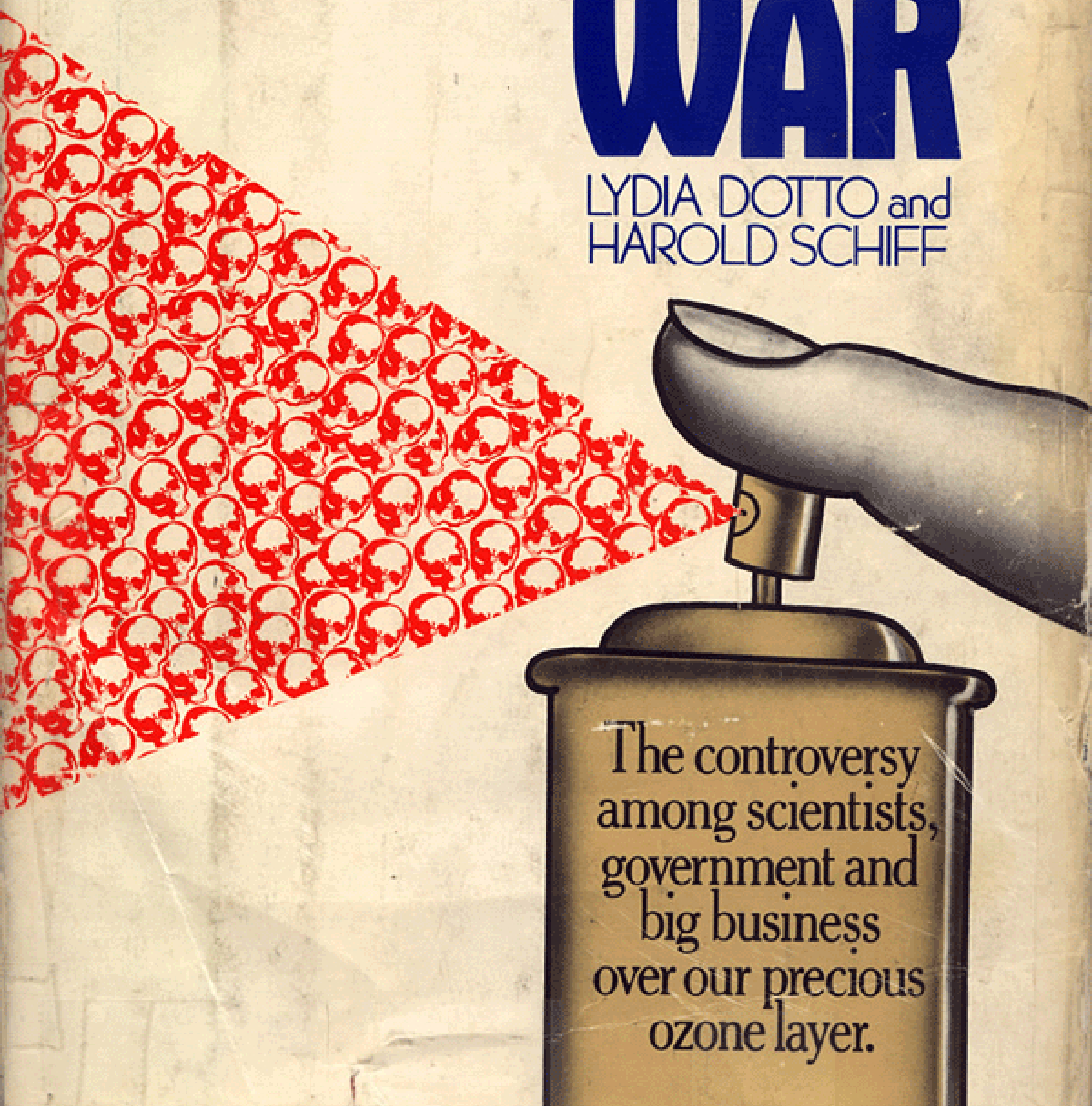
THE OZONE WAR

LYDIA DOTTO and
HAROLD SCHIFF



to his familiar theme that "the presence of these compounds constitutes no conceivable hazard."[1]

The statement could not, of course, have been more ill-advised, and it has many times returned to haunt him. "I boobed," he admits frankly. "It turned out I was sitting on a real bomb." But at the time, he had a very strong philosophical motive for writing the statement. "I'm not a doomwatch sort of person and I was most anxious that a straightforward investigation should not be turned into a doomwatch scare. I could easily have said these things may be a hazard to the health of future generations and this would have probably got me bags of support. I was sort of falling over backward. . . ."

With its implicit challenge to other scientists, Lovelock's statement virtually guaranteed that the fluorocarbon/ozone theory would eventually be discovered by someone. It was certainly what provoked Charles Kolb to start thinking about fluorocarbons late in the summer of 1973. Kolb, a physical chemist with Aerodyne Research Inc., a Boston contract research organization, felt the fluorocarbons had a "large potential for mischief," but he did not immediately perceive the threat to the ozone layer. Though he knew that the chemicals would release chlorine atoms, "at the time I didn't know about the efficiency of chlorine as an ozone-eater." So he did not really start to push his idea until late September, when he heard from Harvard scientist Mike McElroy about a meeting in Kyoto, Japan, earlier that month at which the problem of chlorine

[1] Interestingly, however, a group of meteorologists had already suggested fluorocarbons might be an environmental problem. The suggestion is in a little-known report on a meeting sponsored by the National Aeronautics and Space Administration in August 1971—before even the SST threat was identified. The report suggests that fluorocarbons would be broken up by ultraviolet radiation high in the atmosphere and that there didn't seem to be anything preventing them from getting up there. It also suggested that "an apparently irreversible alteration of the atmospheric composition" might be possible, but does not specifically mention ozone destruction, nor does it identify the mechanisms leading to this destruction. The fact that this idea originated in a workshop on how to measure pollution in the lower atmosphere probably accounts for the fact that it was not picked up by atmospheric chemists concerned with the upper atmosphere; as we shall see later, these two groups tended to be scientifically isolated from each other, particularly at the beginning of the ozone controversy.

in the earth's atmosphere had been discussed by scientists for the first time. However, none of the scientists present at that meeting had identified fluorocarbons as a potential source of chlorine that could destroy ozone. After Kolb learned of chlorine's special talent for destroying ozone, his interest in studying the fluorocarbons took on a greater urgency. In October he submitted a research proposal to the National Aeronautics and Space Administration, but the project was never funded. By that time, Sherry Rowland and Mario Molina, two chemists at the University of California's Irvine campus, were already hot on the trail.

For Sherry Rowland the story really started as far back as 1970. It started imperceptibly and, at least in the beginning, it unfolded slowly. If Rowland hadn't taken a particular train to Vienna; if he had not heard the early rumors about Lovelock's work; if he had not been searching around for something completely different to do; if he had not just taken on a bright young postdoctoral research associate who similarly was looking for something new—so much of what happened might not have, just as easily.

Rowland is formally known as F. S. Rowland or sometimes F. Sherwood Rowland. He prefers Sherry, which mystifies some of his colleagues. When one newspaper story referred to him as Mr. Rowland instead of Dr. Rowland, he wryly commented that he was "not concerned about the use of titles . . . except the occasional Ms. that I receive because of mistaken inferences about the gender of Sherry."

Rowland is a large man, more than six feet tall, with long, graying sideburns and a frequently furrowed brow. He carries his calculator in a hand-tooled leather holster slung around his waist. He wears size 14 shoes. The latter is not normally a statistic that matters to anyone, but Sherry Rowland's feet have acquired a certain notoriety in some scientific circles as a result of an escapade in Russia in 1967. Rowland was among a group of Western scientists to visit the Soviet science city of Novosibirsk that year, and, during their stay, they were challenged to a game of basketball by the Soviet graduate students. Rowland, who had played college basketball, was captain of the Western team.

The Soviets were able to scrounge gym clothes and running shoes for most, but they were not equal to the challenge posed by the size of Rowland's feet. So Rowland trotted out onto the floor barefoot—whereupon the Soviet captain immediately removed his own shoes.

After the game, Rowland returned to the locker room and calmly proceeded to peel a thick, single layer of skin off the sole of one foot. Unperturbed, he slapped the skin back on with cold cream and walked several miles in Moscow the next day. This sort of thing happened to him from time to time; his wife swears that before they were married, she had received letters from him written on the soles of his feet, or portions thereof.

Rowland was, in many ways, a most unlikely candidate as the originator of the fluorocarbon/ozone theory. Worrying about the ozone layer was a going concern long before he got into the game and, like Jim Lovelock, he was not a member of the clique of researchers who had staked out stratospheric chemistry as their special domain. This is one of the most striking features of the ozone controversy—the extent to which "outsiders" played a crucial role in identifying the threats to the ozone layer. Rowland was not an atmospheric scientist. He had specialized in the chemistry of radioactive isotopes and thus, in 1970, found himself in Salzburg, Austria, at an International Atomic Energy Agency meeting on the applications of radioactivity to the environment. He was feeling the need to renew himself scientifically and was at the Salzburg meeting on a fishing expedition for new ideas.

After the meeting, on his way to Vienna, he happened to share a train compartment with William Marlow of the U. S. Atomic Energy Commission (AEC). Marlow was responsible for organizing scientific meetings that brought meteorologists and chemists together, a task that rivaled in difficulty the mixing of oil and water. The ozone issue involved complex problems of both meteorology and chemistry, and the inability of these two groups of scientists to communicate with each other was to become one of the recurrent frustrations in dealing with the problem, and, as we shall see, a source of some spectacular personality conflicts.

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"I had no thought at that point that it was anything except another molecule that had been put into the atmosphere and in obviously very, very low quantities. It wasn't until much later that one has any real feeling that there is anything serious about it."

In fact, he put the whole matter on the "back burner."

It was not until the summer of 1973 that it was revived, when Rowland was going through the annual ritual of preparing his research proposals for the following year. Rowland had been funded since 1956 by the Atomic Energy Commission (now the Energy Research and Development Administration). The AEC was supporting his research on the chemistry of atoms produced in nuclear reactions. However, over the years, Rowland had introduced new subjects into his research, including photochemistry (the action of light on chemicals) and the chemistry of fluorine, both of which laid the foundations for the fluorocarbon work that would follow. In the summer of 1973, Rowland called his sponsors at the AEC and told them he wanted to branch out. They seemed agreeable to the idea. He then wrote a proposal asking for money to study the fluorocarbons—a curious request, in a way, since it really had nothing even remotely to do with nuclear energy; but researchers who have proven to be productive in the field of basic research are often given a relatively free hand, since the benefits of such research are frequently unexpected and can neither be predicted nor planned. The AEC denied Rowland additional funds, but told him that if he wanted to use part of his regular funds for the study, they had no objections.

It is difficult to trace the reasoning that led from Rowland's "casual thought" in January 1972 to an actual research proposal in 1973. Since scientists were interested in using fluorocarbons as atmospheric tracers, Rowland thought that there might be some interesting chemistry in predicting what was going to happen to them. But he is emphatic that there were "no cosmic implications" behind the research proposal to AEC. Though he would later be teased by some colleagues about a subconscious recognition of a hot political issue, Rowland denies trying to jump on any environmental bandwagon. While his proposal to the AEC did suggest that the fluorocarbons would likely get into the upper atmosphere and be attacked by ultraviolet radiation, there was no suggestion that this would pose an environmental problem involving ozone.

Rowland was not unaware of the ozone issue. He had twice invited Harold Johnston, a chemist at the University of California's Berkeley campus, to come to the Irvine campus to discuss the supersonic transport (SST) controversy. This was a fight in which Johnston had been deeply involved for several years; he and many other scientists were engaged in a major $21 million program to study SST effects and were becoming increasingly convinced that large fleets of the planes would indeed pose a serious threat to the ozone layer. But when Rowland first set off on the trail of the fluorocarbons, he had no sense of being about to embroil himself in the ozone war.

On October 1, 1973, Mario Molina joined Rowland's team as a postdoctoral research associate. Molina is a native-born Mexican, the son of Mexico's ambassador to the Philippines. He attended boarding school in Switzerland and universities in Mexico, Paris, and Germany before entering a graduate program at the University of California's Berkeley campus in 1968. He received his Ph.D. early in 1973 for work on chemical lasers. Of the various research projects he talked over with Rowland, the fluorocarbon problem intrigued Molina the most. Atmospheric chemistry was entirely new to him—and it was a bit unusual for a young researcher and his senior colleague to embark on a project in which they were both essentially neophytes—but it seemed exactly the change of pace they were looking for. For Molina, too, the problem was "simply scientific curiosity. We took it as a challenge to chemists to see what happens to these things. We didn't even think about the environmental effects."

Rowland and Molina asked themselves a seemingly simple question: What happens to fluorocarbons once they are released into the earth's atmosphere? Rowland recalls that once they started working on the problem, "it unwrapped very quickly." The first thing they did was to look for "sinks" or removal processes that would destroy the chemicals in the lower part of the atmosphere. From the start, this seemed improbable. For one thing, fluorocarbons are inert—that is, they don't react chemically with anything.

It was their great advantage as propellants, for example, that they could be counted upon not to react with the products being propelled or with the things the products were being propelled at (including humans). But this inertness also eliminated many of the processes that could put the chemicals out of action in the lower atmosphere. Second, there were Lovelock's measurements, which seemed to suggest that virtually all the fluorocarbons that had thus far been released remained in the earth's atmosphere. If this was in fact true—it would later become one of the most contentious points of the controversy—it strongly argued against the existence of removal processes in the lower atmosphere. But Molina set out in search of them anyway.

He tried to think of all the things that could possibly destroy fluorocarbons in the lower atmosphere. The list of possibilities got shorter and shorter without any obvious fluorocarbon "sinks" turning up. It was a systematic approach to the problem and, Molina rather sheepishly admits, "a bit boring. It was rather frustrating because I kept coming up with all sorts of possibilities and working out each of them and saying no, it cannot be very important." The chemicals were not washed out of the troposphere by rain. They were not dissolved in the oceans. They were not removed by interacting with living things. In short, there appeared to be only one fate for the fluorocarbons—they would migrate upward. By November, Rowland and Molina had concluded that fluorocarbons would certainly reach the stratosphere. From about 15 to 20 miles up, no longer well shielded themselves by the ozone layer, the fluorocarbon molecules would be split apart by the sun's ultraviolet radiation. The whole process would, however, be an incredibly slow one; the calculations showed that the fluorocarbon molecules would stay around for 40 to 150 years before getting into the stratosphere and encountering the ultraviolet radiation that would break them up. Rowland knew that his decomposition of fluorocarbons would produce chlorine atoms.

At this point the two scientists debated writing up their research results. They had answered their original question: What happened to fluorocarbons? The chemicals went into the stratosphere and broke down to produce chlorine atoms. But there was a loose end: What happened to the chlorine atoms? Still no "cosmic implications." If Rowland had been asked at that point whether chlorine atoms would attack ozone, he would probably have said yes. But he would not have thought it that important, since there was so little fluorocarbon up there. A tiny amount of fluorocarbon would chew up only a tiny amount of ozone—or so he thought. Still, in the interests of thoroughness, Molina went off to find out what happened to the chlorine atoms. He sat down with pen and paper and worked out the way in which the various chemicals that are in the stratosphere might react with chlorine atoms. The next day, he came back and informed Rowland that there was a chain reaction involving chlorine. The chlorine atom would react with ozone, producing an ordinary oxygen molecule and a chlorine compound. But in another chemical step, the chlorine compound would be converted back to a chlorine atom. Each time this happened, an ozone molecule disappeared and the chlorine atom emerged intact. It would go on to destroy many thousands of ozone molecules before some other process managed to remove it from the stratosphere.

In working out the chlorine chain, Rowland and Molina were not being totally original; the fact that chlorine chain reactions could destroy ozone was already known to other scientists. In fact, Rowland and Molina were soon to find out that the importance of the chain in the earth's stratosphere was at that very moment a subject of debate within the scientific community concerned with the ozone layer (though, ironically, none of these experts had perceived the fluorocarbon threat). But Rowland and Molina did not belong to that community—their professional orbits had never overlapped those of atmospheric scientists—and they knew nothing of the chlorine debate or the scientists who were party to it.

The existence of the chlorine chain was a startling development but still not too worrying to Rowland and Molina. The fact remained that there just weren't a lot of fluorocarbons in the stratosphere. But would it stay that way? In short order, they had concluded that the answer to that question was probably no. Molina only began to get excited when he looked at the fluorocarbon industry's figures on annual production of the chemicals. The calculations indicated that a constant injection of fluorocarbons at the 1972 rates—about .8 million tons a year—would ultimately produce concentrations of fluorocarbons in the lower atmosphere 10 to 30 times the existing levels, amounting eventually to nearly 100 million tons. This in turn would produce prodigious amounts of chlorine in the stratosphere—something like .5 million tons. Their first rough estimates led them to conclude that as much as 20 to 40 per cent of the ozone layer might be destroyed. Moreover, it appeared that the destruction by chlorine would be at least comparable to the natural destruction of ozone and might well become the overriding influence. It might, in short, take over from nature.

They both had the same reaction to this: They had clearly made a big mistake somewhere. Molina thought it couldn't really be the problem it seemed to be because surely someone would have thought of it already. They each went off by themselves to think it over. They meticulously checked everything, did it over and over again. But they could find no flaw, and that's when the penny finally dropped. If this was really all there was to it, they'd found a serious problem.

Alarmed, they sought advice from the people who had been working directly on the ozone problem. Rowland called Hal Johnston and told him: "We've found a chlorine chain and a source of chlorine."

Johnston asked if Rowland knew that the chlorine chain was already known, that in fact there was quite a bit of discussion going on about it among the people working on stratospheric chemistry. "No," Rowland responded, "that's why we called you. We're trying to find out how much of this is known within the atmospheric-science community."

In the week between Christmas and New Year's of 1973, Rowland visited Johnston at Berkeley to find out what was being done by other scientists. Though many researchers had known for years that the chlorine chain would destroy ozone, no one had worried too much about it because there were no known sources of chlorine in the stratosphere. It was not until September of 1973, at the meeting held in Kyoto, that scientists publicly began to argue that chlorine chemistry might have some relevance to the real world, that it might actually be going on in the upper atmosphere and that this could spell trouble for the ozone layer. Johnston gave Rowland preprint copies of two soon-to-be-published scientific papers on the subject, one by a group of researchers at the University of Michigan and the other by a team at Harvard University. But these scientists were not talking about fluorocarbons as a source of chlorine in the stratosphere. They were talking about the space shuttle, a reusable space vehicle being built by the National Aeronautics and Space Administration, scheduled to go into operation in the 1980s. The shuttle's exhaust will produce hydrogen chloride, a source of chlorine atoms.

During their December meeting, Johnston also told Rowland about recently completed work by British researcher Michael Clyne that had the effect of making the chlorine problem even worse than Rowland and Molina had originally calculated. Johnston could not see anything wrong in what they had done, so Rowland left the meeting with a sense of urgency about publishing the data as soon as he could. He found himself in a paradoxical situation. He was about to take off to Vienna for six months on a Guggenheim fellowship, a trip that had originally been intended as a rejuvenating change of scene. He'd intended to use the opportunity to find a new scientific problem to work on that was different from what he'd been doing. The irony of the situation did not escape him; here he was, sitting on the newest and biggest problem he'd yet encountered and one, moreover, that gave every evidence of being a major bombshell. But he went to Vienna anyway and spent his first Sunday there, in the place where it had all started for him, writing the first paper on the fluorocarbon/ozone theory, which he promptly sent off to the British journal *Nature*.

In the paper, Rowland and Molina argued that virtually all the fluorocarbons that had ever been released were still in the lower atmosphere. All of it would eventually—and inevitably—reach the stratosphere, but this would happen slowly. Thus the full impact on the ozone layer would not be felt for perhaps several decades. In the emotionally neutral language of science, the authors also mildly observe that "if any chlorine atom effect on atmospheric ozone concentrations were to be observed from this source, the effect could be expected to intensify for some time thereafter." This was a significant point. The import of what they were saying was that the worst was yet to come and that there was not a thing anyone could do about it. Even if the fluorocarbons were banned immediately, the damage to the ozone layer would get worse before it got better be-

PROLOGUE

There is a certain indignity—if perhaps some poetic justice—in the possibility that human beings could seriously threaten all life on earth through the use of deodorants and hair sprays.

Yet the Spray-can War, despite its macabre zaniness, or maybe because of it, was one of the most significant environmental clashes of recent times—and certainly one of the most spirited.

It had all the proper ingredients of an environmental thriller—warnings of impending disaster, a multibillion-dollar industry at stake, scientific dissension, and (the unsurprising contribution of a bureaucratic society) a plethora of committees. Add just a touch of bandwagon-jumping by federal agencies, legislators, scientific organizations, and research teams, industry associations, environmental and consumer groups, the media, and all levels of government, and you've got the hottest environmental/political show in town.

The essence of the problem can be briefly put: Chemicals called fluorocarbons, once widely used as propellants in many spray cans (and still extensively used as refrigerants) gradually percolate to the earth's upper atmosphere, to a region known as the stratosphere, where they are broken down by the sun's energy to produce chlorine atoms. The stratosphere happens to contain the protective ozone shield, and chlorine atoms chew up ozone in a singularly pernicious fashion. Ozone is a rare form of oxygen which, though poisonous to humans at ground level, is possessed of some useful qualities in the upper atmosphere, mainly an ability to absorb the sun's deadly ultraviolet radiation, preventing most of it from reaching the earth's surface. This radiation, the cause of suntans when taken in small doses is, at greater strengths, lethal to all living things. It is no coincidence that life arose on the land masses of the earth several cause most of the chemicals already in the atmosphere hadn't even begun to do their dirty work.

Other researchers concurred. In September, the University of Michigan researchers published a paper in the American journal *Science* stating that if ground-level emissions of fluorocarbons were halted immediately, the destruction of ozone would peak in 1990 and "would remain significant for several decades." Their calculations indicated that chlorine destruction of ozone could take over from all natural-destruction processes as early as the first half of the 1980s.

Subsequently, the Harvard team published computer calculations that had produced several rather alarming scenarios. If releases of fluorocarbons continued to grow at their then current rate of about 10 per cent a year, the ozone layer would be depleted by 14 or 15 per cent by the year 2000. If fluorocarbons emissions growing at a 10 per cent annual rate were suddenly banned in 1978, ozone depletion would still grow to 3 per cent by the year 1990. And if the decision to eliminate the chemicals was delayed until 1995, "the reduction in ozone could exceed 10 per cent and would be significant for as long as 200 years."

The Molina/Rowland paper did not appear in *Nature* until late June, in part because the man who was handling it apparently disappeared with no forwarding address and the paper languished at the *Nature* offices for some time. Rowland was fretful about this at the time, but in retrospect he is thankful for the delay. It gave him and Molina much-needed time to gather their wits. They were not atmospheric scientists and, essentially interlopers, they felt they had a lot to learn before they crashed the stratospheric chemistry game.

In February they had a brief but enlightening taste of the controversy and even notoriety that was to come when a Swedish newspaper published a story about their theory. Rowland had been discussing the work with other scientists since Christmas, and the gossip mill had soon spread the news throughout the community. One of those to whom he sent a preprint of the *Nature* paper was Paul Crutzen, a Dutch-born meteorologist working in Sweden, who had identified the SST problem. Crutzen mentioned the Rowland/

Molina work in a speech at the Royal Swedish Academy of Sciences, and Katrin Hallman, an alert reporter for the newspaper *Svenska Dagbladet,* picked it up and made front-page headlines with it. (Crutzen had not known that there was a reporter in the audience, and he was a little chagrined at his role in helping Rowland and Molina scoop their own scientific paper.)

No one else in the media picked up the story at the time, but the incident taught Rowland a valuable lesson about the quick reflexes possessed by the fluorocarbon industry. He was still in Vienna at the time, and he heard about the Swedish story from a Du Pont public-relations man based in Geneva. The Du Pont Company is the largest single producer of fluorocarbons, and their Geneva man was rather anxious to discover if the Swedish story was true. Rowland said that indeed it was and proceeded to give the details. When he got off the phone, he went all over downtown Vienna searching for a copy of the Swedish paper and managed to find one for every day of that week except the right one.

Industry's grapevine was an efficient one. In March, during a scientific meeting at Berkeley, Molina happened to share a lunch table with two other people unknown to him. It was not long before he realized the two were from Du Pont—one was Raymond McCarthy—and they were discussing the news they'd recently received from Europe about the fluorocarbon/ozone theory. Molina got the impression that the whole thing had come as a complete shock to Du Pont. The shock was due not to the fact that industry had not considered the environmental impact of fluorocarbons, but to the fact that they had. In 1972, Du Pont had issued an invitation to fluorocarbon manufacturers around the world to attend a seminar on "the ecology of fluorocarbons." The invitations stated that the chemicals were being released into the atmosphere at a rate approaching a billion pounds a year, and they could be either accumulating in the atmosphere or returning to the earth's surface. "It is prudent that we investigate any effects which the compounds may produce on plants or animals now or in the future." The companies funded several research projects, and by 1974 the results of these studies indicated that fluorocarbons posed no major environmental problems in the lower atmosphere. The inert chemicals did not affect plants, played no part in the formation of smog, and were apparently not decomposed by chemical reactions near the earth's surface.[2]

Rowland and Molina, however, argued that this very inertness which seemed such a boon, simply transferred the environmental problem from the lower to the upper atmosphere and made it a global problem to boot. It was therefore hardly surprising that Molina sensed a certain gloomy frustration in the conversation going on next to him. But he did not break in, partly because he felt rather embarrassed at having inadvertently overheard it and partly because he and Rowland were still keeping a low profile. They were trying not to discuss the problem too much until they had some reassurances from the scientific community that they were not completely off the track.

The fluorocarbon story would ultimately generate several thick bindersful of press clippings, but it got off to a surprisingly slow start. Some articles appeared after the Rowland/Molina paper was published, but few of them got beyond California, and the story soon died. Several of the major newspapers, including the New York *Times* and the Washington *Post*, kept the story at arm's length. Later, the *Times*' science writer, Walter Sullivan explained why: There was so much "doomsday reporting" going on at the time that he was not particularly anxious to jump too quickly at this latest prediction of environmental disaster. This decision played an important role in the history of the fluorocarbon story; to the continuing frustration of many newspapers and science writers across the country, a science story often just doesn't become news until Walter Sullivan and the New York *Times* take notice of it.

The fluorocarbon story revived in the fall of 1974 when Dorothy Smith took an interest in it. Smith, the peppery, no-nonsense news manager for the American Chemical Society, is responsible for handling media coverage of ACS meetings. These meetings, which are held frequently and are often very large, can be overwhelming to cover, and Smith does an excellent job of alerting science writers to the highlights and organizing press conferences on controversial and interesting new subjects.

Rowland was scheduled to give a talk on the fluorocarbons at an

[2] Ironically, this was essentially the same process that Rowland and Molina went through.

ACS meeting in Atlantic City in September of 1974, and the abstract of his paper caught Smith's attention. She flagged it as one of the top twelve news stories of the conference and sent out an advance notice to the press. Her use of the word "Freon" in the release elicited a prompt call of protest from a spokesman for the fluorocarbon industry. Industry was becoming increasingly insistent that brand names for fluorocarbons not be used in discussions of the Rowland/Molina theory. That kind of publicity they could do without.

As Smith started arranging a press conference on the fluorocarbon problem for the approaching ACS meeting in Atlantic City, she received a call from the public-relations department of one of the major fluorocarbon companies. "I wouldn't call it pressuring," she recalled later. "He was leaning a little bit, but it was lightly done." The caller didn't see much reason for holding a press conference. He told Smith that there was no proof that the Rowland/Molina theory was correct, that no fluorocarbons had ever been detected in the stratosphere, and that there was no known mechanism to get them up there.

Smith thought this over and decided to check for herself. Normally she does not investigate the scientists she chooses for press conferences that closely—Rowland was, after all, an established researcher at a major university and had been active in the ACS—but she could see that this issue was already a touchy one, and she felt obligated to reassure herself. She called several scientists, and they all assured her that Rowland and Molina's work was not nonsense. One of them told her that Jim Lovelock had actually made one measurement of fluorocarbons in the lower stratosphere, although the data had not yet been published. None of them warned her off holding the press conference, so she decided to go ahead.

The ACS meeting was covered by the wire services, whose dispatches received relatively widespread distribution. But the story did not really take off until September 26, when the New York *Times* ran a front-page article by Walter Sullivan. The story was dominated by a discussion of the computer calculations done by the Harvard group headed by Mike McElroy. Rowland and Molina were barely mentioned in the story. Sullivan, who had not been at the ACS meeting, had not talked to them for the September 26 article, but he did call Rowland later that day for a follow-up story, which ran on September 27. Rowland can be philosophical about this. He realized that the press is constantly in search of the very latest information and the Harvard calculations represented the newest data around. (Rowland was fast becoming "media hip"—as *Rolling Stone* magazine would later refer to him.)

However, the *Times* story did exacerbate a growing feud between the Harvard and Michigan groups. We mentioned earlier the papers written by each of these two groups, giving their calculations of the amount of ozone depletion that might occur from fluorocarbon usage. Both papers were published in *Science:* the Michigan group's on September 27, 1974, and the Harvard group's on February 14, 1975.

At the time that Sullivan wrote his September 26 article focusing on the Harvard calculations, these data had not been published in the scientific literature. In fact, the Harvard paper was not even received by *Science* until three days after the *Times* story appeared, and it was not published in the journal, as we have noted, until February, some four months later. Thus the appearance of the information in the New York *Times* raised some eyebrows; the scientific community takes a dim view of the press usurping the role of the scientific journal.[3] But what really caused annoyance and bitterness was the fact that the *Times* story appeared just one day before the Michigan calculations were published in *Science,* in effect scooping them. (In fairness, it should be pointed out that the results of the Michigan study had also been published in the press. They were referred to in the Ann Arbor *News* about two weeks before they were published in *Science*. However, in this case, the Michigan paper had been accepted and scheduled for publication by *Science* long before; thus it was in the "preprint" stage. While not all scientists approve of press accounts of preprints, there seems to be less disapproval in this case than in the case of results that have not yet even been accepted by a scientific journal, primarily because the latter have not been officially reviewed by scientific peers.)

[3] The following note once appeared in *Physical Review Letters:* "As a matter of courtesy to fellow physicists it is customary for authors to see to it that releases to the public do not occur before the article appears in the scientific journal. Scientific discoveries are not the proper subject for newspaper scoops."

The *Times* article really signaled the beginning of the Spray-can War. Soon the story was picked up by Walter Cronkite of CBS-TV and by the newsmagazines, and there was no looking back.

The controversy profoundly disrupted the lives of the men who started it—Lovelock, Rowland, and Molina. Lovelock found the uproar an anathema. Like many other Britons, he thinks Americans have an unfortunate penchant for excess in environmental controversies, and he speaks with faint disapproval of the furor caused by the fluorocarbon issue in the United States. "The Americans tend to get in a wonderful state of panic over things like this," he once told a British newspaper.

"I respect Professor Rowland as a chemist, but I wish he wouldn't act like a missionary. . . . I think we need a bit of British caution on this."

He illustrated his point by referring to the controversy over mercury in fish. "The Americans banned tuna fish and they blamed industry until someone went to a museum and found a tuna fish from the last century with the same amount of methyl mercury in it."

Lovelock's choice of an example was ironic: He apparently did not realize that the "someone" to whom he referred was none other than Sherry Rowland himself. Two years earlier, Rowland and several colleagues published a paper in the journal *Science* that starts: "The mercury levels of museum specimens of seven tuna caught 62 to 93 years ago and a swordfish caught 25 years ago have been determined. . . . These levels are in the same range as those found in *specimens caught recently.*" To further complete the irony, Rowland had received a modest amount of publicity for this work at the time and was briefly adopted as a champion of sorts by the anti-environmental camp.

Sherry Rowland adapted quickly and well to the intrusion of the world at large into his life. A controversy like that over the spray cans usually earns for its perpetrator a high-profile life-style that demands as much time in the congressional hearing room and the television studio as in the research lab. Some scientists react badly, refusing to make the transition or resenting it when they're forced to, but Rowland seemed to enjoy it immensely.

To all outside appearances, he is almost invariably relaxed and cheerful. He listens patiently, speaks calmly, and does not get hysterical. If Central Casting were looking for someone to play the role that Sherry Rowland played, Sherry Rowland would not be a bad choice. But the ozone controversy could provoke anger and tension in him too, and a perhaps inevitable obsessiveness. At one scientific meeting, he carried a loose and cumbersome bundle of files with him everywhere, as though fearful he would be caught off guard and unprepared to defend his position.

Rowland and Molina are in many ways a study in contrasts. Rowland is large and sometimes rumpled-looking; Molina, a dark, bearded young man, is slight and dapper. Rowland is gregarious and outgoing; Molina is somewhat reserved. But Molina stands his ground at the scientific meetings and the press conferences, challenging his opponents, senior though they may be, with an edge of defiance in his voice.

Still, you could hardly blame him for feeling perhaps a bit queasy at the outset. His Ph.D. was barely a year old when he was tossed into this dogfight, and he and Rowland were working in a field that was new to both of them. Their work was being rigorously scrutinized by the best brains in the business, who were there to pull it apart if they could. That is the way science works; Molina knew this and accepted it, but still it scared him a little.

Nor was Rowland immune to moments of tension, to moments of feeling that his scientific neck was on the chopping block. He and Molina had done a remarkably thorough job—they would be credited for that by many experts—and Rowland felt reasonably confident right from the beginning that they would not run into major obstacles. But there were moments of uncertainty. Once, in an introspective mood, Rowland muttered almost to himself that if the theory were proved wrong, he at least hoped it would not be because they left out something simple or obvious.

If, at the outset of the fracas, they could not see clearly what was ahead for them, they had at least been warned that there were confrontations aplenty to come, that there were few cease-fires in the ozone war. The warning came from Hal Johnston, their counterpart in the SST/ozone controversy. During their conversation at Christmastime, Johnston turned to Rowland and asked: "Are you ready for the heat?"

billion years ago only after the ozone layer materialized in the stratosphere.

The potential consequences of a damaged ozone shield are rather grim. They include increases in the incidence of skin cancer; genetic mutations; damage to animals and plants (including crops), with a consequent upset in the earth's ecosystems; and global climatic changes. The extreme case—destruction of the ozone layer—would mean the end of all life on earth.

Scientists cannot predict precisely what will happen, or when, or how severe it will be. In fact they are more worried about what they don't know than what they do know about the consequences of a depleted ozone shield.

It was only at the beginning of this decade that they began to realize that human activities could alter the amount of stratospheric ozone. The first threat to be identified was the supersonic transport (SST), an aircraft that flies through the stratosphere at faster than the speed of sound. The exhausts from these aircraft are a source of nitrogen oxides (NO_x) which can chew up ozone in much the same way that chlorine atoms do. Then, nuclear weapons and the space shuttle were suggested as potential threats to the ozone layer, after which came the spray cans and even more recently, nitrogen fertilizers.

The scientific attempt to understand just what these disparate technologies were doing to the earth's atmosphere began in earnest in 1971 and it is not finished yet. The complexity of the chemistry going on in the atmosphere ensured that there would be pitfalls and surprises along the way. One of the most dramatic of these, which will be dealt with in more detail later, is worth mentioning here. As of this writing, it appears that the SST is much less of a problem than originally predicted. Continuing research resulted in a steady downward revision of the SST's predicted impact (an unusual and unexpected situation) until finally, data gathered in the last half of 1977 indicated that the SST might actually *produce* a small amount of ozone in the stratosphere instead of destroying it—this, despite an earlier three-year multimillion-dollar study by scientists around the world that concluded the SST was indeed a threat to ozone. Interestingly, the same research that resulted in a downward revision in the case of SSTs led to a substantial upward revision in the predicted impact of fluorocarbons.

It is, of course, the spray-can threat that has attracted most of the public attention. Indeed, the Spray-can War was characterized by a public interest in science that quite startled many of the researchers working on the problem. These scientists, accustomed to being treated with, at best, indifference and often with a veiled kind of hostility ("I never was much good at science"), found themselves actively, even eagerly, questioned by concerned and interested non-scientists. Perhaps it was because many people felt that, while decisions about SSTs and nuclear bombs were out of their hands, decisions about spray cans were not. And they were right—as the dwindling sales of aerosols and the booming business in roll-ons and pump sprays soon proved.

If the impact on the public was one notable feature of the ozone controversy, its impact on scientists was even more intriguing. A wide variety of researchers were attracted to the problem for a number of reasons. Many of them were truly worried about the possibly devastating consequences of ozone depletion. Moreover, the science involved was genuinely challenging: This was not a second-rate research problem. Yet it was also, at a time when society demanded relevance in research, unquestionably relevant. The solutions being sought were important to society and, perhaps more to the point, could clearly be seen to be so by the taxpayer. This had the happy consequence of loosening the purse strings and, with preferential funding, the ozone question acquired the distinction of being "where the action is." But there was also the glare of publicity and the strain of doing science on demand—or what one pressured participant angrily referred to as "science in a goldfish bowl." Nothing in their training had prepared them for this; in fact, they were predisposed to avoid it.

Nevertheless, many scientists expended a considerable amount of time and effort on the ozone problem—and not just on doing research. They testified before state and federal authorities, they served on scientific committees investigating the problem, they dealt with the press. At times, these efforts resembled nothing so much as a traveling road show and, for some, they became very nearly allconsuming. Nor was the pursuit of scientific truth in the matter of human technology vs. the ozone layer conducted without emotion and some acrimony.

This is partly because the scientific process is inherently one of confrontation. New suggestions or theories are often strongly challenged, and success goes to those whose ideas can withstand such attacks. In particular, success goes to those who have the best ideas first. The competition for scientific priority is always present, but is especially intense when the subject in question commands widespread interest and involves far-reaching consequences.

There are scientists who believe—or say they believe—that such confrontations and competitions are strictly impersonal. But this is clearly not true, certainly not in the case of the ozone debate, which was dominated throughout by strong personalities with strongly held convictions. One of them once remarked: "There are some real villains in this thing." It was a sentiment echoed by many others. The irony is that few people agreed on who were the villains and who the heroes.

Some scientists clearly prefer that the story of the ozone controversy be told without reference to these confrontations; they seem to shrink at the idea of allowing the public to see this human side of their work. But the story cannot be told this way, for it would not be the truth. These disputes were an integral part of the ozone story, as much a part of it as the balloon flights, the laboratory tests, the computer simulations, the scientific meetings, and the research papers that made it the "happening" it was.

Of course, scientists were not the only players in the game. One cannot ignore the role played by the aviation and fluorocarbon industries, both of which waged fierce fights to save their respective billion-dollar enterprises, nor that of the government agencies, the Congress, the state legislatures, and even some municipal governments. They all got into the act, partly because it was their ultimate responsibility to decide what must be done, and partly because, for them too, this was where the action and the money and the jurisdictional power resided. Few issues have so completely bridged the gaps among the "three solitudes"—industry, government and basic science—as the ozone controversy did.

The ozone war involved public relations as much as it did science; emotion as much as it did logic. Scientists would be accused of alarmism and publicity-seeking; industry of self-interest and deviousness; environmental groups and their "fellow travelers" of overreaction and hysteria; politicians and government agencies of inaction or self-serving manipulation.

Yet despite all the fireworks, the fundamental issue was a vital one: We simply had to understand the earth's atmosphere, to chart both its resiliency and its fragility, and to come to terms with our own, largely unwitting, ability to damage it. This was a formidable task, and the ozone scare forced us to make a start on it, but the job is far from done. Large global systems like the earth's atmosphere are characterized by exceedingly complex chemical and physical processes that frequently defy an adequate understanding, much less control. Chemicals like the spray-can propellants never existed in nature. In his increasing use of such chemicals, man seems at times to be engaged in some bizarre game of environmental Russian roulette.

It may be that, on a global scale, it is a game in which he is out of his league.

CHAPTER ONE

The Spray-can War Begins

> The known fact is that fluorocarbon propellants, primarily used to dispense cosmetics, are breaking down the ozone layer. Without remedy, the result could be profound. . . . It's a simple case of negligible benefit measured against possible catastrophic risk both for individual citizens and for society. Our course of action seems clear beyond doubt.
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> —Alexander Schmidt, Food and Drug Administration, announcing a phase-out of fluorocarbon spray cans, October 1976.

In a way, Mrs. Lovelock started it all. Back in 1970, when her husband, Jim, decided he wanted to measure fluorocarbons in the earth's atmosphere, no one was much interested—certainly not the people who supplied funds for scientific research in his native England. So Mrs. Lovelock, the family business manager, broke out the grocery money, and her husband built a very sensitive instrument that soon detected minute amounts of fluorocarbons in the atmosphere. These chemicals did not come from nature; they were man-made, and it was not hard to figure out where they did come from. What Jim Lovelock was measuring was largely the accumulation of several decades' worth of hair spray and deodorant propellants, with perhaps a very small amount of refrigeration and air-conditioning coolants thrown in. Though neither he nor his wife could know it at the time, their modest investment in pure scientific research would threaten the billion-dollar refrigeration and spray-can industry and touch off one of the major environmental rows of the decade.

But Lovelock was no environmental crusader. He is an unassuming Englishman with modishly long graying hair and a soft, almost hushed voice. There is a gentleness about him that provides a striking contrast to the rather high-powered American scientists who dominate the ozone controversy. Lovelock is something of an oddity—a freelance scientist who works from a garage-*cum*-laboratory at his home in the rural English village of Bowerchalke. He prefers the precarious life of the free-lancer; security, he feels, kills scientific creativity and this, paradoxically, makes him profoundly nervous. With a Ph.D. in medicine he had been for twenty years a researcher at the British National Institute of Medical Research. It was a tenured civil-service position with a pension, safe and secure, but he could see it winding down inexorably to retirement, and so he got out. His independent philosophy occasionally affronts the sensibilities of the scientific establishment. Until he was able to acquire a respectable address through an honorary professorship at the University of Reading, he had trouble publishing scientific papers sent from his home address. The British magazine *New Scientist* reported that an editor of one scientific journal once told Lovelock, when he asked why a paper of his had been rejected, that "they were always getting crank papers from people living in the country."

When Lovelock set out in search of fluorocarbons, he was not looking to create another environmental scare story. On the contrary, in his early papers, published before the fluorocarbon threat to the ozone layer was recognized, he took pains to dismiss any suggestion that fluorocarbons could harm the environment. Lovelock subscribes to the "pure science" school of thought, which holds that curiosity is the only acceptable motivation for anyone truly worthy of the title "scientist." Motivations other than this make a scientist what Lovelock disdainfully refers to as a "professional science operator." In his scientific papers, Lovelock does note that the fluorocarbons might serve a useful purpose, acting as tracers that would allow meteorologists to track global air motions and wind directions. But pressed for his own motivations, he shrugs his shoulders.

For reasons he does not—perhaps cannot—enumerate, he was simply interested in measuring the chemicals.

Initially, he had trouble convincing anyone that the job was worth doing. His first applications for research grants were turned down flat. There is still a tone of outrage in his voice when he recalls the comments of one reviewer—"probably a distinguished scientist"—who dismissed the grant application as the most frivolous he had seen in a long time. This scientist doubted that fluorocarbons could be measured at all even at much higher concentrations than those Lovelock was claiming to be able to measure, and concluded that even if they could be measured, the project was still a waste of time. Lovelock speculates that there may have been some considerable chagrin at the granting agencies when the fluorocarbon controversy hit its full stride later on.

The refusal of research funds didn't dissuade Lovelock; in fact, it just made him more stubbornly determined to carry out the measurements. So with his wife's blessings and the housekeeping money—for which she received a gracious acknowledgment in one of his scientific papers—Lovelock built his fluorocarbon-measuring instrument. It incorporated an electron capture detector, a device invented previously by Lovelock that is among the most sensitive instruments ever designed for chemical analysis. It improved the ability to measure certain chemicals in the atmosphere by about one million times. Indeed, it was only because the instrument was so sensitive that Lovelock was able to measure the fluorocarbons at all.

Lovelock set up his first fluorocarbon monitoring station at his home. Before long, the family was forced to give up spray cans, not for environmental reasons but because they were interfering with Lovelock's backyard measurements. In 1971 and 1972, Lovelock made further measurements aboard a ship that sailed from Britain to the Antarctic and back, and in January 1973 he published the results in the British scientific journal *Nature*.

He had found only very tiny amounts of fluorocarbon in the atmosphere, and no one got very excited about it. In his *Nature* paper, Lovelock once again pointed out the potential usefulness of fluorocarbons as meteorological tracers, and once again he returned