia Station, New York 23). 27–28 Dec.

Chicago Acad. of Sciences (R. A. Edgren, Chicago Acad. of Sciences, 2001 N. Clark St., Chicago 14, Ill.). 29-30 Dec.

Conference on Scientific Communications (G. L. Seielstad, Applied Physics Lab., Johns Hopkins Univ., Silver Spring, Md.). 28–29 Dec.

Conference on Scientific Manpower (T. J. Mills, National Science Foundation, Washington 25). 28 Dec.

Ecological Soc. of America (W. C. Ashby, Dept. of Botany, Univ. of Chicago, Chicago 37, Ill.). 28–30 Dec.

Honor Soc. of Phi Kappa Phi (L. R. Guild, 634 S. Western Ave., Los Angeles 5, Calif.). 30–31 Dec.

Illinois Geographical Soc. (Miss M. Grant, Morton Junior College, Cicero, Ill.). 28 Dec.

Institute of Management Sciences (M. M. Flood, College of Engineering, Univ. of Michigan, Ann Arbor). 29 Dec.

Metric Assoc. (J. T. Johnson, Ravenswood YMCA, 1725 Wilson Ave., Chicago 40, Ill.).

Mycological Soc. of America (D. P. Rogers, Dept. of Botany, Univ. of Illinois, Urbana).

National Assoc. of Biology Teachers (H. E. Weaver, 202 Men's Old Gym, Univ. of Illinois, Urbana). 26–30 Dec.

National Acad. of Economics and Political Science (J. Rothrock, Pan American Union, Washington 6). 29 Dec.

National Assoc. for Research in Science Teaching (J. C. Mayfield, Univ. of Chicago, Chicago 37, Ill.). 26–30 Dec.

National Assoc. of Science Writers (P. Fraley, Evening Bulletin, Philadelphia, Pa.). 27 Dec.

National Geographic Soc. (W. R. Gray, NGS, 16 and M Sts., NW, Washington 6). 30 Dec.

National Science Teachers Assoc. (R. H. Carleton, NSTA, 1201 16 St., NW, Washington, D.C.). 26–30 Dec.

National Soc. for Medical Research (R. A. Rohweder, NSMR, 920 S. Michigan Blvd., Chicago 5, Ill.). 29 Dec.

National Speleological Soc. (T. C. Barr. Jr., Tennessee Polytechnic Inst., Cookeville, Tenn.). 28 Dec.

Philosophy of Science Assoc. (W. A. R. Ley, Roosevelt College, Chicago, Ill.). 28 Dec.

Scientific Research Soc. of America (D. B. Prentice, 56 Hillhouse Ave., New Haven 11, Conn.). 29 Dec.

Sigma Delta Epsilon (Miss E. S. Anderson, Stratford Hotel, 25 E St., NW, Washington, D.C.). 26–30 Dec.

Society for General Systems Research (R. L. Meier, Mental Health Research Institute, Univ. of Michigan, Ann Arbor).

Society for the History of Technology (M. Kronzberg, Dept. of History, Case Inst. of Technology, Cleveland, Ohio).

Society of the Sigma Xi (T. T. Holme. 56 Hillhouse Ave., New Haven 11, Conn.). 29 Dec.

Society of Systematic Zoology (R. E. Blackwelder, Southern Illinois Univ., Carbondale). 26–30 Dec.

Tau Beta Pi Assoc. (R. H. Nagel, Univ. of Tennessee, Knoxville). 27 Dec.

United Chapters of Phi Beta Kappa (C. Billman, 1811 Q St., NW, Washington, D.C.). 29 Dec.

27–30. American Anthropological Assoc., Mexico City. (W. S. Godfrey, Jr., Logan Museum, Beloit College, Beloit, Wisc.)

27–30. American Astronomical Soc., Cleveland, Ohio. (J. A. Hynek, Smithsonian Astrophysical Observatory, 60 Garden St., Cambridge 38, Mass.)

27–30. American Folklore Soc., Mexico City. (MacE. Leach, 110 Bennett Hall, Univ. of Pennsylvania, Philadelphia 4.)

27–30. American Statistical Assoc., Washington, D.C. (D. C. Riley, 1757 K St., NW, Washington 6.)

27–30. Institute of Mathematical Statistics (weather control), Washington, D.C. (J. Neyman, Statistical Lab., Univ. of California, Berkeley 4.)

28–29. American Chemical Soc. (Div. of Industrial and Engineering Chemistry), symp., Baltimore, Md. (M. A. H. Emery, ACS, 18 and K Sts., NW, Washington D.C.)

28–29. Industrial Relations Research Assoc., Washington, D.C. (E. Young, Sterling Hall, University of Wisconsin, Madison.)

28–29 Mechanism of Interfacial Reaction, American Chemical Soc., annual symp, Baltimore, Md. (H. E. Hoelscher, Chemical Engineering Dept., Johns Hopkins Univ., Baltimore, Md.)

28–29. Lepidopterists' Soc., 10th annual, Ann Arbor, Mich. (E. G. Voss or W. H. Wagner, Dept. of Botany, Univ. of Michigan, Ann Arbor.)

28–29. Northwest Scientific Assoc., Spokane, Wash. (W. B. Merriam, Dept. of Geography, State College of Washington, Pullman.)

28–30. American Economic Assoc., Washington, D.C. (J. W. Bell, Northwestern Univ., 629 Noyes St., Evanston, Ill.)

28–30. American Philosophical Assoc. (eastern div.), New York, N.Y. (L. Garvin, Dept. of Philosophy, Univ of Maryland, College Park.)

28–30. American Physical Soc., Pasadena, Calif. (K. Darrow, APS, Columbia Univ., 116 St. and Broadway, New York, N.Y.)

28–30. Econometric Soc., Washington, D.C. (R. Ruggles, Dept. of Economics, Yale Univ., New Haven, Conn.)

28–30. Western Soc. of Naturalists, Los Angeles, Calif. (Y. U. Amrein, Dept. of Zoology, Pomona College, Claremont, Calif.)

28–31. Phi Delta Kappa, Columbia, Mo. (A. G. Clark, 316 Dalzell Ave., Ben Avon, Pittsburgh 2, Pa.)

28–16. Bahamas Surgical Conf., Nassau. (B. L. Frank, P.O. Box 4037, Fort Lauderdale, Fla.)

January

1–5. Electrochemical Soc., Chicago, Ill. (Electrochemical Soc., Inc., 216 W. 102 St., New York 25.)

1–5. Institute of Geographers, annual conf., Southampton, England. (A. E. Smailes, Queen Mary College, Univ. of London, Mile End Rd., London, E.1.)

3–9. Indian Science Cong. Assoc., 4th, Bombay. (B. W. Prasad, ISCA, Lakshmi Niwas, Georgetown, Allahabad 2, India.)

5–7. Recent Mechanical Engineering Developments in Automatic Control, symp., London, England. (Institution of Mechanical Engineers, 1 Birdcage Walk, London, S.W.1.)

6–8. Northeastern Weed Control Conf., 14th annual, New York, N.Y. (M. G. Wiltse, Chairman, Public Relations Committee, Dow Chemical Co., 916 Shoreham Bldg., 15 and H Sts., NW, Washington 5, D.C.)

7–10. Radioactive Isotopes, 4th intern. symp., Bad Gastein, Austria. (R. Hofer, Isotopen-Laboratorium, II. Medizinische Universitäts Klinik, 13, Garnisongasse, Vienna 9, Austria.)

8–11. Sanitary Engineering Conf., ASCE, Cincinnati, Ohio. (E. S. Kirkpatrick. ASCE, 33 W. 39 St., New York 18.)

11–13. American Acad. of Allergy, Hollywood-by-the-Sea, Fla. (J. O. Kelley, 756 N. Milwaukee St., Milwaukee 2, Wisc.)

11–13. Arctic Geology, 1st intern. symp. Calgary, Alberta, Canada. (D. W. R. Wilson, Arctic Symposium Committee, P.O. Box 100, Calgary, Alberta, Canada.)

11–13. Reliability and Quality Control, natl. symp., Washington, D.C. (N. S. Hibshman, AIEE, 33 W. 39 St., New York 18.)

11–15. Society of Automotive Engineers, annual, Detroit, Mich. (R. W. Crory, Meetings Operation Dept., SAE, 485 Lexington Ave., New York 17.)

11–25. Effects of Atomic Radiation, New York, N.Y. (R. Appleyard. Scientific Committee on the Effects of Radiation, United Nations, New York 17.)

12–15. Society of Plastics Engineers, 16th annual conf., Chicago, Ill. (T. A. Bissell, SPE, 65 Prospect St., Stamford, Conn.)

14–18. American Inst. of Mining, Metallurgical, and Petroleum Engineers, annual, New York, N.Y. (E. O. Kirkendall. AIMMPE, 29 W. 39 St., New York 18.)

17–30. Bahamas Medical Serendipity Conf., 2nd, Nassau. (B. L. Frank, P.O. Box 4037, Fort Lauderdale, Fla.)

18–21. American Astronautical Soc., 6th annual, New York, N.Y. (A. P. Mayernik, AAS, 6708 53 Rd., Maspeth 78, N.Y.)

19–21. American Meteorological Soc., 40th annual, Boston, Mass. (K. C. Spengler, 3 Joy St., Boston.)

19–21. Congenital Malformations, CIBA symp. (by invitation only), London, England. (G. E. W. Wolstenholme, 41 Portland Pl., London, W.1.)

21–23. American College of Surgeons, Louisville, Ky. (H. P. Saunders, 40 E. Erie St., Chicago 11, Ill.)

23–28. American Acad. of Orthopedic Surgeons, Chicago, Ill. (J. K. Hart, 116 S. Michigan, Chicago 3.)

24–29. American Rocket Soc., Princeton, N.J. (J. J. Harford, ARS, 500 Fifth Ave., New York 36.)

25–28. Institute of the Aeronautical Sciences, 28th annual, New York, N.Y. (IAS, 2 E. 64 St., New York 21.)

25–28. Plant Maintenance and Engineering Show, Philadelphia, Pa. (R. S. Wolcott, Clapp & Poliak, 341 Madison Ave., New York 17.)

(See issue of 13 November for comprehensive list)

New Products

The information reported here is obtained from manufacturers and from other sources considered to be reliable. Neither Science *nor the writer assumes responsibility for the accuracy of the information. A coupon for use in making inquiries concerning the items listed is included in the post card insert.*

■ ANTENNA-PATTERN INTEGRATOR uses a synchro-driven sampler to provide 1000 samples of antenna signal for 360-deg rotation of the antenna. The integral sum, proportional to power radiated, is read out on decimal counting units with accuracy said to be ±0.5 percent of maximum count. The instrument may be applied generally to integration of a-c or d-c signals in respect to shaft rotation. (Scientific-Atlanta Inc., Dept. Sci241, 2162 Piedmont Rd., NE, Atlanta 9, Ga.)

■ MICROWAVE WATTMETER is powered by a mercury cell and provides power-measurement ranges of 0 to 1 mw and 0 to 10 mw, or −10 to 0 dbm and 0 to 10 dbm. Measurements over additional ranges may be accomplished through use of couplers and attenuators. Accuracy is said to be ±5 percent of full scale. Dimensions are 6 by 2¾ by 2⅛ in. and weight is 1 lb. (Radar Measurements Corp., Dept. Sci242, 190 Duffy Ave., Hicksville, N.Y.)

■ MICRO-PIPETTER has a maximum capacity of 1 ml and will measure samples as small as 0.001 ml. Accuracy is said to be ±2 percent for a 0.001-ml sample and ±0.25 percent for a 0.1-ml sample. The pipette is hydraulically operated with integral well and valve assembly. All metal parts are stainless steel or aluminum; the pipette itself is Kel-F. Vertical travel is 7½ in.; horizontal travel is 42 in. (Nuclear-Ohio, Inc., Dept. Sci243, Bay Village, Ohio)

■ LABORATORY FURNACE of resistance type operates at temperatures to 5000°F. Heating chamber is 4 in. in diameter and 8 in. high. The furnace uses an inert-gas atmosphere with a graphite heating element and carbon insulation. Power supply, gas control valves, pyrometers, and fittings for cooling are supplied. Automatic temperature controls can be furnished. Power input is 15 kw at 4500°F. (Curtiss-Wright Corp., Dept. Sci245, P.O. Box 689, Santa Barbara, Calif.)

■ HIGH-TEMPERATURE SOURCE is a small plasma generator powered by a standard welding generator. The unit is basically a high-intensity electric arc consisting of a water-cooled tungsten cathode and a copper anode. Commercial bottled gas is used to produce the jet. Temperature range is said to

be from 2500° to above 12,000°F, and operation at 5 kw power level is said to be possible for an almost unlimited period. Available nozzles are 0.25, 0.375 and 0.50 in. in inside diameter. (Avco, Dept. Sci247, 201 Lowell St., Wilmington, Mass.)

■ INDUCTION-HEATING UNIT, specially designed for processing semiconductors and other conductive materials, operates at approximately 4 Mcy/sec with output power of 10 kw. This output is furnished at a work station centrally located on the front panel. Power consumption is 20 kva max, 230/460 volt, three phase, 60 cy/sec, at 90 percent power factor. Cooling-water consumption at full load is 6 to 7 gal/min at 80°F. Outside dimensions are 40 by 40 by 76 in. (Lindberg Engineering Co., Dept. Sci256, 2450 W. Hubbard St., Chicago 12, Ill.)

■ STRIP-CHART RECORDER of potentiometer type is a two-channel recorder with both pens traversing the full width of a 5-in. chart. Range is adjustable from 0 to 9 mv to 0 to 150 mv. Zero can be set anywhere. Chart-speed options range from ⅛ in./hr to 8 in./min. Full-scan balancing time is 1 second, and accuracy is said to be ±1 percent of full scale. Portable and panel-mounting versions are available. (Varian Associates, Dept. Sci257, 611 Hansen Way, Palo Alto, Calif.)

■ PACKAGED TRANSFORMER PRIMARY is a toroidal coil of 400 turns on a strip-wound silicon-steel core. To obtain high alternating currents at a desired low voltage, the proper number of turns is wound through the center opening. Rating is 120 volts, 50 to 60 cy/sec, and 150 va output. The device can also be used in reverse as a current transformer. (Superior Electric Co., Dept. Sci248, 83 Laurel St., Bristol, Conn.)

■ TWO-COORDINATE MEASURING COMPARATOR covers a plate area up to 18 by 18 in. Calibrated screws read to 1 μ along the entire length. Magnification of the observing microscope is variable between 12 and 28. Position of the rotary-stage vernier is readable to 20 sec through 360 deg. A filter magazine permits enhancement of contrast, and a choice of four reticle shapes is provided. Stage position is automatically recorded by depression of a foot switch. (David W. Mann, Inc., Dept. Sci261, Lincoln, Mass.)

■ STRAIN-GAGE INDICATOR is a disk-type, null-balance unit using a servo-driven slidewire to balance an internal bridge circuit. Specified accuracy is ±0.15 percent of full scale. Less than 1 square foot of panel area is required for monitoring. (Baldwin-Lima-Hamilton Corp., Dept. Sci251, 42 Fourth Ave., Waltham 54, Mass.)

■ RECORDING ATTACHMENT permits conversion of two-pan analytical balance for automatic plotting of weight changes. Range is continuously variable from 10 mg to 5 gm full scale. The attachment is designed to be used with a potentiometric recorder of at least 5.5-mv sensitivity. The converter comprises an amplifier and a balance attachment. The latter consists of a differential transformer that produces an a-c voltage proportional to balance-arm deflection, and a restoring coil that applies a field to restore balance. Part of the voltage required to provide the latter field is recorded. The recorded signal is said to be a linear function of weight within ±1 percent of full scale. Effects of balance vibration are filtered electrically. (Fisher Scientific Co., Dept. Sci258, 717 Forbes St., Pittsburgh 19, Pa.)

■ TEST TUBE MIXER accommodates 15- and 16-mm test tubes using a separate shaded-pole motor for each tube. Rotational speed is 3000 rev/min. A special motion is said to create a vortex that produces rapid mixing. Capping of tube is unnecessary. A four-tube and a two-tube model are available. (Kraft Manufacturing Co., Dept. Sci252, 68-60 140th St., Flushing 67, N.Y.)

■ CLOT TIMER is a bench-top unit with a self-contained heat source that maintains reagents at 37°C. Addition and mixing of reagents is synchronized with the start of a digitally reading timer. The timer stops automatically when a transistorized detector circuit senses the endpoint of the reaction. Reaction time is displayed to the nearest 0.1 sec. (Mechrolab, Inc., Dept. Sci249, 601 Main St., Redwood City 1, Calif.)

■ MEASUREMENT MICROPHONES include the model 4131 cartridge with flat free-field response from 20 to 18,000 cy/sec and the model 4132 cartridge with a flat pressure response conforming to A.S.A. standard Z S 24.8-1949 L-type microphones. Cartridges are dimensionally interchangeable with the A.S.A. standard microphone. (B & K Instruments, Inc., Dept. Sci250, 3044 W. 106 St., Cleveland 11, Ohio)

■ AMPLIFIER, completely transistorized, combines high-voltage handling ability with high input impedance. Voltage gain is 1.0000 ± 0.0001. Input range is ±105 volts. Long-term drift is ±300 μv. Input impedance is said to be thousands of megohms and rise times on the order of a few microseconds. (Adage Inc., Dept. Sci253, 292 Main St., Cambridge 42, Mass.)

JOSHUA STERN

National Bureau of Standards, Washington, D.C.

Letters

(Continued from page 1530)

values of zero and 18,000 tons. On 16 July, after the test, our first measurement of what had really happened gave 20,000 tons. That morning General Groves communicated this figure to the War Department, for transmission to the Secretary of War and the President, who were in Potsdam, meeting with representatives of the United Kingdom and the U.S.S.R.

I should perhaps add that when a scientific panel met with the Secretary of War's Interim Committee on 31 May 1945, Secretary Stimson and General Marshall were both present. I remember reporting that we expected one model of the bomb to be many times more powerful than the other, but that we were so uncertain of our ground that they might turn out in quite opposite order. I also remember that on that occasion we discussed a possibility, which had been reported to the Government in 1942, of developing bombs in the multi-megaton range by the use of fissionable material, heavy hydrogen, and natural uranium. Here, too, our early designs were to change with time.

ROBERT OPPENHEIMER
Institute for Advanced Study, Princeton, New Jersey

I have seen a copy of the letter which Robert Oppenheimer has written you correcting statements in Amrine's book, which were developed in E. U. Condon's review (which I have not seen).

I have no way of knowing whether Amrine was misled by statements on the subject made in my book *Churchill, Roosevelt, Stalin*. But I have a sense of responsibility for sharing in the diffusion of this erroneous impression of the anticipated explosive power of the atomic bomb. How I came to overlook the first paragraph of General Groves' memorandum to General Marshall quoted by Oppenheimer I do not understand. I suppose an appropriate penalty—the ultimate proof of my mistake—would be to be blown up by one.

HERBERT FEIS
York, Maine

I would like to correct some of the erroneous impressions a reader would gain from E. U. Condon's "review" of Michael Amrine's book *The Great Decision*, which appeared in a recent issue of *Science*.

This "review" goes far afield from the content of the book, and this despite a confession of little first-hand knowledge. While such frankness may be refreshing, equal frankness would have made Condon add the statement that he is not particularly fond of me.

I would not bother to answer Condon except for the fact that a publication such as *Science* has seen fit to print his "review" without checking his alleged facts. Many are so preposterous as to be obviously incorrect. The conclusions supposedly drawn therefrom present me in an entirely false position.

Condon's charge that I worked "to hobble and frustrate cooperation" with the British simply is not true. Because neither I nor anyone else could even guess at that time how the atomic future might unfold, because I felt that I had no right to give away information developed with United States funds, and because I believed that orderly procedures were essential in such a large and widespread effort, I did not want the previously highly informal communications to continue. These had not been unreasonable before the United States entered upon its major atomic effort. I felt that now specific presidential approval was required for any significant passing of information. I was not alone in this opinion. An executive agreement between President Roosevelt and Prime Minister Churchill, defining British-American relationships in this area, was the first essential to the formalization of the interchange of information.

Yet, it was not until the Quebec Conference of August 1943 that the necessary basic agreement was reached. I was not responsible for the fact that this agreement was not arrived at sooner. As soon as it was signed, proper arrangements were promptly made and the furnishing of information was controlled in accordance with the terms of the Quebec Agreement.

As to Chalk River, Condon ignores the fact that its parent organization was already being established at Montreal before I was placed in charge of the Manhattan project. This laboratory was never a part of the American project. A number of its key personnel were Free French, and France was still under German control. This added to our difficulties. In spite of these complications, the Canadian-British effort always received essential information from us to the extent necessary for the work it was carrying on. The Canadian Government always seemed satisfied with this cooperation. The Royal Commission appointed to investigate Russian espionage in Canada in its report of 27 June 1946 said (page 617):

> From the beginning there was the closest cooperation in scientific research between Canada, the United Kingdom, and, later the United States. While some secrets were not fully shared, as in the case of some details concerning the atomic bomb, the results of continuing research work by scientists in one country were in almost all cases at once communicated to their opposite numbers in the other two.

All available essential data and estimates respecting the bomb were made known to our policy makers when decisions were being made on the use of the bomb. General Marshall, Secretary Stimson, and the President (Roosevelt and later Truman) were kept informed as to the probable effect of the bomb on the conduct of the war. It was my responsibility to keep General Marshall and Secretary Stimson fully informed as to the changes in scientific thinking, including my own, on the probable power of the bomb. And I fulfilled that responsibility.

It is true that I did not know enough physics to insure the accuracy of any estimate, but, I might add, neither did anyone else. We simply did not have in the Manhattan organization a scryer of sufficient ability to do this. The estimates included in my memorandum of 30 December 1944 were based on my latest discussions with, and information from, Oppenheimer and his associates at Los Alamos. I might add that as late as the middle of May 1945 the

responsible heads at Los Alamos felt that the explosive force of the first implosion-type bombs would fall somewhere between 700 and 1500 tons.

Actually my memorandum to General Marshall of 30 December 1944, of which Condon makes such a point, has been completely misrepresented—and this is not the first time, although, with a few omissions (all nonessential for the present discussions), it was published in full with the Yalta papers and is thus readily available to any responsible writer. The first part of this memorandum read as follows (the remainder dealt with informing Admiral Nimitz and the air commander in the Guam area as to the atomic possibilities):

It is now reasonably certain that our operation plans should be based on the gun type bomb, which, it is estimated, will produce the equivalent of a ten thousand ton TNT explosion. The first bomb, without previous full scale test which we do not believe will be necessary, should be ready about 1 August 1945. The second one should be ready by the end of the year and the succeeding ones at . . . intervals thereafter.

Our previous hopes that an implosion (compression) type bomb might be developed in the late spring have now been dissipated by scientific difficulties which we have not as yet been able to solve. The present effects of these difficulties are that more material will be required and that the material will be less efficiently used. We should have sufficient material for the first implosion type bomb sometime in the latter part of July. This bomb would have an effect which would be equivalent to about 500 tons of TNT. During the remainder of 1945 it is estimated that we can produce . . . additional bombs. The effectiveness of these should increase towards 1000 tons each as development proceeds and, if some of our problems are solved, to as much as 2500 tons.

The plan of operations while based on the more certain more powerful gun type bomb also provides for the use of the implosion type bombs when they become available. . . . The time schedule must not be adversely affected by anything other than the difficulties of solving our scientific problems.

For a considerable period prior to this we had fully expected to use the gun-type bomb first and, as we did at Hiroshima, to use it without previous test. Actually, the reading of this memorandum by President Roosevelt was only a minor portion of the conference, and further information was given him later—the next day, I believe—by Secretary Stimson. That the President was fully informed is well illustrated by a statement of Secretary of State Stettinius, who, in his book *Roosevelt and the Russians*, mentioned his meeting with President Roosevelt shortly after this date, and the latter's

telling him on this occasion that if a bomb were dropped on New York City at 42nd Street and Broadway, the resulting explosion would lay New York low.

President Truman was fully aware of the potential power of the bomb from the time that he had his first conference on it with Secretary Stimson and me on 25 April 1945. At this meeting the President was not shown the 30 December memorandum. He did see, among other papers, Secretary Stimson's memorandum of 25 April, the first paragraph of which read as follows:

"Within four months we shall in all probability have completed the most terrible weapon ever known to human history, one bomb of which could destroy a whole city" (Stimson and Bundy, *On Active Service in Peace and War*, vol. 2, p. 635). Could anything have been clearer as to the import of the bomb?

From the very first, the messages to Potsdam were definite and clear as to the magnitude of the Alamogordo explosion. It was described in one of the cables as being visible for over 200 miles and audible for more than 50. Nothing could be further from the truth than the charge that there was any design to minimize the bomb.

I trust that because of the scientific reputation and value of your publication, because unanswered statements, no matter how unfounded, tend to become history, and because of the palpable injustice to me of Condon's "review," you will publish this letter in full.

LESLIE R. GROVES

Darien, Connecticut

In response to the editor's invitation to comment on the letters from J. R. Oppenheimer and General Groves, I would say that I welcome the attention thus focused on Amrine's book. If there are errors in that book, this underscores the need for scholarly historical research on the full story of uranium bomb development.

Groves confirms that he did restrict exchange of information with our British partners—exchange which had been well established before he came on the project. More than a year and a half was lost before Roosevelt and Churchill reestablished that cooperation, in August 1943. How much delay resulted from that unfortunate policy will need to be assessed when the real history is written.

When that history is written one of the least important parts will be that which says whether General Groves is particularly fond of me or vice versa. A complete history will tell of the frantic efforts of one man to retain personal control of atomic energy in America by trying to rush the May-

Johnson bill through Congress; of how he was foiled in this by some of the scientists; and of his embittered efforts to take revenge. Such a history will examine carefully whether these things were related to the doings of the House Committee on Un-American Activities in 1948 and 1949, which deprived this country of the services of so many brilliant young American scientists.

E. U. CONDON
Washington University,
St. Louis, Missouri

Radionuclides and Bone Cancer

A serious error exists in J. G. Kaplan's recent letter (*1*). It was stated that the Russians had observed bone cancers developing in dogs about 3 years after the injection of 0.1 microcurie of strontium-90 per kilogram. Actually, the radionuclide used in these Russian studies was not strontium-90, as erroneously reported by Engstrom *et al.* (*2*), but the much more dangerous thorium-228 (*3*).

The injected amounts of strontium-90 which it has been proved cause bone cancer are much higher than 0.1 μc/kg. Finkel (*4*) found that the incidence of osteogenic sarcomas in 90 mice injected with 44 μc of strontium-90 per kilogram was somewhat higher (6 percent) than that in 150 controls (2 percent), but the probability of this being due to chance occurrence was 20 to 30 percent. In a current study in our laboratory, 60 beagles have been injected with from 0.5 to 100 microcuries of strontium-90 per kilogram. Thus far only one dog, injected with 94 μc/kg, has developed a bone tumor. These results do not imply that lesser amounts of strontium-90 are without effect. However, they do illustrate the enormous difficulty in experimentally determining the consequences of very small injections of strontium-90, such as 0.1 μc/kg.

The maximum permissible body burdens of strontium-90 and radium-226 should be set so as to give the same probability (or improbability) of causing undesirable effects. If the ratio of these limits is based on the observed biological effects of strontium-90 and radium-226 in experimental animals, the maximum permissible body burdens, for occupational workers, of 2 microcuries of strontium-90 and 0.1 microcurie of radium-226 correspond fairly well.

In view of Kaplan's opinion of the "obvious impertinence" of physicists in biological questions, it seems strange that he would base the heart of his argument on the theoretical calculations of Rolf Björnerstedt, who is a physicist.

C. W. MAYS
Division of Radiobiology,
University of Utah, Salt Lake City

References

1. J. G. Kaplan, *Science* **130**, 728 (1959).
2. A. Engstrom, R. Björnerstedt, C. J. Clemedson, A. Nelson, *Bone and Radiostrontium* (Wiley, New York, 1957), p. 133.
3. R. Björnerstedt, in a letter (9 July 1959) citing results reported in *Conference on the Remote Consequences of Injuries Caused by the Action of Ionizing Radiation*, F. G. Krotkov *et al.*, Eds. (State Med. Lit. Press, Moscow, 1956).
4. M. P. Finkel, *Science* **128**, 637 (1958).

Meteorology for Non-Science Majors

There is general agreement that all college students should have an experience in science before they receive their diplomas. Very rarely, it seems, is meteorology recommended as one of the courses the student should take to satisfy his science requirement. This is regrettable in view of the fact that meteorology has within it all the ele-

ments which a science course for the non-science major should have.

Of course there are reasons for the present situation. People have been concerned about the weather since "the year 1," yet the problems involved in making enough observations of the atmosphere to find out what is going on and then devising ways of handling and digesting the masses of data have been so immense that modern meteorology can be considered a rather young branch of science.

Despite the vast meteorological training program of World War II, only a small percentage of these trained men went back into academic life after the war, and of those who did, only a fraction found academic positions in which it was possible to teach meteorology.

The professional academic meteorologist still finds it difficult to disengage himself from the stereotype of the fumbling-bumbling weatherman fostered by the cartoonists in the public press. And his timid dean hesitates, on the basis of scientific respectability, to encourage development of a course in which (he opines) the subject matter is more of an art than a science.

Despite difficulties such as these, meteorology does seem to be coming of age as a valid academic subject studied by many more than just a few specialists. It is of this that I wish to speak, stating what appears to be a case for meteorology.

If a student who is a non-science major is to have only a fleeting experience of science, we first might ask what elements his experience should contain. Certainly his science course should have high interest value; it should leave him with an increased understanding of science; it should illustrate general principles in such a way that their broad applicability may be perceived; it should give him accurate information about the way in which science relates to human activity; it should have maximum carry-over value with respect to situations which he will encounter in later years; and, finally, it should not involve just an accumulation of facts—it should be developed around an integrative theme which provides coherence and continuity. How does meteorology rate according to these several criteria?

1) Every person lives out his life at the bottom of the "sea of air and water vapor" which produces the weather. The weather is of great interest to everyone because it necessarily affects everyone. Therefore it seems to make very good sense for an educated person to have an understanding of the circumstances under which the several phenomena—wind, cloud, rain, lightning, and so on—are produced, and the reasons for them.

2) Meteorology has high carry-over value. The person who learns to identify clouds and to read these "signposts of the sky" will have gained access to a source of lifelong enjoyment denied to other people. Each hour of each day holds interest for one who has become familiar with clouds and their portent.

3) Meteorology is a subject which beautifully illustrates general physical principles. In discussing energy and its transformations, an instructor can start with mass-energy transformations which take place in the sun's interior, lead the student through application of radiation laws across 93 million miles of space to the outer edge of the earth's atmosphere, follow the diminution of the solar beam through the atmosphere, apply the laws of motion to illustrate how the atmospheric circulation is maintained, and so forth.

4) In meteorology, excellent examples of the way pure science relates to human activity are legion. For example, the tornado represents concen-

trated atmospheric violence which often results in deaths and heavy property damage. When we combine the increased understanding of the dynamics of storms of this type (as provided by meteorological research) with the research efforts in electron physics which produced radar and television, and then use radar for storm detection and television to inform the populace about the progress of a storm, death and destruction are reduced to a minimum.

Meteorology should be much more widely offered, and students who are non-science majors should be encouraged to take this course as a satisfying means of completing their physical-science requirement.

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Arctic Waters

Regarding the report entitled "Tritium tracer in Arctic problems," by Giletti and Kulp [*Science* **129**, 901 (1959)] and the subsequent exceptions taken by Barnes and Coachman [*Science* **130**, 273 (1959)], I am not sure whether I am lessening or adding to the confusion with the following remarks, but there are some factors very pertinent to the matter which I feel should be brought out.

As Barnes and Coachman pointed out, the curves in Giletti and Kulp's Fig. 1 do not agree with the reference (*6*) cited in the figure's caption. The figure presents temperature-depth curves for "Icicle" stations, 2, 5, 10, and 11. The first two curves were derived not from reference *6* but rather from reference *5*, cited in the text [namely, L. V. Worthington, *Woods Hole Oceanog. Inst. Tech. Rept. 53-92* (1953)]. The other two curves were drawn from data gathered by Goldstein in 1955. I am not aware that the curves from the last two stations have appeared in previous literature, but the data received some small distribution.

Barnes and Coachman have used a bottom temperature read from the curve for station 11 to show that this water, if it were a mixture of Atlantic water and arctic surface water as proposed by Giletti and Kulp, was at best only 20 percent arctic and 80 percent Atlantic water. Giletti and Kulp replied that this accorded with their theory.

I should like to point out two items. First, the bottom temperature shown in the station-11 curve is actually an extrapolated value, the deepest observation having been made 22 meters above the bottom. This extrapolated bottom temperature is about +0.1°C; the deepest observation shows a temperature of +0.21°C. I am not sure that other extrapolators would have drawn the curve in quite the same manner as it appears in the Giletti and Kulp report.

The second item is, I believe, the crux of the matter. The tritium sample which led Giletti and Kulp to their conclusions was collected at a depth of 400 meters at a location very close to station 11. Therefore it would seem to be only proper to use the interpolated 400-meter temperature of +0.35°C given by Giletti and Kulp for station 11 in carrying out the computations proposed by Barnes and Coachman for figuring the percentages of Atlantic and arctic surface waters.

If this is done, we find that if surface water having a temperature of −1.9°C (which it would have if the salinity concentration were to be raised the requisite amount by freezing) is mixed, in