1 Introduction

This book tells the story of scientific understanding of the stratospheric ozone layer. It is certainly not the first work to be written on this subject! But the approach here is somewhat different. We are looking at the story of a series of scientific investigations. And we are looking at them from the point of view of evidence: what conclusions were drawn, and when? How were experiments designed to try to sort out the different possibilities? What happened to cause scientific opinion on certain issues to change? The first part of the book sets out the history, with these sorts of issues in focus.

This then sets the basis for the second part. Philosophers of science have tried to analyse the way that science is conducted. They have written about the way that theories are devised, become consensually accepted, and then may be revised or even overthrown in the light of new evidence. The history of stratospheric ozone is full of unusual twists and changes. So in this work it is used as a case study: an example we can use to examine how some philosophical accounts of evidence in science might compare with the actual conduct of modern science. The example even suggests some new aspects that differ from the philosophers' accounts.

Does that mean that this is a work without a clear focus? A book that is trying to tackle two quite separate issues, rather than concentrating on one of them? I would certainly hope not. The aim is rather to achieve a sort of two-way feedback that enriches both themes. On the one hand, the philosophical issues can be more clearly brought out when they are related to a real and interesting case in near-current science. The relevance of the several philosophical accounts, and the problems with them, are exposed in a different way when they are applied to actual scientific practice rather than idealised science, and to recent science rather than the science of the past. And on the other hand, looking at the history of a series of scientific investigations from the point of view of collection and presentation of evidence, can provide novel and interesting insights. These insights differ from, and are perhaps complementary to those which are obtained when the history is analysed primarily in terms of political and social issues, a more typical perspective in modern history writing. Examination of the history informs the philosophical analysis; an understanding of the philosophical issues enriches the history.

The main source of material for the analysis of the investigation is the primary scientific literature. The history that is presented and discussed here is the 'official' scientific development of the subject, as presented in numerous peer-reviewed scientific papers.

There is a rationale for approaching the history in this particular way. The philosophical questions that I address later, relate to the basis for evaluation of the evidence, and the justification of the theoretical framework. To examine these issues, it is fair to consider the evidence as presented, at the various stages of the unfolding story. Exploring the accident of the detail of the way the evidence was actually collected, or the way theoretical insights were actually gleaned, might produce rather a different picture. On that account science might appear rather less like a rational enterprise. This approach to the history and sociology of science is an important undertaking in its own right. But I see it as largely irrelevant to the specific issues that are being addressed here. The questions of importance to this discussion relate not to whether new evidence or insight was collected as the result of a rational approach, but rather to whether the construction that is put together in reporting the evidence or insight, after the fact, provides a convincing justification.

Some who have written on issues like this have been largely concerned with questions of vested interest and hidden motive. These might certainly colour the way in which a scientific investigation proceeds. Certain projects may receive funding, which others are denied. A group of scientists might be sensitive to the interests of sponsors and 'put a spin' on their published findings. But similar factors apply in any situation where evidence is presented and conclusions drawn from it. What really matters is whether the evidence leads convincingly or compellingly to the conclusions that are drawn. Scientists do not work in a social and political vacuum. There are certainly possibilities that vested interests, improper motives, or pre-conceived ideas might lead some lines of enquiry to be pursued and others neglected. In extreme cases, evidence may be suppressed, distorted, or fabricated. The concern of others with these issues is a legitimate one, even in examining a scientific investigation. But they are not the main concern of this work. Vested interests may indeed have played a major role in some aspects of the ozone investigations. The issues will be indicated, but any deep analysis left to others.

There is an important problem with trying to use the record of the primary scientific literature as an historical source in this way. It is incomplete. It is incomplete in a systematic way, and in a way that is sometimes

– fortunately rarely – misleading. A scientific paper sometimes contains errors that escape the notice of the referees. Simple miscalculations or transcriptions are of course corrected in errata published by the relevant journal. But there are also significant errors of experimental design or interpretation that arise from time to time. A publication which corrects such an error is often, and justifiably seen as an insubstantial and derivative piece of work, and editors are understandably reluctant to publish such snippets. So in discussion with leading scientists you might hear that 'that paper was flawed', 'that paper was not widely accepted at the time', 'that paper has been discredited', or even that 'the referees really should not have accepted that paper'. And they can point out the flaws to justify such statements. Although the refutations are well known to, and circulate widely within the specialist scientific community, many do not appear in the primary scientific literature, nor even in the review literature.

This underlines the importance of discussions with scientists, and of some of the informal material, in helping to provide a balanced picture.

There is a debate in the Philosophy of Science about the relationships between philosophy, history and science. One view is that philosophers should stand apart from science in prescribing the epistemic standards that science ought to adopt, and the methodologies that are appropriate to this task. They can thereby become an independent arbiter of the performance of scientists. The other view is that philosophers should discern and describe the epistemic standards and methodologies that scientists claim to adopt or actually adopt. By doing this, a more accurate picture of what science actually is emerges, but the philosophers leave themselves with no basis from which to criticise.

Both of these attitudes toward the philosophy of science are fraught with peril.

If we take the first attitude, we are immediately faced with all of the traditional philosophical problems of world view. Should a philosophy of science be based on a realist or an anti-realist ontology? Or can it somehow embrace both? Can parameters be devised for rational scientific methodology while sceptical arguments about the impossibility of any sort of knowledge remain largely unassailable? A path must be traced through these minefields before the specific questions and problems that affect scientific enquiry can be addressed.

Then, even if we succeed in this part of the enterprise, there is a second and much more practical area of difficulty. The demands of logical and philosophical rigour will have constrained the idealised methodology we describe into an artificial enterprise that will probably bear little relationship to the way science is actually conducted. And the work will probably strike few chords with scientists, be of little practical use to the scientific community, and have little practical influence. It is important to stress that this is not necessarily the case. Popper's work, which falls squarely into this mould, has had a huge influence among scientists, and strongly colours the way that they describe and discuss their methodology. But there is plenty of evidence that it does not fit very well with the actual methodology that is adopted in modern science. We will be looking at some of this evidence in later chapters of this book.

The alternative approach is for philosophers rather to recognise that modern science is a huge and relatively successful enterprise that has largely set its own rules and methodologies, and to adopt the task of collecting, describing, systematising, and possibly rationalising the methods that are used and that have been successful. The problem here is that the philosopher who adopts this approach seems to be left without means of handling the traditional philosophical imperatives such as rationality and justification. If the focus is on what science *is*, without a clear model of what science *ought to be*, there is no means of distinguishing good science from bad science. And perhaps the only issue on which there is general agreement among scientists, philosophers of science, historians of science, sociologists of science, and science educators, is that some scientific investigations involve good science and some involve bad science.

Kuhn's account of Scientific Revolutions and Lakatos' account of Research Programmes are among the influential works that can be seen to come from this perspective. The main claim in these works is to describe the actual conduct of science, and there is little in the way of value judgements to enable us to recognise 'good' science. A notion of 'fruitfulness' as a measure of a paradigm or a research programme does emerge: this does seem to be a case of the end justifying the means. Generally these works are less recognised than Popper's by working scientists, and regarded with more hostility.

The approach of this book is to be generally descriptive rather than prescriptive of modern science. But I have tried to maintain some basis for rational examination and judgement. I believe that it is possible to maintain a significant basis for legitimate critical analysis of scientific arguments, and to distinguish good science from bad science, without having to be prescriptive of any ontological or methodological basis. It arises simply from a requirement of legitimate evaluation of the evidence, in the same way that disputes about matters of fact might be resolved in a court of law. The science is clearly flawed, for example, if a particular result is claimed as an entailment of a particular theory, and it can be demonstrated that it is not! Grounds for criticism of the performance of science also remain when it can be shown that parts of the edifice of science rest on improper bases, for example cultural prejudice, political influence of a few leading scientists, fabricated evidence, or the like. There is, in my view, a fundamental requirement that elements of the corpus of scientific knowledge should ultimately be grounded and justified in a reasonable interpretation of observational or experimental evidence. There may also be room for criticism elsewhere in the gap between scientists' claims and performance.

This, then, is the basis on which I have conducted the research that underlies this book. The primary scientific literature which forms the basis for my discussion is supplemented only to a small extent. There are occasional passing references to non-scientific works discussing aspects of the ozone investigation. There have been several books and papers written about the ozone investigation from journalistic, political, or sociological points of view. These secondary sources have been freely drawn on as required to illustrate various points. They are of very widely varying quality, and have not been treated as authoritative sources. This book does not pretend to cater for those whose main interests are in political or sociological questions; these other works should be approached directly.

I include references to scientific reviews and published reminiscences. It would be inconceivable to tackle a project like this without reference to the several reports of the Ozone Trends Panel, for example, or to the Nobel lectures of Molina and Rowland.

I also refer to some unpublished material, some email and usenet newsgroup communications from individual scientists. I conducted a series of interviews in April and May 1996 with a number of scientists who were involved in the investigation in different ways, about their views and their reminiscences. This less formal material is used primarily for illustration, rather than as a central basis for any of my arguments. Much of it has contributed to my own background understanding of the issues, and has perhaps influenced the writing in ways that are not and cannot be directly attributed.

The main focus of this book, then, is on a series of scientific investigations which took place quite recently: between about 1970 and 1994.

In 1987, the governments of many nations agreed to limit, and eventually to phase out the widespread domestic and industrial use of chlorinated fluorocarbons (the Montréal Protocol). This was because of scientific suspicion that continued use of these compounds posed a real threat to the structure of the upper atmosphere. In particular they are supposed to be involved as precursors to chemicals which deplete ozone levels in the stratosphere. Significant loss of ozone from the stratosphere would allow damaging ultraviolet radiation, presently absorbed by ozone, to penetrate to the earth's surface. Because of the potential seriousness of this problem, regulating authorities adopted a standard of caution, and acted before the scientific issues had really been decided. Action on this scale against industrial products, particularly ones which have no direct toxic, carcinogenic, explosive, or corrosive effects, is quite unprecedented.

The background to this decision goes back to the discovery of ozone 160 years ago, and the gradual discovery and investigation of its presence and role in the stratosphere between about 1880 and 1970.

Chlorinated fluorocarbons were developed as refrigerants in the 1930s. They had remarkable properties which led to their being enthusiastically adopted for various applications during the four subsequent decades.

Then, as environmental awareness became an important issue during the 1970s, there were warnings about possible damage to the ozone layer as a result of human activity. First, there was the problem of high-flying planes, and then a warning about inert chlorine-containing compounds.

The last part of the story centres around the discovery and subsequent investigation of the Antarctic ozone hole, which occurred at much the same time as the negotiations that led to the Montréal Protocol. A scientific consensus about the general basis of the phenomenon was achieved in the late 1980s, and about its detailed mechanism in the early 1990s. But there are remaining problems and uncertainties, and stratospheric ozone remains an active area of current scientific research.

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More information

Part I

History of the understanding of stratospheric ozone

2 Stratospheric ozone before 1960

Ozone, O_3, is a highly reactive form of oxygen, which is found in trace quantities both in the natural stratosphere (15–50 km altitude), and in polluted surface air. It was discovered and characterised in 1839 by Schönbein. It cannot easily be prepared pure, but can readily be obtained in quantities up to 50 per cent by passing an electric spark discharge through normal oxygen. Ozone is much more reactive than normal molecular oxygen, and is also very toxic.

The presence of ozone in the upper atmosphere was first recognised by Cornu in 1879 and Hartley in 1880. Its particular role in shielding the earth's surface from solar ultraviolet light with wavelength between 220 and 320 nm then became apparent. Meyer (1903) made careful laboratory measurements of the ozone absorption spectrum. Fabry and Buisson (1912) were able to use these results to deduce the amount of ozone present in the atmosphere from a detailed analysis of the solar spectrum. It was not hard for the scientists to deduce that gases in the earth's atmosphere must be responsible for any missing frequencies observed in the spectrum of sunlight. To produce an absorption in the solar spectrum, a molecule must be somewhere on the path of the light from the sun to the earth's surface. The solar atmosphere is much too hot for any molecules to be present, let alone a relatively unstable one like ozone. There is ample other evidence that interplanetary space is much too empty to be a location for the required quantity of ozone. Therefore the ozone is somewhere in the earth's atmosphere.

Fabry and Buisson (1921) returned to the problem later, having produced a spectrograph better designed for measuring ozone absorption. They measured ozone levels over Marseilles several times a day for fourteen consecutive days in early summer. Their measurements appear to have been quite accurate. They concluded that the thickness of the ozone layer was about 3 mm at STP. That is, if all of the ozone in a column above the observer were warmed to 0°C, and compressed to a partial pressure of 1 atmosphere, it would form a layer 3 mm thick. In current units, this amounts to 300 Dobson units, very much in line with more recent measurements. They also found that ozone levels showed a small but significant irregular variability with time of day, and from day to day.

Measurements taken at Oxford by Dobson and Harrison in autumn 1924 and spring 1925 showed that springtime levels were much higher than autumn, and also showed much greater short term irregular variability than the Marseilles results had (Dobson and Harrison, 1926). Over the course of the next few years they were able to establish a regular annual pattern which reached a minimum in autumn, and a maximum in spring. They were also able to demonstrate a close correlation between ozone measurements and surface air pressure, with high pressure corresponding to low stratospheric ozone (Dobson, 1968b).

Discovery of these variations in ozone with season and weather conditions was of great interest to meteorologists and atmospheric physicists. It immediately raised the problem of discovering a mechanistic link, and a direction of causality between the phenomena. Also, the correlation with surface weather conditions meant that ozone monitoring held some promise as an extra piece of evidence that might become useful in weather forecasting.

The discoveries also stimulated an interest in the wider investigation of regional distribution of stratospheric ozone. Already, ozone levels had been found to vary from place to place, from season to season, and with weather patterns. Systematic collection of much more data was seen as a necessary prelude to any deeper theoretical understanding of a possible connection between ozone levels and climate, weather patterns, or air circulation.

Some effort was made to obtain regular readings from a series of observing stations with wide geographic distribution. The first attempt in 1926 involved measurements with matched and carefully calibrated instruments from stations at Oxford, Shetland Islands, Ireland, Germany, Sweden, Switzerland, and Chile. In 1928 these instruments were moved to give worldwide coverage. The new network included Oxford, Switzerland, California, Egypt, India, and New Zealand. An attempt to set up an instrument in the Antarctic at this stage, in the care of an Italian team, ended in disaster. The Dobson spectrometer finished up at the bottom of the Southern Ocean (Dobson, 1968b).

Between 1928 and 1956 a lot of painstaking work was conducted. The main achievements could be classified in the following areas:

1. The need for a global network of ozone monitoring stations was recognised, and protocols were devised to try to ensure that observations from different stations would be directly comparable.
2. Techniques and instrumentation were greatly refined. Initially the spectra taken had to be from direct sunlight (or, with much less accu-

racy, from moonlight). Methods were developed initially for clear zenith sky, and then for cloudy zenith sky. A comprehensive monitoring network needs methods that will work on cloudy days, or the data from some locations will be very sparse indeed.

3. New techniques were developed to give information about the vertical distribution of ozone. The only information available from a conventional ozone spectrometer is the amount of ozone in the line between the instrument and the sun. This can be readily and accurately converted to 'total column ozone' – that is the total amount of ozone in a vertical column directly above the observer. But there are effects arising from light scattering in the upper atmosphere that can be exploited. Sunlight travels directly from sun to instrument. Skylight travels along one line from the sun to a scattering centre, and another from scattering centre to instrument. Tiny differences between sunlight and skylight spectra can provide information about differences in the amount of ozone along the two paths. If the distribution of scattering centres is known or can be safely assumed, then this data can be transformed to calculate varying distributions of ozone with height. The results are very approximate. But ground-based instruments can provide some vertical distribution information. Development of methods suitable for balloon-borne experiments was a separate aspect of this work. At that time, balloon-borne instruments were the only practical means of directly probing the stratosphere. Attempts to measure ozone in aircraft in 1952 had mixed success – they did indicate (as expected) that ozone levels were very low throughout the troposphere, and started to increase rapidly above the tropopause. But the altitude of the ozone layer was well above the operating height of the aircraft. Very little ozone could be measured at altitudes the aeroplane was capable of reaching.
4. Gradually a picture was built up of the annual and short term variation patterns for stratospheric ozone. A strong correlation of the short term variations with surface weather patterns was established. Some theoretical explanations for these variations and connections were starting to emerge. The situation was seen almost entirely in circulation terms, with low column ozone levels associated with upwelling of ozone-poor tropospheric air, and higher levels associated with downward air movements in the stratosphere.
5. The group of scientists with an interest in stratospheric ozone monitoring gradually increased. The International Ozone Commission was set up in 1948, and atmospheric ozone was one of the major issues addressed in planning the International Geophysical Year (IGY) programme for 1957–8. Unlike most years, the IGY lasted for eighteen