The Ozone Layer

A Philosophy of Science Perspective

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To the memory of Mary Agnes Christie
(14 February 1911 – 17 October 1996)
The main point that comes out of the examination of scientific consensus in the three aspects of the ozone investigations is the primacy of the actual observational evidence. I would suggest that any account of scientific consensus that loses sight of this is likely to falter.

But the first issue—the case of CFCs—also shows that the observational evidence on its own may have little power to convince. It must be assembled and presented to make a case. On at least some occasions the evidence needs to be coupled with a new and convincing theoretical insight to have any power to influence scientific opinion. The evidence that Molina and Rowland used to formulate a case against CFCs was, by and large, not new. But it had no influence until the various strands were drawn together from different corners of the scientific edifice, and new implications drawn from their conjunction. Apart from model calculations of the likely extent of any chlorine-mediated ozone depletion, there was nothing in the evidence on which they based their argument that had not already appeared in the primary scientific literature. But it was assembled from very diverse sources, associated with different sub-disciplines of science. It required the guidance of a strong new theoretical insight to pick out the relevant evidence and put it together.

This part of the story forms a marked contrast with the ‘smoking gun’ result of the AAOE. A particular set of measurements was made, where ozone and chlorine monoxide radicals were simultaneously measured. On one day in twelve, a special set of conditions arose where there was an exact and detailed match between abnormally high chlorine monoxide levels and abnormally low ozone levels. According to current theories, chlorine monoxide levels match the rate of ozone removal, which is quite a different thing to extent of ozone loss. Only for a short period in the spring season would removal rate parallel extent of loss. And on one day during that critical period, the air on the edge of the Antarctic vortex had a laminar structure so that the flight encountered a succession of patches of anomalous polar air on the edge of the vortex. The result was the ‘smoking gun’ plot, which showed a succession of rapid changes in the mixing ratios of both ozone and chlorine monoxide radicals, with an exact match between the changes in the two species. But once those particular measurements had been made, there was no need to assemble other evidence, nor room for further argument. An intelligent lay person with a minimal background briefing can immediately determine that the close correlation between chlorine monoxide and ozone mixing ratios must mean that the ozone depletion is based on chlorine chemistry.

When there is a crucial experiment, it forms a particularly clear-cut basis for a scientific consensus. I have already argued that crucial experiments are much more diverse than has generally been recognised.

The scientific consensus

The view that is promoted by scientists and scientific publications is that scientific consensus arises out of a rational weighing of the evidence, following a healthy and vigorous scientific debate. This may or may not be somewhat naïve as an historical or sociological account of what actually takes place. But there does not seem to be anything in the ozone story that indicates that the consensus or uncertainty at the various stages of the investigation was epistemically inconsistent with a reasonable weighing of the currently available evidence.

An examination of dissenting positions

The consensus view of the Antarctic phenomenon, as outlined in the previous chapters, is by no means universally accepted. There has been a significant body of opinion, particularly in the United States, that the debate has been manipulated and subverted by a part of the scientific community, and that the evidence does not really support the published conclusions. On the one hand, these dissenters have made accusations against the scientists involved in publicising the threat to the ozone layer. Charges have included bias in evaluating the evidence, manipulating conference agendas and the peer review system, and having a hidden agenda connected with the extreme environmental lobby. They see the ozone depletion story as part of an industry concerned with stirring public feelings about forthcoming environmental doom. On the other hand, the scientists from the mainstream position tend to regard the dissenters as having inadequate technical background and understanding of the particular areas of science involved, and as not ‘playing fair’ by the rules of scientific discussion. They see the dissenting views as coming from beyond the pale of legitimate scientific debate.

Although atmospheric scientists generally regard the scientific investigation and debate as having reached a clear conclusion, and do not take this dissenting position seriously, the publicity achieved by the dissenting group has been sufficiently effective that many people believe that the scientific debate is not yet decided.

Most of the arguments brought up by dissenters are mustered in a book by Maduro and Schauerhammer (1992). This is widely recognised as the leading work from the dissenting camp. According to one review:

This book is extremely important since it is probably the best known and most widely quoted text aimed at debunking the concept of ozone depletion and the deleterious effects of CFCs and other so-called ozone-depleting chemicals. The book has been used as a primary reference by Dixy Lee Ray, Rush Limbaugh, and others who dispute the reality of ozone depletion and the effects of CFCs. (Newton, 1995, p. 155)
In another passage from the same source, the book is described as:

The book that appears to have been the single most influential document in the 1990s among critics of the CFC-ozone hole hypothesis. (p. 88)

On the basis of such recommendations as these, it seems to be a good candidate for closer examination as a dissenting source.

The earlier chapters of the book present the main theses:

1. Chlorinated fluorocarbons and related compounds cannot, it is claimed, be important as sources of any chlorine responsible for ozone depletion in the stratosphere, and for the Antarctic ozone depletion in particular, since natural sources of chlorine are so much greater than anthropogenic sources.

2. The Antarctic phenomenon is a natural phenomenon which is not new, not accurately described by the atmospheric scientists involved in the ‘ozone scare’, and not caused by chlorine chemistry.

3. There is no evidence that the Antarctic ozone hole really matters, in terms of increased ultraviolet radiation.

In the later chapters of the book the authors discuss an alleged conspiracy involving environmentalists, large chemical companies, and atmospheric scientists, in the promotion of disaster scenarios. They conclude by outlining a series of technological and engineering projects which they claim could greatly enhance quality of life for the people of the earth. These are presented to ‘give readers who are angry about environmental hoaxes a positive alternative to fight for’.

It is clear from the outset, then, that this is an attack on the orthodox view on all possible fronts. It is quite a different matter from the debate that is normally conducted within an area of a scientific discipline. It would be typical for a dissenting contribution to a scientific debate to challenge a single aspect of the basis of the theory under attack. In most cases this challenge would be backed either with new evidence from an experimental study, or a new insight that comes from a novel development or articulation of part of a rival theory. Maduro and Schauerhammer’s book is not presenting a challenge of this sort. If it is a contribution to a scientific debate, it is clearly a revolutionary one.

This book draws no new observational data into the discussion. All of the authors’ argument is based on published sources or private communications with scientists; there is no new material arising from their own experiments, nor any detailed presentation of significant experimental results that have failed to get into print. At most their claim can be that some of the sources they cite have been ‘overlooked’ by the mainstream atmospheric scientists. The crucial question, of course, is whether this oversight is accidental, arises from different perceptions of what data are relevant to the question, or is part of a conspiracy to suppress alternative views.

It could be that this book ought to be judged non-scientific, or even anti-scientific. Its presentation suggests that the conclusion – a conviction that the story of chlorine-mediated ozone depletion must be wrong – has preceded the evidence. When an analysis is driven by the desire to reach a particular conclusion, there is a major danger that it will step outside the rules of scientific debate. Typically, evidence may be considered selectively and out of context. A large number of arguments, of very variable quality, may be mustered to refute the unwanted theory, in the hope and belief that at least some of them will stick. The invocation of a conspiracy theory is another typical and worrying symptom.

It is important to remember, though, that there is not necessarily anything wrong with the conclusion preceding the evidence per se. Mendeleev was so convinced of the correctness of his periodic law that he felt free to tamper with accepted values of atomic weights, and postulate new, unknown chemical elements, on what was really the flimsiest of evidence (Brock, 1994).

It would be quite wrong to dismiss either the arguments against the orthodox view of stratospheric ozone depletion, or even the conspiracy theory, on these grounds alone. To find that the work has certain similarities in structure with some pseudo-scientific and anti-scientific works, is not to find it scientifically invalid. That would be a form of ‘guilt by association’. The arguments that are put forward must be examined in detail, and judged on the evidence. Because the arguments are numerous, and, in many cases, unconnected, the only way to make a final judgement is by working laboriously through them one at a time. A single valid counter-argument might, in principle, undermine the orthodox view of the situation, even if it were hidden among tens of faulty ones.

This task will not be undertaken here. We will merely look in a broader fashion at some of the main lines of dissent, as presented in the first six chapters.

The political arguments and allegations of conspiracy which follow are both outside the scope of this work, and extremely difficult to evaluate. Conspirators usually seek to avoid leaving much evidence of their activities, and such evidence as there is is often highly ambiguous. The final chapter on great global projects seems totally irrelevant both to their argument and to my analysis.

The first major strand of Maduro and Schauerhammer’s argument is that the release of CFCs and similar compounds to the atmosphere is a
very minor fraction of the total chlorine release, mostly from natural sources. Their figures are generally in accord with those published elsewhere, though there are significant differences in detail.

But figures for the release of chlorine compounds to the atmosphere at ground level do not necessarily parallel those for the origin of stratospheric hydrogen chloride and chlorine nitrate. The crucial question is the efficiency with which chlorine from each of these sources is delivered to the stratosphere. The argument put forward by Rowland and Molina, and accepted as part of the mainstream scientific view, is that CFCs are delivered to the stratosphere with considerable delay, but with almost 100 per cent efficiency, while the chlorine compounds associated with the other emissions are delivered with much lower efficiency. Maduro and Schauerhammer address some of these aspects of the situation, but fail to do so in an accurate or systematic way.

Firstly, their table 1.1 and figure 1.1 contain a single extra entry among the 'Atmospheric Sources of Chlorine'. This is described as 'Chlorine theoretically released by the alleged breakup of CFCs', and is set at 1 per cent of the total annual chlorine release from CFCs. But as soon as an isolated entry like this occurs, the table is no longer inviting us to compare like with like. What is really needed is a second table which contains this entry along with estimates of the amount of chlorine released from each of the other sources that finds its way to the stratosphere. What proportion of the particulate sodium chloride dust formed by evaporation of sea spray eventually arrives in the stratosphere, for example? Nothing is said in the text, other than that tropical storms are capable of carrying sodium chloride particles into the stratosphere. But is this the fate of 1 per cent of salt spray, or 1 part per million, or 1 part per trillion? In either of the last cases, salt spray becomes an insignificant contributor to stratospheric chlorine, as claimed by the atmospheric scientists. This would be so in spite of the fact that it might account for one thousand times as much chlorine release to the lower atmosphere as CFCs. The efficiency of transfer of volcanic hydrogen chloride is discussed, but not in quantitative terms.

There is also a very serious error in this table entry. It is produced by fallacious reasoning, and in effect produces a 1 percent transfer efficiency for CFCs to the stratosphere, compared with the generally accepted figure of 80–100 per cent. The argument runs as follows:

According to the theory, approximately only 1 percent of the CFCs produced on Earth is broken up in the stratosphere every year. (The reason is that CFCs, because they are chemically inert, have lifetimes of more than 100 years in the atmosphere). Therefore a year's production of CFCs would contribute at most 7500 tons of chlorine to the atmosphere. (Maduro & Schauerhammer, 1992, p. 12)

But this completely misrepresents the situation. Certainly only 1 per cent of this year's CFC production can break up this year, but so can 1 per cent of the remainder of the production of every previous year. Nothing can happen to the 99 per cent of this year's production that is not broken up this year; it simply accumulates in the atmosphere awaiting some future year when it will rise to the stratosphere and be broken up. In a steady state situation, if the world production of CFCs had been constant for a century or more, a figure equal to the whole of this year's production would decay this year, even though only 1 per cent of it would actually be CFCs from this year's production. Because the world production of CFCs had, until recently, been rapidly building up from a zero level sixty years ago, and because the efficiency of CFC transfer may be a little less than 100 per cent in the long term, a current figure around 30 percent of annual CFC production would seem appropriate for this table entry. That is a figure thirty times higher than the one shown.3

In the text accompanying the figure and table in the book, another source of atmospheric chlorine is mentioned:

In addition, untold numbers of tons of chlorine enter the earth from outer space, a result of meteorite [sic] showers and cosmic dust burning up as they enter the atmosphere. (Maduro & Schauerhammer, 1992, p. 12)

Why, one might ask, are the numbers of tons untold? A statement like this has no place in a scientific work, where the rules are that if something cannot be referenced and quantified, it should not be mentioned in this way. In effect, it is nothing but innuendo. Perhaps the numbers of tons of chlorine are untold simply because they are insignificantly small. If the proposal is a serious one, an estimate of the required number could readily be obtained by combining estimates of rate of influx of meteoric debris to the earth/atmosphere system with typical chlorine concentrations in meteors of various types.

From a general consideration of chlorine emissions into the atmosphere, the text moves to the question of accumulation of chlorine in the Antarctic:

Therefore, the propagandists conclude, CFCs are arriving at the South Pole in great concentrations, and are being broken down by ultraviolet radiation, releasing the killer chlorine molecules that then poke a hole in the ozone layer. (Maduro & Schauerhammer, 1992, p. 13)

But as we have seen, the atmospheric scientists are saying no such thing. Maduro and Schauerhammer are setting up 'straw men'. The two vital factors that are not properly mentioned and included in the analysis are the relative isolation of tropospheric and stratospheric air masses from
The conventional scientific view differs drastically from the caricatureisation in the quotation above. To summarise once more: CFCs are slowly entering the stratosphere near the equator. They break up as they rise in the tropical stratosphere and encounter strong ultraviolet irradiation. After a series of reactions, the chlorine that they contain is stored in the reservoir species hydrogen chloride and chlorine nitrate. In these relatively unreactive forms, chlorine is transferred through the stratosphere to higher latitudes and lower altitudes. If clouds are present in the polar stratosphere when these reservoir molecules arrive there, further chemical reactions occur on ice crystal surfaces, forming the precursors molecular chlorine and hypochlorous acid, which can break down in visible light – ultraviolet is not necessary – to produce the atomic chlorine and chlorine monoxide free radical species, that can effectively remove ozone.

Notice particularly that, according to the orthodox view, any CFCs that arrive near the South Pole are not involved in any ozone depletion. CFC molecules themselves are totally unreactive. Polar spring ultraviolet levels are too low to break them up into reactive chlorine-containing species. And there is negligible upward transport from the polar troposphere to the stratosphere, nor, indeed within the polar stratosphere. The caricatureisation bears no resemblance to the actual claims of the mainstream atmospheric scientists.

But Maduro and Schauerhammer point out the impossibility of sufficient upward transport of CFCs from the polar troposphere, as if it were an argument against the story of Antarctic ozone depletion due to CFCs. There is no claim in the conventional story that CFCs break up anywhere near the polar regions.

The discussion moves on to volcanic sources of chlorine, examining particularly the possible role of Mount Erebus, a large volcano on the edge of the Antarctic continent. Maduro and Schauerhammer (1992, pp. 14–17) point out that Mount Erebus has been in a state of continuous eruption since 1972. They argue that:

- eruption of Mount Erebus injects 1000 tons/day of active chlorine compounds into the atmosphere;
- because of the extreme dryness of the Antarctic troposphere, the atmospheric lifetimes of these active chlorine compounds are much greater than those quoted as typical for such compounds;
- McMurdo Sound, where the NOZE investigations took place, is just 10 km downwind of Mount Erebus, and so readings of ozone and/or chlorine compounds taken at this base are grossly distorted because of local effects of the volcanic plume;
the implication is then drawn that, in the Antarctic, the chlorine from Mount Erebus is much more abundant than active chlorine compounds from anthropogenic sources such as CFCs could possibly be. The claimed input of chlorine compounds from Mount Erebus may or may not be correct; it certainly can not be deduced from the article cited as the authority without making other assumptions. It is not an unreasonable value, but how it was obtained is not at all clear.

The next problem is that the eruptions of Mount Erebus are continuous and gentle. This is not, by all accounts, the type of volcano that explosively spews ejecta into the stratosphere. Any hydrogen chloride plume from Mount Erebus is unlikely to reach more than a few kilometres altitude in the first instance, and in the polar winter it will be held at low altitude by the strong descent in the polar vortex. The very same arguments that the authors used irrelevantly against polar CFCs ascending into the stratosphere, is a very pertinent argument against a chloride plume from Mount Erebus making a similar ascent.

The third problem is that although the NOZE experiments were indeed based at McMurdo, most of the measurements were relevant to stratospheric processes and concentrations rather than surface ones. Even for the instruments that were ground based, the scientists were at least claiming, according to the normal usage of such instruments and interpretation of their results, to provide a vertical profile of the distribution of ozone and of chlorine compounds. Low level hydrogen chloride, and even the presence of other chlorine compounds at low levels in the atmosphere simply would not interfere with the measurements. Thus, for example, some of the scientists involved could refer to NOZE results in these terms:

Our 1986 measurements of chlorine monoxide showed a strong layer in the vicinity of 20 km [altitude], with a peak mixing ratio of the order of 10^2 times that predicted for this altitude range by normal stratospheric chemistry. (de Zaffra et al., 1989)

More importantly, many of the critical data were not collected at McMurdo. The picture put together by the atmospheric scientists relied on data collected from at least four widely separated Antarctic ground stations, from satellite observations, from balloon and rocket-based measurements, and from the AAOE flights which sampled directly in the polar stratosphere. Cross-checking to ensure that a consistent pattern had been obtained between all of these sources of data was an important aspect of the scientific investigation.

Having raised the issue of Mount Erebus, Maduro and Schauerhammer turn their attention to volcanoes more generally. Their claim is that volcanic inputs of chlorine compounds directly into the stratosphere are much greater than anthropogenic inputs. Their argument to support this claim runs roughly as follows:

- the amount of chlorine released to the atmosphere by volcanoes is very difficult to measure directly;
- in the late 1970s a volcanologist made some chlorine determinations which he interpreted as meaning that previous estimates of chlorine emissions may have been too low, possibly by as much as a factor of 20 to 40;
- large individual eruptions involve the ejection of very large amounts of material. Material from such eruptions is sometimes injected directly into the stratosphere.

The next few pages of their book describe the dramatic effects of some volcanic explosions. Nothing resembling a calculation of the proportion of volcanic chlorine reaching the stratosphere, or even a reference to such a calculation is presented. In discussing the Tambora explosion, the authors get sidetracked onto the later parts of their thesis – chlorine chemistry and radiation issues:

Now if the ozone-depletion-by-chlorine theory were true, such a catastrophic release of chlorine in 1815 should have wiped out the ozone layer completely, flooding the earth with so-called cancer causing ultraviolet rays. Every single man, woman, and child on earth should have suffered from skin cancer (Maduro & Schauerhammer, 1992, p.19).

Note the innuendo in the last clause of the first sentence: are they seriously arguing that ultraviolet rays do not cause cancer? But the more serious problem with this, is that they have refuted their own argument with what they have said a few paragraphs earlier:

... injecting enormous amounts of ash and debris directly into the stratosphere. The volcanic cloud reduced the amount of sunlight reaching the surface of the Earth [my emphasis], lowering temperatures.

An aerosol of volcanic ash is very effective at blocking those ultraviolet rays!

The next group of arguments concern salt spray from the oceans. Assertions that a very large amount of chlorine enters the atmosphere as particulate sodium chloride from the evaporation of salt spray, and that some of this salt makes its way to the stratosphere are uncontroversial. But the proportion of sodium chloride that finds its way to the stratosphere from ocean spray residues is minute, according to the consensus of scientific wisdom. Any estimation of this proportion is completely missing from the argument.

Instead, we are presented with an improper comparison – sodium chloride release to the atmosphere (as a whole) is compared with CFC
break-up in the stratosphere. The figure used for the latter quantity is in any case a factor of 30 too low, because it was obtained from the miscalculation discussed earlier. The figure that is desperately needed for a proper comparison is the size of the sodium chloride injection into the stratosphere, and that figure is not produced.

Those who hold CFCs responsible for a significant increase in chlorine-mediated stratospheric ozone depletion do not need to claim that none of the sodium chloride from salt spray finds its way to the stratosphere; only that the contribution from this chlorine is smaller than that from CFCs – perhaps half as much or less.

Maduro and Schauerhammer (1992, p. 26) complain that 'the ozone doomsday papers' do not refer to the natural presence of chlorine compounds in the stratosphere prior to the manufacture of CFCs, nor do they refer to chlorine from the oceans.

Their claim is largely, but not entirely true. This does not mean that their complaint is justified. There is an obvious reason why the origins of pre-CFC chlorine, or chlorine from the oceans, are not referred to. It is a simple question of relevance. Research papers are required to be concise and focused. The currently accepted views on salt spray, and on the amounts and origins of natural chlorine in the stratosphere were not being challenged. They were part of the underlying corpus of scientific knowledge that was being used as a basis in these papers, and were therefore 'taken as read'. The journal editors would probably have been very unsympathetic to the inclusion of discussion of this sort of material in the papers, in the unlikely event that the authors had seen a need to do so. It is not particularly difficult to find discussions both of ocean salt and of natural stratospheric chlorine in the mainstream atmospheric science literature.

Maduro and Schauerhammer review a paper which they feel supports their case (Delaney et al., 1974). They claim that it conclusively shows that 'vast amounts' of oceanic chlorine reach the stratosphere.

What they do not pick up on is that this paper actually provides them with some information about just how much! Chlorine analyses were done on particles collected in balloon flights at 16 and 18 km in the very lowest part of the stratosphere (as well as at several heights through the troposphere). The measurement was thus specifically of chlorine in particles; gas phase chlorine compounds in the stratosphere were not measured. Mass mixing ratios for chlorine in particles at 16 to 18 km altitude are plotted between 0.03 and 0.11 ppbw. Data for hydrogen chloride gas levels in the stratosphere range from about 0.4 ppbw at the bottom of the stratosphere (16 km altitude) to about 3 ppbw at the top (Warneck, 1988, p. 119). Translating from volume to mass fraction would provide 0.5 ppbw and 4 ppbw respectively. The only conclusion that can be drawn is that salt spray contributes at most 20 per cent (i.e. 0.1 ppbw/0.5 ppbw) of the total chlorine present in the lower stratosphere. (The sodium imbalance the authors refer to indicates that it is probably only half as much!). It is also worth noting that this chlorine is in a particulate form, where it cannot contribute to the Molina–Rowlands scheme, and even its participation in the Antarctic phenomenon may be problematic.

The types of actual particles collected in the study cited do not seem to have been investigated. In a much later paper dealing with the volcanic cloud from the explosive eruption of El Chichón in 1982, Woods et al. (1985) comment that the techniques they used... revealed the presence of NaCl particles (halites), which are rarely if ever seen at these altitudes [i.e. 18–21 km].

Any sea spray residues would, in the absence of further chemical processing, be in the form of halites.

Maduro and Schauerhammer's next arguments deal with exchange of material between troposphere and stratosphere. The discussion seems relatively uncontroversial and only marginally relevant, since there is no denial in any quarter that such material exchange does occur. One suggestion that does seem a little bizarre is that 'many scientists' believe that downward transport of stratospheric ozone is the main source of tropospheric ozone, and might even be responsible for smog alerts. No source is cited for this particular speculation. A significant amount of ozone does reach the upper troposphere by downward transport. Seldom does ozone which enters the troposphere by this mechanism penetrate downward to ground level. The origin of ozone in smog from the interaction between hydrocarbons and oxides of nitrogen is very well established, and not a matter of controversy.

The discussion of methyl chloride from biomass burning is based on data which came largely from interviews and conversations with named scientists. The information might be sound, but it has not passed the filter of peer review.

Although it is not directly relevant, an aside dealing with carbon dioxide in this section illustrates some of the problems with the style of argument these authors use when they do attempt to be quantitative. In an interview, a Brazilian scientist provides a figure of 540 million tons for annual carbon dioxide emissions from burning in the Amazon rain forest. An American scientist, in a private conversation with the authors, suggests that the figure might be more like 4 billion tons, when other sources of carbon dioxide emission associated with deforestation are included. It is then pointed out that the Amazon represents less than half of global
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forest burning. A comparison is invited with an alleged figure of 5 billion tons for the global industrial release of carbon dioxide. The reader is indirectly led to conclude that biomass burning (8 billion tons is implied but not stated) contributes more carbon dioxide to the atmosphere than industrial activity (5 billion tons).

But the figure of 5 billion tons, which is not referenced, is fairly easily found. It is widely quoted in many articles and textbooks dealing with the global carbon cycle, as the carbon content of the carbon dioxide released (e.g. Wayne, 1991, p. 18). But only 27.3 per cent of the mass of carbon dioxide is carbon. Five billion tons of carbon means just over 18 billion tons of carbon dioxide as the annual global industrial output. If the 4 billion tons figure for Amazon forest burning and associated soil release is taken seriously, that might lead to a global figure for biomass burning of 10 billion tons at most. But the figure may equally well be as low as 1 billion tons or so, if the Brazilian scientist's calculations are more accurate! Instead of being larger than the industrial emissions, the carbon dioxide from biomass burning lies somewhere between about one twentieth and one half of the industrial output, if the authors' sources are taken at face value, and the calculations done properly.

The chlorine figures are treated in similar cavalier fashion. A paper by Crutzen and others is cited as providing a figure of 420,000 tonne for chlorine release as methyl chloride from biomass burning. But Maduro and Schauerhammer choose to multiply this figure by at least ten because of alleged satellite surveys that show Crutzen's estimates of rates of deforestation to have been much too low. No source is cited. A figure of 4.2 million tons is submitted as the appropriate figure for annual chlorine release from tropical biomass burning. This figure is then doubled again to arrive at 8.4 million tons because of a rather vague suggestion that wildfires in developed countries in the temperate zone might contribute as much again.

On the balance of the evidence presented, it would probably be fair to concede that somewhere between 420,000 tonne and 8.4 million tons of chlorine are released annually to the atmosphere, largely as methyl chloride, from biomass burning.

The 1994 report of the Intergovernmental Panel on Climate Change provides relevant data that can help to put this methyl chloride release on a more accurate and realistic basis. The figure they provide for total atmospheric content of methyl chloride is 5.0 million tonne, and the atmospheric lifetime is given as 1.5 years. This corresponds to a total annual input of 3.3 million tonne of methyl chloride, or 2.3 million tonne of chlorine as methyl chloride (since methyl chloride is only 70 per cent chlorine). This 2.3 million tonne must include both the biomass burning source – estimated in Maduro's book as 8.4 million tons – and the 'seaweed' source discussed in the following section – estimated in the book as 5 million tons.

The current orthodox view of atmospheric scientists is that CFCs are a major source of stratospheric chlorine compounds, and the dominant source of the increase in stratospheric hydrogen chloride and chlorine nitrate that has led to development of the Antarctic ozone hole.

A careful analysis of the arguments presented by Maduro and Schauerhammer has shown that they are not effective in rationally under-mining that view. On the other hand, they contain numerous clear errors, that in every case lead either to drastic underestimation of CFC contributions to the chlorine burden of the stratosphere, or significant over-estimation of one or more of the natural sources. The authors' own position on the issue looks quite unsound.

Not only is the authors' position unsound, but their arguments on this part of the question must be judged unscientific. Each one is either dogged by vagueness, or flawed by demonstrable error or misinterpretation. None ought to have passed a scientific peer review process.

Maduro and Schauerhammer write at length about the possibility of CFCs being removed from the atmosphere otherwise than in the stratosphere. They describe possibilities of bacterial degradation, absorption in leaves, deposition in the oceans. The description is all in qualitative terms. No attempt is made, either by Maduro and Schauerhammer themselves, or in references to the publications that they cite, to extrapolate any of the findings to provide an estimated size for a global sink.

There is a very simple reason for this. Rowland (1994) summed up the results of CFC monitoring in the lower atmosphere as follows:

By the mid-1980s, CFC increases were large enough to show that the corresponding atmospheric lifetimes must clearly be very long – 50 to 100 years – and no undiscovered tropospheric sinks existed.

To elaborate this a little: the rate of build-up of CFC content of the lower atmosphere was large enough, when compared to the known production figures for CFCs, that it could be ascertained that at most 1 per cent to 2 per cent of the total CFC content of the atmosphere was being removed each year by all sinks (and this can be deduced without any knowledge of what the sinks are). Decomposition by UV light in the stratosphere was by then a thoroughly investigated sink. It was 'known' to be removing 1 per cent of the total CFC content of the atmosphere each year. Therefore all other sinks, whatever they may be, are only removing between 0 per cent and 1 per cent of the total atmospheric CFC content annually; that is they must be responsible for only between 0 and 50 per cent of total CFC.
removal, while the stratospheric sink accounts for between 50 per cent and 100 per cent.

What, then, was the basis of the claimed knowledge of the rate of removal of CFCs from the stratosphere? The main evidence came from stratospheric measurements of CFC levels, which showed fairly constant mixing ratios up to a height of 25 km, and then a very rapid decrease with increasing height (Rowland, 1996, p. 1790). No physical circulation process can account for this. It can only be due to decomposition in UV light. The rate of CFC removal represented by the altitude profile can be deduced from the known rates of material transfer in atmospheric circulation.

The evidence that other sinks for atmospheric CFCs are relatively minor can thus stand quite apart from any study of the alleged sinks themselves.

Rowland (and others among the mainstream atmospheric scientists) leaves an opening for an attack on the ground of alternative CFC sinks, in saying that there are none — as in the quoted passage above. All that the evidence supports, and all that needs to be said for the ozone depletion theories to be valid, is that other CFC sinks, if they exist, and whatever they may be, are very minor contributors to CFC removal, compared to the stratospheric sink.

The second string of Maduro and Schauerhammer’s attack on the orthodox view of Antarctic ozone depletion is a questioning of the reality of the phenomenon itself, or at least of the way it is typically described by the atmospheric scientists.

Maduro and Schauerhammer’s message in introducing their discussion of the Antarctic ozone hole is confused and self-contradictory. Thus we are told on page 120 of their book that Dobson’s Southern anomaly is the same thing as the ozone hole, and on page 121 that it is not. Claims of prior discovery of the ozone hole at Dumont d’Urville in 1958 and at Syowa in 1984 have already been discussed in Chapter 6 of this volume. The former is certainly important to the general thrust of Maduro and Schauerhammer’s arguments, but it is difficult to see any effect that transferring credit for the discovery from the British Antarctic group to the Japanese would have on their case.

From the introduction to their chapter, where the reader is tacitly invited to regard the ozone hole as much the same thing as Dobson’s Southern anomaly, through the early part of the discussion, where the suggestion is that there was an ozone hole in 1958 which disappeared for many years, only to return in the late 1970s, the authors come to the next issue they want to discuss, with another change in position. They seem to concede that the ozone hole is new, or at least more serious than it formerly was. They turn to arguing that it is not man-made, and that chlorine compounds have little or nothing to do with it.

Some discussion of natural cycles and other factors that influence ozone levels is presented. It leads to a pronouncement that the ozone hole is a natural and ephemeral phenomenon. In one sentence (Maduro & Schauerhammer, 1992, p. 127), the data are alleged to ‘suggest’ it. In the next they prove conclusively that the hole was there decades ago.

The evidence and argument to support such a contention simply is not there. At best, there might be some support for it if the 1958 Dumont d’Urville reinterpretation were taken as solid and incontestable fact. But even in that case, a single anomaly of that type might rather provoke a search for unusual antecedents relating to that particular occasion, rather than an induction that the phenomenon is ‘ephemeral’ in a more general sense!

The logical slide from an allegation of error to an allegation of fraud, without presentation of supporting evidence, ought also not be left unmarked.

The rest of this chapter of their book is devoted to possible alternatives to the chlorine account of Antarctic ozone depletion. Various material is presented from the standpoint of the circulation and solar cycle theories. No mention of the AAOE or its results, nor citation of any literature more recent than the AAOE is included. This may be only marginally excusable in a volume published four and a half years after the AAOE, and two and a half years after the full and final publication of its results. Needless to say, the AAOE results themselves completely disarm these arguments, to the point where their main protagonists, Mahlman, Fels, Schoeberl, et al., abandoned them in favour of the chlorine theory.

Having already concluded that chlorine chemistry is not the cause of the Antarctic ozone depletion, and that the chlorine that is not causing the phenomenon is not coming from CFCs, Maduro and Schauerhammer devote their final chapter of ‘scientific’ analysis to whether or not the Antarctic ozone hole really matters.

The central issue that they address is the danger of increased skin cancer incidence. This is implied in the connections drawn by mainstream scientists between decreases in ozone and increases in surface ultraviolet irradiation, and between increased surface ultraviolet radiation and increased incidence of skin cancers. Other alleged effects of increased radiation arising from ozone thinning are also discussed.

The first claim that Maduro and Schauerhammer make is that there is no evidence that the amount of damaging UV-B radiation reaching the
earth’s surface has increased. They cite data collected at a series of US field stations during 1974–85, which show a clear decrease in the total amount of surface solar UV-B radiation through the period.

Their argument is that if ozone levels really decreased by 3 per cent, and if UV-B irradiation increases by 2 per cent for each 1 per cent ozone decrease, UV-B levels ought to have increased by 6 per cent or so. But it is quite apparent that this is based on a simplistic misrepresentation of the atmospheric scientists’ position. The claim for a 3 per cent ozone depletion represents an underlying figure for the chlorine-mediated contribution, which is part of an overall pattern of ozone presence which involves variations with the solar cycle, the quasi-biennial oscillation, and other factors. By the same token, ozone levels are only one of several factors that influence surface UV radiation. Average cloud cover, and general ‘haziness’ (that is, the amount and type of tropospheric aerosol present) can obviously also affect surface ultraviolet levels to a much greater extent than a small variation in stratospheric ozone. Perhaps more important in this case, though, is the question of stratospheric aerosols. Major eruptions of Mount St. Helens and El Chichón in 1982, followed a relatively quiet period for volcanoes in the 1970s. El Chichón, in particular, injected a large amount of material directly into the stratosphere, which had a significant effect on incoming solar radiation, for two to three years following the eruption (Mankin & Coffey, 1984).

More recent data from New Zealand (McKenzie, 1996), show both a significant increase in surface UV-B, and a strong correlation of surface UV-B with ozone levels during the period 1982–90. The influence of any volcanic aerosol on these figures has not been factored out; it may be fortunately absent, since the El Chichón cloud probably did not extend so far South, and the next major eruption, that of Pinatubo, occurred in 1991.

Maduro and Schauerhammer then proceed to point out the relatively small scale of the UV-B dose increase that might be associated with a moderate depletion of stratospheric ozone, with the very large variations of UV-B reaching the earth’s surface at locations at different latitudes, or at different altitudes. They compare the increased risk for an individual with that of moving address a few hundred kilometres closer to the equator.

They briefly dismiss the other point that is sometimes raised – ecological damage because plants and animals may have difficulty adapting to increased UV at any fixed location – by pointing out the versatility of various crop plants in adapting to conditions over a wide latitude range. The latitude range over which a crop species can grow efficiently typically would span a range of UV radiation levels greater than any radiation change brought about by moderate ozone depletion. One part of this debate that they have not addressed is the issue of some of the specialised ecologies in the Antarctic and sub-Antarctic. There, species that may be important in the food chain have had to face very different UV-B levels in the Antarctic ozone hole, from the extremely low ones they have evolved to cope with (Silver & de Fries, 1990, pp. 113–14).

Finally, the authors turn to a claim that UV-B radiation is not as damaging as has been supposed. Their first claim is that, while other skin cancers do show some epidemiological correlation with total UV dosage, malignant melanomas do not. Their playing down of the importance of other forms of skin cancer is a rather bizarre overstatement. Their claim about melanomas is somewhat out of date, and does not tell the whole story of a very complicated connection that is only starting to be clearly understood (Armstrong, 1996).

They conclude the chapter with a review of a number of claimed beneficial effects of UV-B radiation exposure: both generally accepted ones, and some contentious ones. Making a great deal of the fact that UV-B kills certain bacteria seems a little of a two-edged sword; on the one hand it may indicate a means of removing these organisms if they are not wanted; on the other it seems indicative of a general injuriousness of UV-B to living things!

Overall, then, the finding of this analysis must be that, while Maduro and Schauerhammer (1992) adopt (in places) the language and forms of a contribution to a scientific debate, that is not how it should be viewed or classified.

No serious attempt is made to provide any new observational or experimental evidence nor new theoretical insight that might be effective as scientific argument. The arguments put forward lack any real substance. In most cases they contain errors of logic or mathematics, or avoid quantitative detail, or both.

It is unfortunate that a book published in 1992 avoids discussion of the results of the AAOE series of experiments in 1987, particularly when they played such a crucial role in swinging the scientific consensus firmly behind the chlorine-based theories of the Antarctic ozone hole. This failing is compounded by producing old arguments from the standpoint of the solar cycle and circulation-based theories, when the protagonists of these outdated theories had long since abandoned them in the light of new evidence.

The book is not a contribution to a scientific debate. It does not present a serious challenge to the current scientific consensus on chlorine-mediated ozone depletion.

But that does not mean that Maduro and Schauerhammer’s work is not an effective and influential one at other levels. The arguments are presented with a political rhetoric that is very convincing for the uninformed.
reader, the careless casual reader, or the reader who is predisposed to a view similar to theirs. It has played a role in influencing debate at levels other than the scientific: the political debate in the US Congress, and the public debate in the mass media outlets.

Rowland's evaluation is dismissive: he claims that the book is 'a good job of collecting all the bad papers in one place' (Taubes, 1993, quoting from Rowland's presidential address to the AAAS). This is an unfortunate suggestion. Some of the papers quoted by Maduro and Schauerhammer should not be seen as 'bad' in any sense. It is rather the case that they are cited in a way that is out of context, or they represent out of date work which was good at the time, but has since been refuted.

But the atmospheric scientists have found that they cannot afford to be so dismissive in public forums; even if they continue to ignore Maduro and others arguing in a similar way in the strictly scientific forums. And even this latter step lends credence to accusations of a conspiracy by atmospheric scientists to silence their critics, and suppress work that does not fit in with their favoured theories.

One atmospheric scientist suggests that the scientist trying to contribute to a public debate is:

... caught between the exaggerations of the advocates, the exploitations of political interests, the media's penchant to turn everything into a boxing match, and your own colleagues saying we should be above this dirty business, and stick to the bench. (S. Schneider, as quoted by Taubes, 1993)

Some recent secondary commentators have seen the scientific debate on chlorine-mediated ozone depletion as not yet settled, and cited Maduro and Schauerhammer's book as an important document for the minority view.

I cannot accept that their book is a part of a scientific debate, for the reasons that have emerged in this discussion. A more appropriate view is that the scientific debate is now closed, with a clear consensus behind the orthodox views of the ozone hole, and chlorine-mediated global ozone depletion. A public and political debate continues in some quarters, based largely on the same flawed and outdated arguments that Maduro and Schauerhammer present (see, e.g. Clarkson et al., 1994).

Conclusions about the scientific consensus

In the analysis in this chapter, and, indeed, throughout this volume, we have seen how a body of scientific evidence accumulated in the primary literature, and how it shaped various aspects of the debate and eventual consensus about several aspects of the investigation of the ozone layer.

It could not be said that what we have found is a science that is based on a solid and rational ontology or epistemology as might be demanded by philosophical purists. Either models like Popper's are too narrow and rigid to provide a realistic description of science, or this area of science has not yet qualified as sound and mature science, and is unlikely to do so in the near future. Clearly, I prefer the former characterisation.

On the other hand, what we have seen is a science that has made pragmatic and sensible use of such evidence as there is. In most cases the scientific consensus has been backed by proof that would be likely to stand in a court of law as 'beyond reasonable doubt', and in all cases by proof rather stronger than 'the balance of probabilities'.

The caricaturisation of science as a monolithic conspiracy, manipulated by an elite, motivated by issues of research funding and political power, does not fit well with the healthy and vigorous debate that can be seen to have taken place. The way that that debate was settled, and the relationship of the settlement to the evidence is clearly recorded in the primary scientific literature. It can easily be audited by informed outsiders—say other scientists from a neighbouring discipline. In this instance at least there seems to be little ground for complaint.

This is not to say that political and sociological issues are unimportant. There is little doubt that the way scientific institutions and infrastructure are organised produces a strong conservative bias. There may well be instances where personal ambition has managed to distort parts of the scientific edifice to a greater or lesser extent. The dynamics of human social interactions affect bodies like national academies of science or the Ozone Trends Panel no less than they do other similar institutions. But conspiracy allegations are implausible. As an example, the Ozone Trends Panel includes, and has always included prominent scientists who were major players on both sides of a vigorous debate between chlorine and circulation theories of anomalous Antarctic ozone depletion. When such a body arrives at a consensus position in accord with one of these views, and in contradiction of the other, in the light of new evidence, it must be taken seriously. This is even more the case when the new evidence is clearly and publicly presented in a form accessible to anyone who can take the trouble to learn enough background material to follow the more technical side of the argument.

At the end of the twentieth century, there is a firm but provisional consensus about the science of the ozone layer. The investigations that we have explored in this volume have been thorough, and the conclusions appear to be well grounded in and firmly justified by the experimental and observational evidence. I believe that here we have a modern instance of good science.

The status of the evidence on which the scientific consensus is built in
this case cannot be seen as providing firm epistemological justification in a global sense. There is nothing that would disarm the thorough-going sceptic. But there is justification quite sufficient for an auditor who is prepared to go along with the general thrust of current scientific belief to accept the detail of the consensus that has been reached about these phenomena, and to admit it into the general scientific corpus. Good science of the type represented by these investigations can thus be seen as a valid and worthwhile pursuit.

NOTES

1 In this article Rowland claims that Lovelock, and Wilkniss et al. had established a current background of about 1 part in $10^{10}$. The one-year residence time was based on the figure of 1 part in $10^{11}$ given in Lovelock's earlier letter.

2 Molina, M.J., Public Lecture, University of Melbourne, 4 December 1996. In reply to a question from the audience, Prof. Molina stated that he and Rowland had realised from very early that their work had important public policy implications. They took what action they could, within the bounds of professional propriety, to ensure that their work was widely noticed.

3 J.R. Christie, private communication. A very simple spreadsheet calculation was set up. CFC production was assumed to increase linearly from zero in 1934 to its 1975 level, and then to hold that same level from 1975 to 1990. Each year 1 per cent of total CFCs was assumed lost to the stratosphere, and 0.25 per cent lost to all other sinks. These conditions correspond to an atmospheric lifetime of eighty years, and an 80 per cent efficiency for transfer to the stratosphere. The total 1990 transfer to the stratosphere was a figure equal to 28.9 per cent of the 1990 production. Leaving the rate of stratospheric transfer fixed at 1 per cent, the total transfer rose to 30.2 per cent at 100 per cent transfer efficiency, and fell to 25.4 per cent at 50 per cent transfer efficiency.

4 The usage of ton and tonne in this passage may seem a little confusing. In essence, the American authors follow local practice and work in imperial tons. The scientific sources, and the author of this volume, prefer to work in tonne, or metric tons of 1000 kg. An imperial ton is just over 1016 kg. The difference in size between the two units is trivial for most purposes, and certainly for the purposes of the discussion in this chapter.


6 A well known story concerns a minister of religion working with a group of homeless alcoholics. He is supposed to have poured out a small puddle of whisky, and placed a small worm in it. The worm wriggled briefly, and died within a half minute or so. 'Alcohol is a poison!' he thundered, 'What does that experiment show you?' 'If I drink plenty of whisky, I won't get worms,' came the reply from one of his audience.

7 Newton, D.E., The Ozone Dilemma, ABC-CLIO, Santa Barbara, 1995. The conclusion to a chapter entitled 'Ozone layer depletion: myth or reality?' is that 'The debate concerning ozone depletion is far from over'. (p. 22.)