

NATIONAL ACADEMY OF SCIENCES

GORDON JAMES FRASER MACDONALD
1930–2002

A Biographical Memoir by
WALTER MUNK, NAOMI ORESKES, AND
RICHARD MULLER

*Any opinions expressed in this memoir are those of the authors
and do not necessarily reflect the views of the
National Academy of Sciences.*

Biographical Memoirs, VOLUME 84

PUBLISHED 2004 BY
THE NATIONAL ACADEMIES PRESS
WASHINGTON, D.C.



Photo by Roy Porello

Gordon MacDonald

GORDON JAMES FRASER MACDONALD

July 30, 1929–May 14, 2002

BY WALTER MUNK, NAOMI ORESKES, AND
RICHARD MULLER

GORDON WROTE EXTENSIVELY with force and conviction about his life and work; readers of these biographical memoirs will want to learn in his own words of his successes—and of his failures. This is not an exercise in hagiography; to suppress Gordon's weaknesses would discredit his formidable strengths.

Following a discussion of Gordon's early career in the 1950s, we have chosen to feature two major late twentieth-century issues in which Gordon played a significant role. The first issue, dealing with Gordon's resistance to plate tectonics, is excerpted from "How Mobile Is the Earth?" It is excerpted from an essay he wrote shortly before his death.¹ The second, dealing with Gordon's policy work on weather modification and climate change, is excerpted from a set of articles he wrote between 1968 and 1988.² We close with an account of his activities in the 1990s and an attempt to evaluate Gordon's extraordinary accomplishments.

WALTER MUNK WRITING ON YOUNG GORDON

Gordon grew up in San Luis Potosi, Mexico. His father was among the many Scots who emigrated to Mexico; for some years he was an accountant with American Smelting and Refining Company. He met Gordon's mother, an Ameri-

can, while she was working at the U.S. Embassy in Mexico City. As a child Gordon contracted polio, and spent the next 60 years trying to prove to everybody, himself included, that this was not a problem. Gordon's application to Harvard for a football fellowship was a case in point. (He graduated summa cum laude at the age of 20.)

Many years later President Nixon, when questioned about the intellect of his administration, replied, "I have three members of the Harvard class of 1950 on my staff, all summa cum laude." They were Kissinger, Schlesinger, and MacDonald.

After graduation Gordon was among the 20 privileged junior fellows (under Dean McGeorge Bundy, a previous fellow) who were supported to do anything they wished. Gordon spent some of his fellowship climbing in the Alps. On the island of Unst in the Shetlands he stumbled upon a great Arctic skua with a wingspread of 8 feet, and this converted him to bird watching, in fierce competition with Murray Gell-Mann. Gordon himself attributes his subsequent interest in water quality to his early experience in bird life.

Gordon entered Harvard for a degree in chemical engineering, but he switched to geology as an undergraduate, and got his Ph.D. in geology at the age of 25. He then moved across town to an assistant professorship in geophysics at MIT.

I met Gordon while he was a Harvard undergraduate. I was giving a seminar on the variable rotation of the Earth associated with a seasonal change in the high-altitude jet stream (just discovered), feeling reasonably secure that no one in the audience knew anything about this. A student in the first row interrupted with some rude comments about neglect of tides, variable ocean currents, and such like. Four years later I gave a much-improved account at MIT; there he was again in the front row, complaining that I had not answered his questions of four years ago.

We decided to write a book together. The scope grew beyond bounds. To quote from the preface: “The diversity of the subject is appalling. It touches on every branch of geophysics. By the time it is covered, information will have been gained concerning wind and air masses, atmospheric, oceanic and bodily tides, sea level, rigidity and anelasticity of the Earth’s mantle, and motion in its core.” Quoting Gordon,¹ “Walter and I had a mild debate on whether or not to include discussions of continental drift and polar wandering. I (GmacD) argued we should, so as to tweak the geologists into considering limitations on their wilder speculations. The final chapter of *The Rotation of the Earth* takes up the subject of the Earth’s mobility, as we understood it in 1960.” I was moved to learn from Naomi Oreskes, who interviewed Gordon on his views of plate tectonics, that Gordon considered his writing of *Rotation of the Earth* as the most satisfying experience in his scientific career.

GORDON MACDONALD IN HIS OWN WORDS

On Plate Tectonics.³ “In the 1950s, polar wandering and continental drift were controversial subjects, often leading to heated discussions between North American and European geophysicists and geologists. . . . I started serious work on these topics in 1957, when Walter Munk and I began the research and writing for our book, *The Rotation of the Earth*. . . . The final chapter takes up the subject of the earth’s mobility, as we understood it in 1960.”

“When I was an undergraduate at Harvard in the late 1940s, my professors ignored or dismissed (with ridicule) speculation that continents move relative to each other, the poles tip, and convection currents constantly stir the interior of the earth. However, I was very much impressed in 1949 by reading Reginald Daly’s book, *Our Mobile Earth*.”

“Whatever sympathy I had for Daly’s notion of continental drift was overwhelmed by the work of two giants of 20th century geophysics, Cambridge Professor Sir Harold Jeffreys and Harvard Professor Francis Birch. . . . I found Jeffreys’ reasoning about the strength of the earth . . . to be convincing. . . . Elastic materials have what physicists call a ‘finite’ strength, which means that upon the application of a stress . . . they will deform a certain amount in proportion to that stress. But no matter how long the stress is applied the deformation is limited . . . Birch felt that his demonstration of the homogeneity of the mantle in both the upper and lower regions ruled out large-scale convective motions . . . there would be no driving force for large-scale convection. Based on my readings of Jeffreys and my close interaction with Birch, I concluded that the earth indeed possessed a finite strength.”

“In the early 1960s, new observations and interpretations of the sea floor data led to the theory of plate tectonics. According to this theory, low-intensity long-term stresses drive the horizontal motion of the plates. I argued in two papers that the large-scale difference between continents and oceans . . . extended to several hundred miles’ depth . . . and that the mantle possesses a finite strength, as argued by Harold Jeffreys. . . . My insistence that geophysical constraints must be discussed led many participants . . . to dismiss me as a troglodyte who was slowing the convergence of thought that was later to be labeled either as a revolution or a paradigm shift.”

“In all science there is a strong ‘herd instinct.’ Members of the herd find congeniality in interacting with other members who hold the same view of the world. . . . Before the 1950s, the North American herd of geologists found it com-

forting and amusing to ridicule those foreign geologists who advocated continental drift. In the early 1960s . . . (several) respected leaders . . . decided to shift directions and the herd soon followed.”

“The Royal Society sponsored (a meeting) March 19-20 1964. Teddy Bullard, a relatively late convert to drift, presented what he regarded as proof that there was a precise fit between the two coasts of Africa and South America. . . . I once again argued for deep roots to continents and the difficulties these imposed on any drift scheme. Teddy Bullard, in a masterful putdown, responded ‘Many precedents suggest the un-wisdom of being too sure of conclusions based on supposed properties of imperfectly understood materials in inaccessible regions of the earth.’”

“Although I maintained an interest in the structure of the earth’s interior, I had actually begun to disengage from the field of continental drift in 1962, when I was asked to chair a National Academy of Sciences Committee examining weather modification.”

On the Science and Politics of Rain Making.⁴ “Weather modification was one of many areas in which the federal government, through both its armed forces and its civilian agencies, was funding scientific research aimed at improving our capacity to understand, control, and modify the environment. In 1961, I was appointed to the National Academy of Sciences Committee on Atmospheric Sciences (1961-1970) and the President’s Science Advisory Committee Panel on Atmospheric Sciences (1961-1964); three years later I was appointed to the National Science Foundation Advisory Panel for Weather Modification (1964-1967).”

“Weather modification was a highly contested topic, dating back to the establishment in 1953 of the Congressional Advisory Committee on Weather Control. Throughout the 1940s and ‘50s, there had been considerable enthusiasm for weather modification projects. In the early 1940s, Irving Langmuir and Vincent Schaeffer, at the General Electric Company, demonstrated that clouds could be modified by seeding them with dry ice pellets; Bernard Vonnegut demonstrated that silver iodide could do the same (thus inspiring the ice-9 of his brother, Kurt’s, novels).”

“Weather modification was taken up with enthusiasm by those who hoped to use it on behalf of matters ranging from warfare to world hunger. The 1953 advisory committee was charged with evaluating the various government and private initiatives in cloud-seeding. In 1957 this committee reported to President Eisenhower that ‘seeding of wintertime storm clouds in mountainous areas in the western U.S. produced an average increase in precipitation of 10-15% from seeded storms, with heavy odds that the increase was not the result of natural variations in the amount of rainfall.’”

“In 1963, the Committee of Atmospheric Sciences of the NAS appointed a Panel on Weather and Climate Modification to ‘undertake a deliberate and thoughtful review of the present status of activities in this field and of its potential and limitations for the future.’ The report was issued in 1966. The tone was cautious, but the conclusion positive: ‘There is increasing but somewhat ambiguous evidence that precipitation from some types of clouds and storm systems can be modestly increased or redistributed by seeding techniques.’ Statisticians attacked this conclusion. Alexander

Brownlee of the University of Chicago, writing in the *Journal of the American Statistical Association* (June 1967) had the following closing: 'That such nonsense should appear under the aegis of the National Academy of Sciences is deplorable.' My own conclusion, consistent with the panel report, was that there is no in-principle objection to the possibility of weather modification, and in some meteorological conditions, precipitation reaching the ground can be increased perhaps by a substantial amount by seeding."

"Over the next several years I became increasingly convinced that scientists should be more actively engaged in questions of environmental modification, and that the federal government should have a more organized approach to the problem. While such research could take place in both the public and private sector, the government should take the lead in large-scale field experiments and monitoring, and in establishing appropriate legal frameworks for private initiatives."

On Environmental Sciences.⁵ "I felt that changes were needed within the scientific community. The environment was not merely important politically and socially, to my mind, it presented complex and intriguing scientific problems, which the scientific community might be enthusiastic to tackle. Yet they were not. Left to their own decisions, the scientific explorers will push those areas that are exciting or perhaps fashionable. In the past it has been far more acceptable and more praiseworthy to investigate the Earth's deep interior than to puzzle over the problem of predicting earthquakes. In the past, questions of the origin and development of the atmosphere have proved far more attractive than investigations of atmospheric pollution."

“Scientists in the 1960s were generally reluctant to take on society’s problems or to allow for the idea that their research should be directed from without. To do so, they felt, would threaten the purity of scientific research. The widely held views of the time were typified by Michael Polanyi, who wrote, ‘The pursuit of science can be organized . . . in no other manner than by granting complete independence to all mature scientists. The function of public authority is not to plan research but only to provide the opportunities for its pursuit.’ My own view was different: I believed the scientific community needed to find a balance between the pressures from within and without, advancing basic knowledge and translating those advances into tools for society.”

“My experiences with weather modification convinced me that the topic could not be isolated from other developments in atmospheric sciences, and indeed, environmental sciences as a whole. The uncertainty over weather modification illustrated our lack of basic scientific understanding in many areas of environmental sciences. Our ability to modify the atmosphere depends on our proficiency in describing and predicting its behavior. Indeed, it would be both ineffective and perhaps unsafe to attempt weather modification in the absence of the capacity to predict the consequences of such activities in some detail. I proposed at the time that the federal government establish a new agency whose task was to promote and foster research and development in environmental prediction and modification—not just of the atmosphere, but also the oceans and the solid earth. While the agency I envisaged did not come to pass, some of these considerations were addressed when I served on the President’s Science Advisory Committee (PSAC) under Lyndon Johnson (1964-68), and again when I served on the

newly established Council on Environmental Quality (CEQ) under Richard Nixon (1970-72).”

“A critical event in this period was the Santa Barbara oil spill, just two weeks after Richard Nixon’s inauguration. Television media covered the blow-out and the impacts on the birds and sea life on an hour-by-hour basis. The administration faced its first real crisis by quickly appointing a small group of scientists and engineers to recommend solutions. I was a member of that group. At the time, I was Vice Chancellor of the University of California, Santa Barbara, and a holdover member of President Johnson’s Science Advisory Committee (PSAC). I quickly flew to Los Angeles to meet the President. In order to demonstrate to the public that all was well, the President was to walk along the beach, with coverage by TV photographers who would be backing up. I was to be on Nixon’s right, and on his left, Fred Hartley, president of Union Oil Company. As the choreographed walk proceeded, Hartley continually asserted that there had been no damage. He also emphasized that there really was no oil on the beach. Upset at Hartley’s statements, I contradicted him, stating that the tide came in, the tide went out, and each time the tide came in it deposited a layer of oil. Impulsively, I kicked at the sand, sending an oily glob of sand onto a highly strategic area of the President’s trousers.”

“With this incident, ‘environment’ became a central issue of American politics for the next decade. Two of the principal accomplishments of this period were the passage of NEPA—the National Environmental Policy Act—and the establishment of the Environmental Protection Agency. This turned out to be the most important work I did in the politi-

cal domain, an example of how scientists being involved in politics does make a difference.”

“At the time, NEPA’s critics said it was vague and inconclusive. Yet with its clear statement of intent—that it be the policy of the federal government to ‘use all practicable means and measures . . . to create and maintain conditions under which man and nature can exist in productive harmony and . . . [meet] the needs of the present without compromising the ability of future generations to meet their own needs,’ NEPA anticipated the concept of sustainable development.”

“The establishment of the Environmental Protection Agency is another example. CEQ played a major role in this. The framework of laws that today give the federal government authority to protect the environment all came out of the work carried out by the CEQ between 1970 and 1972. But by late 1972, the shadow of Watergate had crept over the White House, and I resigned.”

“People don’t generally think of Richard Nixon as having been a great environmentalist, but he was a very astute politician and he knew that environmentalism was going to be a big issue in the 1972 election. So he wanted to do something about it, to have something tangible to point to, and he took the advice of his scientific advisors (at least in this instance). EPA and NEPA were the result. I was very proud of this work.”

“My work on the CEQ convinced me that environmental problems had to be addressed from multiple angles and required discourses between the environmental scientist, the economist, the lawyer, and the sociologist. We needed

to develop overall strategies and policies for dealing with the whole problem of the environment. These conclusions motivated me in 1972 to become the first director of the Environmental Studies Program at Dartmouth College. The principal mission of this program was to provide an opportunity for undergraduates to assess the seriousness and complexity of environmental problems and to understand how these problems can be solved.”

On the Segue from Weather Modification to Climate Change.⁶ “Why was I optimistic about weather modification, when so many others were skeptical? I considered my optimism justified on three grounds. 1) The basic understanding of the physical processes of the atmosphere had been achieved. The atmosphere was complex, but not mysterious. 2) High speed computers were making it possible to model atmospheric processes, including the effects of cloud-seeding experiments, and 3) A new array of instruments, particularly satellites, was making it possible to observe and detect atmospheric changes. Satellites in particular would soon make global coverage possible. It seemed to me that the nonscientific aspects of weather modification—political, economic, sociological—would prove far more difficult than the scientific ones. At the same time, I also became an advocate for increasing our basic understanding of the environment through the growth of environmental science.”

“Perhaps more important, I became convinced that inadvertent weather modification was already occurring. Like many earth scientists, my initial concern was the opposite of what concerns us today: global cooling. In a 1970 lecture to the Industrial College of the Armed Forces, I said: ‘Apart from changing the character of the air, the vast quantities

of material introduced into the atmosphere may be changing the climate of the planet. While we do not know whether the changes observed result from putting carbon dioxide and particulate matter into the atmosphere, or indicate basic natural changes, it is unmistakable that the atmosphere is cooling off and has been cooling for the past 30 years. The average temperature worldwide has dropped about half a degree Fahrenheit over these last 30 years.’ This perspective was consistent with the geological understanding of the time that we live in an inter-glacial period and are heading towards the next ice age. Our worry was that our actions might be accelerating that journey.”

“Yet, at the same time, we knew that carbon dioxide could have the opposite effect as particulates, and induce global warming. In same lecture I continued: ‘We do know that the carbon dioxide content of the atmosphere has increased by about 10 percent over the last 70 to 80 years, the period of the great industrial revolution.’ Elsewhere I suggested that the addition of carbon dioxide to the atmosphere had produced an increase in the average temperature of the lower atmosphere of a few tenths of a degree Fahrenheit—an increase that might have been greater were it not for the countervailing effects of urban and industrial pollution. The key point was that the long-held assumption that the land, water, and air can absorb waste products in unlimited quantities was wrong. The ocean, the atmosphere, and even the solid Earth had been viewed as receptacles of essentially infinite capacity; now we were recognizing that on a local, regional, and even worldwide scale we might have exceeded nature’s capacity to dilute the effluence of our technology. And we knew too little about the paradoxical effects of warming and cooling to tell what the net outcome might be.”

“We cannot detect changes, either desirable or undesirable, without repeated observations and established baselines. One of the most important and convincing monitoring programs was that of Charles Keeling, who by 1969 was already able to show that atmospheric CO₂ was increasing by approximately 0.2 ppm per year, and that, of all the CO₂ produced by combustion, two-thirds are absorbed by the oceans and biomass, the remaining one-third remaining in the atmosphere. At that rate of deposition, the amount of man-made atmospheric CO₂ was doubling every 23 years. I argued for immediate attention to the issue: to a high priority for increased support for research on inadvertent modification, with particular attention to the effects of altering the thermal balance by changes in the albedo, CO₂ and dust particles.”

“In 1969, it seemed plausible that our activities could either lead to a disastrous ice age or to an equally disastrous melting of the polar ice caps. Interest in the topic mushroomed, and, as a member of the JASON committee, and through the MITRE Corporation, I undertook a series of studies, funded by the U.S. Department of Energy, Office of Energy Research, which convinced me that the scientific basis for the greenhouse effect was sound.”

“Keeling had continued his painstaking measurements of atmospheric CO₂ at a remote site in Hawaii, now the Mauna Loa Observatory, and demonstrated continued exponential growth—with concentrations approaching 350 ppm by the late 1980s. The exponential growth in carbon dioxide levels paralleled the increased worldwide use of carbon-based fuels, while calculations of the expected increase in average temperature of the Earth’s surface since 1900 led to a value of about 0.5°C, matching the detailed analysis of

tens of millions of surface-temperature observations. Given continued growth in fossil fuel use, major climatic shifts could be expected as warming proceeded at an increasing pace.”

“Past climate change—such as the Little Ice Age in 1500-1850 AD—had had a profound effect on human history. But before 19th century industrialization, man’s activities were of too small a scale and too low an intensity to alter global climate. With the mechanization of agriculture and the greatly enhanced use of carbon-based fuels, particularly coal, the situation changed. Both the burning of coal and the greater development of agriculture released carbon that had been stored in the soil and rocks for thousands or millions of years.”

“In view of the heightened interest in long-term climatic change, the question naturally arose as to whether the warming trend would have been noticed if theory had not predicted that it should be there. I was convinced of the warming long before the detailed analyses of the temperature records were available. I had observed that the snouts of the glaciers on the Alps on the south island of New Zealand had moved from sea level to high up the mountain between 1900 and the present. New Zealand, having a relatively isolated geographical setting in the ocean, was more likely to capture longer-term trends than glaciers in more continental regions. Critics will undoubtedly question the reality of the derived warming. Nevertheless, the statistical base for the inference is strong, and the independent confirmation from the Arctic [permafrost] may prove persuasive.”

RICHARD MULLER WRITING ON GORDON'S LATE CAREER

I had known Gordon since the 1980s when I became a member of JASON, a Department of Defense scientific advisory committee established in the 1950s, of which Gordon was a longstanding member. But I had never worked with him on any projects. However, I had a vivid memory of a talk he had given in the late 1980s concerning the state of knowledge of climate modeling, particularly with regard to the cycles of the ice ages. The “standard model” was the Milankovitch theory, as modified by John Imbrie and others. This theory explained the ice age cycles as due to variations in the eccentricity of the Earth’s orbit, and changes in the tilt of the poles, formally known as the “obliquity.” I became interested in this subject, and developed a hypothesis that extraterrestrial dust could affect climate. I asked Gordon whether he could point me to a paper that would convince me that the Milankovitch theory was basically correct. “Yes, and I’ll give you a copy in a moment.” He rummaged through his desk, and gave me a copy of a review paper he had written in 1990.

This paper should be considered a classic (1990). It covered two subjects, the Milankovitch theory and the potential role of marine clathrates in climate. He brought to the work a sophistication in statistical analysis he had learned from one of the fathers of the field John Tukey, and that was far above the standards being used in the field. After I read the paper he lamented that “now there were two people who have read it.” So we began to collaborate.

I remember a day when I did a calculation of some oxygen isotope data with my laptop and showed it to Gordon. He was disturbed. “The 100 kiloyear peak is too narrow,” he pointed out. “The Milankovitch theory doesn’t have such a narrow peak.” His observation became the basis for

a major follow-up effort that we did over the next eight years.

In “Glacial Cycles and Astronomical Forcing” (1997) we extended Gordon’s 1993 insight: that the narrow 100 kiloyear peak was *prima facie* evidence that the cycles of the ice ages were driven by orbital forcing and were not the result of internal changes in the earth or the sun. Our book *Ice Ages and Their Astronomical Causes* (2000) had two goals: to explain in detail all of the aspects of paleoclimate that we had uncovered and to prepare a primer for proper statistical analysis of such data.

In 1990 Gordon left his job with the Mitre Corporation and returned to academia. His passion had evolved over the decades from pure science to the use of science to address world issues. Rather than take a job in geophysics he accepted a position as a professor of international relations in the Graduate School of International Relations and Pacific Studies at the University of California, San Diego. He also served as the research director for international environmental policy at the Institute on Global Conflict and Cooperation on the same campus. He continued as a member of the Board of Directors for the Environmental Research Institute of Michigan and as one of the most active participants in JASON, with much of his effort directed toward environmental issues—which he considered to be central to U.S. national security. His academic research was largely directed toward climate—including some mysterious behavior in the radiation balance of clouds—and work in frozen deep-ocean methane deposits known as clathrates. He argued that they probably played an important role in paleoclimate, since their existence was potentially unstable to changes in temperature and sea level. The role of clathrates is still largely mysterious, but Gordon is recognized as one of the first people to draw attention to them.

Gordon loved the environment at San Diego, but he was not happy with his administrative burdens. He had little help and had to spend much of his own effort organizing visitors and meetings. Those duties limited his ability to teach, to study, and to continue his environmental investigations; so he resigned. Then in a strange twist he accepted a job with far greater administrative burden but one that had potentially greater impact on world affairs: director of the International Institute for Applied Systems Analysis (IIASA) at Laxenburg, on the outskirts of Vienna, Austria. He saw this institute as an organization that could exert important influence on its member countries.

IIASA was in serious financial difficulties, and Gordon applied his incredible intuition about the stock market to the institute's portfolio, outperformed the professional investors, and greatly enhanced the institution's finances. He worked hard to expand membership in IIASA, and managed to get Norway to join. He hoped other countries would follow. He upgraded the mathematical standards of the institute, insisting on rigorous statistical methods. He refused to reappoint people whom he considered to be "dead wood," but this made enemies, leading to an acrimonious parting with the institute's Governing Council.

CONCLUDING THOUGHTS

The book on Gordon is yet to be written. We recall his academic career, from chemical engineering and geology at Harvard (his *summa cum laude* was the first in the department), to an assistant/associate professorship in geology and geophysics at MIT, to a full professorship (at the age of 29) at the Institute of Geophysics at the University of California, Los Angeles, to a professorship in physics and geophysics at the University of California, Santa Barbara, to the chair in environmental studies and policy at Dartmouth,

to the Institute of Global Conflict and Cooperation (IGCC) at the University of California, San Diego, to the directorship of the International Institute for Applied System Analysis (IIASA). Interspersed is a one-year residence at NASA and seven years as vice-president and chief scientist with the MITRE Corporation. Add to this his many, many services on national and international committees, and the enumeration alone would fill the allotted pages of this biographical memoir.

Gordon was elected to the National Academy of Sciences in 1962 at the age of 32!

We have chosen to emphasize two major late twentieth-century issues in which Gordon played a significant role: plate tectonics and climate. On plate tectonics, in the long run Gordon's opposition to Earth mobility turned out to be in error; he based his reasoning on a model Earth of finite strength rather than the high-temperature creep of nearly (or partially) molten material. In any event he chose not to be one of the late jumpers on the bandwagon. Perhaps he was just stubborn. But Gordon was not alone; he was joined by other distinguished geophysicists, such as Maurice Ewing and Harold Jeffreys (as well as by various aging directors of Soviet geology institutes).

On weather and climate modification Gordon was one of the earliest scientists to call attention to carbon dioxide as a specific problem, to push forward the scientific understanding of the likely impacts of increased atmospheric CO₂, and to bridge the science policy divide.

A final word needs to be said on Gordon's contributions to national defense and intelligence issues. Gordon served on JASON for 37 years; during the Vietnam War he chaired a JASON committee on designing the "McNamara Fence" (an instrumented frontline for preventing enemy intrusion into South Vietnamese territory), probably the

earliest version of the instrumented battlefield. Gordon was passionate about JASON, where he brought his formidable intellect to bear on an enormous diversity of problems. He always spoke his mind, driven by his insatiable curiosity in a multitude of fields and his ability to convey the excitement of the research endeavor to scientists and lay people, to politicians and bureaucrats.

Gordon chaired the MEDEA Committee (initially the Environmental Task Force) of the Central Intelligence Agency, a brainchild of Senator (later Vice-President) Gore for the application of "overhead assets," that is, information collected by intelligence satellites, to the solution of environmental problems. Here the intelligence and academic communities, two disparate communities meeting initially under conditions of mutual mistrust, developed over the years a feeling of trust and respect. Gordon was at his best, combining his unique environmental background with patience and perseverance. In 1994 the CIA presented Gordon with the Seal Medallion, the highest civilian honor of the agency.

In the early 1990s after leaving MITRE and having changed his interests and residence so often, he found himself without a stable home base. Neither the Graduate School of International Relations and Pacific Studies of the University of California, San Diego, nor the Governing Council of IIASA, nor his body now weakened by childhood polio, could accommodate to his singular style of work and living. He was reluctant to use a walker or wheelchair, and Austria was not friendly to his developing handicaps. It became yet another burden on his energy, and that of his wife, Margaret. Upon his return to the United States from Vienna Gordon took up residence in Cambridge, Massachusetts, but without formal association with any of the institutes or departments he had helped to develop, or indeed had founded.

We remember Gordon for his warm friendship, insatiable curiosity, and powerful intellect. He was an inspiration to his students and to all who knew him. He was never dull. In the words of Freeman Dyson, “It is bad for the world that Gordon’s informed and critical voice is silent.” Gordon is survived by his wife, Margaret Stone MacDonald, three sons, one daughter, and five grandchildren.

NOTES

1. G. J. MacDonald. How mobile is the Earth? In *Plate Tectonics: An Insider’s History of the Modern Theory of the Earth*, ed. N. Oreskes with H. Le Grand, pp. 111-27. Boulder, Colo.: Westview Press, 2001.

2. Most of the prose in this section is taken directly from Gordon’s articles (with minor editorial modifications or in a few cases sentences added to clarify points based on e-mail communications in the autumn of 2001) and my conversation with Gordon at his home in Cambridge, Massachusetts, on October 17, 2001.

3. These are short selected quotes from “How mobile is the Earth” in *Plate Tectonics: An Insider’s History of the Modern Theory of the Earth*, ed. N. Oreskes with H. Le Grand. Boulder, Colo.: Westview Press, 2001. According to Margaret McDonald, this was Gordon’s last serious writing.

4. Excerpted primarily from G. J. F. Macdonald. Science and politics of rainmaking. *Bull. At. Sci.* 4(1968):8-14, with additional materials based on *Weather and Climate Modification*, Report of the Panel on Weather and Climate Modification of the National Academy of Sciences, pub. no. 1350 NAS./NRC, Washington, D.C., 1966; MacDonald. How to wreck the environment. *New Sci.* 25(Apr. 1968):180-82; and MacDonald. Weather modification as a weapon. *Technol. Rev.* 78(1975):56-63.

5. Excerpted from MacDonald, 1965. Environmental sciences—problems and prospects. *Trans. Am. Geophys. Union* 46(2):373-76, 1965; G. J. F. MacDonald. Environment: The evolution of a concept. In *Yesterday, Today and Tomorrow: The Harvard Class of 1950 Reflects on the Past and Looks to the Future*. Arlington, Mass.: Travers Press, 2000; G. J. MacDonald. Environmental studies program: Dartmouth College. *Chemosphere* 5(1977):185-205; and conversation with Naomi Oreskes, Cambridge, Massachusetts, October 17, 2001.

6. This section is compiled from several of MacDonald's extensive writings on climate change from the late 1960s onward. These include: Science and politics of rainmaking. *Bull. At. Sci.* 4(1968):8-14; Weather modification. *Sci. J.* 4(1968):39-44; Our beleaguered environment. In *Perspectives in Defense Management*, autumn 1971, pp. 11-20; How to wreck the environment. *New Sci.* 25(Apr. 1968):180-82; Caring for our planet: How man endangers the planet. *Current* 114(1970):17-24; Climatic consequences of increased carbon dioxide in the atmosphere. In *Power Generation and Environmental Change*, ed. David Berkowitz and Arthur Squires, pp. 246-62. Cambridge, Mass.: MIT Press, 1971; Pollution, weather and climate. In *Environment: Resources, Pollution, and Society*, 1st ed., ed. W. W. Murdoch, pp. 326-36. Stamford, Conn.: Sinauer Associates, 1971; The long-term impacts of increasing atmospheric carbon dioxide levels. JASON Technical Report JSR-78-07 and JSR 789-04. Arlington, Va.: SRI International, 1982; Scientific basis for the greenhouse effect. *J. Policy Anal. Manage.* 7(3)(1988):425-44.

SELECTED BIBLIOGRAPHY

1953

Anhydrite-gypsum equilibrium relations. *Am. J. Sci.* 251:884-98.

1956

Quartz-coesite stability relations at high temperatures and pressures. *Am. J. Sci.* 254:713-21.

1958

With L. Knopoff. The magnetic field and the central core of the earth. *Geophys. J. R. Astron. Soc.* 1:216-23.

With L. Knopoff. On the chemical composition of the outer core. *Geophys. J. R. Astron. Soc.* 1:284-97.

1959

Calculations on the thermal history of the earth. *J. Geophys. Res.* 64:1967-2000.

1960

With W. H. Munk. *Rotation of the Earth, a Geophysical Discussion*. Monograph in Applied Mathematics and Mechanics. New York: Cambridge University Press.

With F. Gilbert. Free oscillations of the earth. 1. Toroidal oscillations. *J. Geophys. Res.* 65:675-93.

Stress history of the moon. *J. Planet. Space Sci.* 2:249-55.

1961

Interior of the moon. *Science* 133:1015-50.

With N. F. Ness. A study of the free oscillations of the earth. *J. Geophys. Res.* 66:1865-1911.

1963

With K. Hasselmann and W. H. Munk. Bispectra of ocean waves. In *Time Series Analysis*, ed. M. Rosenblatt, pp. 125-39. New York: Wiley and Sons.

The internal constitutions of the inner planets and the moon. *Space Sci. Rev.* 2:473-557.

With W. H. K. Lee. The global variation of terrestrial heat flow. *J. Geophys. Res.* 68:6481-92.

1964

The structure and strength of the inner planets, space exploration and the Solar System. *Rendiconti della Scuola Internazionale d. Fisica, E. Fermi*, XXIV Corso, pp. 60-141, Academic Press.

1965

Origin of the moon: Dynamical considerations. *Ann. N. Y. Acad. Sci.* 118:739-82.

Continental structure and drift. *Philos. Trans. R. Soc.* 258:215-27.

1968

Science and politics of rainmaking. *Bull. At. Sci.* 24:8-14.

1975

Weather modification as a weapon of war. *Technol. Rev.* 78:56-63.

Intentional and unintentional modification of the atmosphere. In *Organizing for Global Environmental and Resource Interdependence*. Report by the Commission on the Organization of the Government for the Conduct of Foreign Policy, June.

1982

Ed. *The Long-Term Impacts of Increasing Atmospheric Carbon Dioxide Levels*. Cambridge, Mass.: Ballinger.

1990

Global climate change. In *Global Climate and Ecosystem Change*, ed. G. J. MacDonald and L. N. Sertorio, pp. 1-96. New York: Plenum.

1997

With R. A. Muller. Glacial cycles and astronomical forcing. *Science.* 27:215-18.

2000

Environment—the evolution of a concept. In *Yesterday, Today and Tomorrow—The Harvard Class of 1950 Reflects on the Past and Looks to the Future*, ed. G. S. Mumford. Arlington, Mass.: Travers Press.

With R. A. Muller. *Ice Ages and Astronomical Causes: Data, Spectral Analysis and Mechanisms*. New York: Springer.

2001

How mobile is the earth? In *Plate Tectonics: An Insider's History of the Modern Theory of the Earth*, ed. N. Oreskes, pp. 11-27. Boulder, Colo.: Westview.

