JOHN J. NANCE

AUTHOR OF BLIND TRUST AND ON SHAKY GROUND

WHAT GOES UP

THE GLOBAL ASSAULT ON OUR ATMOSPHERE

CHAPTER 2

1. High-flying jet engines leave behind NO (nitric oxide) and NO_2 (nitrogen dioxide).

2. Mario Molina had received his early education in Switzerland, graduating from the University of Mexico with a bachelor's degree in chemical engineering, then attending both the University of Freiburg in Germany, and the Sorbonne in France before electing to study for his Ph.D. under Dr. George Pimentel at the University of California at Irvine, where he joined Sherry Rowland's research group. Molina had just turned thirty at the time.

3. CFC's are not lighter than air, they are heavier, which seems a contradiction. The molecular weight for CCl_3F, for instance, is 137.5 versus a molecular weight of 29.0 for "air." As Professor Rowland explains it: "The mixing of molecules occurs in the atmosphere in large air masses, taking heavy and light at the same rate. When eventually caught in an updraft, they all go up together. The motion is random, up, down, down, up, up, etc., and eventually a string of 'ups' [propels] the CFC's into the stratosphere."

4. Rowland and Molina's findings at that point are summarized as follows: "No tropospheric sinks; photolysis in the stratosphere, but only after 40–80 years for CCl_3F, and 75–150 years for CCl_2F_2; and photolysis released Cl atoms."

5. Molina actually went to the lab to repeat and extend the measurements of UV-C cross sections for CFC's, since data developed by Du Pont had been incomplete. Cross sections provide the quantitative knowledge needed about the rates at which molecules decompose (photodissociate). Both he and Rowland assumed that if a molecule intercepted UV-C, the molecule would fall apart in *every* case. A year later they proved that assumption.

CFC's react with the class of ultraviolet wavelengths roughly classified as UV-C. UV-C is generally defined as wavelengths of ultraviolet below 230nm. UV-B, the wavelengths of UV so damaging to carbon-based DNA on Earth's surface, is roughly defined as 280–320 nm, while UV-A is in the range of 320–400 nm. The service provided by O_3 (ozone) molecules that has permitted life as we know it to develop and flourish on Earth is the continuous absorption of wavelengths of UV-B in the 280–320 nm range. That absorption of energy is translated through the breakage of the molecular bonds into heat energy at stratospheric heights, which gives rise to the inversion heat layer in the stratosphere that in itself defines the vertical delineation of the stratosphere.

6. The stratospheric answer: Chlorine's reaction with ozone is about a thousand times more likely than its reaction potential with anything else. So chlorine reacts with ozone to give ClO—chlorine monoxide.

7. The chlorine atom is a free radical too, with seventeen electrons. ClO has twenty-five electrons.

8. Sherry Rowland explains the process this way: "When chlorine is liberated from CFC's, it comes off as *atomic* chlorine, Cl. The reaction with ozone is the first step in the chain, giving ClO. The second step is ClO + O, completing the first cycle of the chain reaction."

9. Other scientists had done similar work, but they were in the stratospheric research arena, and unknown to Rowland and Molina. At the same time that Mario Molina was working with these figures, the same catalytic chain was being debated by others in relation to the stratosphere. No one, however, had found a source for massive amounts of chlorine. There was nothing as yet in the literature, though Harold Johnson had received a preprint of a paper on the subject from Cicerone and Stolarski.

10. When Molina and Rowland began calculating the atmospheric lifetimes of CFC's in the 40–150 year range, they routinely calculated how much CFC would be in the atmosphere after 300–400 years of continuous emission. Those numbers were much higher than the known 1973 concentrations, so they almost immediately escalated to and focused on the future problem.

11. Two months before Mario Molina's figures reached their startling values regarding ozone destruction, there had been a scientific meeting in Kyoto, Japan, during which Richard Stolarski and Ralph Cicerone proposed that chlorine chemistry might pose some sort of threat to the ozone layer. Teams at Harvard and the University of Michigan were preparing papers on the subject by December of 1973, but neither was considering chlorofluorocarbons as a source of chlorine in the stratosphere. In her book *Ozone Crisis,* Sharon Roan details the original involvement of two scientists—neither of them chemists—who would become a major part of the ozone controversy for the next decade, Cicerone and Stolarski, both of whom determined in 1973 that the space shuttle's exhaust would produce chlorine that would attack and deplete as much as a tenth of a percent of the world's supply. The official reaction was "So what?" but unofficially NASA was frightened enough to suggest that such findings really didn't need to be published, since they might touch off another SST-style brouhaha. NASA remained supportive of their research, however, if scared to death of its potential for public image problems.

12. Harold Johnston held his post despite clandestine political efforts through the board of trustees from within Governor Ronald Reagan's administration to fire the pesky chemist for opposing federal SST policies, and, it was assumed, for being a general environmental nuisance.

13. Rowland remembers it this way: "Yes, we knew that we were going to get a lot of heat, but until you're actually in it, you don't *really* know what it means."

14. The energetic Rowland spent the first week in Vienna renting an apartment, writing the CFC–ozone paper, and simultaneously joining the International Atomic Energy tennis team.

15. The paper was received in mid-January and published about five and half months later. Referee approval came after four months. The rest of the time was prepublication processing.

16. Dr. Paul Crutzen had mentioned the upcoming paper in a talk in Sweden, not realizing that a newspaper reporter was in the audience. On the basis of that reference and a preprinted copy of the Rowland-Molina paper, the reporter wrote an article that headlined the finding. The article got the immediate attention of a Du Pont public relations executive in Europe, who was aghast that Rowland and Molina had used Du Pont's trademark name, "Freon," in connection with what he considered a "ludicrous" theory. After an upset phone call from the man, a chastened Sherry Rowland changed the wording to substitute the generic word "chlorofluorocarbons" for "Freon." In her excellent account of this sequence in the book *The Ozone Crisis,* Sharon Roan reports that in that first phone call, and in subsequent contact, the Du Pont man never seemed to be in the least concerned that the point of the Rowland-Molina paper was, in effect, that his company was producing a product that might be disastrous for mankind.

17. The others were Pennwalt, Union Carbide, Allied Chemical, Racon, and Kaiser Industries. There were a total of twenty producers in the world, but the U.S. group made up about half of world production by volume.

18. In fact, in the sixties, a rash of teenage deaths from "sniffing" or breathing the CFC's in spray cans profoundly upset Du Pont and its senior executives. Though the incidents resulted from purposeful misuse of the product and did not indicate any toxicity, the fact that even the safest of products produced by the most careful of companies could still be tarred with the public image of causing unnecessary death was an excruciating problem.

19. The stratosphere was considered only with respect to global urban smog issues.

20. On the basis of a friend's recommendation, Sherry Rowland sent preprints of the paper from Vienna to Ralph Cicerone and to Paul Crutzen, neither of whom he knew. The key to the calculations was the CFC cross sections in the UV, and this Rowland gave to Cicerone.

21. Scientific journals such as *Nature* and *Science* strongly discourage prepublication publicity by threatening to cancel publication of an article if newspaper articles appear in advance.

22. They also prepared a 150-page support paper.

23. Rowland and Molina also put the expected ozone depletion estimate (with indefinite CFC production continuation at the then-current production levels) at 7 percent to 13 percent. This pair of figures is important because the future calculations were usually done with the same premise: indefinite continuation of CFC emissions at present levels.

24. The media—and especially science writers—had been bombarded with the "end-of-the-world-crisis-of-the-week" for several years, and this seemed like yet another routine doomsday theory.

25. The Wofsy-McElroy findings were not published in *Science* for several more months.

26. The U.S. aerosol market in 1974 had risen to a production level of almost three billion cans annually, about 80 percent with CFC's as the propellant. That works out to around fifteen cans per person per year—sixty per family. When Sherry Rowland and his wife threw their spray cans out in 1973, they found fifteen in their house, and didn't consider themselves big aerosol fans.

27. This was a fact that gave Richard Nixon's friend Robert Abplanalp an additional headache of massive proportions. Abplanalp had just suffered through the demise of the Nixon presidency, and now the basis of his multimillion-dollar fortune was not only being threatened financially, but excoriated as the worst example of environmental rape: the inexpensive plastic valve on the top of each CFC-loaded spray can was manufactured by, or under license from, Abplanalp's Precision Valve Corporation. Abplanalp was so spooked, he took the extraordinary step of writing to the chancellor of the University of California at Irvine asking that Professor Rowland be prevented from further public discussion of the issue. The request was firmly denied.

28. IMOS membership was entirely government employees, one each from about fourteen federal agencies. One of the co-chairmen, Warren Muir, is quoted by Sherry Rowland as saying that he had initially looked on the committee as an opportunity to finally rule against a crackpot kind of environmental alarm. He quickly converted when confronted with the facts, however, and became a strong leader.

29. Chlorine nitrate is very hard to handle because it reacts very rapidly with any water in a lab experimentation system. It took researcher John Spencer several months to learn how to handle it cleanly. When the subject of chlorine nitrate came up later in the midst of Rowland and Molina's ozone-chlorine research, they had a four-month head start on everyone else in being able to test their ideas about the compound as a stable sink for chlorine.

30. The formal rule making was published in the *Federal Register* by the Environmental Protection Agency, the Consumer Product Safety Commission, the Department of Health, Education, and Welfare, and the Food and Drug Administration

on Friday, March 17, 1978 (Part II). This was the final rule. There was a previou rule requiring warning statements on consumer products that contained CFC's whicl was published on April 29, 1977 (42 FR 22017) that had gone into effect on Octobe 31, 1977. Yet another Notice of Proposed Rulemaking came out on August 20, 1978 for sunscreen drug products sold over the counter, standardizing the sunscreen rat ings and product quality with a numerical rating that we still see on such product today.

31. The book even came after the public—and Ms. Burford—had knowledge of th ozone hole over Antarctica.

CHAPTER 8

1. Adrian Tuck, who shaved his mustache sometime later, characterized it as more of a "Mexican bandit" style rather than the author's firsthand description of "walrus." He does admit, however, to being barrel-chested.

2. The degradation of the diffuser plate was from exposure to the sun, and it was a well-established fact as early as 1980. The ozone calculations, in other words, were adjusted to compensate for the downward drift of the readings. The question before the Trends Panel members, however, was whether the algorithm which Arlin Krueger and Don Heath were using to adjust for the drift was nevertheless still leaving a bias in the adjusted data.

3. The evolution of the ideas that later coalesced into a theory to explain the Antarctic ozone destruction by means of heterogeneous reactions on the surfaces of ice crystals began long before Joe Farman's paper was published. (Please see note 13 in Chapter 3 for a fascinating look at the history of this theory and its evolution.)

4. The extremely detailed calculations they were involved in producing were not just a reevaluation of previous work, but an entirely new statistical calculation which was threatening to overturn a long-held conclusion arrived at years before by professional statisticians: that there was no ozone loss trend found in the Dobson station readings. Du Pont's McFarland, for one, wanted to be absolutely certain that the potential conclusion that there *was* in fact a statistically significant loss of ozone in the Northern Hemisphere didn't just originate with the 1982–83 results which had been presumably affected by El Chichón.

5. The resulting paper, "Observations of the Nighttime Abundance of OClO in the Winter Stratosphere above Thule, Greenland," by Susan Solomon, G. H. Mount, R. W. Sanders, R. O. Jakoubek, and A. L. Schmeltekopf, was published in *Science* on October 28, 1988. With the data taken in February 1988, the paper written by and submitted in July with an October publication date, that was about as fast as a scientific team could report through the refereed journals.

In the 1989 expedition, Jim Anderson would find as much OClO over the Arctic as over the Antarctic.

6. While the worldwide Dobson network measures a ratio of UV-A (which is not absorbed by ozone) to UV-B, the direct-reading UV-B meters measure only UV-B light striking the sensor from any angle. Thus, while decreased Stratospheric ozone would be detected by a Dobson even in the presence of increased ozone presence

around the reading site, a UV-B meter would simply average the Stratospheric loss of ozone (as increased UV-B) with a low-altitude gain in ozone (due to smog and pollution which would absorb the added UV-B reaching the surface), and perhaps report a steady level of UV-B which would falsely imply that there had been no change in Stratospheric ozone.

7. At one point, Graham was barely prevented from pouring twenty million dollars into an off-the-wall scheme to destroy the CFC's already in the atmosphere by stimulating the ionosphere with radar! Even if enough radar energy could be focused on the ionosphere to energize such a process, there is a rather substantial flaw in the plan: CFC's never reach the ionosphere!

8. The previous statistical approach had assumed that if an ozone loss were to occur, it would be the same for all seasons of the year. When that assumption is coupled with the realization that winter Dobson readings are extremely "noisy" compared to more stable summer readings, then it makes sense to weight the data so that the more stable summer readings are predominant. If it's the same for all seasons, this is a sound methodology. If there is any difference in terms of seasonal ozone loss, this method can yield significant errors.

The actual summer readings do, in fact, show at the most very small changes in ozone levels, and so this had become the statistical conclusion for the entire data set from the world's Dobson stations. *But,* the winter figures were showing significant losses. Yet those losses were hidden by the use of the stable summer data as the predominant authority.

Another aspect of the problem was the extensive delay in publishing what data existed. The 1986 WMO-UNER report, for instance, was quoting published evaluations of ozone data through 1979–80 because updates incorporating 1984 and 1985 figures had not been worked up for publication.

9. This is a controversial issue. Another view is that the Antarctic vortex is more of a flow reactor than a containment vessel, and as a flow reactor, it spins off ozone-poor air to the mid-latitudes. In other words, mid-latitude ozone losses might be more the result of mixing ozone-poor polar air with mid-latitude air than the result of any significant increase in mid-latitude heterogeneous reactions.

10. Chairman Heckert sent Senators Baucus and Durenberger a second letter justifying his previous letter by saying that the March 15 Trends Panel report was new evidence that simply updated the previous evidence, ignoring the fact that the previous "scientific evidence" was not considered valid. The letter, as quoted by Sharon Roan in her book *Ozone Crisis,* ended with: "While we believe the short-term risks to health and the environment from CFC's is negligible, we nonetheless have concluded that additional actions should be taken for long-term protection of the ozone layer." In addition, Heckert, for inexplicable reasons, excluded Senator Stafford from the letter's distribution list, and from any personal visits by Du Pont representatives. The small-minded snub was shrugged off by Stafford, but well-noted by his colleagues.

11. There is another aspect of "missed-opportunity" to this reticent attitude as well: Any other CFC company could have had a ten-year jump on Du Pont in the HCFC replacement fluorocarbon market if they had either been more environmentally conscious, or more astute as marketers.

12. Compounding the time problems was the fact that while some scientists have clear voices and can think and speak with sparkling clarity on their feet, public speaking is not a required subject in a Ph.D. curriculum, and some of the presenters simply couldn't communicate rapidly or clearly, leading to additional questions and lost time.

13. According to Adrian Tuck, the "cornfield meet" was really over the question of whether or not the Antarctic vortex is better approximated as a flow reactor (the contention chiefly of Tuck, Mike Proffitt, Dan Murphy, and Ed Danielsen), or as a containment vessel (the contention of Dennis Hartmann, Mark Schoeberl, and as

assumed by Jim Anderson for his kinetics analysis). The confrontation is still ongoing—there was a major clash over it at the AASE meeting at Charlottesville, Virginia, in June 1990. The next Antarctic mission, in fact, will probably be targeted to address this question.

14. Jagged, irregular surfaces are even more effective than flat surfaces—such as ice flakes rather than large crystals.

15. Sherry Rowland and Neil Harris would later do a serious study of this subject in conjunction with the readings from Arosa, Switzerland, which would make a case for heterogeneous reactive ozone losses caused by volcanic debris acting as a catalyst.

16. Maggie Tolbert was not the first scientist to tackle what is still an unresolved issue. According to Mario Molina (as he told his audience at Snowmass), it is clear that sulfuric acid particles are rather *inactive* at lower latitudes. The question is whether they become activated *outside* the polar stratosphere before they are transformed in the polar regions to PSC's—Polar Stratospheric Clouds.

17. To be precise, the Rowland and Molina predictions concerned potentially catastrophic ozone losses in the upper stratosphere at lower latitudes in the middle of the next century. What Snowmass validated through the findings of NOZE-1 and -2 and the Punta Arenas expedition was catastrophic ozone loss in the lower Stratosphere at high latitudes in the last half of *this* century caused by a never-postulated permutation of chlorine loading of the atmosphere. Yet the most valuable element of the Rowland and Molina postulations of the mid-seventies—from a societal point of view—is that an alarm was raised at that early date for the first time over CFCs and their potentially destructive effect on Earth's ozone layer. Snowmass did validate that connection—that CFC chlorine loading has now been proven to have had a significant effect on the ozone layer by whatever means at whatever altitude and latitude. CFC's have placed the chlorine in the Antarctic stratosphere in such concentrations that the ozone hole was made possible, and for CFC producers, there was and is no way around that reality. Before NOZE-1 and -2, and Punta Arenas followed by Snowmass, there was still some defiance left in the CFC producers as they continued to blindly maintain that there was no proof of a smoking gun connection between their product and negative planetary impact. Snowmass put the smoking gun on display.

18. In fact, the 1989 Arctic Ozone Expedition showed that ozone depletion was occurring at a rate of 1 percent *per day*, but the meteorology broke it up before heavy losses could occur. With even more chlorine headed toward the stratosphere, the rate of ozone loss by the year 2000 is expected to be considerably faster. Air, with substantially depleted ozone, drifting over the populated regions of the north is an even more probable legacy of CFC production.

APPENDIX I

Update on The Main Players in this Story

As of the spring of 1991 . . .

Dr. Sherwood F. (Sherry) Rowland continues to serve as a full professor of chemistry at the University of California at Irvine, and continues to fill the role of elder statesman in the ozone-chlorofluorocarbon issue. Contrary to the professional difficulties he encountered previously for his advocacy for CFC limits, he is now one of the most sought-after figures in atmospheric chemistry worldwide. In October 1987, Dr. Rowland received the prestigious Charles A. Dana Award for pioneering achievements in health, and in 1989 the Science and Technology Foundation of Japan awarded him the 1989 Japan Prize, one of the world's top honors in scientific research, which also carries a $400,000 monetary award.

Dr. M. J. Molina, now Professor, Department of Earth, Atmospheric and Planetary Sciences and Department of Chemistry at MIT, continues his energetic and pace-setting research in areas of atmospheric chemistry. Dr. Molina has received the Esselen Award of the American Chemical Society (1987) and most recently the Newcomb-Cleveland prize of the American Association for the Advancement of Science for his elegant 1987 paper in *Science* describing his work on the Antarctic Ozone Hole chemistry (postulation of the ClOOCl "dimer" and the methods of chemical denitrification and dehydration in PSC's). He continues to be a major player on the world stage of understanding the chemical processes threatening our atmosphere.

Dr. Susan Solomon continues to be one of the key researchers at the Aeronomy Laboratory of the National Oceanic and Atmospheric Administration's Boulder facility, and one of the world's pacesetters in atmospheric chemistry. Despite her professional modesty, her contributions to this all-important field are difficult to overemphasize.

Dr. Robert Watson, Director of the Office of Space Science and Applications of the Earth Science and Applications Division of NASA in Washington, D.C., has accelerated his worldwide stewardship of a host of research progams involving global atmospheric change and ozone depletion. His schedule would kill the average twenty-year-old.

Dr. Joe Farman retired in 1990 after a distinguished career spanning the better part of four decades, most of it with the British Antarctic Survey. He lives near Cambridge, England.

Dr. Stephen Schneider continues his research and climate modeling as well as his international advocacy of climatological responsibility as a leading climatologist with the National Center for Atmospheric Research in Boulder, Colorado. As a speaker, communicator, and author, he spends much of his professional time focusing the nations of the world on the hazards of inadvertent climate modification.

Dr. James Hansen, Director of the Goddard Institute for Space Studies of NASA in New York City, continues to pursue his team's research projects in advanced climate modeling while remaining accessible to the media and the public. His office remains above Tom's Restaurant in upper Manhattan near Columbia University.

Dr. James Anderson, who continues as a professor at Harvard, is pushing the pace-setting research of his various team members into new and innovative methods of upper stratospheric sampling, including the use of unmanned drone aircraft that can stay aloft for days at a time and fly literally to the poles and back gathering invaluable information on the chemical and dynamical realities of our changing atmosphere. He has managed at least one vacation in Idaho during the past year.

Dr. Adrian Tuck, who is no longer hirsute (possessed of a mustache, whether walrus *or* Mexican bandit), remains deeply involved in the issues of Arctic and Antarctic ozone destruction from his office at NOAA's Aeronomy Lab in Boulder.

APPENDIX II

Acknowledgments

The scientific method involves the constant testing and evaluation of ideas and theories constructed to explain the universe in which we find ourselves. For the writer-journalist-researcher who would penetrate a scientific discipline without benefit of a Ph.D. in that field (with the intent and purpose of explaining it to his readership), the process is the same. The initial ideas and tentative conclusions of the writer must be tested brutally and constantly against the realities of the science *as seen by those within that science*!

It would be virtually impossibl , then, to write a quality book of this scope and caliber without a major contribution of time and cooperation by the professionals in the various sub-disciplines of atmospheric science. While the ultimate conclusions and the thesis of this work are entirely mine (and while the responsibility for ultimate accuracy of the facts and conclusions is also mine), a lion's share of the credit for whatever effectiveness *What Goes Up* achieves in terms of increasing international understanding of these vital issues belongs to a great number of people in the scientific community who have been unfailingly patient and generous with their time, their reference material and papers, and the benefit of their intellect. Chief among them are the following individuals:

Dr. Sherwood F. Rowland—Professor of Chemistry, University of California at Irvine

Sherry Rowland had been described to me as friendly and patient long before I met him in Snowmass, Colorado, during the week-long conference in 1988, and indeed, those qualities were instantly apparent when this writer, whom he did not know, approached him in the lobby of the conference center between sessions with what (at that stage in the research) were extremely basic questions. With professorial ease and interest, out came his pen, and he began to diagram for me on the pages of my notebook chemical reactions that a first-year chemistry student would find rather pedestrian. As my questions increased in complexity over the next few days (and, I hope, in sophistication over the following two years), Professor Rowland's patience never flagged, and his profound interest in accuracy never waned. When I sent a majority of this manuscript to him for comment and technical correction (as I did with a circle of senior scientists), I did expect that he would, as promised, spend some time looking over the book. I was not prepared, however, for the comprehensive and detailed pages of handwritten notes I received back from him within a few weeks—voluminous explanations of the events I had described as well as notations refining my understanding of the chemical truths and discoveries covered—pages which must have taken many hours to prepare, and which will retain an honored place in my library of research material. While I must emphasize that the words and conclusions in this book are entirely mine and must not be imputed to Dr. Rowland or any other party except where quoted, his gracious assistance has been quite pivotal, and entirely definitive of a true professor.

Dr. Mario J. Molina—Professor, Department of Earth, Atmospheric and Planetary Sciences and Department of Chemistry, MIT.

Mario Molina's willingness to spend time with me at Snowmass, explaining yet again the key struggles in the ozone equation over the prior fourteen years despite the pressures of the moment, was followed by unflagging patience in taking my phone calls, and providing careful analysis and markup of the many pages of this manuscript I asked him to review. His contributions to the accuracy of this work are many, and are greatly appreciated.

Dr. Susan Solomon—Atmospheric Chemist, Aeronomy Laboratory, National Oceanic and Atmospheric Administration, Boulder, Colorado

My first contact with Susan Solomon came in the form of my phone call out of the blue to her Boulder office. In my roles as an airline safety analyst, and earthquake safety advocate, I, too, deal with constant, unsolicited calls for information and comment from the media and fellow journalists day in and day out, and I try very hard always to be receptive and helpful whether I know the caller or not. Susan, however, set new standards for me in terms of graciousness and receptivity. Her willingness to explain yet again what she had probably detailed to reporters a thousand times over about her subject—and her willingness over the following years to field my calls, check the manuscript, and meet with me several times in Boulder—have been invaluable assets to this work.

Dr. James Hansen—Director, Goddard Institute for Space Studies, NASA, New York City

I had never met or talked with Jim Hansen before the day I walked into his office in upper Manhattan to keep an appointment made the week before from Seattle. Because his life had been altered rather drastically the year before by the intense glare of publicity, I found him to be very guarded and reserved in his direct statements, but very willing and able to provide me with voluminous copies of papers, articles, statements, and other material which are really vital to researching such a book as this. In later phone calls, however, and in his review of the chapters I sent to him for comment, he has taken as much care and time as Sherry Rowland in fairly and carefully explaining anything and everything I wanted to know. The thread of intense personal integrity and intellectual honestly which I have come to admire so much in such individuals as Sherry Rowland, Mario Molina, Susan Solomon, and their fellows in this discipline, is exemplified by Jim Hansen.

Dr. James Anderson—Professor, Harvard University

As with his fellow world-class scientists, Jim Anderson—from talking with me in Snowmass through fielding my phone calls and manuscript review requests—has been unflaggingly helpful and friendly. His vacation home in Idaho is not too distant from my own home in the verdant forests of Western Washington, and I probably felt more acutely than most the sacrifice of his having to leave such a place to shoulder the task Bob Watson handed him prior to the Punta Arenas expedition.

Dr. Robert Watson—Director, Office of Space Sciences and Applications Division, NASA, Washington, D.C.

"You haven't met Bob Watson yet?" asked the senior scientist of me at Snowmass. "Boy, are you in for a treat!"

I'm not sure I believed that at the time, but it turned out to be an understatement. Through the substantial assistance of Dr. Watson's associate Flo Ormund, we finally arranged to pin him down for a Washington interview which was supposed to run only an hour on a crisp, cold January day. Three hours of nonstop information and impassioned discussion later, I wobbled out of his office carrying tapes of the interview which would literally fill a binder on my shelf when eventually transcribed. Dr. Watson is indeed a national asset, and this book has been greatly assisted by his participation.

Dr. Adrian Tuck—Atmospheric Scientist, Aeronomy Laboratory, National Oceanic and Atmospheric Administration, Boulder, Colorado

No matter how dedicated the journalist, and no matter how important the book being prepared, having a writer camp in your office with a tape recorder and notebook for several hours simply wipes out a substantial portion of the working day. This is especially true when the request for such an interview comes at the last moment, by phone, from a Denny's a mile distant. I very much appreciate Adrian Tuck's receptiveness to that particular intrusion, and his friendliness and helpfulness at Snowmass and through several phone calls, as well as his help with review of portions of the manuscript. And I particularly appreciate his keen sense of humor and taste in classical music.

Dr. Stephen H. Schneider—Head of the Interdisciplinary Climate systems group of the Climate and Global Dynamics Division, National Center for Atmospheric Reserch (NCAR), Boulder, Colorado

The day I invited Steve Schneider to lunch following an interview, he ended up with little more than a snack from the cafeteria at NCAR, so intent was he on answering my questions and assisting my understanding of the global warming issues in the time he had between other commitments. I still owe him a decent lunch. Dr.

Schneider has reviewed pages of this manuscript on airplanes and fielded telephone calls on the run to help me, and I greatly acknowledge and appreciate his help and his personal insight, and especially his candor.

Dr. Joe Farman—Scientist, British Antarctic Survey (Retired)

For the time that Dr. Farman spent with me at Snowmass discussing his pivotal contributions to the discovery of the Antarctic Ozone hole, and for the time he has spent on the phone with me prior to his retirement in 1990, I am very grateful.

In addition, the following people were of great assistance with the research writing, checking, and logistics behind *What Goes Up*:

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Ms. Sharon Roan—Author, Writer
Dr. James Rosen—University of Wyoming
Dr. Mark Schoeberl—NASA
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Dr. Richard Stolarski—NASA
Dr. N. Sundararaman—World Meteorological Organization, Geneva
Dr. Steven Wofsy—Harvard

I would also like to thank Steve Schneider's secretary, Leigh, and Jim Hansen's secretary, Carolyn Paurowski, for their kind assistance in arranging interviews and sending materials.