

PLACES AND PERSONS

THE WASHINGTON SCIENCE SCENE

Soon after my arrival in Washington, I was advised to join the American Chemical Society, which made me automatically a member of the local section of the Chemical Society of Washington, and also to join the Philosophical Society of Washington. When the regular meetings started in the fall of 1924, I was introduced to a number of the members and officers. I have never regretted the time I spent during the next 25 years in participating in the activities of scientific societies and informal colloquia. These activities brought to me the acquaintance and, indeed, the friendship of many very interesting people, vastly extended my education, and gave me much intellectual pleasure.

Joseph Henry, the first Secretary of the Smithsonian Institution, expressed my feeling more generally and more elegantly years before I was born.

Man is a sympathetic being, and no incentive to mental exercise is more powerful than that which springs from a desire for the approbation of his fellow men; besides this, frequent interchange of ideas and appreciative encouragement are almost essential to the successful prosecution of labors requiring profound thought and continued mental exertion. Hence it is important that those engaged in similar pursuits should have opportunities for frequent meetings at stated periods....Furthermore, a society of this kind becomes a means of instruction to all its members, the knowledge of each becoming, as it were, the knowledge of the whole.

In the first three decades of this century, the scientific life of Washington was young, vigorous, and uncomplicated. Organizations such as the Bureau of

These descriptions are excerpted from an as yet unpublished autobiography by R. E. Gibson.

Standards, the Naval Observatory, the Coast and Geodetic Survey, the Bureau of Chemistry of the Department of Agriculture, the Surgeon General's Office, the Hygienic Laboratory, the Carnegie Institution, the Naval Research Laboratory, the Geological Survey, the Smithsonian Institution, the Bureau of Ethnology, and others had attracted men of outstanding scientific caliber who, while engaged in research inspired by practical problems, retained a deep interest in the fundamentals of their specific disciplines, keeping themselves abreast of these disciplines not only for their own intellectual satisfaction, but also as sources of material for their applied research. These men and women pooled their knowledge and experience in the meetings of the various scientific societies.

Let me give a description of two of the societies. The oldest, the Philosophical Society of Washington, held its first meeting on March 13, 1871, with Joseph Henry in the Chair. The society "embraced all sciences, except those, if they be sciences, of speculative thought." In those days, a well educated, intellectual man could be expected to take an intelligent interest in the whole spectrum of the sciences, from mathematics to descriptive biology. Recognizing this, Joseph Henry in his first presidential address (November 1871) remarked:

With so many facilities as exist in the City of Washington for the pursuit of science, this Society would be derelict in duty did it fail to materially aid, through communication of thought and concert of action, the advancement of the great cause of human improvement.

Today, people are still trying feverishly to implement, without much knowledge of the past, the wis-

Geophysical Laboratory, Carnegie Institution of Washington.



dom of Joseph Henry. Although the Philosophical Society made provision for the establishment of sections to accommodate members with specialized interests, that proved to be inadequate to cope with the rapid advances in all branches of science. As a result, specialists formed societies of their own – the anthropologists in 1878, the biologists in 1880, the chemists in 1884, the entomologists in 1884, the geographers in 1888, the geologists in 1893 – so that for a long time, the chief interests of members of the Philosophical Society lay in the fields of the more mathematically dependent sciences: physics, astronomy, geodesy, geophysics, meteorology, etc.

Prior to the end of World War II, the meetings of the Philosophical Society of Washington were devoted chiefly to papers and informal communications by members or local people introduced by members. It formed a valuable forum where an investigator could present a recently completed piece of work for the edification and criticism of his fellows, or where interesting pieces of information gained by reading or experience could be aired.

Furthermore, through the bulletin of the Society, and later, through minutes published in the *Journal of the Washington Academy of Sciences*, the priority date of the presentation of a paper was established. (That was before the days when newspapers became a common vehicle for the early publication of scientific results and also before the days when the lecturer from out of town, with accompanying expenses, became popular.)

The Chemical Society of Washington was organized in 1884, some nine years before the national society of which it is now a local section. The Society is very proud of its seniority and always adheres to its original name. I enjoyed very much the meetings of, and the social contacts made in, the Society. The intellectual fare served up at its meetings was choice, the membership consisting of chemists, mainly hand-picked men and women attracted to Washington by senior officials of international reputation and by the opportunity to tackle vital and interesting problems. In those days, the regulatory activities of the federal government were minimal.

Compared with the somewhat serious and staid members of the Philosophical Society, the chemists were a rather earthy and convivial bunch. They smoked pipes, enjoyed a glass or two of beer, played poker, and had an inexhaustible supply of diverting anecdotes in addition to taste for music and the other arts. I found their company much to my liking. In those days the Chemical Society was run largely by members from the Bureau of Standards, the Bureau of Chemistry of the Department of Agriculture, and the Geophysical Laboratory. I was soon involved in various committees and was elected president in 1931, succeeding H. T. Herrick of the Bureau of Chemistry, a man who was not only a good chemist and administrator, but who always could be relied on for the latest in bawdy anecdotes.

WASHINGTON PHYSICS COLLOQUIUM

The year 1926 saw the rise of quantum mechanics with its profound implications for the future course of theoretical physics and chemistry. Gregory Breit (Department of Terrestrial Magnetism, Carnegie Institution of Washington) read the articles of Heisenberg, Schrödinger, and other pioneers in this field as they appeared in the journals. In order to crystallize his own new knowledge and to share it with his colleagues, Breit organized a small colloquium that met every two weeks, sometimes more frequently, in the library of the Department of Terrestrial Magnetism (DTM). Usually about a dozen young physicists from laboratories all over Washington were in attendance.

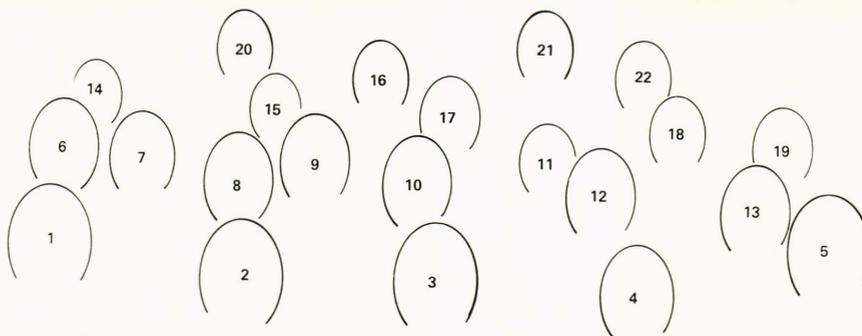
After Dr. Breit left Washington to become professor of physics at New York University in 1929, the Washington Physics Colloquium kept its unorganized identity, meeting at irregular intervals. The advent of George Gamow (1934) and Edward Teller (1935) at The George Washington University saw an increase in the frequency and a broadening of the discussions at the Colloquium's meetings. Several discussions stand out in my memory. One evening, Sterling B. Hendricks gave a fascinating and masterly account of his work on the X-ray examination of certain clay minerals, correlating the structure he found with those properties of clay recognized as of practical importance to some and a nuisance to others interested in the use of soils.

Another memorable meeting was held on January 26, 1939. Professors Niels Bohr and Enrico Fermi were present. The discussion centered around the experimental work of O. Hahn and O. Strassman in Germany on the bombardment of uranium with neutrons. Bohr and Fermi told of the interpretation of those results that had just been given by Otto R. Frisch and Lise Meitner, who both used the term "atomic fission" for the first time. Much excitement, modified by some skepticism, prevailed. That evening, R. B. Roberts, R. C. Mayer, and L. R. Hafstad repaired to their laboratory at DTM to search for the extremely high energy particles that Frisch and Meitner suggested would be produced when uranium was bombarded with neutrons. By the end of the meeting, Roberts, Mayer, and Hafstad were able to demonstrate the existence of these very high energy particles to Bohr and Fermi. It was thought at the time that that was the first time the experiment (which pioneered the Atomic Age) had been done in the United States. Subsequently it was learned that Fowler and Dodson had observed these particles on the same day at The Johns Hopkins University and that a day before (January 25), Dunning and his co-workers had successfully performed similar experiments at Columbia University.

The last meeting of the Washington Physics Colloquium was held at DTM on April 29, 1954. It was not so much a meeting as a reunion of participants who went on to distinguished careers in industry, universities, and government establishments.



The Washington Physics Colloquium at its final meeting on April 29, 1954. The diagram identifies those who attended (including three directors of The Johns Hopkins University Applied Physics Laboratory).



- 1. ROSS GUNN**
Naval Research Laboratory; Director, Physical Research, U. S. Weather Bureau.
- 2. HAROLD C. UREY**
The Johns Hopkins University; Professor of Chemistry, Columbia University; Nobel Laureate.
- 3. RALPH E. GIBSON**
Geophysical Laboratory, Carnegie Institution of Washington; Director, The Johns Hopkins University Applied Physics Laboratory.
- 4. LOUIS R. MAXWELL**
Leader, Basic Research, Naval Ordnance Laboratory.
- 5. SAMUEL K. ALLISON**
Carnegie Institution of Washington; Director, Institute for Nuclear Studies, University of Chicago.
- 6. JOSEPH KAPLAN**
The Johns Hopkins University; Director, Institute of Geophysics, University of California at Los Angeles.
- 7. GREGORY BREIT**
Department of Terrestrial Magnetism; Professor of Physics, Yale University and the State University of New York at Albany.

- 8. EDWARD TELLER**
The George Washington University; Director, Lawrence Livermore Radiation Laboratory.
- 9. FREDERICK S. BRACKETT**
Department of Agriculture; Smithsonian Institution; National Cancer Institute.
- 10. OTTO LAPORTE**
University of Michigan; National Bureau of Standards.
- 11. LAWRENCE R. HAFSTAD**
Department of Terrestrial Magnetism, Carnegie Institution of Washington; Director, The Johns Hopkins University Applied Physics Laboratory.
- 12. FRED L. MOHLER**
Chief, Mass Spectroscopy Section, National Bureau of Standards.
- 13. WILLIAM F. MEGGERS**
Chief, Spectroscopy Section, National Bureau of Standards.
- 14. LYMAN BRIGGS**
Director, National Bureau of Standards.
- 15. WILLIAM H. CREW**
Naval Research Laboratory; U.S. Naval Academy; Dean, College of Engineering, Air Force Institute of Technology.

- 16. PAUL D. FOOTE**
National Bureau of Standards; Director of Research, Gulf Oil Corporation.
- 17. KARL F. HERZFELD**
The Johns Hopkins University; Professor of Physics, The Catholic University of America.
- 18. EDWARD O. HULBURT**
Director, Naval Research Laboratory.
- 19. NORMAN P. HEYDENBURG**
Department of Terrestrial Magnetism, Carnegie Institution of Washington.
- 20. FERDINAND G. BRICKWEDDE**
National Bureau of Standards; Dean, College of Chemistry and Physics, Pennsylvania State University.
- 21. MERLE A. TUVE**
Director, Department of Terrestrial Magnetism, Carnegie Institution of Washington; Director, The Johns Hopkins University Applied Physics Laboratory.
- 22. STERLING B. HENDRICKS**
Department of Agriculture; Home Secretary, National Academy of Science

THE NATIONAL DEFENSE RESEARCH COMMITTEE

The declaration of war between Britain and Germany in September, 1939, started a train of events which would lead inexorably to profound changes in the political history of the world and the lives of all its inhabitants, including myself.

As far as I was concerned, these changes took place very slowly; indeed, for a year, little effect on my daily routine was noticeable. In fact, I was in one of the most productive phases of my scientific career. I had found that studies of liquids and liquid solutions subjected to changes of temperature and pressure that kept their specific volumes constant produced results which threw new light on the actions and reactions of their constituent molecules and on the molecular structure of liquids in general. Not only were we able to obtain the usual pressure, volume, and temperature relations and from them calculate a number of thermodynamic properties, we also were able to measure the effects of pressure and temperature on the absorption spectra of colored solutions, and on refractive indices of organic liquids.

The war seemed very far away, but it was coming closer and by mid-1940 I began to get worried about what I should do. As a British subject my first thought was to return to Britain and volunteer for some war service. But since I had no contacts in Britain to steer me into an appropriate job, I gave up the idea and looked around for some way of contributing to the war against Hitler & Co. in this country. The National Defense Research Committee (NDRC) seemed to furnish such an opportunity.

In 1937, a committee established by the National Research Council to advise on "Scientific Aids to Learning" brought together periodically in Washington a number of highly competent scientists and engineers, among whom were Frank Jewett, James B. Conant, Richard C. Tolman, K. T. Compton, and Vannevar Bush, then president of the Carnegie Institution of Washington. After each meeting, they discussed their concern about the trend in international affairs, particularly the rising power of Hitler.

In early 1940, their worry became acute. They were concerned that an extensive war was imminent, that it would be a highly technical struggle, that sooner or later the United States would be drawn into it in some way, that we were in no way prepared, and that the current military system would never fully produce the new instrumentalities that would be needed and that were possible in the state of science as it then existed. Here and there in the Army and the Navy there were brilliant, generally young, far-sighted officers who realized the dire need for new weapons and equipment, who had sound ideas on how they might be developed, but who were powerless to act because of lack of high-level interest and financial support brought about by the prevailing euphoria. In arsenals throughout the country, built in World War I to de-

velop and supply new weapons and equipment and now supported practically on a caretaker basis, there were a few perceptive voices crying in the wilderness, brilliant and knowledgeable people living in technological isolation. Even the medical and dental services associated with the Army and the Navy had fallen so low in competence as to be the scorn of their professional colleagues in civil life.

Into this atmosphere was born the NDRC, for which Bush obtained the proper government authority, thanks to his acquaintance with men like Oscar Cox and Harry Hopkins, who had the ear of President Roosevelt and the know-how to get things done in Washington. An order signed by members of the Council for Defense on June 27, 1940, established the NDRC and specified as members the President of the National Academy of Sciences, the Commissioner of Patents, representatives from the Navy and Army, and persons to be nominated by Bush: Conant, Compton, and Tolman. Each member was given responsibility for a division of the Committee's operations and authority to establish sections dealing with specific problems and to seek the cooperation of the keenest scientists and engineers in the universities and in the scientific and industrial institutions throughout the country. The response to their requests for assistance was amazing.

I should emphasize that a concept fundamental to the whole policy of the NDRC and later the Office of Scientific Research and Development was that of partnership of military and civilians in the development of equipment to support the Armed Services. Partnership is a relation between equals as opposed to the traditional master-servant or buyer-supplier relationship. Decisions concerning the planning and execution of military operations belonged to the Services, who knew that business decisions concerning the potential and conduct of technological developments belonged to the knowledgeable civilian agencies. The gray area of the requirements for and the potentials of military technology was the subject of dialogue between equals.

L. H. Adams put me in touch with his friend, R. C. Tolman, who was organizing Division A of the National Defense Research Committee, which was concerned with armor and ordnance, and I began to spend a small fraction of my time studying military problems of interest to him. When I first talked with Tolman, he had established three sections within Division A.

Section T consisted of an expanding group of enthusiastic scientists and engineers on leave from various universities and industrial concerns who, under the direction of Merle Tuve, were moving heaven and earth to develop a shell fuze that would set off the shell when it received a reflected microwave signal from an airborne target. Bush, Chairman of the NDRC, had backed this venture even though it seemed to have less than one chance in ten of success. His confidence in the team and his insight were amply vindicated in a very short time.

Section B, under the chairmanship of John E. Bur-
chard, a professor of architecture at M.I.T., was
concerned with armor and other means of passive
defense.

Section H, headed by C. N. Hickman, an engineer
from Bell Telephone Laboratories, was engaged in

the development of rockets for military purposes.

I became a member and later assumed the posi-
tions of Vice Chairman and Deputy Chief of Section
H, as well as Director of Research of the newly estab-
lished Allegany Ballistics Laboratory, for the dura-
tion of the war.



OFFICE FOR EMERGENCY MANAGEMENT
OFFICE OF SCIENTIFIC RESEARCH AND DEVELOPMENT
1530 P STREET NW.
WASHINGTON 25, D. C.

January 31, 1946

Dr. R. E. Gibson,
Director of Research
Allegany Ballistics Laboratory
705 21st St., N. W.
Washington, D. C.

My dear Dr. Gibson:

It is my understanding that this is the last day that you officially carry the title which I have used in addressing you. While you may lose this title you can never lose the respect and admiration which you gained while carrying out your duties as Director of Research. No one should be a better judge than I concerning the stupendous undertaking which you so successfully carried to completion. It is fortunate indeed that your health permitted you to carry this heavy load. When you assumed these duties you relieved me of a task that it is doubtful that I could have endured. At the same time you made it possible for me to direct my energies along a line for which I was better qualified. I trust that the day may come when you will forget the terrible hardships and remember more of the interesting incidents.

Let me assure you that, great as were your contributions as Director of Research, they can not overshadow those you made as Vice-Chairman and Deputy Chief of Section H. Without your help the section would have foundered at an early date. Much of the work you did was of a type which was of the utmost importance yet of an uninteresting nature. For this work we all owe you a great debt of gratitude.

Of equal importance or perhaps more so were your contributions as the leader in propellant research in America. All the major contribution to this art emanated from you. It is fortunate that you have been persuaded to continue work along this line.

As member of the Section and the Division you also were of great assistance in the rocket development program. When one reviews your wide range of activity and the many duties you assumed during the emergency, it is hard to believe that any one else could have done half so much.

It was a great pleasure to work with you and I hope that the closing of Section H will not mark the end of our associations.

Sincerely yours,

Original signed by

C. N. HICKMAN, Chief,
Section H, Div. 3
NDRG

CNH:adm

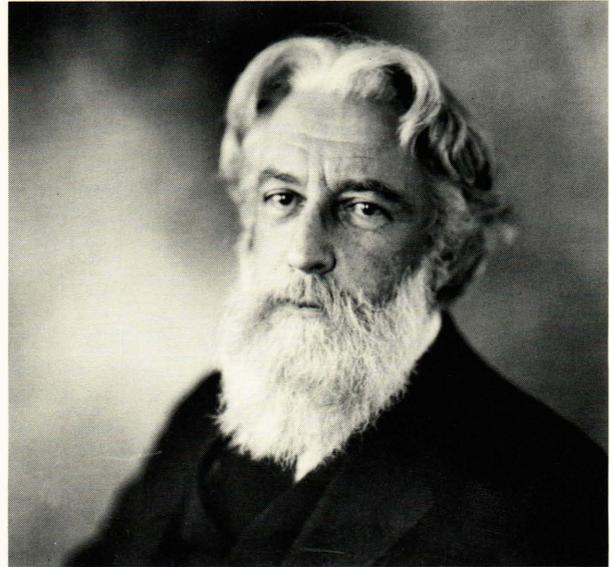
HENRY STEPHENS WASHINGTON (1867-1934)

Dr. Henry Stephens Washington was one of the most remarkable men I ever met, a polished gentleman, a well-read scholar, full of curiosity, a tireless researcher, and an unabashable linguist. He spoke French and German fluently and well; he spoke Italian fluently and badly. I remember a conversation between Washington and a visiting Italian count that took place in the Cosmos Club. Washington started off fluently in Italian, the count looked more and more puzzled and finally said most politely, "Shall we continue the conversation in English?" Incidentally, Washington was science attache at the U.S. Embassy in Rome during World War I.

Henry Washington was born into a well-to-do family in New Jersey in 1867. He was educated at Yale, studying physics, geology, and mineralogy, as well as the usual humanities and languages. His advanced studies at Leipzig earned for him the degree of Doctor of Philosophy, after which he spent some time at the American School of Classical Studies in Athens. From 1886 to 1888 he taught physics at Yale, but then his interest in archaeology took over and he spent the next five years excavating sites in Greece. For that and other archaeological research, for example diggings in Morocco, he was elected a member of the French Academy. After two more years on the faculty of Yale, this time teaching mineralogy, he embarked on a long series of geological, vulcanological, and petrological explorations that took him to Greece, Asia Minor, Italy, Spain, Brazil, and the Hawaiian Islands. In all those travels he also acquired a knowledge of the cultural habits and contributions of the countries visited and, in particular, developed a critical appreciation of their foods and wines. He was particularly fond of the cuisine and vintages of Italy.

Somewhere around 1910, he invested his money in a company to exploit deposits of black diamonds (largely used for grinding and cutting operations in industry) that had been found in Brazil. The project failed, Washington lost all his money, and – about the same time – his wife of some 17 years ran off with an Englishman. Undaunted, he sought and found a new career in 1912 when he joined the staff of the Geophysical Laboratory and pursued vigorously his studies of the chemical composition of igneous rocks, which had for many years been one of his fields of interest. Over the next dozen years, he became a leading authority on techniques for the chemical analysis of igneous rocks and on ways to synthesize experimental results of such analyses made all over the world into a systematic classification of rocks by the chemical composition and into an estimate of the chemical composition of the crust of the earth as a whole. His book on the chemical composition of igneous rocks is monumental in scope, thoroughness, quality, and authority.

His courage was outstanding in every way. Adventures in strange lands he took in his stride. He had



Henry Stephens Washington

very little patience with the foibles of society, not at all because of his ignorance of society, on any level, but because he knew it too well. He lived his own life.

When I knew him, he lived in rooms over a restaurant on M Street (it may have been N) near Logan Circle kept by an Italian named Tarino and his wife. According to Dr. Washington, they served the best Italian food in Washington and, judging from the few times I dined with him there, I can well believe him. Prohibition was not well received by Harry Washington, who genuinely regarded it as an insult to civilization in depriving mankind, or at least the American *genus homo*, of the highest products of art – vintage wine. However, he dared and drank.

Washington was a very striking man. His strong personality shone through his eyes, deeply set in a classical face, surrounded by a shock of long grey hair and a full grey beard. An excellent portrait of him, painted in his laboratory, hangs in the long gallery of the Cosmos Club, of which he was one of the best known and most popular members.

ARTHUR L. DAY (1869-1960) ROBERT SOSMAN (1881-1967)

Drs. Arthur L. Day and Robert Sosman were director and assistant director, respectively, of the Geophysical Laboratory of the Carnegie Institution of Washington. The two men had many characteristics in common. Both were skillful and meticulous experimenters; their findings never needed to be repeated; both were "hard" thinkers who never expressed an opinion until they were certain it was founded on facts; both were instinctively good administrators. There, however, any resemblance between the two men ended, as far as I could see. Dr. Day was a very urbane person, quite sensible of the

customs and requirements of society. Dr. Sosman affected a taciturn manner, lived his own life as he figured it out, and cared very little what people thought.

Day, a New Englander, had been a very shy, sensitive young man, according to what E. S. Shepherd told me, who had set his ambitions very high and had the self-discipline and determination to achieve them. For example, by dint of great effort and self-control, he became one of the most polished and poised scientific lecturers I have known, and I have heard many in my time.

Day received his higher education at Yale: B.A., 1892; Ph.D., 1894; and instructor in physics for three years. He then joined the staff of the *Physikalische-Technische Reichsanstalt* in Charlottenburg, Germany. There he became interested in the physics and chemistry of rocks, particularly in the accurate measurements of high temperatures – techniques of utmost importance for the development of a quantitative knowledge and understanding of phenomena involving the melting and crystallization of rocks and their constituent minerals. While in Germany, he married Helene, daughter of the famous German physicist, F. Kohlrausch. Day returned in 1901 to the U.S. where, as a physical geologist, he took a leading part in the establishment of a program to investigate the fundamental physics and chemistry of rock formation in the technological branch of the U.S. Geological Survey.

A story is told that upon being very favorably impressed with young Day's qualifications for the post, the authorities of the Survey invited him to come to Washington from Germany for an interview – a formality, and a long and laborious undertaking in those days. Day is reported to have refused to make the journey on the grounds that he had already supplied all the necessary information about his education and experience and that he felt the interview would add little, if anything, to the record. He got the job anyway, but the incident illustrates the determination and the disdain for formality that characterized his lifelong attitude toward administration.

Shortly after its foundation in 1902, the Carnegie Institution of Washington started looking for promising and important areas of fundamental research that it could assist with its funds, even to the length of establishing a new center where the research could be pursued on a long-term basis. The work of Day and his colleagues on the Geological Survey attracted the interest of the trustees and led in 1906 to the establishment and construction of a handsome new building, the Geophysical Laboratory, on what was then a very rural site in the District of Columbia, to support and extend this work.

Dr. Day was appointed director of the Laboratory, which opened for business in July 1907 with a staff consisting initially of Allen, Shepherd, White, Rankin, Wright, and Clement. Dr. Sosman joined the group a year later; within a few more years, Day had added more names that afterward became internationally famous to the roster of the Laboratory.

The first 10 years of its life were really halcyon days for the Geophysical Laboratory. The staff were all young, worked hard, and played hard. Within 10 years, the Laboratory had acquired an enviable reputation for the quality of its research output in a variety of fields.

Led by Drs. Day and Wright, tennis became a popular game among the staff. Two excellent clay courts were laid down behind the Lab, members of the staff doing most of the work. The Cosmos Club, Harvey's, and other Washington taverns were the scenes of many stag dinners at which members of the Lab were joined by selected kindred spirits from government agencies. The Metachemical Club, whose name was inspired by John Johnston, met at irregular intervals to discuss any problems beyond chemistry and served as an outlet for the ingenuity of its members in inventing gifts for their guests or for each other. In all of these activities, as well as in the broader activities of the Washington scientific societies, Day played a leading role.

When the United States entered World War I, in 1917, the resources of the Geophysical Laboratory were immediately redeployed to tackle pressing national problems, and they were needed. Not only had this country and its allies been completely dependent on Germany for its supply of optical glass, but the

A story about J. Willard Gibbs was told me by Dr. Day. Gibbs was one of the leading scientific thinkers of the 19th century. Working in almost complete isolation, he developed rigorously and in detail the consequences of the two simplest laws of thermodynamics applicable to an understanding of the behavior of physico-chemical systems, as well as the formulation of statistical mechanics. He published his outstanding work in an obscure journal, *The Transactions of the Connecticut Academy of Science*. He has generally been regarded as one who pursued science for its own sake, regardless of the approbation of his fellows. On his return to Yale after some years of study in Germany, Day, as a friend of Gibbs, was commissioned by the Prussian Academy of Science to present to Gibbs a document expressing the Academy's high appreciation of his work and a diploma signifying that Gibbs had been elected to honorary membership in the academy, which was at that time one of the most prestigious scientific bodies in the world. Day carried out his assignment and was flabbergasted by Gibbs' reaction. Tears streamed down his face as he said, "If this had only come 20 years earlier, I could have accomplished so much more," showing how much the approbation of his colleagues really meant to him.



Arthur L. Day (left)

Robert Sosman (right)

knowledge and skills necessary to produce glass of the quality needed for optical instruments, e.g., binoculars or range finders for the Armed Forces, were nonexistent in the U.S. at that time. Some, like Morey, attacked the problems of the chemical composition of glasses; others went to supervise processes in glass works such as those in Charleroi, Pa., or Corning, N.Y.; others like Adams and Williamson attacked problems in the annealing of glass blocks to remove striae produced by strain. Fenner was in charge of Spencer Lens Company's Optical Glass Plants in Hamburg, N.Y. Wright contributed to the technique for the manufacture of high-grade optical glass for military instruments.

In the midst of this patriotic work of the Laboratory, a discordant note was heard. Mrs. Day (nee Kohlrausch) loved her Fatherland, an affection that was only increased when the U.S. entered the war on the other side. I never met her, but she must have been a fairly strong-minded woman, since she kept no secret of her patriotism from their American neighbors, much to the embarrassment of her husband. In October 1918, Dr. Day resigned from the directorship of the Laboratory and became a consultant to the Corning Glass Works. Dr. Sosman was persuaded to become director, or at least acting director.

In April 1920, on the urging of the president of the Institution, the staff of the Laboratory, and especially Dr. Sosman, Day returned to Washington and resumed the directorship of the Laboratory, continuing in that capacity until his retirement in 1935. Meanwhile, Mrs. Day and their three daughters returned to her homeland, leaving their son in America.

Although the separation must have hurt him sorely, Day never showed or expressed any resentment, but his social contacts with the staff diminished noticeably. He became more and more involved in his studies of volcanoes and of earth movements in California, all of which involved expeditions to far-

off places. Sometime later he married his secretary, Miss Ruth Easling. Dr. Day lived to the age of 91.

As I have mentioned before, the personalities of Day and Sosman were very different. Sosman generally wore a very austere aspect, seldom smiled, and very seldom laughed. John Johnston, who was a contemporary of Sosman among the famous group of young scientists attracted to M.I.T. by A. A. Noyes, told a story about Sosman. Among the topics discussed during their lighter moments was the definition of special units. The suggested unit of pomposity was the Bogert (Marston Taylor Bogert, professor of chemistry at Columbia and a well-known figure in the American Chemical Society). However, it was noted that the feasible working unit would have to be the "micro-Bogert." In searching for a unit of joy, the group suggested as appropriate "the Sosman after one bottle of beer," but felt that a working unit should be the "kilo Sosman." Actually, deep down in Sosman's personality was a highly sophisticated sense of humor that enabled him to smile at himself and to accept and smile at, but not adopt, the fads and fashion of others. For example, he resisted all social pressures to own an automobile; he calculated that it was cheaper to take taxis, walk, or take the train. He was a most systematic person, keeping concise but clear notes of all meetings, conversations, papers read, etc. His book *The Phases of Silica* was a masterpiece of erudition, the result of years of reading and digestion. He was so systematic that he had a super system that prevented him from getting into a rut by too much adherence to any one system. For example, after leaving the Geophysical Laboratory (1928) to become assistant director of the U.S. Steel Research Laboratory in Kearney, N.J., he lived in Westfield, N.J., taking a commuter train to work. He had worked out a formula by which his seat and car on the train were chosen each morning in as near a random fashion as possible. He did not want to become a slave of habit.

During his stay at the U.S. Steel Research Laboratory, and afterwards, during his postretirement position as professor of ceramics at Rutgers University, Dr. Sosman became very fond of dining out in New York City. True to form, he chose the restaurants he patronized very systematically, and after each visit made copious notes of the quality of their cuisine, decor, etc. These he compiled into a small brochure entitled *GUSTAVADEMECUM FOR THE ISLAND OF MANHATTAN, A Checklist of the Best Recommended or Most Interesting Eating Places, Arranged in Approximate Order of Increasing Latitude and Longitude. Prepared for the Convenience of Mathematicians, Experimental Scientists, Engineers and Explorers*. For a number of years, Sosman distributed copies of this remarkable document to his friends, the first being dated "New Year's Eve, 1940." It contains a table of more than 280 eating places (later extended) and gives address, type (French, American, Hungarian, etc.), price index, lighting level, sound level, qualities of food, service, types of customers, looks of waitresses – in all, some 18 items of information coded to give the student their most exact meaning. The latest one is dated St. Theodotus Day (the patron saint of innkeepers), 1953. It lists some 370 restaurants.

NEVIL VINCENT SIDGWICK (1873-1952)

Nevil Vincent Sidgwick was pure Oxford, born and bred. His parents and other ancestors had been connected in one way or other with Oxford and its University for generations. He was educated at Rugby School and Christ Church College, Oxford. In 1895 he earned a first class degree in chemistry and in 1897 a brilliant first class in *Literae Humaniores*. After a few years' study in Germany he returned to Oxford, staying at the university in various positions associated with the titles don, tutor, Fellow of Lincoln College, and finally University Reader in Chemistry.

I have forgotten the exact circumstances of our first meeting; it must have been around 1930. He was then a slight, wiry, scholarly looking man, with gray hair and mustache. He had a rich wit, a sense of humor, a prodigious memory, and a very sharp tongue, all of which made him a *raconteur par excellence*. His book, the *Organic Chemistry of Nitrogen*, published in 1910, had established his reputation as a chemist and as a master of English prose.

I am told that up to the age of 40, Sidgwick stuck very close to Oxford; indeed, except perhaps for an occasional visit to the Continent, he did not leave England. That insularity was shattered in 1914 when he joined a party of British scientists bound for the meeting of the British Association in Australia, but the effects did not show up for more than 10 years. World War I intervened and, like practically all his colleagues at Oxford, he became involved in the war effort.

Among those colleagues was F. A. Lindemann, much later Winston Churchill's chief scientific men-

tor in World War II. Lindemann had already invented the Lindemann compass and joined the Royal Flying Corps (later the R.A.F.) as a private. His wartime exploits became legendary. Sidgwick, who knew Lindemann very well, was full of stories about his adventures. One deserves repeating.

The tailspin was the deadly enemy of the flyer, military or otherwise. If a plane got into a tailspin, the pilot and passengers were doomed. Lindemann, already a physicist of high reputation, had gotten interested in aerodynamics and turned his attention to the tailspin during his stay in the R.F.C. He worked out a theory of how tailspins were generated and what might be done by the pilot to pull out of one safely. He was so pleased with his theory that he learned to fly a plane. After some experience, he took it into the air, deliberately went into a tailspin, came out of it, landed safely, said to the skeptical, but amazed spectators, "See?" and proceeded to write down notes of the experiment.

After the war, Sidgwick turned his attention to physical chemistry, notably the question of how atoms stick together to form molecules, liquids, and solids, using the rapidly evolving models of atomic structure. In 1927, he summarized his research in a book, *The Electronic Theory of Valency*. This attracted great attention and, in particular, Dr. Sidgwick was invited to be the George Fisher Baker Lecturer in chemistry at Cornell University for 1930-31. He was welcomed enthusiastically by faculty and students alike. His lectures really made history in American chemistry. During the latter part of his stay, he was invited by the graduate students to stay at the Telluride House, part of a nationwide association of the most promising graduate students in chemistry. There he met Lee G. Davy, who took on the role of his "equerry." Being from the far west, Davy persuaded his charge to visit that region of the country, converting what had been a mild love affair between the Oxford don and the United States into a passionate attachment. From that time, until his death in March 1952, Dr. Sidgwick missed no occasion to visit the U.S. Indeed, except for wartime, when the government made heavy demands on his services, his visits became almost annual.

I remember vividly being seated in the garden of Balliol College, Oxford, with him on a lovely summer morning in 1933. We were surrounded by the beauty and the historical association of that famous college. What did Dr. Sidgwick talk about for nearly an hour? – the beauty and the grandeur of the Sequoia forests and the High Sierras of western America.

His rooms in Lincoln College were unique in that they contained a completely equipped private bathroom. He had had it installed after his first tour at Cornell. His private library, the room where he met his students, was most interesting. Not only did it contain an impressive array of works on chemistry in many languages, together with classic works in practically all the disciplines, it also contained all the

books ever written by P. G. Wodehouse, which Sidgwick read time and again.

After World War II, Dr. Sidgwick turned his attention to inorganic chemistry, studies that resulted in a third book, *The Chemical Elements*, published in 1950. Thus he left the world three classic works, one each in the main branches of chemistry – organic, physical, and inorganic.

CHARLES SNOWDEN PIGGOT (1892-1973)

Probably the most colorful person I met at the Geophysical Laboratory was Charles Snowden Piggot, who was born in Sewanee, Tenn. in 1892. After being educated at a private military school in the South and the Boys' Latin School in Baltimore, he attended the University of the South, an excellent school where his father was a professor. He received the B.A. degree from that institution in 1914, did graduate work at the University of Pennsylvania for two years, and joined duPont as a chemist, where he remained until 1919. From 1919 to 1920, he studied at Johns Hopkins and received the Ph.D. From 1920 to 1925, he was a research chemist with the U.S. Industrial Alcohol Co., except for the period 1922-23, when he held a Ramsey Memorial Fellowship at University College, London, studying under Prof. F. G. Donnan. During that time, he became acquainted with many aspects of London society, including *la vie de boheme*. Piggot joined the staff of the Geophysical Laboratory in the fall of 1925 to study the radioactivity and the relative amounts of daughter isotopes in rocks, one objective being the determination of various rocks and minerals.

Piggot had a very sharp mind, a very retentive memory, and almost instant recall. His 10 years of experience in different university and industrial laboratories provided him with a very well-stocked mental inventory that served him well as his career at the Laboratory progressed. Furthermore, he had all the charm of an educated southern gentleman, and made friends easily among his colleagues, to whom he could go for advice. He was an excellent raconteur and conversationalist, and I can remember very pleasant and informative evening discussions held either in his office or mine. He was also a very good experimenter, loved to work with his hands, was an excellent carpenter and cabinetmaker, and, indeed, with his own hands remodeled at least two houses.

Two of his scientific projects deserve mention here from the historical point of view. In the course of his study of the age of rocks by determining the ratio of radioactive and other isotopes found in samples, Piggot got very interested in an unconventional method of chemical analysis that was proposed by Dr. Fred Allison, head of the Department of Physics at Alabama Polytechnic Institute as a result of Allison's studies of the effect of magnetic fields in rotating the plane of polarization of a plane-polarized beam of light passing through a liquid solution. (The phenomenon was first discovered by Faraday.) By varying the magnetic field around a tube that contained a so-



Charles Snowden Piggot

lution through which a beam of polarized light was being observed under crossed Nicol prisms, Allison and his co-workers noted faint signals that could be correlated exactly with the atomic species known to be in the solution and even with isotopes of the same element. Dr. Piggot prepared a series of solutions whose composition was known only to him and sent them to Allison for analysis. As I remember, the results reported by Allison agreed completely with the composition Piggot had put together. As a result of that experiment and many consultations with Allison, he installed an "Allison apparatus" in the attic of the Geophysical Laboratory. I had a chance to experiment with it and thought I saw the "Allison effect." Allison's published work came in for great criticism; some people could reproduce it, others could not. Many of the scientists of the "establishment," including Harold C. Urey, were violent in their condemnation of the alleged phenomenon. A Nobel prize was at stake. If Allison's observations had been accepted, he would have been hailed as the discoverer of deuterium, an isotope of hydrogen. Some people even impugned his honesty, but this I discount entirely. I met Fred Allison on a number of occasions, and I can say emphatically that to me his honesty and integrity were beyond all shadow of doubt. The Allison effect is still somewhat of a mystery; it dropped from sight, but a thorough study of it in the light of today's knowledge could be rewarding.

In 1935, Piggot extended the sphere of his radioactivity studies by promoting a project to obtain relatively large core samples of the ocean floors for analysis. Although some oceanographers had obtained small samples from the shallower parts of the sea, Piggot saw a way to go after much larger game. I have used the word "promoting" because the magnitude of Piggot's project required fairly large-scale engineering cooperation and use of a substantial ocean-going vessel.

His idea was to mount a core drill (some 12 feet long) to a gun charged with a suitable explosive. The “gun” consisted of a massive cylinder of steel bored to accommodate the explosive and the drill. The whole assembly was to be lowered from a ship and, when the tip of the drill touched the ocean floor, the charge was to be fired, driving the drill into the underlying material (sediments, rocks, etc.). The drill was lined with a long brass cylinder in which the core sample was collected. The brass cylinder was removed from the drill immediately on recovery, labelled appropriately, and used to store the sample until it arrived in the laboratory for sectioning and analysis. I saw many of those cores stored carefully in the attic of the Geophysical Laboratory.

Charlie succeeded in persuading the Institute to provide a modest amount of funds, specified a diesel-powered winch and a tapered steel cable (the cable increased in thickness from the bottom to the top in order to obtain the necessary tensile strength with the lightest weight), designed the gun and the drill with the help of his friends at du Pont, and prevailed on appropriate authorities to accommodate his gear on a cable-laying ship. He made several expeditions, one of which I remember was a traverse of the Atlantic Ocean, which resulted in 8 to 10 core samples, some as long as 8 feet. Needless to say, they were a gold mine of geological information and provided Piggot with plenty of material for his radioactive studies.

When it became apparent that the U.S. would become involved in World War II, Piggot joined the Navy, receiving a commission as an officer in the Bureau of Ordnance. Before the end of the war, he had risen to the rank of captain and retained that rank for many years after in the Naval Reserve. For several years (1950-52) he served as Naval attaché at the U.S. Embassy in London, a post that brought him into close contact with the scientific and social life of London. Both Charlie and his wife enjoyed that immensely. Later he served as head of a National Research Council mission to India.

Charlie Piggot was a very active member of the Cosmos Club, its secretary for 13 years (1936-1949) and its president in 1957. Throughout his career, he was a well-known, active, and influential member of the club. He died in 1973 at the age of 81.

GEORGE GAMOW (1904-1968)

In 1934, Dr. C. H. Marvin, as president of The George Washington University, had an idea for strengthening the physical sciences there and enhancing the university’s prestige at a fairly low cost. He would import a theoretical physicist of international reputation. No extension of the physics laboratories would be needed. Dr. Marvin set his sights high – very high indeed – and succeeded in persuading George Gamow to accept a professorship of theoretical physics. Gamow’s coming to Washington was an excellent move, as it affected and inspired Washington scientists in general. As far as strengthening the physics department at George Washington, it left

something to be desired. Gamow lectured once or twice a week. Throngs of eager students of theoretical physics did not converge on the lectures. Like most men of great genius, Gamow was an individualist whose powerful thought and fertile imagination sent out radiations that enlightened the whole world, but reflected little on the environment in which he happened to be based.

Gamow was instrumental in having Edward Teller invited to George Washington as professor of physics in 1935, an appointment that had a tremendous impact on the scientific life of Washington. Although he did not have the creative genius of Gamow, Teller was probably a much abler scientist in that he was knowledgeable in almost all fields of science and had a phenomenal facility with the techniques of exact thought—for example, mathematics. Gamow and Teller had worked together in Europe with great success. Teller could grasp quickly the novel and often abstruse ideas emerging from Gamow’s brain and apply exact reasoning to examine their applications fully. Very few people could do that. (Years later, Robert C. Herman at the Applied Physics Laboratory proved to be another scientist whose collaboration with Gamow was outstandingly productive.)

The fame of George Gamow is widespread; biographies have appeared in public print and been recorded in books or learned memoirs. I shall confine myself to recollections, mostly personal, extending over the years when he was in Washington.

Gamow was a Russian giant whose high-pitched voice contrasted violently with his tall broad-shouldered figure and distinguished face and head, surmounted by a shock of yellow gold hair. He spoke English very fluently, but with an accent that could hardly be called Russian. Like many of his country-



George Gamow

men, Gamow was an accomplished linguist and lectured in French, German, Spanish, Portuguese, and Dutch, as well as Russian and English. It was said that after two weeks in Denmark he gave a lecture in Danish. One of his friends said that Gamow really spoke one language, namely, Gamovian, of which, however, there were many dialects: the English dialect, the French dialect, the German dialect, and many others. He wrote English well; indeed, his style was distinctive and appealing. His popular books, for example, the *Mr. Tomkins* series, which were fundamental discussions of rather abstruse scientific topics, were widely acclaimed for their combination of exactness and simplicity.

Gamow was born in Odessa, U.S.S.R., in 1904, educated at the Normal School in that town from 1914 to 1920 and later at the University of Leningrad, which awarded him a Ph.D. in 1928. The period of his education spanned the years when revolutions completely destroyed the old Czarist regime and several moderate interim governments.

From 1928 to 1931, Gamow held several fellowships at Göttingen, Copenhagen (with Niels Bohr), and Cambridge. His sojourn at Cambridge had very far-reaching effects on the Cavendish Laboratory. By applying the quantum theory to study the nuclei of atoms, Gamow discovered the “tunnel effect,” which, in essence, said that there was a considerable probability that the nuclei of atoms could be disintegrated by bombardment with particles whose energies were much lower than theory predicted for the binding energies of electrons and protons in atomic nuclei. Cockroft and Walton in the Cavendish Laboratory had developed an instrument that could accelerate particles to 800,000 volts; however, feeling that that electric potential was not enough to give particles sufficient energy to disintegrate even light atoms, they wanted to build a machine that could generate 1.2 million volts, an achievement they knew to be within their power. Ernest Rutherford, head of the Cavendish Laboratory, who was convinced by Gamow of the validity of his new theory, told Cockroft and Walton to forget about higher voltage machines for the time being and go ahead bombarding atoms of lithium with particles generated by their 800,000-volt apparatus. It is reported that he also added that, if they did not do so within a month, they would have to leave his laboratory. Thus Cockroft and Walton were propelled into lasting fame figuratively by the toe of Rutherford’s boot, for their experiments were successful and they became the first to disintegrate an atom (lithium) artificially.

From 1931 to 1933, Gamow was a Master in Research of the Academy of Science in Leningrad, during which time he married Loubov Wochminzewa (known as Rho), a very sprightly, intelligent, and courageous woman. Rho’s parents and all her relatives perished in the revolution when she was a small child, and she grew up as one of the “wild” children with no fixed abode. Despite that, she was well educated and, indeed, became a chemist of no mean abil-

ity. During that time, Gamow made up his mind that he wanted to leave Russia permanently. I don’t know why, because I never heard him express any political views. To succeed in his intent, Gamow had to pit his wits against Stalin and the Politburo, for his reputation was by then known in the highest circles of the government, who decided that he was too much of an asset to Russia to let go. The Gamows’ first attempt to escape was a failure. Being on holiday at the Black Sea resort of Odessa, they procured a rubber boat and set out across the Black Sea for Turkey. Halfway across, they encountered a violent storm. George paddled and Rho bailed furiously, but to no effect; the storm carried them back to Russia and washed them up exhausted on the sands near Odessa. I remember vividly listening to the Gamows’ description of these and other adventures.

The Soviet authorities were afraid to shoot or imprison so great an ornament to Russian science, so Gamow returned to work at the Academy with renewed determination that he and Rho would leave Russia for good. In 1933, George obtained by some machinations a permit for himself and Rho (which was the real trick) to attend the Solvay Conference in Brussels. Thence he went to Paris and London, where he lectured at the universities, spent a few months at the University of Michigan, and finally ended up at George Washington, where he was to stay for 22 years.

As might be expected from one who had successfully outwitted the heads of the Soviet government, Gamow feared neither God nor man. He was a law unto himself, and his manners and human relations were at times naive and brusque to the point of rudeness, although often amusing.

George Gamow mapped out an ambitious career early in life. He decided to devote himself sequentially to three major problems: (a) the problem of the very small, the atom; (b) the problem of the very large, the universe; (c) the problem of animate life.

By the period 1930 to 1935, his work had opened the way to solving the problem of the nature of atoms. His papers on the fluid hypothesis for an atomic model led the way to the present theories of nuclear fusion and fission.

Around 1952, his studies had led him to an hypothesis for the origin of the universe, namely that all started with a giant atom that became unstable and exploded, sending out in all directions atoms and fragments of atoms of the elements as we now know them. With R. C. Herman and Ralph Alpher of the Applied Physics Laboratory, he worked out detailed consequences of the theory – for example, the relative abundance of different elements in the universe, the temperature in outer space, etc. Cosmologists refer to this picture as the “Big Bang” theory of the origin of the universe. For many years it was treated with condescension or neglect, to say the least. However, in 1978, workers at Bell Laboratories, studying the spectra of microwave noise from outer space, confirmed Gamow’s theory. It is now accepted, and

his genius is being posthumously recognized more strongly than ever.

By the mid-1950's, Gamow was turning his attention to fundamental problems in biology and published a paper on a triplet system of protein coding. Of that phase of his work, I know very little first hand, so I cannot really comment on the significance of his studies. But it is now regarded as the key idea that broke the genetic code.

Gamow took very little, if any, part in the development of the atomic bomb, although much of this development depended on his early theoretical work. The Department of the Navy and, I believe, the National Defense Research Council, used him as a consultant, but I remember nothing about the nature of his work in this field. In fact, with the security regulations that then existed, I may have known nothing about it.

In 1956, Gamow left George Washington officially to accept a Chair of physics at the University of Colorado in Boulder, a position he held until his death in August 1968. He and Rho were divorced in 1956. Their son, Igor, was well launched on a career as a ballet dancer. In 1958, George married Barbara Perkins, with whom he lived very happily for 10 years, as may be gathered from a letter she wrote to me after his death.

I quote from my letter to Mrs. Gamow dated 23 August 1968:

The telegram informing me of the sad news of George's death came as a great shock to me. I cannot believe so vital a person is gone. Indeed, he has not! George, more than anyone, grasped the pattern of creation. He opened up the secret of the very small, the atom; he propounded a pattern of the immense, the universe; and he illuminated the mystery of animate life. Above all, he gave to the average man, the Mr. Tompkins' of the world, the opportunity to share with him the grand products of his disciplined imagination. George is one of the few immortals of this age....

And her reply,

Your kind letter was really wonderful, giving the essence of George in its sympathetic tribute. We all did love and admire him, and it seems such a special pity that he should go just when he had returned to thinking so hard about pure science.

I do thank you for your comforting words and your real appreciation of this unusual man.

EDWARD TELLER (1908-)

Edward Teller became professor of physics at The George Washington University in 1935, after studying and lecturing in Göttingen, Copenhagen, and London. Both Edward and his wife Mitzie (Augusta) were very charming, highly intelligent, modest people who soon accumulated a large circle of friends in Washington. At that time, Edward did not smoke

nor drink any alcoholic beverages. Although he had a good sense of humor that frequently showed itself in his keen appreciation of sophisticated jokes, he was of a very serious turn of mind and his extreme intolerance of shoddy thought or action at that time made him almost painfully honest.

It did not take long for students at George Washington and scientists in Washington in general to find out that Teller excelled first as a lecturer and second as a collaborator in solving research problems. His command of English was extraordinary and his lectures were models of clarity and systematic organization of what, in other hands, might be a complicated subject. Moreover, all his lectures were delivered without a single note. Teller is one of those rare individuals who can think through, organize in detail any subject, simple or complex, and retain a complete systematic picture of the results, all in his head. For Teller, writing or lecturing is just a routine final step when he empties on paper the fully developed contents of his mind.

I remember a good example of Teller's powerful mental processes. Sometime after he came to Washington, Dr. Joseph Meyer, professor of chemistry at Johns Hopkins, asked Edward for a critical appraisal of a paper he was writing on the theory of the condensation of gases to liquids, based on statistical mechanics. The argument was based on very complex mathematical reasoning – so intricate and bulky that Meyer observed wryly that the only way he could hope to have it published was to skip over portions of the mathematics with the statement, "This formula leads 'obviously' to the next step, namely..." Teller said that in order to help Joe, he must first understand the theory thoroughly and in detail; he suggested that they take a walk together in Rock Creek Park during which Meyer would read to him his complete mathematical study. They walked and talked somewhat over two hours, Teller said he now understood Meyer's theory completely, that he was satisfied it was sound, and that there were a few details that he could suggest be corrected or clarified.

During his stay in Washington (1935-41), Teller took a delight in helping others develop elegant solutions to problems they had conceived or encountered in experimental work. Of the many examples that occur to me, I shall mention one. Drs. Paul H. Emmett and Stephen Brunauer, both of whom had been members of F. G. Cottrell's outstanding group at the Fixed Nitrogen Laboratory, had studied experimentally the adsorption of gases on the surfaces of solids in an attempt to clarify the understanding of the mechanisms of heterogeneous catalysis. An important point was the development of an accurate and reliable method for measuring the surface areas of porous or finely divided solids. They got Teller interested in the problem and, as a result, the Brunauer-Emmett-Teller paper on the subject became and still remains a classic in the field.

Edward Teller is an accomplished pianist and erudite musician. He knows almost every note in the

works of his favorite composers – Bach, Beethoven, and Brahms. He plays the piano largely for his own amusement, being very reluctant to perform for an audience. He says he is not good enough. However, I remember at least one occasion when he held a number of us enthralled by his mastery of the piano. It was a warm summer Saturday evening. Teller had given a lecture to the Philosophical Society, and he and Mitzie had adjourned with a number of others to our house. Teller was exhausted; as usual, he had expended an enormous amount of his emotional energy on the lecture, although to the listener his discourse had seemed to be an effortless, polished performance. While the rest of us were relaxing over a drink, Teller asked if he could relax by playing the piano. For the next two hours, he held us spellbound. Not only was his technique superb, but he played with a feeling that revealed a deep understanding of the music and the composer's intent. He first played Beethoven's *Pathétique Sonata*, then talked about some of its finer points that I had overlooked, even though I knew the sonata very well. After other pieces, Teller got up from the piano bench, completely relaxed, a new man. I asked him how he became such an accomplished pianist. His answer was illuminating. "My mother forced me to practice when I was a small boy." In these days, when parents are urged to refrain from directing the energies of their children and to encourage self-expression without learning the techniques, that reply was very interesting.

In many ways Teller was ahead of his time in the application of nuclear physics in other scientific

fields. He was very keen on interesting chemists and biologists in the use of radioactive isotopes as tools for research. He met with little or no success. Chemists and biologists were not yet ready; Teller had a solution looking for a problem. Twenty years later, the chemical and biological journals were full of papers describing the use of radioactive isotopes as tools for solving research and routine problems in chemistry, biology, and medicine.

In 1941, Teller left George Washington and, indeed, disappeared from view into the scientific fortress at Los Alamos. Indirectly, I learned that he had made a significant contribution to the development of the first atomic bombs and had been one of those who witnessed the Trinity test at Alamogordo in 1945.

We next saw the Tellers during a visit we paid to Los Alamos in 1949. I was invited to give a lecture on the potential of the TRITON cruise missile then being developed at APL to deliver nuclear warheads very accurately over long distances (that was before the days of POLARIS). One evening, we had dinner with the Tellers. Mitzie was the same as ever, but a great change had come over Edward. Gone was the young man of the Washington days. He had lost none of his conversational charm and wit, but the iron of the development and deployment of the atomic bomb had entered his soul, and we found a hard-boiled executive. The cold war had revived in him the fear and detestation of the Russians, born from firsthand experience in his early days in Hungary, that accounted for his intense desire to develop and deploy the hydrogen bomb.

FOREWORD to the *APL Technical Digest*, Vol. 1, No. 1, 1961

The primary mission of the Applied Physics Laboratory, The Johns Hopkins University, is research and development leading to the design and construction of systems to provide timely and competitive solutions to operational problems confronting the United States Navy. In support of this mission, its work extends from pure research in specific fields of physics, chemistry, and engineering through the rational design and test of prototype systems, to the evaluation of these systems under operational conditions. In short, the technical objectives of the Laboratory are understanding on the one hand – useful working hardware on the other.

Results of its work are published in a number of media; they appear in scientific and engineering journals, are disclosed as patents, or appear in special Laboratory reports. For the most part they appear as papers written by specialists for specialists. This practice will continue. However, the diversity of the Laboratory's activities and of the vehicles for their publication requires a synthesizing agent to preserve the overall pattern of its activities, not only for the benefit of its own staff, but also for the benefit of scientific and engineering colleagues throughout the world. It is the purpose of the *Technical Digest* to accomplish this synthesis by presenting in one periodical results from many fields, expressed in terms that excite the interest of those who are not specialists in the particular field and at the same time invite the critical examination of those who are.

The *Digest* is an attempt to solve one aspect of the ever-increasing communications problem. It is my confident hope that it will succeed in this attempt by helping its readers to recognize more clearly the interactions of the technical knowledge in the fields it covers.

R. E. Gibson