A Nuclear Winter's Tale

Science and Politics in the 1980s

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Preface

Highly speculative theories of worldwide destruction—even the end of life on Earth—used as a call for a particular kind of political action serve neither the good reputation of science nor dispassionate political thought.

—Edward Teller, "Widespread after-effects of nuclear war," *Nature* 310 (23 Aug. 1984), 621–624, quote on 624

For myself, I would far rather have a world in which the climatic catastrophe cannot happen, independent of the vicissitudes of leaders, institutions and machines. This seems to me elementary planetary hygiene, as well as elementary patriotism.

—Carl Sagan, "Nuclear war and climatic catastrophe: Some policy implications," *Foreign Affairs* 62 (winter 1983–84), 257–292, quote on 286

The concept of nuclear winter (NW) burst upon the consciousness of the American public toward the end of 1983. The idea was all the more alarming because until then a nuclear winter had not been among the anticipated effects of nuclear war.

The story did not begin there, of course. Many research specialties, pursued over many years, provided the background of personnel, information, techniques, and ideas that coalesced in the shocking concept that fires from nuclear explosions could produce enough smoke to block sunlight for extended periods of time. Added now to the horror of the immediate effects of nuclear war was the threat of long-term reductions in continental temperatures, extending to noncombatant nations, such that the living would have additional cause to envy the dead.

NW was (and remains) a controversial concept. All scientists did not agree on the technical assumptions or on the value of the computer programs needed to simulate the effect. But the validity of the NW prediction is not the central issue in this book. As a historian of science, it is not my task to solve scientific controversies but to report and analyze them. Thus, I trace the story of the several scientific specialties that came together to suggest the concept of NW, and then the subject's development as a specialty of its own. Basic sciences such as physics and chemistry led to interdisciplinary subjects, including meteorology, ecology, and the effects of nuclear weapons.

Further, some scientists, and others, objected to the "marketing" of NW in the media. They questioned the behavior of scientists, the standards of the scientific community, and the manner in which science policy was formed.

Because the consequences of nuclear war were of concern to the government and the general public, suggestions for avoiding NW meshed tightly with the ongoing national security debate and were often regarded as politically motivated. The Reagan administration's hard-line military stance and its budgetary realities dictated some aspects of the scientific program. In the historical trade this influence by society on science is now called "social construction of science."

In this book I seek to describe three lines of NW activity: science, popularization, and politics. If ever they were uncomplicated pursuits, they certainly were not by the second half of the twentieth century. No longer largely self-contained, they interacted continually. And there were nuances, such as the different international and national styles of politics and the scientific community's concern about its self-image.

I am conscious of my inability to tell the "entire" story. The science would require a technical text, which is not my intention. Another constraint is that today so much business is conducted by telephone, in face-to-face discussions, and by means of unpreserved correspondence that much of the story cannot be documented. For the politics, I can rely upon printed sources and some unpublished materials, but I can only guess at the motivations of many individuals. In short, this is a normal historical work. In it I strive for accuracy, fairness, and a balanced presentation of the subject while nonetheless expressing my point of view. But I do not pretend to cover the topic in encyclopedic fashion; rather, I use the materials available to me, and I recognize that my picture inevitably is incomplete. It is, moreover, biased in the sense that I focus primarily on the issue of smoke in the atmosphere and its effects on the climate. Other serious consequences of nuclear warfare, such as the downwind distribution of radioactive fallout and pyrotoxic materials and increased ultraviolet radiation reaching the Earth's surface once the skies have cleared somewhat, are mentioned only in passing.

Careful readers will note that I use the abbreviation NW in a variety of ways. It may refer only to the climatic consequences of urban fires or also to the effects upon agriculture and Earth's population. In other cases, it may refer to people (such as "NW supporters") who accepted as valid the dire predictions of global cooling and the need for political action to prevent such a tragedy. I believe that the meaning is obvious in each instance and that the shorthand is useful.

In writing the book, I consciously rejected "structural" arguments that give dominant weight to economic considerations, technological imperatives, ideological forces, geopolitical hegemonic struggles, domestic politics, or bureaucratic inertia, recognizing that each of these things played a part but none predominated in framing attitudes or determining events. My "thesis," if such a commonplace explanation of events is worthy of the term, is that normal human behavior set the stage and gave the actors their lines. Scientists investigated subjects that interested them and then debated the results; politicians sought to take advantage of new events and at the same time defend their long-time interests.

After World War II, when science and technology were recognized as extremely important to society (not just to national security), agencies and structures were created to encourage and manage the flow of technical advice to government. In this story about NW, the bureaucratic process worked, but the picture is a messy one nonetheless. Some agencies had to be prodded to action, while others worked at cross-purposes. Guidance from outside the government and its usual circle of advisors was both welcomed and rejected. Perhaps because the US is so large, with so many government and private entities, the process could not have been more rational. It is logically frustrating to observe the chaos that ensued as scientific ideas attempted to influence policy positions; at the same time, it is democratically exhilarating that there were so many pathways for such attempts.

Just as I noted above that I did not intend to write a science textbook, it is useful to note that I did not intend to write a political science manual. While I recognize that there are flow charts describing the relationships among various government agencies, and that analyses of lobbying efforts exist, I see no deterministic picture of NW information flowing in prescribed ways from source to receptacle. It is a muddled, chaotic, controversial, and very human story of a scientific idea moving through the scientific community and the government bureaucracy.

As I just intimated, beyond the main story about the development of this scientific specialty I see NW as an exemplar of twentieth-century interaction between science and society. In many cases, if not most, the lone scientist, working with small apparatus and on a small budget, has given way to Big Science, with its team research, large machines, generous funding, high visibility, and frequent political-military-economic impacts. This has been an enormous change in the workings of science and in science's dealings with the rest of society.

For most of recorded history, scientists interacted with government far less than did the clergy, the military, lawyers, and merchants. Among the ancient and medieval exceptions were Archimedes, Hero of Alexandria, Galen, astrologers, and alchemists. Courtly patronage of natural philosophers (scientists) grew during the Renaissance. In early modern times, Galileo and Lavoisier were prominent for their connections to the ruling elite. In the nineteenth century, some governments supported geology, mapping, public health, and other "useful" activities. But the total number of examples of such interactions was relatively small. By the twentieth century, however, science was far more integrated into daily life. It not only provided intellectual value (in the form of ideas about nature that added to a nation's prestige); it also offered concepts and devices that were important to a nation's economy and security.

With the increased prominence of science, the public came to the unsettling realization that scientists often disagreed, and that the vaunted "scientific method" was not an infallible tool for reaching the "truth"—whatever "truth" meant. Experiments were challenged for the validity of the data they produced, and the meanings of the results were disputed even more. And when the results affected public policy, politics entered the discussion. Scientists became still more polarized, and the public could wonder whether their personal views on war, peace, social welfare, and economics influenced the way they read the consequences of their experiments and their analyses.

Of course, NW was not the only area of science to have an enormous potential impact on society; molecular biology, weather modification, medical research, and agricultural research are other examples. But I find the story of NW intrinsically interesting and important, and I see it as a useful illustration of my view of the current complex relationship between science and society.

More particularly, NW serves as an example of the problems faced by scientific Cassandras. Scientific prophets of disaster must first make themselves heard; then they must convince fellow scientists, the public, and government that they are correct. But doing so is enormously difficult. New interpretations of scientific data step on the toes of people with established careers. Proposed policies or innovations may introduce new dangers (for example, ozone depletion caused by supersonic aircraft, the spraying of malathion—a carcinogen—to combat Medfly infestation). Onerous changes in the operation of businesses or in daily life may be required (for example, industrial pollution controls, bans on DDT and on barbeque lighter fluid). Large sums of money may be needed, and may have to be redirected from existing appropriations (as in the case of a Superfund cleanup). And a call to action may be a call to topple a long-held political conviction (military superiority, freedom to make and sell products, freedom to dump wastes). Those who accepted the validity of NW and then sought to effect changes in national policy found, like Chicken Little, that some leaders might be solicitous but few were disposed to respond with action to the cry that the sky was falling.

The chapters in part I provide background information about the arms race and the state of the several scientific specialties that led to the point at which NW could emerge from computer printouts. The chapters in part II describe the birth of the concept, the initial efforts to popularize it, concerns within the scientific community about its own standards and procedures, and various political maneuvers and policy proposals, all before the end of 1983. Part III (the heart of the book) explores developments in 1984 and 1985, the years that saw the greatest amount of NW activity. Chapters in this part treat such topics as public relations; congressional hearings; administrative meanderings; debates between critics and supporters of NW; new research on computer models, on fire phenomena, and on ecology; several reports by prestigious committees; the diminishing role played by Soviet scientists; and policy proposals of many kinds. The chapters in part IV finish the historical story, including the uncertain lessons learned from the 1991 oil well fires in Kuwait; in these chapters I attempt to place NW in the larger context of providing scientific advice to a government that does not always want it.

This book is not only for the relatively few historians, sociologists, and philosophers of science interested in contemporary issues; it is aimed also at scientists and the general public. Thus, I avoid the in-house "science culture war" debates about theoretical and methodological approaches to the study of the scientific enterprise.

I was fortunate in my efforts to gather information for the book. Many of the most prominent scientists investigating NW gave generously of their time for interviews, provided correspondence and documents, and in other ways responded graciously to my inquiries. I warmly thank them collectively for their assistance, while accepting responsibility for any errors. These "actors" and others who gave support include Thomas Ackerman, Margot Alexander, Steven Van Beek, Hans Bethe, George Bing, Linton Brooks, George Carrier, Curt Covey, Paul Crutzen, Sylvia Curtis, Freeman Dyson, Paul Ehrlich, Joann Eisberg, J. J. Gertler, Christopher Griffith, Jacob Hamblin, John Harte, Tsuyoshi Hasegawa, Frank von Hippel, John Holdren, Marcel LaFollette, Barbara Levi, Bruce Lewenstein, Harold Lewis, Fredrik Logevall, Michael MacCracken, Robert Malone, Patrick McCray, Roger Meade, Eugene Miya, Jeannie Peterson, James Pollack, George Rathjens, Alan Robock, Carl Sagan, Jacob Scherr, Stephen Schneider, David Simonett, Tina Skandalis, Jeffrey Stine, Starley Thompson, O. Brian Toon, and Richard Turco.

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