European Commission

Air & Climate








CONVERSATIONS ABOUT MOLECULES AND PLANETS, WITH HUMANS IN BETWEEN

by Frank Raes

with
Sir John Houghton
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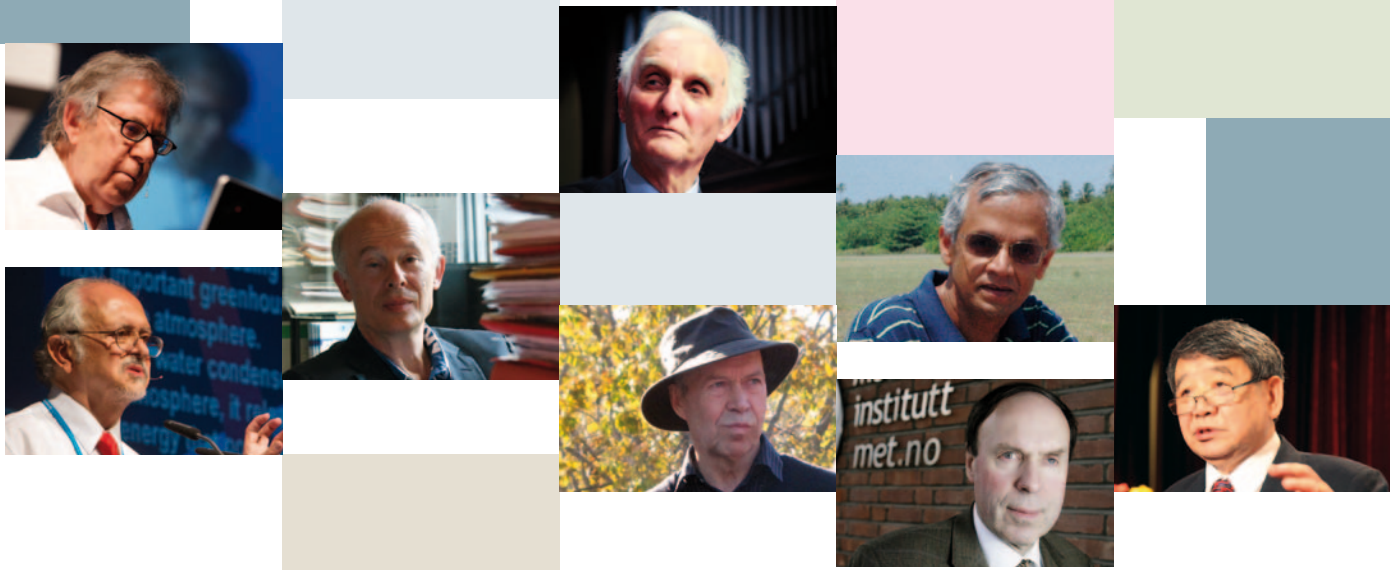

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Prologue

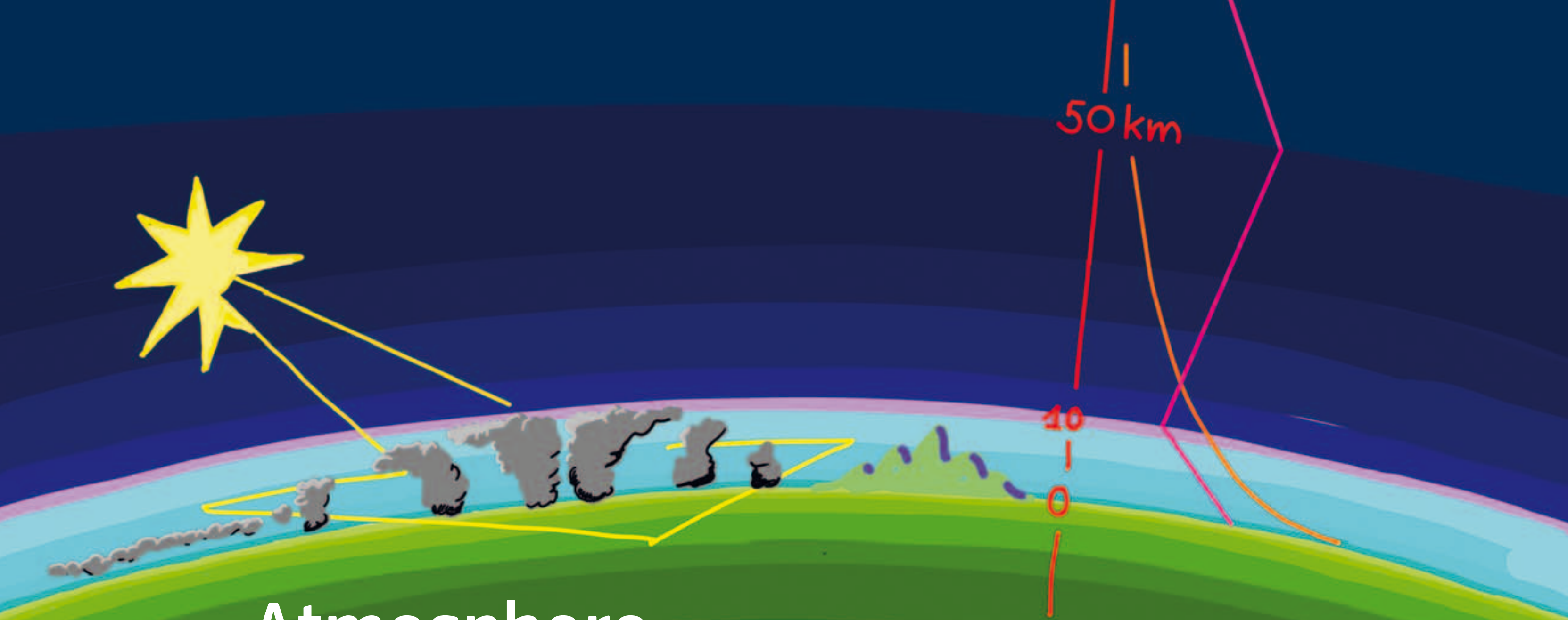
THIS is a collection of conversations with some of the 'fathers' of air pollution and climate change science. Through careful observation and a mix of intuition and critical thought, they have uncovered some of the workings of our planet: its atmosphere and climate. They were generally driven by curiosity and gradually by a concern that mankind might indeed have a devastating impact on Earth and the life it hosts. Their findings were so important that they could not stay in the ivory tower of scientific research. They personally reached out to decision makers and the public at large because in the early days, the seventies, there were no Greenpeaces or World Wildlife Foundations that could help them with this. Many of the phenomena that were observed early on – the loss of ozone in the stratosphere, climate change and air pollution – are now matters of concern throughout the world.

Society is diverse, so the interaction between science and society is also diverse. The conversations in this book shed light on the many ways that science and society can meet. Science has an impact on the way we live not only through the technological innovation to which it leads, but also through the elaboration of new understandings and concepts that enter our thinking. Our connectedness with nature, re-discovered by making the various connections explicit, is one of the understandings that emerged from the scientific research talked about in this book.

The conversations took place between 2009 and 2011. These were tough times for both climate change science and policy. The Intergovernmental Panel on Climate Change, which had synthesised what was known and what was still to be explained about climate change, came under attack in 2009. The climate negotiations in Copenhagen, later that year, that were supposed to have resulted in an international agreement on how to address climate change, failed. These events illustrated how science is but one voice in the broader discussion of how to tackle a collective problem, where a lot is at stake for many different people. But in the aftermath of these events it also became clear how scientific research can provide factual evidence that helps to steer society onto a sustainable path.

If there is one single thing that is common to all those who took part in these conversations, it is a belief in the scientific method – that critical way of looking at the world without ever being complacent. A believe that, using the words of Bruno Latour with a somewhat different emphasis: matters of fact are still important when dealing with matters of concern.

I thank my illustrious colleagues John, Paul, Anton, John, Jim, Ram, Mario and C.S. for the time they have taken to talk to me. They have been an inspiration, an inspiration I want to share with others.



The **Atmosphere** formed as a result of degassing and volcanic activity during the steamy early life of the Earth. Initially the molecules present in the Earth's atmosphere, e.g. carbon dioxide (CO_2), water vapour and nitrogen, were the same as those found on Venus or Mars, but the appearance of life on Earth drastically changed that. Plants turned CO_2 into oxygen through photosynthesis, with the result that today 21% of the atmosphere consists of oxygen (O_2) molecules and only about 0.03% of CO_2. The levels of oxygen and carbon dioxide have been through regular cycles for at least the past million years, due to a balanced interplay between photosynthesis and respiration (which turns O_2 back into CO_2). Fossil fuel combustion during the past couple of centuries has been disrupting this equilibrium, leading to an increase of CO_2 molecules in the atmosphere.

The number of molecules per unit of volume (which is felt as pressure) decreases with altitude. Half of the molecules of the atmosphere are found within 5 km of the Earth's surface. The atmosphere is extremely thin compared to the 6 370 km of the Earth's radius.

At the surface of the Earth, the air is heated from below to a temperature of about 14°C on average and then cools to about -50°C at an altitude of 10-15 km. That makes this layer of the atmosphere unstable, with warm air rising from below. This lower part of the atmosphere is called the troposphere (τροπειν: Greek for circulate or mix). It is the layer in which we live, where we experience the weather and where most of the air pollutants generated by human activities accumulate.

Between 10-15 km and about 50 km above the Earth's atmosphere, the air heats up again because ozone molecules absorb incoming ultraviolet (UV) solar radiation. That layer is stable, with the higher warm air remaining at the upper level. It is called the stratosphere (στρατα: Greek for layer). The presence of ozone in the stratosphere is of utmost importance as it absorbs the UV solar radiation that is lethal to life on the Earth's surface.

Climate is averaged weather. While on a particular day the weather can be better in London than in Rome, when averaged over several years Rome's climate is certainly sunnier and warmer than that of London.

Climate is the result of how energy from the sun is absorbed by the Earth, distributed by the motion of the atmosphere and oceans, and eventually re-emitted back into space. About 70% of the incoming sunlight is absorbed by the atmosphere and the dark parts of the Earth's surface. The remainder is reflected back into space by clouds, by the molecules and aerosol particles in the atmosphere and by the bright parts of the Earth's surface. The absorbed light is transformed into heat and re-emitted as infrared radiation. That radiation is partly absorbed by molecules like water vapour and carbon dioxide (CO_2), thereby trapping heat and leading to warming. This is called the greenhouse effect.

As long as CO_2 is constant in the atmosphere, the amount of energy coming in is equal to the amount going out; no net energy is added to the Earth-Atmosphere system and its temperature remains constant. Over the past million years the global mean temperature cycled regularly between glacial and interglacial periods, with global mean temperatures around 10°C and 14°C respectively, in resonance with the CO_2 concentration in the atmosphere that cycled between 0.020% and 0.027 % respectively. This shows a significant sensitivity of global mean temperature to changes in the composition of the atmosphere.

At present, the Earth is in an interglacial period, the Holocene. Due to fossil fuel combustion and deforestation, the concentration of CO_2 in the atmosphere is rising and approaching 0.04 %. This has led to an enhanced greenhouse effect and extra global warming of 0.8°C. With ongoing emissions of CO_2 and other gases and particles, there is a real possibility of the planet warming by another 2 degrees before the end of this century.

Sir John Houghton

The conversations with Sir John took place on two occasions: in the euphoric aftermath of being awarded the Nobel Peace Prize for the IPCC, the Intergovernmental Panel on Climate Change, and two years later, after the IPCC had again come under vicious attack.
In early 2009 some hundred climate scientists met in Hawaii to discuss the possible content of the next assessment report of the IPCC. There was a slight sense of euphoria at that meeting. In the previous four reports, written over the previous 20 years, IPCC scientists had painstakingly put together evidence that showed that mankind is changing the Earth's climate, and that had led to them winning the Nobel Peace Prize in 2007. Some wondered how the IPCC could proceed from there, if there would be enough material to produce a new and useful report, if the focus would not have to shift from getting a better understanding of the climate system to finding solutions to tackling the problem. But the atmosphere at the 2009 meeting was one of bold confidence: it was believed that, in the years ahead, inspiration and creativity would again do the job. New data would become available, looked at from all possible angles, critically discussed, and tested against what was already there. This would eventually lead to new insights. The feeling was that another Nobel Prize was within reach. After all, there were still the prizes for Physics and for Chemistry to win.
The IPCC is generally seen as the brainchild of Sir John Houghton and a few others. It was the first ever attempt to present scientific evidence about a large societal problem to governments, and have it scrutinised by them. After all, governments would eventually have to do something about the problem. It was the first time scientists would have to explain what they knew and what they didn't know, and expose themselves to the criticism of people who were not their peers. It was far from obvious that this could work at all.
In the course of 2009, the IPCC became the target of attacks by lobbying groups that had an interest in denying its results. The fiercest attack was strategically staged right before the climate talks in Copenhagen, in the autumn of 2009. At the opening session of that summit a delegate immediately made reference to the allegations of fraud, once more shedding doubt on climate science. Some time afterwards, I asked Sir John for his views on these attacks.

Sir John Houghton (born 1931) was Chief Executive of the UK Meteorological Office from 1983 until 1991, and founder of its Hadley Centre for Climate Research.

He was co-chairman of the Intergovernmental Panel on Climate Change (IPCC), which received, together with Al Gore, the Nobel Peace Prize in 2007.

AT the Hawaii meeting, there was an up-beat atmosphere among the IPCC scientists. Later that year the IPCC was subjected to harsh and unjustified criticism. This was something you had to go through before. How did you experience it this time around?

In fact, a massive misinformation campaign against the science of human-induced climate change already began after the Climate Convention was signed by the world's nations at the Earth Summit in 1992. Those behind the campaign and their style and tactics are well described by the historians Oreskes and Conway*. They are those with vested interests who are determined and well funded to oppose scientific findings and confuse the public. They are assisted by the media which tends to favour sensationalism rather than truth.

*Naomi Oreskes and Erik M, Conway, *Merchants of doubt*, Bloomsbury USA, 2010.

The IPCC has come particularly under attack. In the fall of 2009, hacked emails of scientists at the University of East Anglia generated big media headlines about IPCC fraud. There were also isolated errors in a report of 1 000 pages. Regrettable of course, but they did not affect any of the IPCC's conclusions in any way. Three official investigations into these emails, one chaired by an ex-chairman of Shell, all cleared the scientists of any fraud or even of scientific impropriety – results that the media reported, if at all, on their back pages! I personally have had to fight against a widely publicised fabricated quotation attributed to me that implied exaggeration on my part and on the part of the IPCC.

The misinformation campaign continues but I believe that truth is stronger than fiction and that honest, evidence-based science will win through, hopefully sooner rather than later.

I also remember your optimism regarding President Obama. You saw his nomination as a sign that things were changing.

I believe that President Obama has been doing what he can, but his hands are tied because of the attitudes of both Houses of Congress. It must be very frustrating for him, as it is for the rest of the world. The refusal of the United States to take seriously the need to massively reduce its very large carbon emissions is the biggest blockage standing in the way of progress towards finding a solution to this very urgent problem. I hope the necessary action will be taken by the US before it is too late.

Back to Honolulu, early 2009, when I first talked to Sir John. We dropped out of the ongoing meeting and sat down in a quiet corridor of the conference centre. After our conversation we returned to the meeting room and found our colleagues waiting for a plenary lecture that was *about to begin. There was complete silence, and everything was very still. It felt like entering into a vacuum or a sea of invisible ideas produced by the meeting of all those minds. It was an atmosphere not unlike the one in which the talk with Sir John had ended, when it had taken an unexpected flight from the material into the spiritual.*

Sir John, you can look back on a life filled with initiative and achievement. Could it have turned out differently?

I think I always wanted to become a scientist. As a child I was always fiddling around with mechanical things. The countryside and nature also fascinated me. Long trips on my bicycle come to mind; the enormity and beauty of nature inspired me a lot.

Were you inspired by anyone in particular?

I had very good teachers. For my last two years at high school I followed the 'maths and physics' course with only one other boy in the class. The physics teacher was very occupied with his other teaching duties and allowed us largely to learn on our own with the aid of textbooks and old examination questions. Every few days we would go to him with the problems we could not solve. He was wonderful. He would also let us use the laboratory at the back of the classroom, a brand new laboratory where we would play and experiment for hours with the demonstration devices and other pieces of equipment. It was a wonderful introduction to teaching ourselves.
And then I went to Oxford at the age of sixteen – very young for those days when most students were well into their twenties, having served in the war. I was a kid to them, but I soon became their friend. I learned a lot from them and eventually became top of the class!

Why did you start atmospheric research?

I became attracted by the work of Gordon Dobson and Alan Brewer in the physics department. They where instrument builders, and applied their skills to measure chemical tracers in the atmosphere in order to follow atmospheric circulation. The possibility to send instruments on balloons and aircraft fascinated me. I was attracted to working in a small group rather than joining a large team such as those working on nuclear physics research.
I became Alan Brewer's first research student. The aim of our work was to advance knowledge. We were interested in the stratosphere simply because it was there and there was so little known about it. We equipped and used an old military aircraft that could fly into the stratosphere up

Our work was advancement of knowledge. We were interested in the stratosphere simply because it was there and there was so little known about it.

A few of us immediately saw the potential of using something like Sputnik for making atmospheric measurements.

to an altitude of 40 000 feet.

From Oxford I went to work in a government laboratory at the Royal Aircraft Establishment, Farnborough, where I continued to build instrumentation for aircraft flying into the stratosphere, this time to measure how much of the infrared radiation from the sun reached different atmospheric levels.

And then, in 1957, the Russians launched the first Sputnik. I was doing my infrared measurements of the atmosphere from a cottage belonging to the Farnborough research laboratory, while that thing was hanging there up in space. A few of us immediately saw the potential of using something like Sputnik for making atmospheric measurements. Going into space could provide coverage over the whole globe twice a day. What a wonderful dream for someone like me who was dedicated to trying to measure and understand the workings of atmospheric circulation. We immediately started to think about what instruments we could possibly put up there.

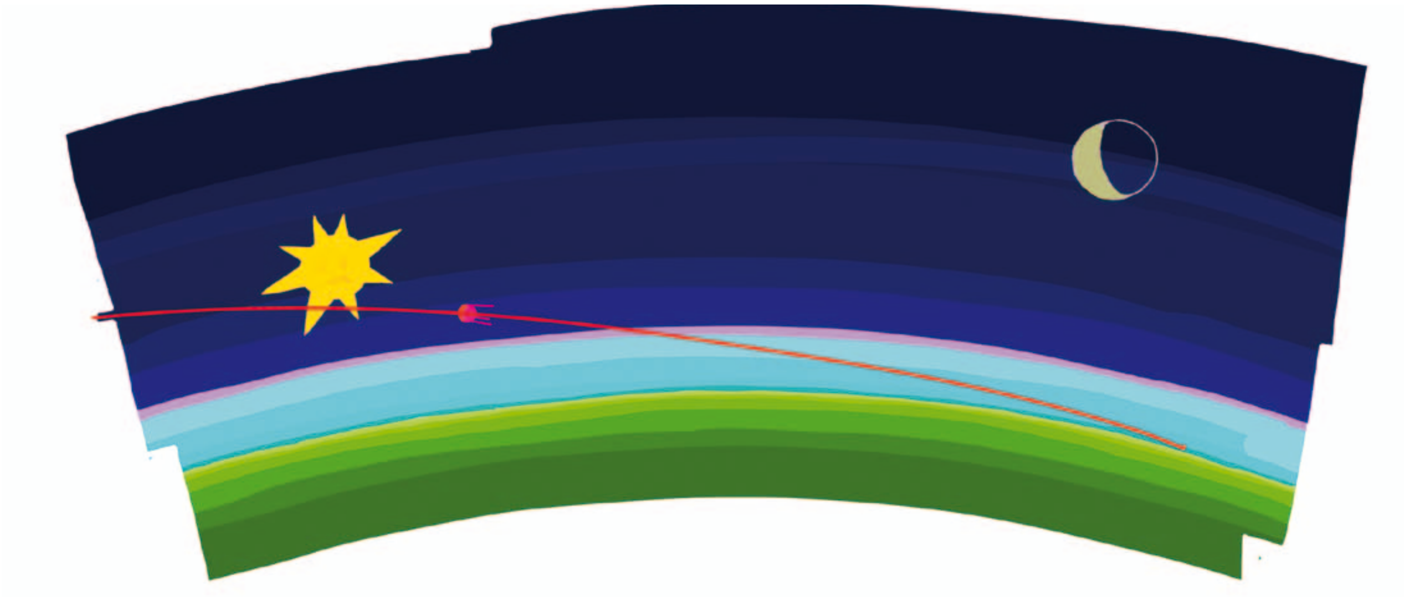
In 1958, the Americans responded to Sputnik with Explorer 1. NASA was founded in the same year. Explorer 1 was an immediate scientific success as it led to the discovery of the Van Allen radiation belts around the Earth*. In that same year Gordon Dobson retired from Oxford leaving a vacancy which enabled me to return to Oxford. I began to work there with Desmond Smith, who had been at Farnborough with me. We continued to pursue the dream of putting instruments into space. By the early 1960s we were able to start to working with NASA on their NIMBUS satellite programme.

*see p. 103.

Satellite measurements are common now. What were the difficulties in the beginning?

To be fitted on a satellite a device could only weigh a couple of kilos and consume very few watts. It was quite tricky engineering. We also had to work with the space industry following strict deadlines, otherwise the satellite would go without us. Our first proposal to NASA was turned down. We were told that we didn't understand nearly enough about satellites, and we were asked to develop our skills by carrying out test flights using balloons. Well, that's what we did, and our first satellite instrument was launched on NIMBUS 4 in April 1970. In fact we mounted instruments on four successive NIMBUS missions, all devoted to observing infrared radiation emitted by the atmosphere from which the atmospheric temperature structure below the satellite could be inferred. The instrumentation on the last of the series, NIMBUS 7, also measured atmospheric composition.

The Earth's first artificial satellite, Sputnik, was launched in 1957.



Engaging in transatlantic collaborations was not routine either.

We had to go to the US three or four times a year, because NASA integrated all of the experiments. My first trip to the US was by ship! The Americans loved to come to Oxford, they loved the ambience, the old colleges, the entertainment we laid on for them (laughs). It was a wonderful partnership. They were very generous to us; they gave us all sorts of things we couldn't get in the UK. They would bring over instrument components in their pockets. In those days much of our communication was made by sending letters. But thinking about it, I can't say we worked any less efficiently than today!

You studied atmospheres far away from where people live, the stratosphere, the mesosphere even the atmosphere of Venus. In the meantime, air pollution was very bad and killed people.

I have experienced the London smog! I knew how bad it was, and I did pay attention to the problem although I did not actively study it. In the UK, policy was in any case being enacted during the fifties to avoid smog events like the one in 1952 that led to the deaths of several

I have experienced the London smog of the fifties, without playing an active role in studying it. In any case, policy was being enacted to avoid smog events.

thousands in London. My research work was simply different; you could say it was pure curiosity-driven research. But our stratospheric work rapidly became as relevant as the London smog.

London, Battersea power station.

Around 1970 Paul Crutzen came to Oxford on a scholarship. He was very smart and had a good nose for selecting interesting research topics. He studied ozone chemistry, the processes of its formation and destruction. The simple explanations didn't work and Paul was thinking about more complicated chemistry. It just so happened that an American scientist, Dave McCray, with whom I was corresponding, had sent me the results of balloon measurements that showed the presence of nitric acid in the stratosphere. I showed Paul the spectra, simply saying that he might want to have a look. He did and this led to the paper* linking stratospheric ozone destruction to nitrogen oxides that eventually won him the Nobel Prize. But his discovery also

*see p. 29.

made many of us aware of the potential impact humans could have on something so essential to protecting life as the stratospheric ozone layer. Soon after, that research proved important in evaluating the possible problems associated with stratospheric aircraft such as the Concorde. It was then that Paul began to get involved with the media and policy makers.

The science I have done has been partly driven by my feeling of responsibility towards society. For me, the environment was given to us as a garden to take care of, not to exploit and destroy. In the seventies I got involved in discussing the effect of a nuclear war and whether it could affect the climate on Earth. I thought that scientists were jumping on a bandwagon, and that they overstated things. That the climate could go berserk was for me a trivial reason to avoid a nuclear war. The real reason is of course because millions of people could die and havoc would be created. And when scientists came to conclude that there would not be a large effect on climate, some seemed to be saying that perhaps we could have a nuclear war after all! This was completely nutty.

Instead of nuclear weapons, climate change became the threat; a 'weapon of mass destruction' as you would later call it. How did the climate change issue emerge?

The advent of observations of the Earth's atmosphere from space led to the discovery of all sorts of new phenomena within the global atmosphere that challenged our understanding. I gave my first climate talk in 1967 to the British Association for the Advancement of Science. I mentioned how the increase in carbon dioxide could potentially have a significant effect on climate, but nobody had much of an idea as to what the effects might be, aside from an increase in temperature. We also didn't realise that a change in climate could become really dangerous for humans and ecosystems. The increase in carbon dioxide concentration was still low at that time, but we took note of it.

Climate research rapidly became a global endeavour. It was coordinated first by the Global Atmospheric Research Programme led by the World Meteorological Organization, among others, and then by the World Climate Research Programme, created in 1979, of which I became the second chairman.

Were you moving from science into management?

I didn't particularly look to become a manager. I had been in Oxford for 30 years. After the NIMBUS programme, NASA's Space Shuttle Programme had taken over and atmospheric

Reshaping the Appleton lab was really a matter of generating new ideas. I didn't have to force anybody to stop doing what he was doing.

research in NASA was getting less priority and progressing more slowly. Furthermore, if you are in the same position for a long time, things first climb to a peak and then tend to decline. So I eventually decided it was the right moment to move on. And, in fact, running and reshaping the Appleton lab was a fascinating job. It was really a matter of generating new ideas. I didn't have to force anybody to stop doing what he was doing; they were happy to join new and exciting programmes and address challenges. It was hard work though.

Then, in 1983, I joined the UK Met Office as Director. This was a wonderful opportunity to further both observations and modelling in the pursuit of a better understanding of both weather and climate.

The Met Office belonged to the Ministry of Defence, and that is a tough place to go into as you can imagine. I had a big battle on my hands right at the start. Margaret Thatcher had come to power in 1979. She was keen on research, because she was a scientist herself. But she was also keen on defence and had raised the defence budget. By 1983, when I entered the Met Office, there was an economic crisis, and the honeymoon period was over. On day one I was asked to fire 100 people. I just said, 'Why do you tell me now? You should have told me before! No way can I take that action at the start!' I fought that battle and fortunately I won. That was very important. Surrounded as I was by people from the military, I somehow felt that if I could win battle number one, battle number two might be more easy.

In November 1988 the IPCC held its first meeting. You, Bert Bolin and only a few others, are called the fathers of the IPCC. Do you agree with that?

You are right especially to mention Bert Bolin. Jim Bruce of Canada was also crucial to setting it up. But as you can imagine there were many other people involved too. My task was particularly to get the Scientific Working Group off to a good start, firstly by setting up a small but effective Technical Support Unit at the Met Office to manage the process, and secondly by creating a process that involved as many scientists as possible with appropriate worldwide representation. It is, after all, a global issue and global ownership of IPCC work was essential if our assessments were to be generally accepted.

That idea of involving non-scientists in a scientific assessment didn't go down very easily, did it?

What we wanted to do was indeed really new; I had to do a lot of persuading. You see, there were a lot of scientists around who would say, 'What are we getting ourselves into? We know far too little to engage with policy makers and advise them!' And I would respond, 'Well I am Director of a meteorological office and every day I have to send somebody in front of the television camera to talk about tomorrow's weather. Can you imagine him turning around and saying, "I can't do this, because the weather is too uncertain?"' (laughs). The point I wanted to make was that people came to accept that forecasting the weather was an uncertain business, and they needed to do the same with climate change. We had to distinguish carefully between what we knew with reasonable certainty and all the things about which we were very uncertain. So there were many voices out there, for and against, and I felt as if I were the conductor of an orchestra and I enjoyed that. As the General-Director of the Met Office I quickly learned that delegation was key. I told my directors to direct rather than spend their time in writing notes and sending them around to one another in brown envelopes (laughs). Working with the space industry also taught me how to do things on time; a mission was planned to be launched on a certain day, and if you were not ready you were not on it! Bert Bolin, in fact, had the same background: he had been managing scientific research in the European Space Agency. He was a real leader, soft spoken in his way, but a leader. He knew what needed to be done and he did it. I believe we applied those skills as chairmen of the IPCC. We gave people responsibility. They liked that, and the response was enormous.

It is often said that scientists and policy makers do not speak the same language. How would you describe this problem?

We speak a different language, but it doesn't mean we cannot communicate! It was basically up to us scientists to write down our results in a way that could be understood by policy makers. I had become used to that because I had been doing other jobs, which included talking to ministers. But for most scientists this was something new; they were not used to being told that something was unclear. They were also afraid that dealing with policy makers would take a lot of time and complicate their lives.

But it was absolutely essential that reports were written in a language that could be understood by government people from across the world. The IPCC reports were written by scientists who were experts in their field. The summary of these reports was meant for policy makers. To get the language right we would work with former scientists who had moved into government. But then came the official plenary meeting, with about 300 people, delegates from hundreds of governments and simultaneous translation into six languages. The summary was reviewed line

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Policymakers Summary

We are certain of the following:

- there is a natural greenhouse effect which already keeps the Earth warmer than it would otherwise be
- emissions resulting from human activities are substantially increasing the atmospheric concen trations of the greenhouse gases carbon dioxide, methane, chlorofluorocarbons (CFCs) and nitrous oxide These increases will enhance the greenhouse effect, resulting on average in an additional warming of the Earth's surface The main greenhouse gas, water vapour, will increase in response to global warming and further enhance it

First lines of the Summary for Policymakers of the First Assessment Report of the Intergovernmental Panel on Climate Change, 1990.

Cambridge University Press, 1990.

by line. It was a fascinating process. Delegates would ask, 'What do you mean by this sentence?' And scientists were forced to think of better ways to express themselves. The draft summary would change quite a bit, and it eventually read much better and became stronger. Even the scientists came to acknowledge that. But more importantly, the summary *for* policy makers became the summary *of* the policy makers: it was their document – they owned it!

It should be clear that we never did allow a political interpretation of the science. Some countries would make plenty of comments with the sole intention of weakening the scientific statements. At one plenary meeting the delegate from an oil producing country complained, 'I have made all sorts of comments over the week, and I didn't have a single one agreed. Can I please have this one included?' And I said, 'Absolutely not! This is a scientific meeting. If you want to change something you need to make a scientific argument, not a diplomatic one!'

You called the IPCC the greatest experience in your life. Were there any difficult moments?

There were many difficult moments. The most difficult time I had was at the IPCC plenary meeting in Madrid that met to agree the 1995 Report. The biggest question to be addressed was whether observations had yet detected a global increase in atmospheric temperature due to the increase of greenhouse gases that could be distinguished from natural variability. The unanimous and carefully crafted conclusion of the meeting after many hours of debate, was: 'The balance of evidence suggests a discernible human influence on global climate'.

The draft chapter presented to the meeting on the detection issue spelled out the scientific evidence in detail but was inconsistent in its conclusions and needed some reworking. The meeting gave clear instructions to its lead author, Ben Santer, regarding the changes necessary for the chapter to be acceptable. These were duly made. However, three months or so later when the IPCC Report was published, the Global Climate Coalition, a US-based NGO largely supported by big oil and coal interests, which had taken part in the Madrid meeting, identified differences between the draft and final texts of the detection chapter, and accused the IPCC of major dishonesty and fraud. These accusations were also voiced by the Wall Street Journal which ignored the messages Bert Bolin and I sent to them explaining that all the changes had been authorised by the IPCC at its Madrid meeting.

The worst part of the episode was that the Global Climate Coalition waged personal attacks against the chapter's lead author, Ben Santer, in a nasty way that was completely 'below the belt'. I let it be known that if they wanted to attack somebody they should have attacked me, not Ben, as I had been the chair of the meeting that approved the changes. But Ben was an easier target for them and suffered badly as a result. In the end, the scientific community came to his and the IPCC's defence in an article in the Bulletin of the American Meteorological Society. So scientific integrity was saved. Also, not a single government supported the claims made by the Global Climate Coalition. But sadly, some continued going after Ben for a long time afterwards.

A year or two later, I came across the Global Climate Coalition leader who had been behind the attacks. He stood in the middle of a corridor. As I walked towards him, he proffered his hand and I said, 'Don, I don't think I want to shake your hand'. And he, with his deep voice, said, 'Why should you not want to shake my hand?' 'You have been so unprofessional', I said, and he flew into an enormous rage. I could not have said anything that would have hurt his pride more. He prided himself on his professionalism, on doing the job he was paid to do, which was to destroy the IPCC.

That was not your happiest moment …

No … But when Margaret Thatcher invited me to present the first IPCC report to her Cabinet, *that* was a happy moment. I went to Downing Street and had twenty minutes to explain the findings to her – it was the first time that an overhead projector had been used in the Cabinet Room! I was told not to worry if she interrupted me early on. But she remained silent, and I became known as the man who kept Mrs Thatcher quiet for twenty minutes! After my presentation she said 'But it is the growth of the world population that is the problem, isn't it?', showing her concern for the much wider problem of how to keep living on our planet in a sustainable manner. And that is indeed the ultimate challenge we face.

That meeting with her Cabinet was on a Monday in May 1990. On the Friday of that week she was to open the Hadley Centre for Climate Research at the Met Office, the creation of which she had supported strongly. It has since become arguably the leading climate research centre in the world. I was amazed at the time she devoted to preparing the speech she would deliver at the Centre's opening – I was summoned with two or three others to Downing Street two more evenings that week to personally help her to prepare it. My wife complained that I spent more time with Mrs Thatcher that week than with her!

You can say what you like about Margaret Thatcher, but she was actually very supportive. She helped to set up the Hadley Centre, persuading her government to raise the money for it. She understood it was important.

The IPCC received the Nobel Peace Prize, not the one for Physics or Chemistry. What are your thoughts on that?

The more you think about climate change, the more you realise it is a very big issue for the world. Even if we manage to stabilise the temperature to within 2 degrees above pre-industrial levels, the world will be a different place. Sea level will have risen significantly, water availability will have changed in many places, the risk of floods and droughts will have increased substantially and many ecosystems will have disappeared. Hundreds of millions of people who can't make a living where they are today will be displaced. It is beginning to happen already, for instance with people from small islands in the Pacific. Major disruptions will be widespread. We, the rich countries, will have to help the poor, because we have become rich by burning fossil fuels and have been the main cause of the problems.

We can do what is necessary; what we need is the will to do it. For instance, to change the way in which we get our energy. We have the knowledge and the technology, and the money involved is not exorbitant! A new, modern, largely renewable energy production system would be more efficient, would not need all that coal and oil and in the long term would pay for itself. The oil and coal lobby have mounted an enormous campaign of misinformation, and they still have a lot of power. Many highly respected people, even former government ministers, lobby or write books, ignoring the evidence and rubbishing the science of climate change and the work of the IPCC. So there are still obstacles, but there are lots of good things going on as well. We have some good leaders – we need more.

Climate change is of course only one of the issues. We just experienced another one, the financial crisis, showing how greed has taken over at a scale that was previously unimaginable. Money has been steadily flowing from the poor to the rich. And it is the poor in the world who will be disproportionately disadvantaged by the impacts of human-induced climate change. If we want to help the poor in tackling climate change and help them with other related issues such as food production, clean water, etc., that flow of money will have to be reversed. Nobody's life in the world is safe, unless the lives of the two billion poor in the world are made safe as well.

It was amazing and wonderful that the IPCC, a scientific body, could be awarded a prize for *peace*. To bring about more peace in the future is an enormous challenge – so much more has to be done, and science can and must play its part.

You keep persevering, despite the difficulties at political level, despite your respectable age.

Some of my colleagues still say, 'You are crazy, wasting your time, politicians will never agree!' (laughs). But I am optimistic. I have seen what the global scientific community can do through the IPCC – and what therefore should be possible in other international fora. When people with different backgrounds and cultures come together with the right attitudes, realising that something big is at stake, you've already come a long way. That's another reason why the IPCC was awarded the Nobel Peace Prize. The second reason for my optimism is that I see that the technology is there and that industry is keen to invest in it if a proper political framework is provided. And my third reason is that I believe God gives us the inspiration to be good stewards of his creation, and that's where Christian faith comes in.

When people with different backgrounds and cultures come together with the right attitudes, realising that something big is at stake, you've already come a long way. That's another reason why the IPCC was awarded the Nobel Peace Prize.

How do you reconcile a rigorous scientific attitude with your Christian faith?

I grew up in a very Christian family. My father never accepted the concept of evolution, because he thought it was against the Bible. But I was not happy with that apparent conflict, and by the time I went to Oxford I recognised both science and Christian faith as searching for truths that could be put alongside each other in exciting ways. Science asks 'how' questions and has been fantastically successful in its voyage of discovery, discovering the laws of nature, for instance of gravity, of motion, of thermodynamics and the strange laws of quantum mechanics. Science does not create these laws, it discovers them. Where do they come from? That is not a 'how' question but more of a 'why' question that is outside the realm of science to answer. Albert Einstein used to talk about the 'intelligence behind the universe', and he was happy to call that intelligence God. Many other scientists also believe in an intelligent Creator. With God as Creator, our science becomes God's science, with science providing perspective about God too.

Does the Creator God have personal qualities?

For me, that is probably the most important question I can ask; I feel bound to expect the answer to be a resounding 'Yes'. When God created us with the personalities and the abilities we have, he did it with a purpose; he did it because he wants us to know Him. And in my very small way, I believe God has helped me to do just that and to bring me closer to Him. I have felt the strength of that relationship not just in my faith but also in my science. For instance, IPCC science has been exposing the fact that we humans are not caring for the Earth as we should, and has been suggesting which actions we need to take. That responsibility is felt by many scientists whether they believe in God or not. But for me, the belief that I am not tackling this problem on my own but with God as my helper, and the One to whom I am responsible for carrying it out with absolute integrity, honesty and trust, values essential to the scientific process, has been a tremendous strength.

Of course I cannot talk loudly about my faith at IPCC meetings, because I might immediately be accused of being biased by a personal belief. But there is really no conflict between tackling climate change in an honest and rational way, and believing in a God who shares our concerns.

Environmentalism is often called a religion, in a denigrating sense.

Yes, people would say, 'Oh, it's a religion…' – a put-down implying that they think caring for the environment has nothing to do with real life. I've been arguing that religious faith can co-exist with science in our care for the environment. Both science and religion rely on strong ethical values. Bringing science into it helps to keep our religious feet on the ground and bringing God into it provides inspiration and motivation for action!

I have helped to set up the John Ray Initiative, an organisation in the UK that brings people together to discuss the relationship between environment, science and religion, but in a rational way. John Ray was the father of natural history in Britain; he would regularly meet with Isaac Newton, Robert Boyle, the architect Christopher Wren and other giants of those days. They would exchange their latest experiments as they investigated the workings of nature. Many of them were devoted Christians and realised that in their science they were investigating and learning about God's creation.

Religion has a great deal to do with real life. For Christians, the fact that God sent his own Son into the world demonstrates His profound interest in the material world. It is through that world that he wants us to discover Him and to know Him.

(February, 2009, Honolulu)

Paul Crutzen

All atmospheric chemists must have some memory or another of Paul Crutzen. He has had professional careers both in Europe and the United States, is widely travelled, and went to see people as much as people came to see him. He once told the story of wanting to visit a colleague. The colleague said he wouldn't be around, but Paul went anyway and found him in his office. He could not accept such behaviour and openly called it cowardice. Being a Dutchman he is straightforward. He is very outspoken and is never shy about letting it be known whether he likes somebody's research or not. On the other hand, he makes no distinction between students, professors, young and old, and that makes everyone feel at ease.

*I first met Paul 25 years ago, when I worked on atmospheric aerosols, a then obscure area of atmospheric chemistry. With my doctoral student Rita, I went to visit his institute. We were two unknown young researchers but we were still invited into his office. When I said I would soon join the Joint Research Centre, he said, 'It is time that Centre got some people with ideas!' It was a qualified encouragement, but I felt encouraged anyway. That was 25 years ago, and things have since changed a lot at our Centre.**

Paul was very demanding, but also very generous with his ideas and insights; at project meetings he would often think aloud, so you could follow his thought process or understand why he was asking certain questions. It was often truly fascinating. The difficulty came when you wanted to reproduce his line of thought back at home, on your own.

A beautiful memory: I met him at a meeting a couple of weeks after he was awarded the Nobel Prize for Chemistry. We reached to each other from a distance and firmly shook hands. 'I got a kick out of that!' I said, and in his eyes and smile I recognised the pleasure and pride of a little boy who had climbed a tree and, despite the warnings, had made it to the top.

The conversation below began during a conference in Wageningen and continued by mail and over the phone. When I met him later during the climate talks in Copenhagen, he immediately asked how my book was going. He asked it in the same way he would have asked a student about the task that he, Paul, had given him after his last trip – Paul's ideas usually occurred during transatlantic flights.

Paul Crutzen (born 1933) received the Nobel Prize for Chemistry in 1995, together with Mario Molina and Sherwood Rowland.

He was Director of the Max Planck Institute for Chemistry in Mainz from 1980 until 2000.

*see p. 139.

IN your Nobel Prize acceptance lecture of 1995, you gave a beautiful account of your youth and the transition from a boy who didn't like chemistry, to an engineer and bridge builder, and finally to the scientist you always wanted to be. The period spent as a bridge builder seems a bit at odds with the others. How did that come about?

It interested me. As a boy, apart from reading about world explorations, astronomy, Jules Verne's and Karl May's books, I also read extensively about bridge, dyke and tunnel constructions, of which we have many in Holland. But the main reason was that, due to a strong fever, my grades in the final exam in high school were not good enough to qualify for a university grant. Those grants were very hard to obtain at that time; it was only six years after the end of the Second World War and a few years after the end of the colonial war in Indonesia, which had been a large drain on Dutch resources. I did not want to be a further financial burden on my parents. My father, a waiter, was often unemployed, and my mother worked in a hospital kitchen. So I chose to study civil engineering and eventually got a job as a construction engineer at the bridge-building bureau of the City of Amsterdam. However, I rapidly became disillusioned with that job. In the meantime I married a Finnish girl I had met during one of my hitchhiking trips through Europe. We moved to Sweden where I joined a construction company in the City of Gävle. I stayed there for a year, becoming once again dissatisfied.

What was wrong with the construction business?

Nothing really, only that I came to realise that what I needed was an environment of learning, such as one finds in a university setting. I was simply curious and I knew I needed freedom of thought. In 1958 I got a job as a computer programmer at the Department of Meteorology in Stockholm University. I had not the slightest experience of such work, but neither did anybody else; computer programming was a brand new field in the fifties! I profited greatly from the generosity of Bert Bolin, the then Director of the Department, who allowed me to attend courses while doing programming jobs for researchers at the university. This meant that I did most of my studies at home, during evenings and weekends, spending any free time with my wife and two children. At the age of 26 I was finally on the right track. Still, it would take another six years before I could start my own research on the chemistry of stratospheric ozone.

Why ozone in the stratosphere? Weren't there enough problems at the Earth's surface?

I always thought it to be good practice to start with something simple before moving on to

If most atmospheric scientists had continued studying photosmog chemistry in Los Angeles, the destruction of stratospheric ozone might have been discovered too late!

Downtown Los Angeles.

more complex systems. The chemistry of the stratosphere, even though not fully understood in those days, was definitely less complex than the troposphere where you have many more gases to deal with. Furthermore, I was mostly interested in pure science related to natural processes so I picked stratospheric ozone as my PhD subject, without the slightest idea of what lay ahead. With hindsight, if most atmospheric scientists had continued studying photosmog chemistry in Los Angeles, where it had first been a problem, the destruction of stratospheric ozone due to man's activities might have been discovered too late! This shows how basic research can become very relevant.

I should also mention that a lot of the research carried out at the Department of Meteorology was relevant to society. The main topics were atmospheric dynamics, cloud physics, the carbon cycle, and studies of the chemical composition of rainwater. The acid rain problem was largely discovered in Stockholm, through the work of Svante Odén and Erik Eriksson. Several researchers at the Department, including Bert Bolin and my good friend and fellow student Henning Rodhe, became heavily involved in the issue, which drew considerable political interest at the first United Nations Conference on the Environment in Stockholm in 1972. Bert

Bolin went on to set up the IPCC, the Intergovernmental Panel on Climate Change. He was my great mentor and his death in 2007 was one of the saddest moments in my life. It came just a few days before the IPCC, his life's work, was awarded the Nobel Peace Prize.

While you were working on your PhD you spent two years in Oxford (1969-1971). That period was important to the work that led to your Nobel Prize.

Those years in Oxford were possible thanks to a grant from the European Space Research Organization. My first goal was to get acquainted with radiative transfer and the use of instrumentation on satellites to derive the concentration distribution of chemical constituents in the atmosphere. Methods for measuring such distributions were developed by the research group of Professor, now Sir, John Houghton. One day, John showed me a spectrum of nitric acid that had been obtained by researchers at Denver University using balloon-borne instrumentation in the stratosphere. The presence of nitric acid (HNO_3) in the stratosphere clearly showed that nitrogen oxides (NO and NO_2) should also be present, thus supporting my theory that stratospheric ozone is controlled by NO and NO_2. The two years I spent in Oxford were of the greatest importance for my career. In spite of the fact I was a newcomer I was able to make a major contribution to stratospheric chemistry. And although I initially studied natural processes, I soon got the feeling that humans could somehow interfere with them. That feeling was made explicit in a 1972 AMBIO paper*, and the title of my PhD thesis in 1973 eventually read, 'On the Photochemistry of Ozone in the Stratosphere and Troposphere and Pollution of the Stratosphere by High-Flying Aircraft'.

*Crutzen, P.J., 'SST's – A threat to the Earth's ozone shield', *AMBIO*, Vol. 1, 1972, pp. 41-51.

What is the thought process that leads to a major scientific discovery?

That is very hard to describe. One needs a critical mind, interdisciplinary thinking, good intuition, a good mentor and total concentration on the subject. You wake up and go to bed thinking about it. Early morning hours can be especially productive. The fact that I travel a lot must also play a role. I hardly sleep on intercontinental flights, so that gives me plenty of time to think and elaborate on what I might just have heard at a meeting. That's productive too.

The role of nitrogen oxides is remarkable: in the stratosphere they catalyse the destruction of ozone. In the troposphere the opposite is the case. I am happy having contributed to this knowledge.

The influence of nitrogen oxides on the atmospheric ozone content

By P. J. CRUTZEN*
Clarendon Laboratory, Oxford University

(Manuscript received 5 November 1969, communicated by Dr. C. D. Walshaw)

SUMMARY

The probable importance of NO and NO_2 in controlling the ozone concentrations and production rates in the stratosphere is pointed out. Observations on and determinations of nitric acid concentrations in the stratosphere by Murcray, Kyle, Murcray and Williams (1968) and Rhine, Tubbs and Dudley Williams (1969) support the high NO and NO_2 concentrations indicated by Bates and Hays (1967).

Some processes which may lead to production of nitric acid are discussed.

The importance of O (^1S), possibly produced in the ozone photolysis below 2340 Å, on the ozone photochemistry is mentioned.

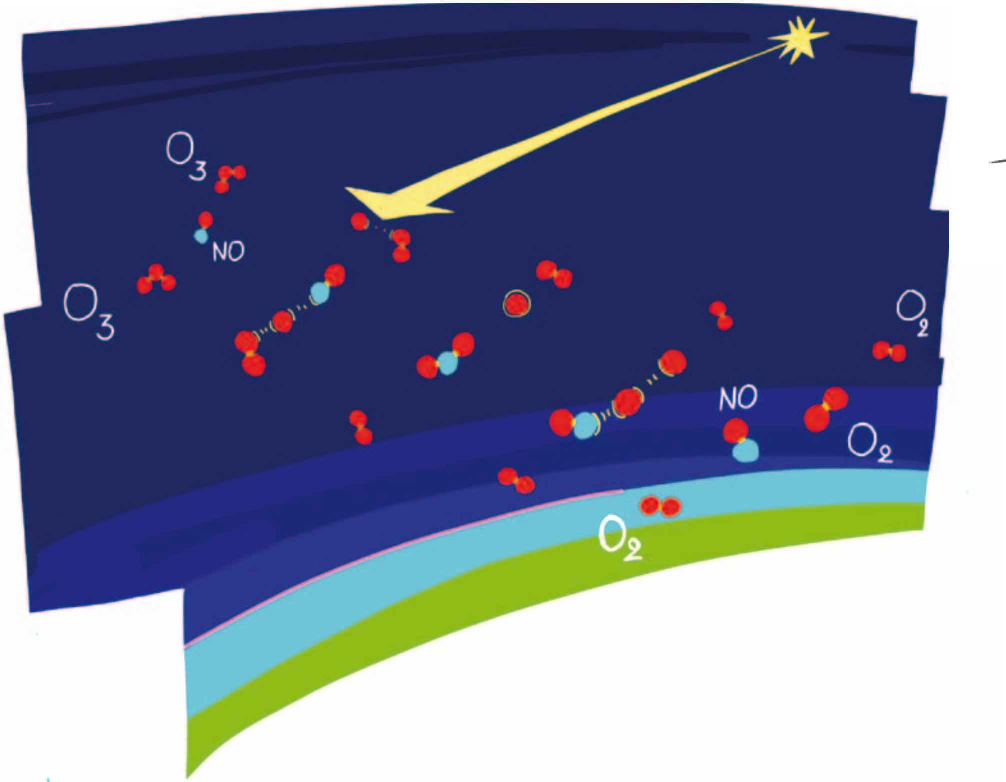
Quarterly Journal of the Royal Meteorological Society, Vol. 96, 1970, pp. 320-325.

What is the role of intuition, of somehow knowing how the system works, without having all the evidence?

Yes, intuition is a powerful force, but I don't think it comes out of nothing. It is important to be able to keep looking at an issue, the environment for instance, in an interdisciplinary way. I think that is what I have been able to do.

The basis for explaining stratospheric ozone is the role of catalytic reaction cycles. Is that an idea you got from another field, biology for instance?

Before I studied stratospheric chemistry, David Bates and Marcel Nicolet had already introduced catalytic reaction cycles in the mesosphere. Their catalysts were H, OH and HO_2 derived from the photolysis of H_2O. They did not see that catalysis by NO and NO_2 destroys ozone in the stratosphere, although Nicolet had listed the relevant reactions in his papers. The catalytic role of NO_x is remarkable: in the stratosphere NO_x catalyses the destruction of ozone. In the troposphere the opposite is the case. I am happy to have contributed to this knowledge.



$$O_3 + NO \rightarrow NO_2 + O_2$$
$$O_3 + h\nu \rightarrow O_2 + O$$
$$NO_2 + O \rightarrow NO + O_2$$
$$\overline{2O_3 + h\nu \rightarrow 3O_2}$$

Ozone destruction!

What was your relationship with Nicolet like? You were a newcomer, he was an established scientist, one of the fathers of (upper) atmospheric chemistry.

Let me answer with the following anecdote. Nicolet claimed that my ozone production scheme in the troposphere could not work because of the presence of clouds. One beautiful day, at a conference, I was heading to an evening reception organised in a local castle. I entered the hall, which was still empty. Nicolet entered at the other side of the hall. I found myself alone with Nicolet in that big hall, lit by the evening sun. I thought of greeting him with the words, ‘Hello Professor, where are the clouds today?’ But I didn’t, out of respect.

When did you first realise that your work on stratospheric ozone was of importance for society?

That was when I read a report from a major conference in the US in which it was claimed that nitrogen oxides, NO and NO_2, could not affect the ozone in the stratosphere. Clearly the expert scientists who participated in the conference had not read my 1970 paper. That same report gave information on NO_x emissions by the planned supersonic aircraft, and I knew immediately that stratospheric ozone was in danger. Harold Johnston from Berkeley came to the same conclusion. Soon John Houghton and I were visited by people from British Aerospace, the manufacturers of Concorde. They were of course very sceptical. The first test flight with this supersonic aircraft flying into the stratosphere had already taken place in 1969. Major research programs had begun in the US, France and Great Britain that confirmed our findings on the role of NO and NO_2. US Congress stopped funding the development of the American supersonic aircraft in 1971, most likely for economic reasons. However, the British-French Concorde operation started anyway with a few commercial transatlantic operations in 1976.

Eventually, stratospheric ozone was partially destroyed not by NO_x but by chlorofluorocarbons (CFCs).

And that was a close call! If the chemical industry had developed compounds based on bromine instead of the CFCs, which are based on chlorine, we would have faced catastrophic loss of ozone. Bromine chemistry destroys stratospheric ozone much faster than chlorine chemistry. Ozone loss would have occurred not only over the Antarctic, but everywhere and during all seasons during the seventies already. That would have been before the atmospheric chemists had developed the necessary knowledge to identify the problem and the appropriate techniques for the necessary critical measurements. Noting that nobody had given any thought to the atmospheric consequences of the release of chlorine and bromine before 1974, I can only conclude that mankind has been extremely lucky!

Do you think genuine scientific questions can result from immediate concerns for our society and our world?

Yes, clearly. Take the case of the CFCs. It had already been hypothesised that chlorine atoms could also enter a catalytic cycle and lead to the destruction of ozone, very much like my NO and NO_2 molecules. Many of us were therefore looking for sources of manmade Cl-atoms.

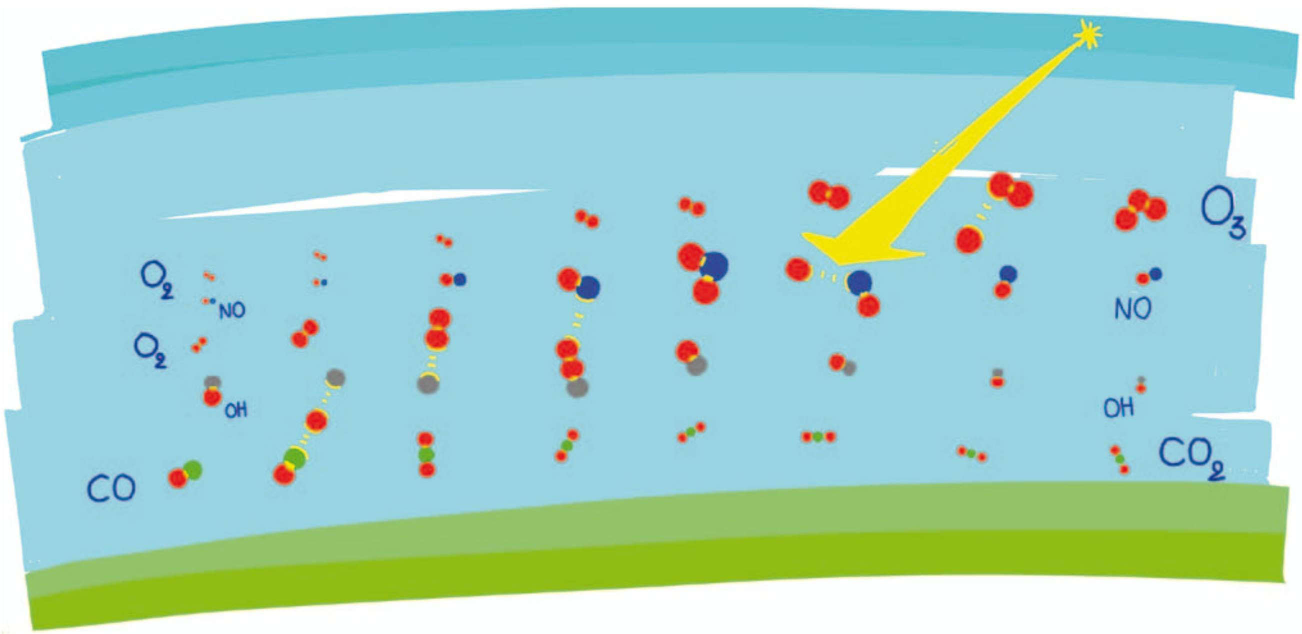
$$CO + OH \rightarrow CO_2 + H$$
$$H + O_2 \rightarrow HO_2$$
$$HO_2 + NO \rightarrow OH + NO_2$$
$$NO_2 + h\nu \rightarrow NO + O$$
$$O + O_2 \rightarrow O_3$$
$$\overline{CO + 2O_2 + h\nu \rightarrow CO_2 + O_3}$$

Ozone production!

Exhaust emissions from space shuttle boosters were one small source. I was looking into DDT and other pesticides that contain Cl-atoms. But it was Mario Molina and Sherry Rowland who, in 1974, convincingly showed that Cl-atoms could be produced in the stratosphere by the photodissociation of CFCs. Later, the appearance of the ozone hole over the Antarctic called attention to the role of chemical reactions on the surface of ice particles in the stratosphere. These are essential in the activation of chlorine. Currently the physical and chemical properties of aerosol particles in the troposphere receive a lot of attention, primarily because of their negative impact on human health, but also because of their implications in climate change. All this led to questions that could only be answered by fundamental research.

Initially you pursued your scientific research very much on your own.

My first research collaborations were with two persons with whom I did not get along. One would not come into the office until midday. The other one had an approach to doing research that I could not agree with.



What was your approach?

You cannot be too complacent, accepting too rapidly what people claim without all the necessary proofs. You need to be critical, but also to keep a positive mind.
But yes, in the beginning I thought I might not get along with people very well. Thus, when I was contacted by Jack Fishman to do a PhD together, I nearly fell off my chair. I hesitated, but after some reflection I agreed and we began collaborating on the role of NO and NO_2 in tropospheric ozone chemistry. That collaboration turned out to be very pleasant and productive. We showed that – contrary to common belief – ozone in the troposphere does not mainly emanate from the stratosphere, but was produced in-situ through the oxidation of CO, CH_4 and other hydrocarbons, with NO and NO_2 serving as catalysts.
Since then I have educated about 30 PhD students, including Susan Solomon, Jos Lelieveld and Frank Dentener. I have been blessed with excellent students.

How did you select your students?

I always looked at their grades at high school, for instance. That turned out to be a good indicator.

Did you learn from your students?

Definitely! They were true collaborators and we learned a lot from one another. For instance: I have always been a theoretician. I would sometimes go to field campaigns but had never turned a knob of an instrument, I was not trained for that. Jack Fishman and Susan Solomon did work with instruments. Together we could prove, based on solid data, how ozone was destroyed in the stratosphere and how it was produced in the troposphere.

You have worked extensively in the US and in Europe. Are the approaches to scientific research different?

There is no US way or European way of doing research; when you deal with basic research questions there is only one way of tackling them. It is certainly not true that Europeans are better at discovering a problem and US scientists better at tackling it. Of course US scientists had larger research budgets. In our area, for instance, that gave them the advantage of being able to use satellites to observe ozone at a global scale.

*Graedel T.E. and P.J. Crutzen, *Atmospheric change: an Earth system perspective*, W.H. Freeman & Co., New York, 1993.

Together with Tom Graedel you wrote the textbook 'Atmospheric Change: An Earth System Perspective'*, which you published nearly 20 years ago. It is still a masterpiece in balancing the breadth and depth of the issue. You also wrote a shorter version of that book, meant for the general public. Was that successful?

Tom was very much behind that publication. We both thought that, because changes in the Earth System have such important consequences, some knowledge of its characteristics and trends should be part of the arsenal of every educated citizen. But Tom certainly knew better than I how to put together an appealing book, including nice graphs and pictures and so on. I don't know about the sales of the short version, but I guess it is difficult to write a bestseller in our field.

Did you always have the freedom of thinking that you had hoped for in the early days?

Freedom in research depends on how you are able to share your own ideas with others.

Since I started carrying out research, I have never come across a situation in which I was told what to do or what not to do. As I mentioned, I was lucky to meet Bert Bolin at Stockholm University who immediately opened up many opportunities. The strategy at NCAR, the National Center for Atmospheric Research in the US, and the Max Planck Institute in Germany was also that of giving full freedom to their researchers. Your freedom in research depends on your surroundings, but also very much on yourself and how you are able to share your own ideas with others.

Winning the Nobel Prize is the greatest recognition of one's work. You also become a public figure. I can imagine that the many obligations take quite a bit of your time and freedom. How did you handle this?

You are right. Especially during the first years after winning the Prize a lot of time was spent in celebrations, scientific commissions and appearances in the media. A lot of people like to get close to a Nobel laureate, and that can sometimes be annoying. But overall it was of course a positive experience, and I still feel humbled by the recognition we received for our work. It was a very happy moment in my life; it came as a total surprise, and I was able to share the occasion with my family. The Prize also gave a boost to atmospheric chemistry research. A lot of funding became available to set up new research groups in the Netherlands and elsewhere.

I remember you saying 'I am ready to stick my neck out again'. It sounded as though you had done this before and that it had caused some trouble.

Pointing out the potential depletion of stratospheric ozone by NO_x emissions from supersonic aircraft was the first time I stuck my neck out. The second time was in the 1970s, when I postulated that the many fires that would be burning in urban and industrial centres as a result of nuclear war would lead to large emissions of black smoke, causing absorption of solar radiation in the atmosphere and creating darkness and rapid cooling of the Earth's surface. This 'nuclear winter' scenario, would make agriculture impossible. Thus, more people would die from the indirect than from the direct consequences of a major nuclear war. More recently I showed that the production of biofuels to offset climate warming caused by the burning of fossil fuels may have the opposite effect when the release of N_2O to the atmosphere, as a result of nitrogen fertiliser use, is taken into account. And finally I initiated an intense debate on the pros and cons of injecting sulphur particles into the stratosphere in order to cool the climate.

Should a scientist wait until all the evidence is available before becoming outspoken about a certain issue, e.g. climate change?

Often, there is no time to wait. But I have seen an interesting development. Earlier, say more than 3 decades ago, scientists were much more conservative in communicating their findings to the press and politicians. There was also a general belief that mankind's power to affect the environment was so much smaller than nature's. This attitude has changed drastically. Now, scientists speak out much earlier and organisations such as the IPCC provide international coordination of their ideas.

What are your views on the slow process of setting up an international climate policy?

I am disappointed. There was great hope that under the Obama administration things would change significantly. Recent developments give a different impression, but I hope I am wrong in my pessimism. Maybe new technology will become more readily available for all nations in the world, and make the preservation of environment and climate feasible. The task is in any case enormous: a 70-80% reduction in CO_2 emissions would be required by the middle of the century in order to stabilise the greenhouse gases in the atmosphere. Instead, they are increasing by 2–3% per year. Given such figures it is hard to be optimistic. This is why I came up with the suggestion of studying the release of sulphate particles into the stratosphere in order to

I very much doubt that sending rockets up to spray sulphur particles into the stratosphere is a viable method to engineer the climate.

cool climate. It is in fact an idea that goes back to a Russian scientist. However, it is a method to be used as a last resort. I very much doubt that sending rockets up to spray sulphur particles into the stratosphere is a viable method to engineer the climate; there are too many potential side effects and too much room for international conflict. It all needs thorough investigation.

Are there any other issues about which you would be willing to stick your neck out?

Well, ten years ago I was at a meeting in which the chairman kept talking about us living in the Holocene, the interglacial epoch that started ten thousand years ago. I kind of spontaneously stood up and reacted that we were living in the Anthropocene! I wanted to point out that we humans had changed the planet to such an extent that we could not possibly compare it with how it was thousands of years ago. That's how my interest in the Anthropocene started, as a result of a spontaneous reaction, but ever since I have been thinking about the issue, gathering the data that would illustrate the large human-induced changes in a range of areas. I found out that others had been talking about the 'anthropozoic era' for instance. Teilhard de Chardin wrote about the 'noösphere' or the 'world of thought'. They all indicated the growing role of humanity in shaping its own future and environment.

I felt the idea was not being developed with enough scientific rigour to find out what it really meant. It means, for instance, that we have indeed moved into a new geological epoch. Consider that such epochs are often characterised by great changes in fauna and flora as can be seen in geological deposits. Well, we are now witnessing a rapid extinction of species and loss of biodiversity, and we are also drastically changing the surface of the planet through agriculture and urbanisation. All this will definitely be visible in the geological record millions of years from now. The International Commission on Stratigraphy, which defines the geological time scale, has taken interest this issue.

concepts

Geology of mankind

Paul J. Crutzen

For the past three centuries, the effects of humans on the global environment have escalated. Because of these anthropogenic emissions of carbon dioxide, global climate may depart significantly from natural behaviour for many millennia to referring to the "anthropozoic era". And in 1926, V. I. Vernadsky acknowledged the increasing impact of mankind: "The direction in which the processes of evolution must proceed, namely towards increasing consciousness and thought, and forms having greater and greater influence on their surroundings." Teilhard de Chardin and

The Anthropocene

The Anthropocene could be said to have started in the late eighteenth century, when analyses of air trapped in polar ice showed the beginning of growing global concentrations of carbon dioxide and methane.

Nature, Vol. 415, 2002, p. 23.

It is not just a matter of a new name, I suppose.

It is a paradigm shift; a change in the way we should carry out our research. Until now, modern scientific research has always tried to remove the observer (i.e. man) from its studies, in the name of objectiveness. I also started out by studying the natural chemical composition of the stratosphere, but soon found out that man did influence it. We now acknowledge that man does have enormous impacts on the planet; from climate change to DNA. In fact, man has become central to the workings of the planet and cannot be ignored in our studies of nature.

How long will the Anthropocene last?

We don't know, there has never been a similar epoch before. We are actually moving into completely uncharted terrain, while rocking the planet quite heavily. That can be scary. We are now trying to define the safe boundaries within which humanity can interact with the planet. Whatever the outcome, I believe we will have to live differently. With countries worldwide striving to attain the 'American Way of Life,' citizens of the West should pioneer a modest, renewable, mindful, and less materialistic lifestyle. But also, if you look at how technology and cultures have changed since 1912, it seems that almost anything could happen by the year 2112. I am confident that the young generation of today holds the key to transforming our

Citizens of the West should pioneer a modest, renewable, mindful, and less materialistic lifestyle.

energy and production systems from wasteful to renewable and to valuing life in its diverse forms. The Anthropocene will obviously last as long as humans do. The awareness of living in that Anthropocene, and being able to steer it in a constructive way, could inject some desperately needed eco-optimism into our societies.

What gives you the strength to do what you do?

The importance of the task, the beauty of science and discovering how nature works: Nature with a capital N, if you like.

(July, 2009, Wageningen; January, 2012, Mainz – Ispra)

Anton Eliassen

From the outside, the Palais des Nations, headquarters of the United Nations in Geneva, is not the most elegant of architectures. It is the result of a compromise reached by a commission that oversaw an international competition back in the 1930s. For political reasons, the overseeing commission could not decide on a winner, even though Le Corbusier was one of the participants. It eventually asked five architects of five different countries to join forces. The result is a mastodontic building in a style that is ironically enough not too far removed from that of Albert Speer. The inside of the building, on the other hand, has a lot of charm; large halls with high ceilings and windows, art deco furniture and tapestries, clocks by Philippe Patek. The wandering, nearly floating, of delegates between meeting rooms, or between meeting rooms and the bar, enhances the quietness that is typical of grand environments. Beyond the large windows: rolling hills, the lake and the shining Mont Blanc on the horizon.
I visit the Palais every year in order to attend the annual meeting of the EMEP Steering Body. EMEP, the European Monitoring and Evaluation Programme, is an international programme that was set up at the end of the seventies to study the acid rain problem and to come up with suggestions as to how to solve it. Over the years it has developed into a programme that successfully deals with various types of air pollution that are spread across national boundaries. One of the founders of EMEP is Anton Eliassen.

Anton Eliassen (born 1945) was Director of the Norwegian Meteorological Institute from 1998 until 2010, and president of the European Centre for Medium-Range Weather Forecasts Council from 2003 until 2010.

He was a member of the EMEP Steering Body, which supports the UN Convention on Long-range Transboundary Air Pollution, since its inception in 1979 until 2010.

YOUR father, Arnt Eliassen, was a meteorologist, as you are. I expect that your early career developed in quite a predictable way.

I did indeed grow up in a home that was frequently visited by famous meteorologists. These were the early days of meteorology as a science. American meteorologists often came to Norway and spent time there, because Norwegian scientists, including my father, played a significant role in developing the science. I got to know many of these famous scientists: Jule Charney, Ed Lorenz, etc. But I was a kid in those days and didn't understand what they were talking about!

When I started studying it was on the cards that my future career would be in something to do with natural sciences and mathematics. But I found that I was not that good at mathematics and that I didn't have my father's abilities in this area. The idea of becoming just a lesser copy of my father did not appeal to me. Therefore, I tried to study other subjects. I still ended up in meteorology, but focused on physical meteorology and cloud physics, rather than dynamic meteorology.

It is a public secret that you never did a PhD.

That's right. When I finished university there was a vacancy at the Norwegian Institute for Air Research, NILU, and I started working there. NILU had just had a project funded by the OECD on the long-range transport of air pollution. It was to be proved, or disproved, that sulphur emitted by power plants in the United Kingdom could travel all the way to Scandinavia and be implicated in the acidification of its lakes. Very few knew much about this stuff. That's how it went, I suppose. I started in that field because of a vacancy.

But to be a professor in Norway you don't need to have a PhD, you need a paper written by an international committee saying that you have professorship qualifications. When I applied for a part-time professorship they looked at my papers from EMEP which I had published by then and found that they were enough. So I was adjunct professor for meteorology for 20 years, lecturing on turbulence and diffusion at PhD level.

Did your father present meteorology as a pure science, or did he also talk about its usefulness for predicting the weather or for describing the transport of air pollutants?

We didn't discuss meteorology very much at all. He was the type of person who would assume that I understood more than I really did. So I was nervous and discussions were always difficult.

Most of his students had the same problem. He was a demanding person, but never explained what his demands were. In the beginning of my career he didn't understand what I was doing with these air pollution issues. He understood that I was attending some important meetings, but he was not really satisfied with that. I remember that he was reasonably satisfied when I was appointed as professor. And when I became a director at the meteorological office, he said: now this is fine. As he grew older, my father also became very selective about with whom he liked to discuss; only with those that really understood. So we didn't discuss meteorology a lot. Dynamic meteorologists were a bit arrogant. They thought that dynamic meteorology was highbrow stuff and that physical meteorology, air pollution and dispersion, was lowbrow. Dynamic meteorology deals with systems that determine their own fate. In air pollution and dispersion the path is already given and what you are dealing with is a simple continuity equation.

Was the difference between dynamic and physical meteorology the same as the difference between basic and applied science?

Well, when we started with air pollution and atmospheric transport and chemistry, we started from practically nothing, and so it was definitely basic science! It was clear to me that you could use a meteorological model to describe the dispersion of air pollution, because I had learned the hard way through my father. If you knew the equations to describe the transport of temperature, vorticity, and so on, why couldn't you use them to describe the transport of air pollutants? I was surprised to find that it hadn't been done before. So in the OECD project we started straight away on that. In a way it was a simple merging of two fields that, for many strange reasons, hadn't yet spoken to one another.

The OECD project took many years, and it was hard work. Our boss was a good initiator of projects, but not a good finisher; he left you one third of the way into the project. My job was to describe atmospheric transport in the best possible way. I struggled with Eulerian and Lagrangian representations. My results initially depended on whether the reference grid of my Eulerian model was parallel to the prevailing wind direction or not. That was of course unacceptable, so I settled on the Lagrangian trajectory approach as it didn't cause such problems.

Others had started similar work on long-range transport, for example the Meteorological Institute of Stockholm University (MISU), under the leadership of Bert Bolin. He was originally a dynamic meteorologist. British scientists were also involved of course. Their expertise was in the area of the modelling of pollution transport after accidental releases, particularly in the nuclear field, such as the Windscale accident back in 1957. It was actually strange that so many scientists believed that sulphur would not travel more than 50 km in the atmosphere before it was deposited. Arthur Chamberlain, of the UK Atomic Energy Authority at Harwell, had already measured the deposition velocity of SO_2 using radioactive tracers to be about 1 cm/s. A back-of-an-envelope calculation made it clear that SO_2 had a lifetime of a day or so, long enough to travel beyond the UK.

In our description we needed to consider that SO_2 could be taken up by cloud droplets, and be removed by precipitation. The dynamic model we used didn't include clouds, so we needed to develop some simple schemes. But in order to do so in a sensible way, you needed to know how the fundamental processes worked; how SO_2 was dissolved into the droplets and how the oxidation process proceeded, and so on. This is just to explain how what we did in the early days was very much basic science. Many of the problems we needed to tackle are still on the research agenda today!

Palais des Nations, headquarters of the United Nations in Geneva.

Even though you describe your work as basic science, it must have caught the interest of the industries responsible for emitting SO_2?

Our work, and that of others, was thrown out at international meetings. High level administrators, especially from the emitting countries, were present at these meetings, such as the chief of the Warren Spring Laboratories, responsible for air pollution issues in the UK, and the chief of the UK Central Electricity Research Laboratory, the main laboratory of the power industry running the power stations and hence directly implicated in the SO_2 emissions. They would participate in the discussion because our results obviously meant a lot to them.

Did this involvement of stakeholders help the science?

That was indeed a first example of stakeholder involvement, and I can give you examples about how the scrutiny of our scientific work by people with a much larger view of the subject did indeed help to improve the science. For instance, when we finished the OECD project in 1976, we went to the OECD headquarters in Paris to get the report approved. The chief of the Warren Spring Laboratory, who was on the steering committee, dismissed the report as being totally unscientific and refused to approve it. We thought that our director would step in and defend the report, but he kept silent. We left the meeting rather depressed, and went to a café. I tried to figure out what was wrong. I realised that in the model we had made a faulty assumption: we had assumed that pollution emitted from a stack would be immediately homogeneously mixed in the air up to an altitude of one kilometre. In reality, this mixing occurs gradually and the concentrations in the surroundings of the stack would remain higher for a while and deposition in this area would also be higher. So our model underestimated the deposition near the sources and overestimated the long-range transport to other areas or countries. That evening I started developing a new scheme which we presented the next day to the OECD people. They gave us another year to correct the results and finish the report. They didn't give extra money but that was only fair (laughs).

Despite the overestimation of long-range transport of sulphur to other countries, the issue still grew into an international one.

Our final report still showed that there was a transboundary issue. I was asked to report on it in front of a World Meteorological Organization Commission. The WMO subsequently decided to hold a large international meeting. Since I was already the rapporteur on the issue, I was nominated the chairman of that meeting. Everybody came, Americans, Russians and Henning Rodhe. I was a bit intimidated. I was in my early thirties then, and chairing 144 participants was no mean feat. The meeting was held in Sofia. In order to secure the participation of Dick Derwent, an atmospheric transport modeller from Harwell, I had to invite another scientist from Harwell, because they were not allowed to travel to a communist country unless there were two of them. That is how John Garland, a younger colleague of Chamberlain, was also invited to talk about dry deposition. Because of him the meeting became a turning point: at the end we all agreed that sulphur could travel quite far.

In the meantime, there were also proposals at the diplomatic level as to how to deal with the issue of the long-range transport of air pollutants.

The relationship between East and West, between the NATO and Warsaw Pact countries, was very bad in the seventies. But that played to our advantage. The first talks between Nixon and Brezhnev aimed to ease the tensions. They resulted in the Helsinki Conference for Security and Co-operation in Europe. That was in 1975. The Conference produced an agreement to cooperate on armament control, human rights and economic affairs. The economic affairs chapter included a subchapter about cooperation on environmental issues. The implementation of the Helsinki agreement was a disaster and, in fact, led to confrontational policies between East and West. Both sides, however, saw the danger of non-communication and were eager to show to the world that they could work together. It was the Russians who proposed that the focus be directed to environmental issues, and the Norwegian and Swedish advisors made the long-range transport of air pollutants their priority. Since there was no major alternative for something that the East and West could manage to do together, our proposal was accepted. That eventually led, in 1979, to the signing of the Convention on Long-range Transboundary Air Pollution. But before that, EMEP had gotten involved in the study of scientific issues related to long range transport of air pollution. The EMEP Steering Body met for the first time in 1975, here in the Palais.

What was it like to have Eastern and Western scientists working together? Was there distrust among them?

The decision was taken to set up two Meteorological Synthesising Centres (MSCs), one in the West at the Norwegian Met Office, and one in the East at the Moskow Hydro-Meteorological Institute. Both Centres had the task of calculating the atmospheric transport of sulphur across

Europe. In the beginning we could operate very much in a purely scientific mode, but the scientists from the East faced severe limitations. Most of them weren't allowed to travel. There were also some strange constraints: they couldn't provide their emission fields, but only the transboundary fluxes. This meant they could not tell us how much sulphur was emitted in the various parts of the East, but could only tell how much of their sulphur was crossing the Iron Curtain from East to West. This was something they had to calculate, of course. It gradually became more political when the UN Convention on Long-range Transboundary Air Pollution came into being, and the participating countries started to negotiate emission control protocols. At one time I chaired a high level meeting between Americans and Russians in order to persuade the Russians to share their emission data. Gro Harlem Brundlandt was present as the Norwegian minister for the environment. It quickly became clear that not I but she was the real chairperson of that meeting (laughs). I worked with her to try to convince the Russians to provide their data, but they refused. Alessander Pressman, the chief of MSC-East, told me that his hierarchy thought that an emissions map would disclose the location of their major industrial installations, and hence be a map of targets for the US' 20 megatonne nuclear bombs (laughs). He didn't say this at the official meeting, but back in his hotel room, over vodka. I told him that the US satellites had long since identified the locations of their installations. But that didn't help. This reaction seemed very exaggerated to us.

So it was possible to speak openly with the Russians?

Since most of the colleagues at MSC-East were not allowed to travel, we had to go to them in order to really talk about science. We visited Moscow every year. We seldom went to the institute, but to the private flats of Pressman's people, and discussed everything there. That was not really legal, but somehow Pressman was able to arrange for permission. He was head of the labour union at the Hydro-Meteorological Centre; he was very important but knew exactly how far he could go. Pressman was special. He was also an example of brinkmanship. He led the calculations of the effects of the fallout of the nuclear bomb testing in Nova Zembla in the mid fifties. He flew around the test sites and eventually died of testicular cancer, while he was still MSC-East chief.

And you just talked about science?

Strictly and only about science (laughs). I always had fun asking Pressman to pull out the telephone plugs before we started.

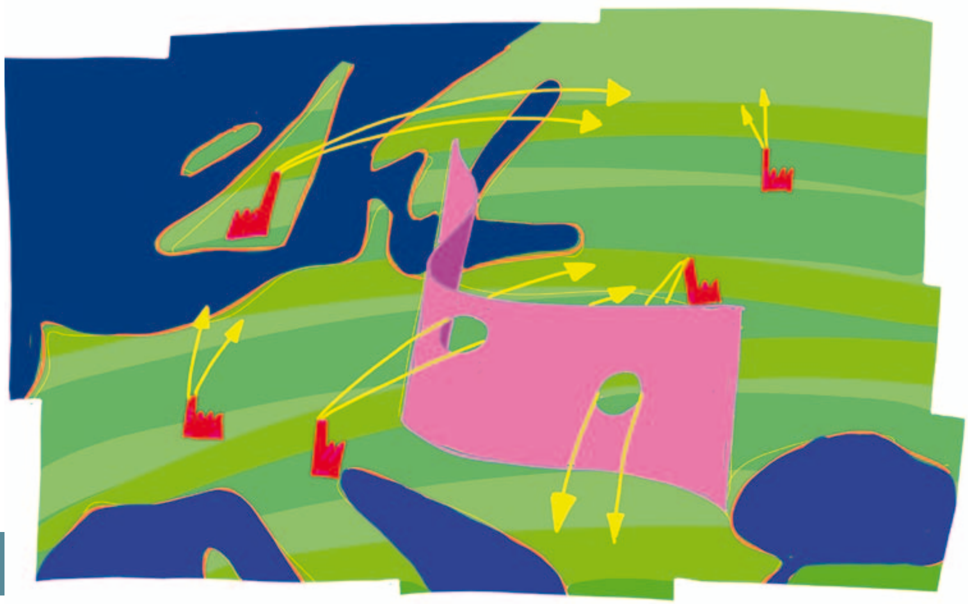
We certainly weren't afraid of a nuclear attack; we simply couldn't imagine that sulphur emissions had anything to do with such things.

Did the Russians give you their sulphur emissions in the end?

As I said, the Russians calculated the fluxes of sulphur compounds across the border between the Western and Eastern countries. Romania, however, was not part of the Warsaw Pact. Bulgaria was, so the iron curtain made a strange loop down there, and there was no way you could calculate the fluxes across that line. So we never used these official flux data, even though they were sent to us twice a week by telex. Anyway, it was hopeless. Little by little we created our own estimates of the emissions in the Eastern Block. First they were mere guesses. When the Convention was ratified, in 1983, it became obligatory to report national totals, and we used those. Gradually we were also picking up information that we really shouldn't have had. During our meetings, the people from MSC-East would happen to 'forget' papers on the table, and gave us all sort of hints that we needed.

And did you give the Russians your emission data?

Our data were public. We certainly weren't afraid of a nuclear attack; we simply couldn't imagine that sulphur emissions had anything to do with such things (laughs)!



I was once in a meeting where you gave a speech and explained why a scientist would enter this area of policymaking and politics. But I got distracted and didn't hear your answer. Now is my chance to hear you again.

(Laughs). In a way it was my job. EMEP was set up to quantify the transport of air pollutants, and obviously this quantification was needed to negotiate the emission control agreements. It was important for countries to know how much of their air pollution they had generated themselves and how much came from their neighbours or countries further away. So I realised that I had to sit through these negotiations, to listen. Sometimes the negotiators would stumble on some of the science. One delegation would, for instance, say that what you breathe in a city comes only from that city and not from other parts of the country, let alone another country. I then tried to give information that I knew even the scientists of the delegation that was causing trouble would agree to. I found that I had talent for expressing scientific information in a way that negotiators could understand. That was, by the way, not a talent my father possessed, so I found I could contribute in my own way, which was very nice.

Scientific information was important then. Today one could have the impression that the special role of science has been eroded. That it has become just one other part of the discussion, together with the issues of economics, employment, stakeholder interests and so forth.

The protocols under the Convention developed so fast, one after the other in quick succession, that the negotiators always needed the latest scientific information. So, science was in prime demand; all the other information that the negotiators used regarding economics and so on, was in fact old and well established. The time spans between the protocols were often too short to allow us to think through our results thoroughly.

Do you mean that decisions were often taken without decision makers being totally sure about the emissions and their impacts?

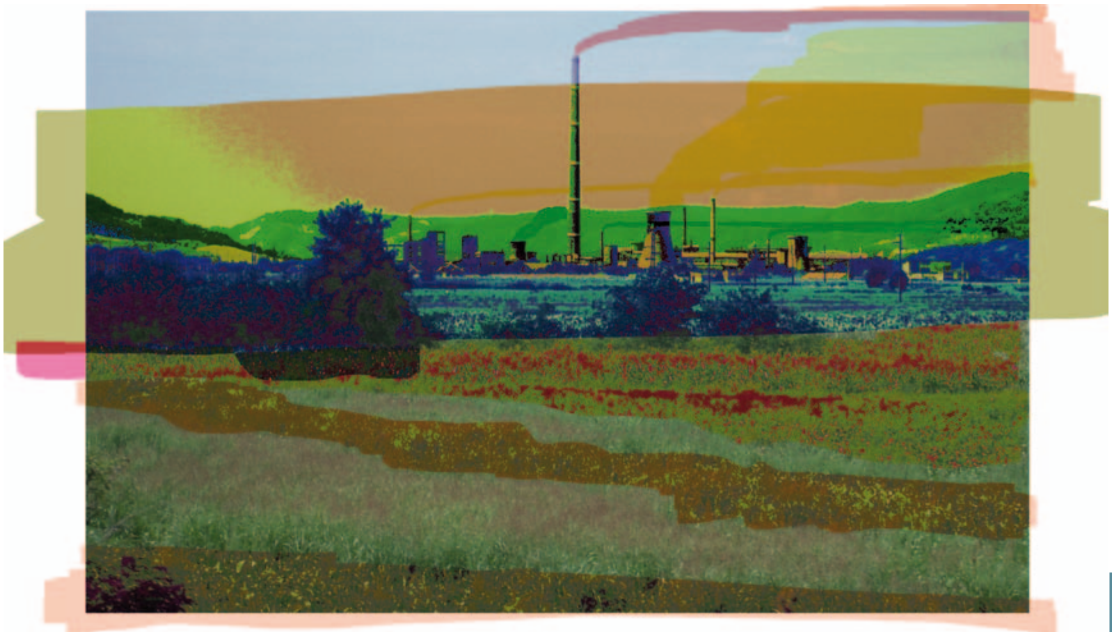
There will always be uncertainties. If you look at the wind fields that disperse pollutants in the atmosphere, they are dragged out in such a complicated way that you cannot say which smokestack pollutes which tree, 400 km away. That is impossible. The issue is really to what degree of geographical resolution you want to develop your control strategy. As long as the areas and countries for which you develop a control strategy are sufficiently large, you cannot be too far wrong. The reductions of emissions in, say France, would certainly benefit trees somewhere in Germany. It is not necessary to say which tree exactly: if it is not this particular tree, it will be another.

I am sure that in developing the various protocols and designing the control strategies we didn't push the scientific data further than they could be pushed. The negotiators knew that if they were using our scientific data for something they shouldn't, I would say so. I once put a stop to the negotiation of the second sulphur protocol because I had figured out that the way the model was set up gave too much weight to a single area near Lofoten in Northern Norway. To keep the sulphur deposition below the critical load in that particular area required very strict emission reduction measures in the whole of North West Europe. But I knew that the area was not sensitive at all; there was not a single trout up there to die from the sulphur deposition (laughs)! So I went into the meeting and instead of sitting behind the flag of MSC-West, I sat at the table of the Norwegian negotiators and I said, 'I withdraw the Norwegian sensitivity map and will return to you with better data later. Thank you very much.' So everything stopped for six months. That caused some immediate trouble, but led to increased confidence in the long run. In the negotiations that followed I could just sit there, and if I shut up, they would proceed (laughs). It was a strange feeling. I thought I had too much influence for just one person, and I did not like that, but at the same time I felt that I was on safe ground, because we looked very carefully at the robustness of the scientific results, or if you like, at the degree to which they could lead to no-regret policies.

We learned something from the experience I just referred to. We learned that a single environmental target or a single environmental condition should not determine the required actions too much. Later protocols, such as the Gothenburg Protocol, did not look at the control of sulphur deposition alone, but at the control of a range of issues simultaneously. In this way one can be more confident that the calculated emission reductions would benefit the environment in one way or another.

You often read that the success of the Convention and its Protocols was not so much because of the underpinning science, but because industry was eager to move to a more efficient, cheaper and less polluting energy production system. There was also the move towards nuclear energy during these years. Do you agree with that?

I think there were indeed a number of factors that helped. The Convention is a success story, it has been up to now anyway. Since it was agreed at the highest possible political level, between Nixon and Breshnev, scientists in the East were allowed to work on it and they were asked to do it well. The Russians had a great tradition in the science of atmospheric dispersion, which was very helpful. The ambience was rather optimistic in the negotiating bodies. In some way we felt we were ploughing new ground and doing something important. Of course all of this would not have happened for just a few fish in Norway. The Germans found out about their 'Waldsterben'. Germany was traditionally a source country of emissions but all of sudden it became a receptor country as well, and so it agreed to reduce its emissions. That change in attitude came about overnight, around 1983, and invigorated the whole process. Without that



Copşa Mică, Romania; once one of the most polluted towns in Europe because of a black carbon factory and metalworking smelter. Air pollution levels dropped after closing most installations in the nineties and installing modern emission control systems on the remaining ones. Toxic metals that have accumulated in the soil remain a problem.

In the seventies and eighties, Europe was really very dirty, the air was visibly dirty, and it was clear to everybody that something needed to be done.

change we wouldn't be where we are now, and Norwegian trout would definitely have died out! It is true that the Convention was in some sense lucky because, with the ongoing development of technology, the emission reductions came about by themselves. You must also realise that in the seventies and eighties Europe was really very dirty, the air was visibly dirty, and it was clear to everybody that something needed to be done. The protocols were only a stick for some countries, while for the big countries the carrot was really the economic gains that could be made by cleaning up their industries. Communism fell, which also helped. The old power plants in the East emitted a lot, and they are gone now.

I can't give you an exhaustive explanation of why the Convention was successful. Some have written books about this. I am inclined to conclude that the success really lay and still lies with the people involved. Some were really great talents in steering discussions and expressing themselves in clear and beautiful language. They could be Norwegian, Swedish, German or Russian, it didn't matter. The good matching of personalities counted for a lot. The ambience was just very good. You know that at UN meetings, delegations sit in alphabetical order. So, in the early days, the USA and the USSR were sitting next to one another. The UK delegate, also sitting nearby, smoked a big pipe and his fumes were drifting to the USA and USSR delegates. They jokingly complained to the chairman that the UK was not only causing pollution in Scandinavia, but in their countries as well (laughs).

It seems as if you were always at the right place at the right time throughout your career. Was it just that, or did you also feel real concern for the environment, the forests and the trout?

Of course I thought that it was bad that the fish died and the forests were damaged. But being too environmentally inclined could influence the scientific work. I am rather strict about keeping that distinction. It is a basic requirement that your personal feelings should not bias your science. So, environmentalism did not play a role in my work.

You referred to the Norwegian trout. I always heard about fish dying in Swedish lakes. Was there some sort of competition between Norwegian and Swedish scientists?

Yes there was, and that is a bit of a sad story. People at the Meteorological Institute of Stockholm University were bitter that they were not involved in MSC-West. I realised that it was wrong of us not to have involved people like Henning Rodhe. After all, he was one of the very first who understood the issue of the long-range transport of sulphur. It is still very important that young students understand the Rodhe and Grandell paper of 1972*, about how precipitation removes aerosol particles from the atmosphere. But in those days it wasn't up to me to decide who should be included in the project.

*Rodhe H. and J. Grandell, 'On the removal time of aerosol particles from the atmosphere by precipitation scavenging', *Tellus*, XXIV 5, 1972, pp. 442-454.

At a certain point you had to leave NILU and move to the Norwegian Met Service.

That was because of some institutional meddling. To be able to provide support to the Convention, EMEP and MSC-West had to go via a UN body such as the World Meteorological Organization. However, NILU is not a meteorological service and is therefore not linked to the WMO. Therefore MSC-West was moved to the Met Service and I had to move there with it. But the Met Service didn't really want to work on air pollution. It just wanted to calculate the weather for next Friday and draw figures to show how the temperature and precipitation had changed from 1950 to date. So, coming from the outside with a project on the environment was not popular, and I was not even sure we would survive in that environment. The only way to secure EMEP was, I thought, to try and create some power base in the traditional structure. One day they needed a new director for research: somebody who understood the science, but also somebody who could behave at international meetings. I had that international experience from my work in the 'Palais' here, and that's how I got the job.

Are you a scientist, a teacher, or a manager?

I am not a scientist any more, but I know what science is about. I left science gradually and around 1990 I realised I would never do anything scientific again. But I still have some ideas and I still have some manuscripts in my desk, you know! I am clearly a manager now. I am Director of the Norwegian Met Office and what I do is make meteorological data available to Norwegians. That is what the taxpayer expects. I still come to the EMEP meetings here; they are often very good, very interesting.

You have attended these meetings for about 30 years now. That shows quite a commitment.

Yes, and I still feel committed. I want to see that the process keeps going reasonably well. If I can help in a straightforward way, I will still do so. I still have some informal influence within EMEP, but I am very careful how I use it.

What advice you would give to young scientists who enter the area between science and policy?

I mentioned already one basic truth, namely that your personal feelings should not bias your science. Others include to always be completely honest, and not to hide data that do not fit with your model calculations. Don't make excuses; if it doesn't work, it doesn't work! This can be very hard. During the acid rain days, there was considerable political pressure on the scientists in several countries, the UK in particular. There are two people I knew well that did not give in to such pressure. One was Barry Smith from the UK Met Office and the other was Bernard Fisher from the Central Electricity Research Laboratory.

How did that political pressure work?

(Ponders for a while.) Once I was invited to the Central Electricity Research Laboratories to present our air pollution model. Having travelled in the morning, we were first served lunch. My host was trying to make me drunk before my lecture, by pouring me a large single malt whisky. Bernard kicked me on the leg under the table, to warn me not to drink it. That was good, because all CERL's star scientists were attending my lecture, sitting on the first row (laughs)! These were the tricks they played on you. They were interesting times. The editorial office of the journal *Atmospheric Environment* was also at CERL, so they had an important say on what could be published. Bernard Fisher was never promoted. Barry Smith, on the other hand, was treated well by the Met Office. He became a 'special merit scientist', and I was told that this is like being next to God (laughs).

Was there also pressure from the opposite direction, to be more outspoken about the acid rain problem?

I told you about how discussions at the OECD headquarters had improved our first estimates of long-range transport of sulphur from the UK to Scandinavia. The result of that improvement was that the estimated deposition of sulphur from the UK in the UK increased, while deposition of the UK emissions in the Nordic countries decreased. When I was asked to present these new results to the Norwegian ministry for the environment, I was accused by the ministry of being a traitor and of giving in to UK pressure. My boss in the Met Service supported this accusation and I and others were left to defend ourselves on our own. It was hard to be accused of being a traitor in a public meeting.

What other advice would you give to young people?

Well, I would say…. (pauses for a while): it is important to speak clearly and honestly, to formulate the science so it can be understood by everybody. I think you can learn this to a certain extent, learning by doing. It is important to think before you speak. Forget yourself. Never think you might be making a career, or promoting your particular piece of science. Always think about the issues, think about solutions, think about how we can give information that will help the policy makers. If you think about yourself, if you are self-centred, some of your statements will immediately have an undertone that will be caught by everybody, and you will no longer be considered to be neutral. Be concise and only say what counts and leave out the rest. You should say things because the negotiators need it, not to show that you know it. When you have given the message that is important to the meeting, then shut up. Some people are completely unable to do this.

Always think about the issues, think about solutions, think about how we can give information that will help the policy makers.

This is also related to culture, I guess. Norwegians and Flemish use fewer words compared to our Mediterranean colleagues.

Yes, and there is probably not much you can do about that. I told some people a hundred and fifty times, but it shortened their interventions by three and a half percent (laughs).

In the early days, studying air pollution was basic science, you said. Today the work in the EMEP centres is more applied science. Is there a risk of losing contact with basic science?

It is clear that EMEP doesn't do basic research. It carries out operational modelling and therefore you need to simplify your model. You can't have a thousand chemical reactions in the model, or the model would need to run for a year to describe one day. You can't do basic science yourself but need the blessing of the basic scientists. The problem is that the scientific purists will accuse us of oversimplification. You must realise that the word 'simple' in the US has also the connotation of 'naïve' and 'stupid'. At one stage, some US purist called our model a 'Mickey Mouse model' (laughs). He went on to call it a 'comedy of errors', and I knew that this was the title of a Shakespeare play in which everything that can go wrong does go wrong.

Our model was simple but certainly not stupid. It is actually an art to know which terms you can omit; intuition is needed in order to simplify. But if you know which terms are large and which terms are small, you can simplify and still infer something useful from the simple model. By being operational and applied we get dirt on our hands, for sure. There is a large gap

> ***If those in the basic sciences were willing to see the value of applying a simple model rather than attacking it for being simple, that would help a lot.***

between those who have dirt on their hands and those that never want to get their hands dirty. If those who work in the basic sciences were willing to see the value of applying a simple model rather than attacking it for being simple, that would help a lot.

What personal talents have been particularly useful during your career?

I think I have wide but not necessarily deep scientific competence. I mean, I do have a thorough understanding of atmospheric turbulence, but I don't have a similar understanding of all the scientific matters that come into play. I still enjoy using information from several disciplines in trying to solve questions. Modelling was a way of integrating what we already knew in new ways.

What did you learn during all these years?

Well, you learn not to be nervous when speaking in public. In heated public discussions you learn to think three times before you reply. You learn not to be arrogant, and to accept that other people know some things better than you do. That's very important. Never take anything for granted. Be happy with what you are able to achieve. That is what I learned. Of course I met a lot of interesting and beautiful people from various fields. That is fascinating. Discussing with economists about what is a public or collective good. Even discussing with legal experts from the implementation committee can be fascinating (laughs).

Your finest moment?

I never recognise fine moments when they are happening, I only recognise them in their aftermath. The signing of the Gothenburg Protocol was probably such a moment. You might find it odd, but I was particularly happy that I had the idea of bringing Jennifer Logan over, at the signing of the Gothenburg Protocol, to speak about transport of air pollutants between continents. Then I was happy when I became Director of the Norwegian Met Service. I had always been a stranger in the Met Service, because of EMEP, and now, one year before I retire, I still feel as though I am a stranger there. I am a bit surprised that everything went so well (laughs)! It was fun. I was just lucky to stumble across this rapidly moving area. I didn't really do much more than what I should have, but I am grateful that I have been allowed to do these things.

(September, 2009, Geneva)

John Schellnhuber

When organising a conference on atmospheric chemistry in 2001 in Turin, I thought it would be a good idea to include an introductory talk showing how atmospheric chemistry fitted into the overall Earth System. I invited Hans Joachim Schellnhuber, who everybody calls 'John' and who had just published the essay 'Earth System Analysis – The Scope of the Challenge'. John accepted. A month before the conference, he called and had to cancel his participation as his wife was very ill. 'I will send you my best collaborator', John added, 'but that doesn't mean I don't owe you a presentation anymore'. A year later I invited John to the Joint Research Centre where he inspired a fully packed auditorium with his views and vision on Sustainability Science. In the evening, over dinner, we talked not about science but about everything that it might relate to: education, inspiration, poetry and courage. We talked about the many layers life is made up of, touching also on death. It was one of those rare moments in which you realise that, while on the one hand you are physically confined to a table in a small pizzeria, on the other hand the mind is able to roam universes.

Later, we became involved in climate change research projects and, each at our own level, in the preparation of an international climate treaty. International climate policy had received a boost in 2007 at the Bali UNFCCC conference with the agreement on a two-year work programme which would be finalised in Copenhagen with the signature of a 'Copenhagen Protocol', the successor of the 'Kyoto Protocol'. There was enormous hype before the Copenhagen conference of the parties, the COP15, not only in the media but also on the street. Neighbourhoods and cities, and action and study groups held meetings, distributed pamphlets, or filled billboards to persuade politicians to 'save the planet'. It was 'now or never!' And the politicians reacted. At least 180 Heads of State, from Obama to Mugabe, went to Copenhagen and joined film actors and pop stars, Nobel Prize winners and business CEOs in a universal delirium. In the end, however, the hopes did not materialise.

I met with John one month after the Copenhagen debacle, at a meeting in which scientists exchanged ideas with artists about climate change and what to do next. 'We need new ideas, new images to make the scientific message clear', John said in his talk.

John Schellnhuber (born 1950) is founder and Director of the Potsdam Institute for Climate Impact Research since 1993.

He has been a member of the German Advisory Council on Global Change since its inception in 1992, and its chairman since 2008.

WERE you really in such a bad mood in Copenhagen that we couldn't do the interview?

Not really. But on that last Saturday morning we had a wrap-up session with the German delegation including the environmental minister. Directly afterwards, I had to rush back to Germany to appear on national television, where I provided the initial interpretation of what had – and what had not – come out of the COP. I felt that it was important to do so because I wanted to strike the right balance. Copenhagen, and I do believe in this, was not the end of the climate negotiations and certainly not the end of the world. In fact, all major negotiation tracks have simply been extended.

But without a deadline.

That is true. There is neither a mandate nor an expectation to come to an agreement at the next COP in Mexico this year. But in the end, if there is genuine willingness at the respective national levels to make a difference, you don't need a deadline. Look at the deadline they set in Bali two years ago, aiming for a finish in Copenhagen. That was even confirmed by the Bush administration and that didn't help either. A legally binding global agreement would be fantastic, of course. But even this would be no guarantee of climate stabilisation at a manageable level. Would you send UN forces to the country that doesn't meet its emission reduction targets under that agreement? So, again, it's a matter of willingness, not of deadlines or signatures.

Isn't the deadline set by the climate system itself? If we want to stabilise the climate at 2 degrees above its pre-industrial temperature, global emissions of greenhouse gases must be reduced by 2020.

This is a deadline of sorts, resulting, inter alia, from the rates of reduction that can realistically be achieved after emissions peak. So the sense of urgency is backed by science, and this will hopefully drive clear-minded people to action. Science has made the difference time and again, probably much more so than lofty political agreements about timetables.

Politicians don't seem to see the urgency of the climate change problem. 2020 is only ten years from now.

Some do. But I have been thinking quite a bit about this since Copenhagen. There is both a moral and a time issue involved, and both create tremendous inertia in the behaviour of

people and the making of politics.
The moral issue goes as follows: if you brought your child to the school bus, and the driver said there was a 50% chance of an accident because something was wrong with the engine, nothing on Earth would make you put your child onto that bus. Climate change undoubtedly creates, with more than 50% probability, the risk of destroying the life of some child in some region that is heavily hit by anthropogenic warming at the other side of the planet – the life of a child who is not even born yet and who you will never get to know. Acting to save that anonymous life is a really tough test for our moral standards, even if you believe every word of what science says about climate disruption.
With respect to the time issue: If you tell a smoker that if he or she continues to smoke, their life will be shortened by many years (as supported by all clinical surveys), he or she might say that cancer will only strike at sixty, perhaps, so they will not give up smoking now. It is crazy, isn't it? Even when your own survival is at stake it seems far too inconvenient to change your habits now and to reap the benefits later. So it is not that people are wicked or dumb or not perceptive of scientific insights, there is simply this inertia related to the demi-god 'convenience'. That is why I am here today, for instance; we need powerful images to overcome our personal inertia. Artists might shock us out of our easy chairs.

In Copenhagen you were present as the advisor of the German government, as a scientist, but also as a citizen. How do you keep these roles separate, or don't you even try to keep them apart?

As an advisor to the government, I tried to provide the scientific insights that strengthened our arguments. Germany has a very ambitious climate change policy, with a goal of 40% reduction of emissions by 2020. This is unparalleled in the industrialised world to date. So I supported Germany´s environmental minister Norbert Röttgen (who is a very reasonable man, by the way) when he proposed that the EU should aim for a 30% reduction in emissions by 2020, unconditionally. That suggestion was indeed discussed among the European heads of state in Copenhagen in the final stage of the conference to try to unblock the diplomatic standstill. But the countries of Eastern Europe did not join in.
As a scientist, on the other hand, I try to demonstrate that the non-linearities in the climate system – the so-called tipping elements – are still not being properly researched, never mind things like a runaway greenhouse effect. Earth will hardly evolve into something like Venus

whose oceans have all evaporated, but there might be episodic runaway effects with a self-amplifying global warming of a few degrees.

That is not something you can talk about during negotiations.

Certainly not, but the issue is part of my responsibility as a scientist. Once you become an expert in a field of immediate relevance to society, you have to start asking yourself: do I really look for the important issues, or do I just keep on maximising my citation index? If you do mainstream work, you are almost guaranteed that you will be part of a wonderful network and that you will receive nice awards and funding, and all this is very nice. However, it is very difficult to get appropriate funding for studying highly nonlinear feedbacks in the Earth System, simply because hardly anybody dares to look at the issue.

Still, I believe that it is our responsibility as a scientific community to study those feedbacks and see whether we can firmly rule out the runaway effects we talked about. This type of research has to be led by the most senior and advanced people in the field, as they can afford to risk being ridiculed. I just organised a Nobel Laureates Symposium in London, where one 'Nobel man' stated that the global climate change problem can only be solved by global governance. He can afford to say so. If a post-doctoral student were to say this, everybody would laugh; it would be called hyperbolic or naïve. So we have the paradoxical situation that the most established scientists have to be more curious, more provocative and more courageous than the others.

You are usually quite clear and outspoken in your talks. This morning you said that if we pass the 2 degrees guardrail, the Greenland ice sheet will melt, leading to as much as a 7-metre sea level rise in a few hundred years. But looking at current emission trends and with the CO_2 that we already have in the atmosphere today, science tells us that we are already heading for a 2.4 degrees rise in temperature. Shouldn't you then, as an established scientist, say that it is already too late, and that we must start building dykes and moving entire populations while we still can in an orderly way.

I would be wrong to simply say it is too late. Looking at the physics, staying below the 2 degrees guardrail is difficult but possible. It is still important to limit further greenhouse gas emissions and global warming. But it is a complicated situation. I have been thinking about this predicament for quite some time and have always believed that we need a two-pronged strategy:

mitigation and adaptation. This strategy was reflected in our scientific concept in 1992 when I became Director of the Potsdam Institute for Climate Impact Research and coined the phrase 'Avoiding the unmanageable and managing the unavoidable'. The idea came to me during a meeting here in Brussels at the Academy of Sciences.

I was at that meeting. You stood up and spoke the phrase, and then you added, almost theatrically, 'I will say it once again: avoiding the unmanageable and managing the unavoidable'. I was sitting next to André Berger and he whispered, 'That man is a genius.'

(Laughs). André is too kind. Anyway, if the world does warm up by 2 degrees it will be a different one from the one we live in today. We are speaking about degrees Celsius! An increase of 3 degrees would bring about dramatic changes with sea level rising by many metres in the long run. If temperature increases by more than 3 degrees we may well lose the Greenland ice sheet, the Amazonian rainforest, the West Antarctic ice sheet, and so on. Some people have undertaken an analysis of the costs and benefits of a combined mitigation and adaptation strategy, and they have arrived at an 'optimal' CO_2 concentration of 750 ppm. However, such calculations are dangerously misleading as there is little scientific evidence behind them. I think that, because of the non-linearities in the system and the related tipping points, it is worth fighting for each tenth of a degree.

Will people more readily adapt than mitigate?

The difference between adaptation and mitigation is that you engage in adaptation for yourself and your neighbours, whereas you engage in mitigation basically for others, living far away, some 50 years from now. So people will instinctively prefer to adapt, and part of that adaptation will happen automatically. The question is whether we need organised global action for this, in the same way as we try to orchestrate mitigation. Do we need to set up new institutions, new information systems, new behavioural codes to make it easier to live in a warmer world? And, by the way, such systems can only be designed successfully if one knows what to adapt to. Therefore, the 2 degrees target is crucial for two reasons: it is a firewall against nasty nonlinear processes, but it also makes it possible to prepare for something that is completely veiled by uncertainty. And even if we do breach the 2 degrees guardrail, I would recommend, for the sake of adaptation, that another target be set – perhaps 3 degrees – so that we can still make relevant projections and prepare ourselves.

Why doesn't mitigation come about more automatically? There are many immediate benefits of reducing fossil fuel use: it will reduce local air pollution immediately, it will reduce our dependency on oil from the Middle East, etc.

I agree with all these points, yet climate change remains the biggest concern. I think that without that issue, there would not even have been this push for renewable energies. The imminent peaking of cheap oil production has been a volatile element in the public debate for a while, but in the meantime we discovered tar sands and shale gas which offer the possibility of transcending the fossil resources in the entire Middle East. So there is not really an exhaustion of geological fuels in sight. That, in turn, has further accelerated the discussion about climate change in the past few years. But I would like to give a twist to this argumentation. In the future, emissions reduction might well be driven by energy policy rather than by climate policy. Once there is sufficient economic momentum and the change of the energy system picks up speed, this concrete transformation is bound to take over the political process.

Now this illustrates once more that we, as scientists, are facing the challenge of telling a very complex story in a compelling way. We have to convey a sophisticated message in a simple manner.

Do you never feel that, when you tell the climate change story, you go beyond your mandate as a scientist? For instance when you said this morning 'We are going to lose everything'.

I was more or less citing from the final text of the Nobel Laureates' Symposium held in London last May, signed by some sixty laureates: physicists, chemists, geneticists, economists, writers, the Dalai Lama, Gorbachev, etc., which reads 'We cannot wait until it is too late. We cannot wait until what we value most is lost.' This might sound like a non-scientific statement. But if you think of the impacts on a city like Venice, on the Alpine glaciers, the Amazonian forest, on the risks jeopardising human development, thus affecting coming generations in general and in particular poor countries exposed to climate change, this statement is actually very well supported by science.

As researchers and scholars we could decide to put only our facts on the table, publish them in *Nature Geoscience*, and then leave it to the world to decide what to do with that information. We might just state that sea-level rise could be such and such, and let people find out for themselves that this in fact means the end of the most beautiful city in the world. But I think the problem is just too big to allow you to retreat to this ivory-tower mentality. Scientists have an obligation to explain their findings to the public and to decision makers when those findings actually matter for the fate of entire societies. Future generations would not forgive us if we drag our feet on this.

Scientists have an obligation to explain their findings to the public and to decision makers when those findings actually matter for the fate of entire societies.

Several metres of sea-level rise will not come about immediately but maybe in 1 000 years. Playing the devil's advocate I could say that that's too bad for Venice, but I hope that in the next 1 000 years humanity will build new cities, new pieces of beauty that people can admire and cherish.

A thousand years is actually not such a long time. Today, we still have very tangible traces of cultural activities of the past thousand years all around us. If we keep carrying on with business as usual, which would lead to 4 or 5 degrees planetary surface warming, sea level could rise over time by tens of metres, so we would lose a great part of our cultural heritage. You cannot defend the roughly one million kilometres of shorelines in the world against a meltdown of the ice sheets. More generally, I feel that it will be next to impossible for a higher form of civilisation supporting 9 billion people to exist if we do not curb global climate change. This is because a number of crises will coalesce in a fatal conspiracy: the loss of coastal zones, severe problems with agriculture due to shifting precipitation patterns, and eventually the depletion of affordable energy, which we failed to replace by renewables. A lot of people will have to migrate, most likely generating international tensions and local conflicts. Look at the consequences that the relatively marginal Palestinian-Israeli conflict has had at the global level – we cannot even manage that! With unabated climate change, our civilised institutions, trying to maintain a fair and just world, may simply collapse. And that means 'losing everything' in my view.

Now you seem to be speaking more as a concerned citizen than as a scientist.

At my age, I am blessed to have a two-year-old son. With the perpetual advancement of the medical sciences, he has a reasonable chance of witnessing the world in 2100, that is, the main target for the scenarios and projections we discuss in the IPCC reports. It is up to us to set the stage, over the next 20 years or so, for either the most benign or the most disastrous of those scenarios. I want my son to see and enjoy Venice, for example, the way you and I have been able to see and enjoy it – a miracle on Earth. So yes, I am also a concerned citizen, but being a scholar I will not say things that are not in line with scientific evidence. You can talk about impacts and what it would mean to people and what the moral implications would be, but you should always stick to the facts and figures. One should never mix up roles. The role of science is to provide information for necessary transformations, but it is up to democratically elected decision makers to call for or to call off those transformations.

If I step just a little bit outside of my circle of colleagues and friends, I notice that few people followed what happened in Copenhagen. Are you optimistic that we can create a tipping point in society, and trigger a big change to which everybody will contribute?

I think that less than a few percent of the entire population will make the initial difference. Eventually, of course, transformations cannot be left to 'change agents' and will need the approval and support of the majority of citizens. The pioneers create small pockets of change in our society; these may be artists, journalists, scientists, particularly committed citizens or even some of our politicians. These are the ones who can push across tipping points and create an irreversible change in the overall socioeconomic dynamics. The German Advisory Council on Global Change (WBGU), which I chair, just did an analysis of how the Industrial Revolution came about. Why didn't it take place in China or among the Incas? We found that you need a very special social configuration where a very small fraction of the powerful adopts innovation, something that doesn't generate an immediate benefit. In that first Industrial Revolution, it was the English gentry who took the lead: well-to-do people who could afford to spend a lot of time thinking about problems, performing experiments and designing machines – the very same people who founded clubs like the Royal Society. These clubs were not about making money. They could only be created in times of prosperity, as a luxury or a fashion emerging from a network of like-minded people. And these pioneers were able, in a sufficiently open and liberal country, to change the intellectual fabric and the industrial metabolism of society. Today, of course, the burning question is how quickly such a transformation can be instigated.

In our outreach as scientists, do we have to target this elite, the members of the Rotary clubs?

That is something that every scientist has to decide for him or herself. The Rotarians might not be the prime audience, but school teachers are a fairly safe bet. However, my perception is that very few scientists see the necessity of communicating their findings to a wider public. But why not convey your stories? Some of them are fascinating. Like the Indian summer monsoon and the nonlinear dynamics behind it.

Does it have to do with age? We did a study within a network of atmospheric physicists and chemists*, and found that younger people are definitely more eager to reach out with their work to the public, to schools etc., without expecting anything back. The older generation, ours, would typically make sure their results enter the IPCC and, with this, they consider their outreach toward society done.

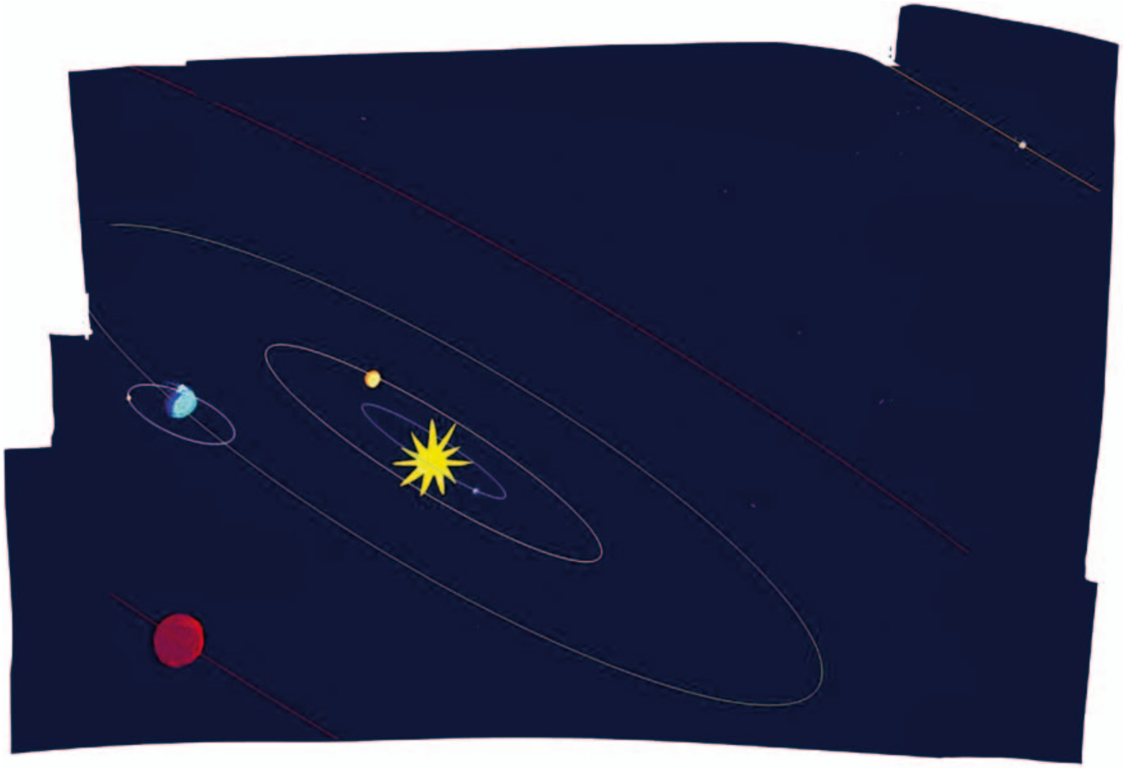
*Pereira A.G. et al., 'Atmospheric composition change research: Time to go post-normal?', *Atmospheric Environment*, Vol. 43, 2009, pp. 5423-5432.

I agree that age is among the relevant factors. I see, for instance at our institute, that we attract more and more young and bright people who wish to engage in climate-change studies. One young doctoral student finished a German high school at the age of sixteen. She had jumped three classes ahead of her age group. Then she studied mathematics and physics in Göttingen University and graduated at the age of twenty. She read an interview I gave in the German weekly magazine *Die Zeit*, phoned me and asked whether I had a job for her. I said, 'You are young, you are smart, you are ambitious, so why don't you pursue a normal career? In less than five years you will become a professor of mathematics. Everything is set for you.' And she answered, 'But I want to do something useful!' You see? The young and bright still want to address the challenges of science, but at the same time they want to do something for future generations. That might be particularly true in Germany. After the Nazi catastrophe, German science was traumatised, since the knowledge community had failed to resist being instrumentalised by appalling politics. So the generation after the war thought they could only preserve their dignity by staying as far away from politics as possible. For the young generation, that is no longer an option. This is a time in which you have to be brilliant, but you also have to be responsible. And if you are truly brilliant, you run less risk of using the facts in the wrong way. I really believe that. I believe that mediocre people, whether in science or society, are more tempted to twist the facts.

This is a time in which you have to be brilliant, but you also have to be responsible.

You have always been 'top of the class' as well. Was there anything in your childhood that indicated that you would become a scientist, an Earth System scientist?

Thinking back to my earliest 'professional' interests, I reckon I wanted to become an archeologist, excavating amazing things. The book 'Götter, Gräber und Gelehrte' (Gods, Graves and Scholars) was a best-seller in those days. In my youth I was mainly interested in three things: Firstly, football, which was absolutely my first priority (laughs). Secondly, nature. I loved to roam through the forests, mostly on my own. I grew up in a beautiful landscape in Bavaria, and I was happiest when I was allowed to go wandering in the afternoons. And thirdly, finding out



It was maybe just the beauty of the solar system.

about the world and how it worked. I was, without knowing it, fairly talented in mathematics and I loved to read. I would devour just about every written item I could get hold of, entire libraries. Very soon I became interested in big discoveries, for instance, how the planetary system was set up. But that was not driven by the desire to become a scientist. I was just curious. I don't know, it was maybe just the beauty of the solar system. Then, at the age of fourteen, I started to read Einstein and Kant.

You read the scientific papers of Einstein at the age of fourteen?

Yes, out of sheer curiosity. I did not immediately understand everything, of course. But some of the thinking stuck in my mind. I first read something simple, 'Mein Weltbild', which is a collection of Einstein's popular and even political writings, but also an introduction to his work and to that of others. Over time, I became acquainted with relativity theory and quantum physics. I didn't do this to become smarter than my peers. I didn't even tell other people about it.

One day, my mother told me that they would send my elder brother to university, but that they didn't have enough money to support me as well. It was not long after the war, I was raised in a poor family, and that was how it was. But she also told me about a scholarship that existed for the exceptionally gifted. In order to get one, you had to pass all of your exams with the

maximum scores. So I made up my mind that if this was the only way, I would win this scholarship. And I did. I finished high school with the highest honours possible in Bavaria. But then I had to choose what to do at university. Well, I was interested in history. But eventually I went for what was probably the most challenging combination, mathematics and physics.
And it is one of the strange coincidences of my life that my office on the historic Telegraph Hill campus in Potsdam is the place where Einstein's field equations were solved for the first time.

You never became arrogant?

I don't think so. There are quite obviously people in the world who are brighter than I am. Through my positions as Director of the Potsdam Institute and chair of the German Advisory Council on Global Change, I have had the privilege of meeting many Nobel laureates. Murray Gell-Mann, for instance, who worked out the theory of quarks and now resides at the Santa Fe Institute, where I am an external professor. That man knows just about everything. So there are good reasons to be modest. But you also have to find out, I think, whether there isn't something you can do better than others. That is almost an obligation, something we owe our creators. Of course, in the beginning of my career, I didn't think about that obligation, or the responsibility to help solve the big problems of the world. It was curiosity coupled with a sporting element of trying difficult things that set my course.

There are good reasons to be modest, but you also have to find out whether there isn't something you can do better than others. That is almost an obligation.

In those days, the main science subjects were mathematics, physics, chemistry, etc. There was not yet the awareness that a broader and more integrated approach was needed to understand some of the world's problems. How did that awareness develop for you?

A number of scholars contributed to the development of what is now called 'Sustainability Science' and has become one of the most dynamic contemporary research fields. As a community initiative, it was spearheaded by my friends and colleagues at the US National Academy, including most notably Bill Clark, Bob Kates, Billie Turner and Pam Matson. But it is probably true that I also helped to build some of the theoretical foundations back in the early 1990s, dubbing it 'Earth System Analysis for Sustainability'.
Why did that happen? Well, two factors allowed me to make my contribution. I already told you that I love nature. I often feel humbled when confronted with the beauty and grandeur to be found on our planet. While studying physics on the sidelines, I travelled extensively through Africa, sometimes missing an entire semester. I really got acquainted with 'creation' there. You

stand on the ridge of the Central Rift Valley, and look to the west over the Congo Basin and to the east towards the Nyiragongo Volcano, spitting its fire into the evening sky, while below, the elephants graze in the shimmering haze ….. It was unbelievable, all those colours, the noises, the scents. It was like getting a glimpse of heaven, and it was quite shocking. You suddenly understand where you come from. This must not be destroyed!

At another occasion, during the time of the Sahel drought in the early 1970s, I volunteered to distribute milk powder, sugar and flour to the Tuareg community in Niger and Mali. I witnessed what happens when the environment interferes with culture. The Tuareg are an extremely hardy type of people, but their culture was completely shattered by a few years of drought, by an unusual fluctuation in the regional precipitation pattern.

These experiences did not make an environmentalist out of me, but they became deeply engraved in the back of my mind nonetheless.

Many of us are engaged in global change research, without having travelled the globe and without having experienced it. Would we carry out our research differently were we all to travel first?

Travelling has certainly been a defining experience in my case. After wandering for five years, intermittently of course, I really felt I had grown up. These were very low budget trips. You sleep in a trench next to the road. Or you are stuck in the middle of nowhere, waiting for a bus, not knowing whether the vehicle will even come that day, or what it will look like. And somebody is sitting next to you, somebody in ragged clothes, and you suddenly realise that this human being is suffering from leprosy. This is in fact a common sight in certain regions; these poor souls are part of everyday life. Through such experiences you develop a completely different perception of what fate means, of what wealth means, of what happiness means.

In Africa, for instance, I travelled sometimes in the company of two or three other young people, sometimes alone. I went to very dangerous places where tyrants posed as benevolent statesmen. When you happened to cross the path of a policeman in such a country, you had better hide because he might rob you, or put you in jail to be able to hold you to ransom. On the other hand, the native Africans are generally wonderful people, so, as a white European, I felt extremely embarrassed and ashamed by systems such as Apartheid that existed in South Africa at the time. All these impressions and emotions were highly conflicting and bewildering, but I had to sort them out. In the end this made me stronger as a person, and I still benefit from that.

I imagine that writing your rather mathematical essay 'Earth System Analysis – The Scope of the Challenge'* requires something more than impressions and emotions.

Yes indeed. In those years, nonlinear dynamics was on the scientific rise; chaos theory, as they called it then. I was lucky to be in Regensburg, where we held seminars on complex dynamics, on the self-organisation of systems in a state far from equilibrium. The Prigogine theory was developed here in Brussels. And so I became highly interested in complex systems theory. Later, I was able to make a few original contributions to that field.

There were several converging strands, which were rather hidden to me since I was not yet fully aware of them. Working on complex systems and nonlinearities soon made me think about the climate system – which epitomises all chaos systems. Just as one plus one equals two, ambition and capacity resulted in my commitment to environmental systems analysis, especially at the planetary level where the climate is formed after all.

I guess I became an Earth System analyst because I realised that with global warming something really big was going on, and that it would require a multidisciplinary approach to understand it. My personal experiences had given me a qualitative gut feeling, an initial understanding of the world around us, and I made it more quantitative by applying the methods of nonlinear dynamics. You have to be able to think about processes in complex systems. Physics and mathematics provide you with the intellectual structures and the methodologies to identify the dominant processes.

*Schellnhuber H.J., 'Discourse: Earth System Analysis – The Scope of the Challenge', in *Earth System Analysis: Integrating Science for Sustainability* (Eds. H.J. Schellnhuber, V. Wenzel) Springer-Verlag Berlin Heidelberg, 1998, pp. 1-195.

For many people, back in 1998, 'Earth System Analysis' came out of the blue. Fifteen years later it is still a source of inspiration for trying to formalise the concept of sustainable development.

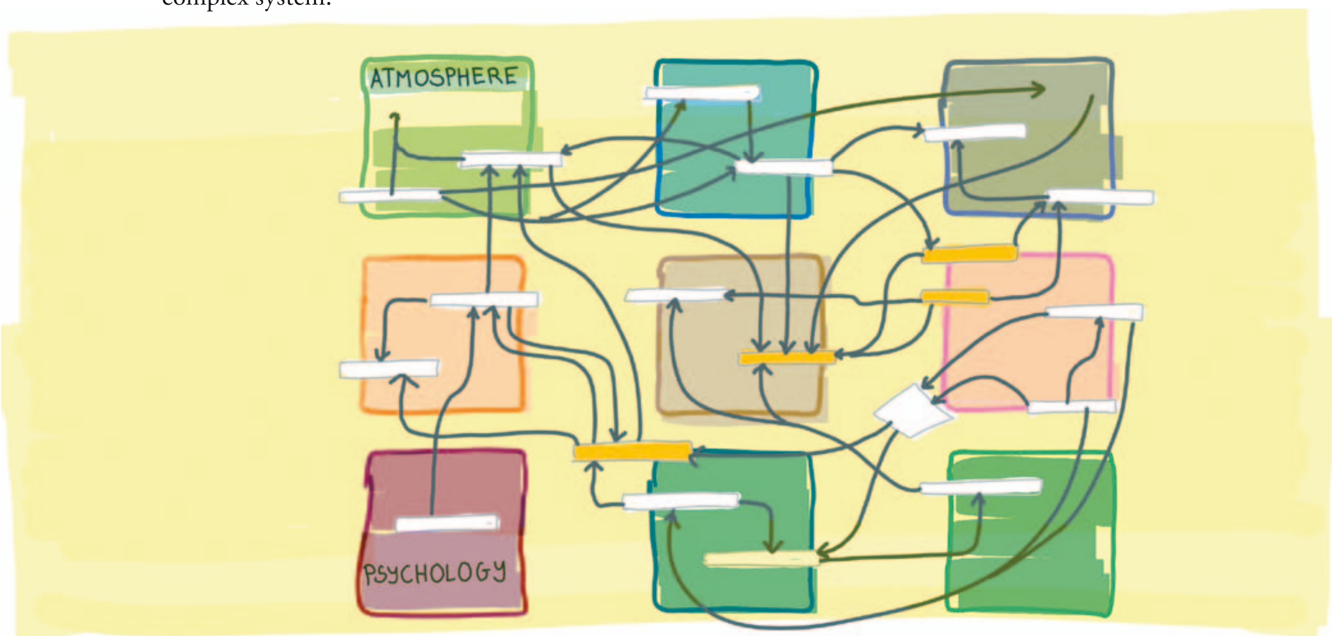
Let me tell you how the essay came about. When I was a young professor at Oldenburg University, I met the son of Jim Lovelock, Andy, who was doing a masters thesis there. So I got involved with the Gaia community. Some of them were completely nuts, but others, like Jim Lovelock himself, were extremely bright people. The crucial point is that 'holistic' thinking can be just a shallow substitute for scientific creativity. On the other hand, I always thought that a bird's-eye view combined with rock-solid methodological knowledge could be really interesting. In other words: genuine systems analysis. So my love affair with the latter began back in those Oldenburg days.

When I was asked to set up the Potsdam Institute in 1991, I needed to develop a guiding

Holistic thinking can be just a shallow substitute for scientific creativity, but a bird's-eye view combined with rock-solid methodological knowledge can be really interesting.

concept for it. Back then, people had a vague feeling that it could be worth studying climate impacts, but no-one had a strategy for this. Klaus Hasselman, one of the world's brightest climate scientists, was chairperson of the founding committee, and even he would just suggest that we add a bit of socio-economic analysis to our climate physics. So I had to develop a vision and a scientific concept for the new institute starting from some key questions: what would an ideal interdisciplinary research capacity look like; how could one tackle a problem like global warming with all its ramifications; what do we absolutely need to understand and what could we afford to ignore for a while?

A spaghetti diagram of relationships in a complex system.



I started to create spaghetti diagrams to illustrate all possible interactions. I still have one of these charts. It is really beautiful, actually a piece of art (laughs), with hundreds of different colours. I thought, this stuff is something for which systems analysis is really needed. I kept thinking about it, and when we had the first meeting of our scientific advisory board, with wonderful scholars such as Klaus Hasselman, Colin Prentice, Martin Parry or Ernst Ulrich von Weizsäcker, I had to give the introductory lecture. In an intellectual frenzy over only a few days I created a mathematical typology of sustainability paradigms, something that still applies, I think. And this is how the concept for the Potsdam Institute came into being. Then it took me another two or three years to fully develop the relevant mathematics and put it all into that essay.

Soon, some heavyweights in the United States, especially Bill Clark and John Holdren from Harvard, became interested in these ideas, and that was certainly instrumental to my election to the US National Academy of Sciences a few years later – quite an honour. My essay is still there as a scientific *pièce de résistance* and some colleagues, also at the Potsdam Institute, are trying to operationalise it.

Did you have a mentor in this process?

Not really. But I was lucky to find a lot of supporters. These included, for instance, Walter Kohn, a Nobel laureate in chemistry, and the founding Director of the Institute for Theoretical Physics in Santa Barbara. He was sort of my supervisor when I was a postdoc over there in California. I already mentioned Klaus Hasselmann and Bill Clark, who have been brilliant colleagues and wonderful friends for a long time now. Paul Crutzen, another Nobel laureate, provided generous support and encouragement at various points in time. And during my term as Research Director of the UK's Tyndall Centre I built excellent relationships with David King, Nick Stern and Diana Liverman, to name just a few.

These are all benevolent people who do not go in for undermining ideas that aren't their own – unlike what often happens in the scientific community. Without at least a handful of powerful supporters you cannot succeed. Yet I developed and wrote that essay entirely on my own. I guess I was never a mainstream person. Not that I would go out on the streets and get myself put in prison, like some of our illustrious colleagues do.

Are you, on the other hand, ever criticised about the fact that you talk about all these negative things to come in too relaxed a way, sometimes even with a smile?

It is a question of personality, or mentality, how you deal with that. I tend to be self-ironic, because there are people who are brighter than I am, and even these chaps are no more than halfwits when it comes to unravelling the mysteries of the universe or the complexity of climate change. So we have no reason to think too highly of ourselves.

List of Frequently Used Mathematical Symbols

$\mathcal{A}$ Anthroposphere

$\mathbf{A}$ Macro-state of the Anthroposphere $\mathcal{A}$

$\mathfrak{B}$ Bundle of Paths

$\mathcal{B}$ Global Brain

$\mathbf{C}$ Coevolution Space

C Class of paths ($C[\mathbf{S}]$)

$\mathfrak{C}(\mathbf{P}_1 \curvearrowright \mathbf{P}_2)$ Geo-cybernetic Corridor from $\mathbf{P}_1$ to $\mathbf{P}_2$

$\mathfrak{D}, \mathfrak{K}, \mathfrak{R}, \mathfrak{r}, \mathfrak{S}, \mathfrak{s}$ Domains and Subdomains

$\mathcal{E}$ Overall Earth System

Γ Manifold (Separating Domains in $\mathbf{C}$)

$\mathcal{H}$ Human Factor

L Lagrange Function

$\mathfrak{M}, \mathfrak{M}^*, \mathfrak{M}^{**}$ Pool of Control Options

$\mathbf{M}, \tilde{\mathbf{M}}, \mathbf{M}^*, \ldots$ Management Sequences ($\mathbf{M} \in \mathfrak{M}$)

$\mathcal{M}$ Executive (Management) Component of Global Subject

$\mathcal{N}$ Ecosphere (Nature)

$\mathbf{N}$ Macro-state of the Ecosphere $\mathcal{N}$

$\mathcal{P}_i$ Paradigm

$\mathbf{P}(\ldots)$ Path in $\mathbf{C}$

$\mathbf{P}$ Point in Coevolution Space ($\mathbf{P} \in \mathbf{C}$)

Π Trajectory in $\mathbf{C}$

Q Scalar Quality Functional

$\mathcal{S}$ Global Subject

$\mathbf{S}$ Generalized Equilibrium

t, τ Time

$\mathfrak{U}$ Accessible Universe in $\mathbf{C}$

$\mathcal{V}$ Value System of Global Subject

Schellnhuber's mathematical arsenal to describe the Earth System.

Springer-Verlag, 1998.

And telling sad and serious stories in a relaxed way helps to get the message across. Even taxi drivers who happen to listen to an interview with me on public radio tell me that they appreciate that I tell my tale in a calm way, not screaming that the end of the world is nigh. But that does not mean I smilingly do not care about the issues I talk about. The tragedy is that the end of the world as we know it may actually not be that far away.

You are Angela Merkel's advisor on climate change issues. She seems to be knowledgeable about the climate issue. She was Secretary of State of the Environment …

… and she holds a PhD in theoretical physics. When the WBGU developed the idea of the 2 degrees guardrail back in 1994, I sat with her for several hours and talked her through the relevant formulas like the logarithmic dependence of the greenhouse effect on carbon dioxide concentration. She would ask for explanations until she understood. That was quite unusual for a politician.

Is she deeply concerned about the problem of climate change?

I think so. She does not seem to be a very emotional person, but she has a very strong sense of responsibility. And she is a tough woman. I would assume that now that she is in office, she feels the weight of responsibility and wants to serve her country in a way that will also be appreciated in the future. She is the daughter of a protestant clergyman, so her strong moral standards are not accidental.

It is often said that there is a communication problem between scientists and politicians and the public in general. There is a 'language' problem. Do you use a special language with Merkel?

Not really. But I do think that you have to communicate differently with each audience. With Merkel, when I go to her and we have a face-to-face meeting in the Chancellery, I bring along a stack of slides, no more than 20, printed on paper. I provide the latest information on issues such as methane release, the dwindling of the Arctic sea ice, the learning curves for renewable energy technologies, and we talk them through. However, you can only proceed in this way with somebody like her, who has had formal training in the natural sciences, whereas with other politicians you have to talk exclusively about costs and benefits, investments, competitiveness, and so on. When giving talks to the public or to schools, it helps to show scientific authority (deepens his voice), 'I am a member of the US National Academy of Sciences', and so on. But you always need to tell a good story. And sometimes it may happen, for reasons you can't explain, that you fall completely flat. I have had audiences where I wasn't able to ignite anything, and felt tremendously frustrated about that.

Do you select your audiences?

Well, I talk to a lot of people, but obviously I can't talk to everyone. I like to talk to young people, but I also enter into discussions with the CEOs of big companies like British Petroleum.

You seem to be a public figure now in Germany.

Maybe. My next-door neighbours recognise me now, at least (laughs).

How much time do you spend on outreach initiatives?

I really try to limit that. You know, I have an institute to run, I am chairperson of several scientific advisory bodies, and I am a member of a number of academies. Some eighty percent of my working time is actually spent on research and concept development. I still have several ideas about which I try to motivate people at my institute to do some in-depth investigation. I left the field of quantum physics, but there is still one approach I have to pursue there, and I hope to find a good postdoc student for that.

Where do your ideas come from? Do you particularly nurture contact with the big sea of ideas out there? You already mentioned the importance of travelling.

I think you need to have a good hard disk in your head, where you store all sort of things, and these can come from all areas of life. I like to associate things that don't seem to belong together. People might have tackled one or the other, but they did not have the methodology to make the connection. So often it is as simple as that: carrying an entire bag of methodologies and associations along with you.

Actually, I get my best ideas when I shave in the morning, so I must not grow a beard (laughs). So it happens usually in the first half hour of my day. But there is no recipe for having bright thoughts. This is in fact a scary thing. I sometimes worry about losing my entire creativity because I don't know where it comes from. Ideas seem to pop up like particles from the quantum foam. You know there is a theory that spontaneous fluctuations at the sub-atomic scale lead to the creation of everything – or rather every thing.

I am also inspired by talking to people. Somebody might say something, and I would think, hmm, where could one go from there. But again, very often these things happen in very trivial situations, such as boring meetings at renowned academies (laughs).

But it is not enough to have this background noise of ideas. You have to catch the idea and pin it down, operationalise it. For this, a good walk in the forest is just perfect: fresh air, enhanced blood circulation. I usually try to write the idea down in a compact way, possibly in quantitative terms, a formula, an equation. How does it work with you?

Well … I consciously keep time apart, also working time, I must admit, to do things that have nothing to do with my normal job. Like having this conversation − nobody asked me to do this. But I thought it was a good idea to talk to people like you about how certain things are done. Somebody gave me once a book 'Writers never die' with a collection of interviews with Flemish and Dutch writers who had turned eighty...

I am not even sixty!

I know, but your name is linked to so many of the big ideas in our science, that it seems that you have been around for a long time.

That is not entirely flattering (laughs). But seriously, I was fortunate to work on occasion with very smart and interesting people or to be involved, rather accidentally, in a number of interesting processes. Like that meeting by a Swedish lake, initiated by Harvard colleagues back in the winter of 2000. After long and heated debates we wrote a paper for *Science*: the now famous 'Sustainability Science' article, with the splendid Bob Kates as the first author.

As mentioned before, I introduced the idea of a 2 degrees guardrail to Angela Merkel back in 1994. So it was a German guardrail in the beginning and became a European climate target later, in 1996*. The basic idea was to view the climate problem in the inverse way: start out from the question of what a safe operating area in climate space with manageable impacts might be, then calculate the corresponding concentrations of greenhouse gases in the atmosphere, and finally determine the admissible emissions corridors to ensure these admissible concentrations. That was the 'tolerable windows' concept. It struck me one morning before I had to address an important meeting, and I didn't really have anything new to talk about. But operationalising that idea turned out to be an extremely difficult mathematical problem involving complicated techniques like differential inclusions. We published the pertinent science in a paper in Climatic Change in 1999*.

The 'budget approach' is another concept on which I stuck my neck out. The German Advisory Council on Climate Change, which I chair again now, published a report in 2009, echoing the latest findings that less than a trillion tonnes of CO_2 should be emitted worldwide by 2050

*At the 2010 climate talks in Cancun, the 2 degree Celsius target was adopted by the United Nations.

*Petschel-Held G. et al., 'The tolerable window approach: theoretical and methodological foundations', *Climatic Change*, Vol. 41, 1999, pp. 303-331.

if we want to maintain a decent chance of not transgressing the 2-degree limit. The climate machinery works in such a way that the cumulative carbon input largely determines the warming of the atmosphere. But we took this insight much further. Once you accept that you have a defined amount of total carbon available to run global civilisation, you naturally have to ask how to divide this cake in an equitable way among the people of the Earth. The obvious first choice is an equal-per-capita allocation. Based on these fundamental assumptions, you can immediately calculate what each country can emit over the next 40 years under such an equitable scheme. An attractive trait of this approach is its flexibility, since each nation can decide how to spend its total carbon share over time. So the 2020 limit, which we discussed before, becomes a lot less stringent.

Take the US, for instance. They cannot possibly be de-carbonised over the next 10 years, so they might first overspend their credit and go into negative emissions later using afforestation programmes or other sustainable carbon management schemes. Or they may buy emission certificates from low-carbon countries like India. We ran a number of environmental-political scenarios that were all very enlightening, but also a bit shocking. This ‘budget approach’ is a new take on international mitigation. It could be the reset key for climate negotiations, based on the best science. It is clear that in Copenhagen the old way of doing climate business, based on a strict silo mentality of the rich and the poor countries alike, utterly failed. So we need a fresh new start, based on fundamental principles such as equity.

You have set up the Advanced Institute of Sustainability Science, using the model of the Institute for Advanced Studies in Princeton. Will the Einsteins of this century come from sustainability science?

Einstein will remain unique forever, but yes, the sustainability subject will attract beautiful minds. Today, combining brilliance and relevance is the most exciting challenge for young people. We are founding new disciplines or, better, inter-disciplines, that become ever more important – this is a new dawn for science.

I am member of the German National Academy of Science, which only exists since 2009. Before that there were close to a dozen regional academies, including the Berlin-Brandenburg Academy of Sciences in my immediate neighbourhood. However, I never became a member of it. The social scientists said I was a natural scientist and the natural scientists maintained that I was a social scientist, dealing with ill-defined matters such as the economic impacts of climate change. They couldn't agree whether it was worth taking me on board. This epitomises the rigid thinking of the past.

In ten years´ time it might well be the other way around; in order to become a member of an academy, you will have to prove that you can think across the disciplinary boundaries. Indeed, there is a paradigm shift going on. That shift will go even further and help to re-liaise with the arts, with moral philosophy, and so on. Not that you will perform better mathematics with moral philosophy, you still need to be able to write down the correct equations. But it will be stimulating, I think. As with the classic eras of Greek culture, the Renaissance, and the Enlightenment, when all the strands come together we may restore a certain universality of thinking, perhaps.

At the meeting here, I have the impression that we scientists want to use artists to put our message across, to help us, as you said, to speak to people's hearts using appropriate pictures. But what about the reverse: can art lead to a deeper understanding of the world?

I do not know of any concrete example of this. But Heisenberg, one of the founding fathers of quantum mechanics, always said to his students that before we try to understand something, we must have a picture, a metaphor for it. Artists undoubtedly see things in a different way and can come up with unusual pictures. So I could imagine that if artists were involved at a very early stage in creating new ideas, when we are still splashing about in that quantum foam, they might help to create the right pictures. But this would have to happen early in the creation process; once we have collapsed our intellectual wave function onto an operational scheme it is too late. So we should explore those initial phases together with artists. Hmm, that is an approach that is just crazy enough to generate something unusual, and it crossed my mind while talking to you. So thank you very much! (Laughs).

Thank you!

(January, 2010, Brussels)

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Veerabhadran Ramanathan

Ever since Paul Crutzen had mentioned that he was working with the Scripps Institute in San Diego, adding that Ramanathan was his 'god' there, Veerabhadran Ramanatan had become a bit of a mythical figure for me. His long name, which looked and sounded more Indian than Indian, undoubtedly added to that.

We worked together on a project sponsored by the United Nations Environment Programme that aimed to reduce global warming by reducing emissions of carbon dioxide and other gases that have an impact on climate. Ramanathan, known as 'Ram', had been the first to realise that other gases aside from CO_2 could contribute to the greenhouse effect. These included CFCs, ozone, and black carbon particles emitted from incomplete dirty combustion processes.

His scientific work has brought him all the way from the atmosphere of Venus to the atmosphere of Indian villages and homes. 'At least there has been some direction in everything I did!', Ram joked.

In 2011, Ramanathan visited the Joint Research Centre for a project meeting. We had agreed to have an interview on the Friday afternoon, when everything should have calmed down. But at lunchtime we met some other colleagues, got into discussions, started looking at data and visited labs. The entire day was spent like this and Ram had to catch a plane later that evening. We therefore decided to do the interview by telephone the following week, when he was back in San Diego. The interview was eventually concluded six months later on the fringes of another meeting at the headquarters of the World Meteorological Organization in Geneva.

Veerabhadran 'Ram' Ramanathan (born 1944) has been a professor at the University of California, San Diego, since 1990.

He chairs the science team of the Atmospheric Brown Cloud Program sponsored by the UN Environment Programme since its inception in 2002.

A couple of years ago, during one of your presentations, you said, 'I am tired of preaching, I want to do something'. Was that a thought that came to you overnight?

In my case it was a bit of a slow process that started in 1998, when we carried out the Indian Ocean Experiment, INDOEX. During that experiment we went on several flights from which I saw vast brown clouds of pollution covering most of the Arabian Sea and the Indian Ocean. We were particularly interested in what effects these pollution layers could have on climate. You can work on the greenhouse effect, but you will never see it with your own eyes, you can only go through temperature records, etc. But seeing these huge pollution plumes with my own eyes made me reflect on the human activities causing them and the personal lives of the hundreds of millions of people living below them. After all, that pollution came from India, the country of my birth, and it was clear that the country was both causing and suffering from that pollution. I thought about this for some time. My own research and that of other colleagues linked the pollution to primitive cooking stoves: millions of such cooking stoves. Little by little I became personally involved in the study of this. I spent my summer holidays in the village where I had seen my grandmother cook on those mud cooking stoves, burning wood and cow dung. I remember her cooking for several hours, and in the end she would be too exhausted to join us for dinner. The indoor smoke she had been exposed to had also definitely had an impact.

All this was mulling away in the back of my mind. But if there is a single moment that really had a defining impact on my involvement, it was that climate talk I gave in early 2007 at the United Nations in New York. Ban Ki-moon had invited about a thousand high school students from all over the world to listen to people like James Hansen, myself and others. Opening that 'general assembly' was Ban Ki-moon's first duty as UN Secretary General. After my talk, an African girl stood up and asked what concrete steps I was taking to help solve the climate change problem. For me, that was the straw that broke the camel's back.

Before we talk about 'the doing', can you tell me what 'the preaching' consisted of?

Preaching is when you tell people what to do. I say that we have to reduce the emissions of greenhouse gases. I say this to the US Congress, but also during talks in churches, schools and Rotary clubs. I tell the public that each of us can and must do something; we don't have to wait for the government to act. That's preaching.

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Kathmandu, Nepal.

There is quite some debate about whether scientists should or should not simply stick to the scientific facts.
I had turned sixty around that time, and I realised I could not just stay within my scientific boundaries. I also had to think and talk about what we can do about climate change. So yes, I crossed a line, and that was a personal choice.

What did you start doing differently after you came back from your talk at the UN?

On a very personal level, I started to take the bus part way and then walked to work. I switched my house to solar power. But the Surya project, I feel, is my way of really doing something. In that project we work, with very little funding so far, in an Indian village of 2 000 inhabitants, and provide people with better cooking stoves. These improved stoves still burn wood and cow dung, but they do it more efficiently than the old stoves did. There are less black carbon particles in the fumes produced, and the temperatures are higher, so the cooking time is reduced. We also measured the levels of black carbon pollution in the village, before and after installing the improved stoves, to determine the effectiveness of the new stoves on the health of the local people and also on regional climate. The INDOEX data show us that the absorption of solar radiation by black carbon heats the atmosphere and cools the surface underneath. That has impacts on the formation of clouds and precipitation. After INDOEX I conducted more field campaigns using unmanned aircraft. These exercises revealed how vast brown clouds of black carbon were surrounding the Himalayas, heating the air and then settling on the snow. When we inserted all of this data into climate models, the simulations showed how the brown clouds were disrupting the monsoon rainfall and, anomalously, warming the Himalayas. I realised that the brown clouds were posing a regional as well as a global problem. They could have severe impacts on the melting of the Himalayan glaciers, with all the associated consequences for the millions of people depending on them for their drinking water. These findings made me even more determined to take action.

You talked about your grandmother. What were conditions like for your mother?

You know, when I was ten, my father found a job with a tyre company and we moved to Bangalore, where things were a bit less primitive than in the villages. My mother and my aunts all switched to kerosene stoves forty years ago. That freed them from hours of hard labour, which was an important incentive. There are 400 million Indians in the middle classes now, and none of them cook with mud stoves.

What about the other 700 million Indians? Is there a link between poverty and environmental degradation?

Half of the world's population doesn't even get the opportunity to pollute! They simply cannot afford fossil fuel.

During the past three years I started reading literature on that subject, and I started working with local people and NGOs. I should have known, but I was shocked to find that half of the world's population doesn't even get the opportunity to pollute! They simply cannot afford fossil fuel, which remains the main cause of environmental pollution. So, in a way, we have to be careful about introducing kerosene and LPG stoves. That is why we focus on introducing improved and more efficient cooking stoves that still burn wood and cow dung. These are, in principle, renewable fuels, and hence are good for the climate, and they are free. They will have to remain free, because people can hardly afford anything else!

Understanding the social aspects of making changes is very important. In the Surya project we have a whole team just studying the social aspects. In the village where we work, people do use the improved stoves, but they still want to use an open fire for certain things. For instance, they would still prefer to boil milk for two to three hours and using cow dung to fuel the fire. Milk is very important in the Indian diet, and it might simply be a matter of taste; just like we in California like our barbeques and you in Italy prefer a pizza baked in a wood fire oven.

Let's come back to your career for a moment. How did a boy like you from a little Indian village become a world class scientist? Did somebody help you?

There was nobody in my family working in the science area; my father worked for the army before he became a salesman, selling tyres. But there was certainly one thing that helped to train my mind. Until I was 11 years old, we lived in small towns in the south of India, where everything was taught in the local Tamil language. Then we moved to Bangalore, which was a major city that had been used by the British as an army post. There was still a lot of British influence there and the language used at high school was English. I didn't understand what was happening in the class or what the teachers were talking about. I dropped from being first in the class to the lower half. I lost the habit of listening to the teachers, and started learning and figuring things out on my own. That habit remained with me and helped me to switch from field to field, in India and also in the US. I was never afraid of making these changes. At each switch I taught myself everything I had to know.

How did you get into science?

After studying engineering, I worked for six months in the refrigerator industry. I didn't like it, but I learned about all aspects of refrigerating, including freon gases, and that would turn out to be important in my later work (winks). Anyway, I hated the job and returned to higher education, but without really knowing what I wanted to do or what I was good at. I studied just enough to stay above average. My passion was in sports. I wanted to play tennis for India! But I was not good enough for that. So I was just floating around like most twenty-year olds. I joined the Indian Institute for Science for my graduate work and built a Mach-Zehnder interferometer, an instrument for measuring small temperature differences in air. I built it from scratch, based on my readings of scientific papers, and it worked! That gave me the confidence to carry out original research and I soon realised that that was my calling. It was still very much an engineering type of work, but I acquired a taste for scientific research, for making measurements and discoveries.

In your biography that appeared in the Proceedings of the National Academy of Sciences, you say that you moved to the US because you wanted to drive a big fast car. Was it that the only reason?

Well, in those days everybody wanted to go the US, mainly in order to enjoy the good life. So that was also my choice. But I also realised I didn't want to be a PhD student in India. It is difficult to switch fields in India, and I wanted to move away from engineering and do something in basic science: physics. To get into the US, I applied for a job in a tyre company, which I got, but it just so happened that by the time I started making plans to leave for the US, the company went bust. So I decided to go straight to graduate school and get a PhD. I also understood that research in the US would be very different. You know, in those days, Indian research and Indian companies where not really interested in innovation, but simply in copying things from the West. In research groups in the US there was a real sense of going for new things.

One of your first tasks in the US was to study the atmosphere of Venus. It is striking that so many climate scientists have started out by studying Venus or Mars. How do you explain this?

When you look at Venus, you basically look at the radiation that it emits, and you have to understand radiation transfer very well in order to understand something more about the planet. From radiation measurements you can learn about the composition of its atmosphere, its temperature and the greenhouse effect that must exist to explain that temperature. Seen from the Earth, Venus and Mars are in fact nice and relatively simple systems that allow you to understand some very basic things about the balance between radiation coming in and radiation leaking out, and the effect that has on temperature. And that, of course, is the basis of climate science. On the contrary, the people who built models of the Earth's atmosphere were much more interested in using fluid dynamics to explain and predict weather patterns. There was very little radiation physics in their models, because it was thought that you didn't need good radiation physics to forecast the weather. In Europe they have always put more physics into the weather forecast models, which I thought made them superior. When I later took a job at NCAR, the National Centre for Atmospheric Research, I had to struggle to get radiation physics put into the NCAR weather forecast model. In fact, I first worked together with a visitor from Australia, Dr Kamal Puri, who had brought his own atmospheric model along. I worked after hours and at night to put my radiation code into his model. This helped obtain a remarkable improvement in the simulation of the jet stream. That was quite a result, because at the NCAR they had been trying to do this for the past ten years, without getting anywhere. NCAR abandoned their model overnight and took our model which eventually became the first NCAR community climate model: CCM0. That was in 1983. Today they are on version CCM5.

To move from instrument builder to climate modeller is quite a step.

Working with models was a tremendous help for me, because suddenly I had a global perspective. I had no education in atmospheric dynamics, but I played with the model, changed input parameters and observed how the atmospheric dynamics would change. So I learned step by step, and this brought me to where I am today.

Don't you think it odd that your scientific focus went from the atmosphere of Venus all the way down to the atmosphere of Indian villages and homes?

(Laughs) At least there has been some direction in everything I did!

Do you think it possible that, if you and others had focused straight away on the problems that people face on Earth rather than on issues on distant planets, we could have avoided a number of environmental problems much earlier?

That is difficult to answer. From my personal experience I would say no. Global climate change is a big problem, and if you hadn't studied the planets first, you wouldn't have had the arsenal of models, nor the mindset, to figure out what is happening with the Earth. If I had attended

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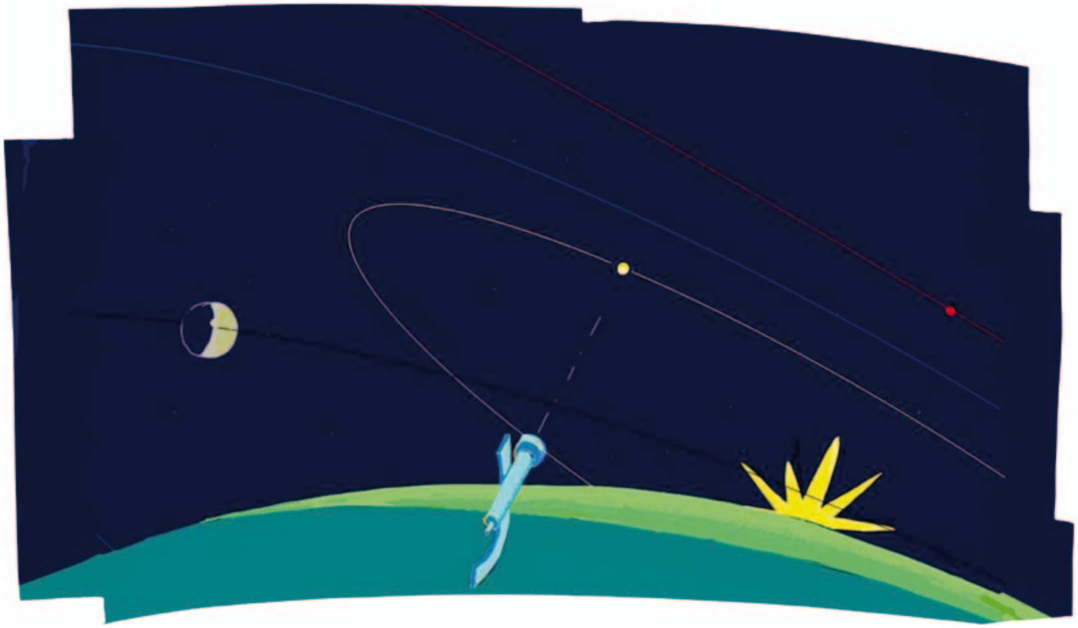


a traditional meteorological school, I would not have had that global view. Looking at Venus and Mars did give that planetary view, which you also need when thinking about planet Earth. Then in my own career, in a rather random way, I moved top-down from the global to the local scale.

… while others have started with local air pollution and subsequently realised that air pollution could have continental and global dimensions. The two approaches are now probably coming together.

That's right. Still, from my own possibly biased perspective, I believe that the top-down approach will not only lead to managing the climate change problem, but also to the solution of local pollution problems. NGOs have been trying to solve the local air pollution problems in India for decades. When we go to Indian villages, we often find that improved stoves had been installed decades ago. It was already very clear in those days that giving people better stoves would be good for a variety of societal reasons: health, emancipation of women, etc. But it is one thing to give people a better stove, it is another to make sure they will maintain it and use it properly. That did not happen, because there was no dedicated policy, or no money to ensure that the new stoves were used properly. With the climate change problem and the urgency to do something about it, there is now more attention and more funds to support the poor in a way that will also benefit the environment, and do so in a more sustainable way.

Your discovery that CFCs are extremely potent greenhouse gases is an example of linking a very specific human activity, refrigeration, with a planetary problem, climate change. How did that discovery come about?

We have to go back to my early years in the US. After having obtained my PhD in planetary sciences from Stony Brook University, and despite the importance I now give to that area of study, I could not immediately find a job in the field. By chance I joined a group at NASA, Langley in Virginia that was working on spacecraft re-entry; that had at least something to do with atmospheric physics. Then two random things happened at the same time. In the same hall where I was working, some engineers were preparing for the next generation Earth Radiation Budget Experiment, in an attempt to make satellite measurements of how much radiation was going into the atmosphere-Earth system, and how much was leaking out. A bit like the problem I had been working on for Venus. I discussed the more physical aspects of such an experiment with them, and eventually I helped to design the experiment. NASA Langley was not a prime spot for atmospheric science research, so a trip was organised to the NCAR in order to explain the experiment and get support from the big names in atmospheric research. Although I was not officially part of the team, they took me along with them. During our short stay at the NCAR, there was a seminar by either Molina or Rowland. The seminar described their early work on CFCs and how these molecules could destroy ozone in the stratosphere. That immediately caught my attention, because in India, during my job at the refrigerator company, I had seen how freons, the CFCs used as refrigerants, could leak from fridges into the atmosphere. My job at the company was, in fact, to prevent those leaks. Hearing that CFCs could be implicated in some atmospheric problem made me think about their potential role in radiation transfer. I had done a quantum mechanics course as part of my PhD, and so I knew that any polyatomic molecule would have a complex absorption spectrum of infrared radiation. You can call this a flash of insight if you like. Until then, only carbon dioxide from fossil fuel burning was implicated in the man-made greenhouse effect and hence climate change. That idea was about a century old. Here, there was the possibility that other man-made gases were also involved. My PhD advisor discouraged me from looking into it, but I was determined to

Greenhouse Effect Due to Chlorofluorocarbons: Climatic Implications

Abstract. *The infrared bands of chlorofluorocarbons and chlorocarbons enhance the atmospheric greenhouse effect. This enhancement may lead to an appreciable increase in the global surface temperature if the atmospheric concentrations of these compounds reach values of the order of 2 parts per billion.*

V. Ramanathan, *Science*, Vol. 190, 1975, pp. 50-52.

investigate it further. Studying CFCs was not my day job at Langley, so I worked on it at night; there was no other way. I wrote to people at companies like Dupont who had made infrared absorption measurements of CFCs, without telling them what I was working on, and they sent me their data. I needed these absorption spectra in order to calculate the greenhouse effect. When I reached the result that one kilogramme of CFCs in the atmosphere was 10 000 times more effective in atmospheric warming than 1 kilogramme of carbon dioxide, I was completely shocked. I didn't believe the numbers myself.

I sent a paper on my findings to *Science* anyway expecting that it would be trashed immediately. Who was I? I had just had come to the US and finished a PhD. I was dumbfounded when *Science* accepted my paper, and shocked when the most famous science writer of the time, Walter Sullivan, wrote a front page story of my paper in the *New York Times* the same day as the publication of the paper. I was in a dream world; when I read the *New York Times* piece, I knew the finding would change the field because it linked chemistry with climate, and it showed that we are changing climate in more than one dangerous way. But resistance was very strong and very few believed it. Ralph Cicerone was one of the reviewers of my paper, and I started working with him on the issue of non-CO_2 greenhouse gases a decade later. We were pioneering a new area of climate-chemistry interactions. We were just a handful of scientists at that time.

Was that also the period when you first met Paul Crutzen?

I still remember the day I met him. In fact, I always met him at nighttime, because that was when the NCAR computers were free. Only two or three people would be there in the basement and Paul was one of these. He was already a division director. For me, as an Indian, a division director is a big shot, but Paul would come to the basement and do actual work! He knew me because of my paper on CFCs. We talked and started to collaborate on writing a paper* that linked the increase in tropospheric ozone as a result of air pollution to climate.

*Fishman et al., 'Tropospheric ozone and climate', *Nature*, Vol. 282, 1979, pp. 818-820.

You ended up collaborating with Paul for decades. What makes for a good scientific collaboration?

I have met a lot of brilliant people in my life; at one time Chandrasekhar, the astrophysicist, was my neighbour. I also follow biographies of scientists. Of all the people I have met I consider Paul to be the most original mind. It is in my nature not to believe what I read; I have to figure it out for myself. Paul was also like that and so it was exciting to work with him. For instance, the whole INDOEX experiment got started with us meeting for lunch at Scripps in an outdoor café and bouncing ideas back and forth. In 30 minutes we had designed the whole experiment, which eventually attracted over 200 scientists from the US, Europe and India.

How did you come to study air pollution, dirty soot aerosols and black carbon?

Well before INDOEX, I led CEPEX, the Central Pacific Experiment. That is a story in itself. CEPEX followed my hypothesis of the existence of a Planetary Thermostat*. I called this hypothesis the Unified Theory, because it unified all meteorologists against it (laughs). After the publication of the hypothesis in *Nature* with my then post-doc student Bill Collins, there were so many papers attacking it, by Wallace, by Pierrehumbert, that *Nature* said that there were to be no more papers on Ramanathan's thermostat! The National Science Foundation, NSF, however, reasoned that if an idea can upset so many people, there might be something to it. There was a remarkable science administrator there, Jay Fein, who really cared about exciting science. He invited me to write a proposal for an experiment to test the hypothesis.

*Ramanathan V. and W. Collins, 'Thermodynamic regulation of ocean warming by cirrus clouds deduced from observations of the 1987 El Nino', *Nature*, Vol. 351, 1991, pp. 27 -32.

One of the reviewers of the proposal wrote that you could not have an experiment led by Ramanathan because 'he was a theoretical guy: he could not even distinguish an aircraft from a bird!' NSF then introduced me to Joachim Kuettner, a legendary German pilot who held the world record for altitude with a glider during the Second World War. He had migrated to the US, and had also helped the famous rocket scientist von Karman to escape. Kuettner became the project director for the Apollo moon landing programme. But his main interest remained in meteorology and turbulence because in his heart he was still a glider pilot. So he came to NCAR and directed all the major field experiments. He took me under his wing and became my mentor. Although I was 45 years old then, I became his student, and it gave me a backdoor entry into experimental work again.

How did the Planetary Thermostat work, according to your hypothesis?

A remarkable feature of the ocean is that it shows tremendous temperature gradients in time and space, but its average over a season never reaches a temperature of more than 28.5 degrees Celsius. The reason I got interested in that was because I felt that if I wanted to understand what the planet was going to be like when it heated up, I had to look at it where it was already hot. That took me to the Western Pacific warm pool: once you cross the date line you find this homogeneous pool of very warm water at 28.5 degrees, all the way from the Western Pacific to the Indian Ocean. I started wondering why the temperature did not go beyond 28.5 degrees and why the ocean did not become super hot. The conventional wisdom was that it was due to the evaporative cooling of the ocean: when the ocean warms, the evaporation of water from the sea surface increases exponentially, and as a result the water starts cooling rapidly. Since it was my habit not to believe what I read until I could see it for myself, I started looking at evaporation data and these did not confirm what most people believed. That is when I started looking for another explanation and stumbled onto cumulonimbus clouds. I knew that on a hot summer day, relief comes in the form these huge towering clouds. My work with satellite data had shown that the thick cirrus anvils from the cumulonimbus clouds reflect an enormous amount of sunlight. I went back to look at the warm pool with satellite data and it showed plenty of clouds being generated by the warm ocean. I used satellite radiation budget measurements and eventually claimed that the clouds were regulating the temperature: the warmer the ocean, the more clouds there were, the more sunlight was reflected, and the more cooling took place. This was a negative feedback such as that found in a thermostat. People were upset, because they were in the middle of a 15 million dollar experiment, TOGA CORE, to explain the warm pool as a solely dynamic phenomenon. They were not making any radiation measurements, and I was saying that everything was caused by radiation! So there were huge intellectual fights. I'm afraid I didn't know how to handle this and it became personal. It was my fault that it all became very emotional.

Did CEPEX eventually confirm your hypothesis?

I will give you a biased version of the story: my version. It was shown that the evaporation rate was lowest over the warmest part of the ocean, refuting the evaporative cooling hypothesis. We also showed that it was relatively dark over the ocean, even during the day, because of the clouds. In fact, sailors that travel in that area would tell you that too. Anyway, I didn't want to say that we had proved our hypothesis, I wanted somebody else to do that. Myself, I got distracted by another anomalous observation. And now we come to black carbon. We had measured that the solar radiation we observed at the sea surface was about 5% to 7% lower than predicted by theory or models. In other words, the sky over the sea surface was much darker than predicted. I became interested in this darkness, and knew that some so far unknown phenomenon in the atmosphere was absorbing the sunlight. I called the discrepancy between data and models the 'missing physics', but the community called it 'anomalous absorption' because my advisor showed that the discrepancy I observed over the warm pool was also observed in other locations on Earth; and so I went from one controversy to another (laughs). Discovering the cause of this anomalous absorption became an obsession of mine in the early to mid 1990s. It seemed that black carbon aerosols could explain that extra absorption. At that time I hadn't heard much about black carbon. I would have looked at the dynamic physical features of clouds, but Paul came in at this point and said that we needed to study pollution in order to understand black carbon. And that is how we got started with black carbon, brown clouds, the Indian Ocean experiment and all the way to the cooking stoves!

With INDOEX you came back to India, after nearly 30 years, to do actual research work. Had anything fundamentally changed since your time at the Indian Institute for Science?

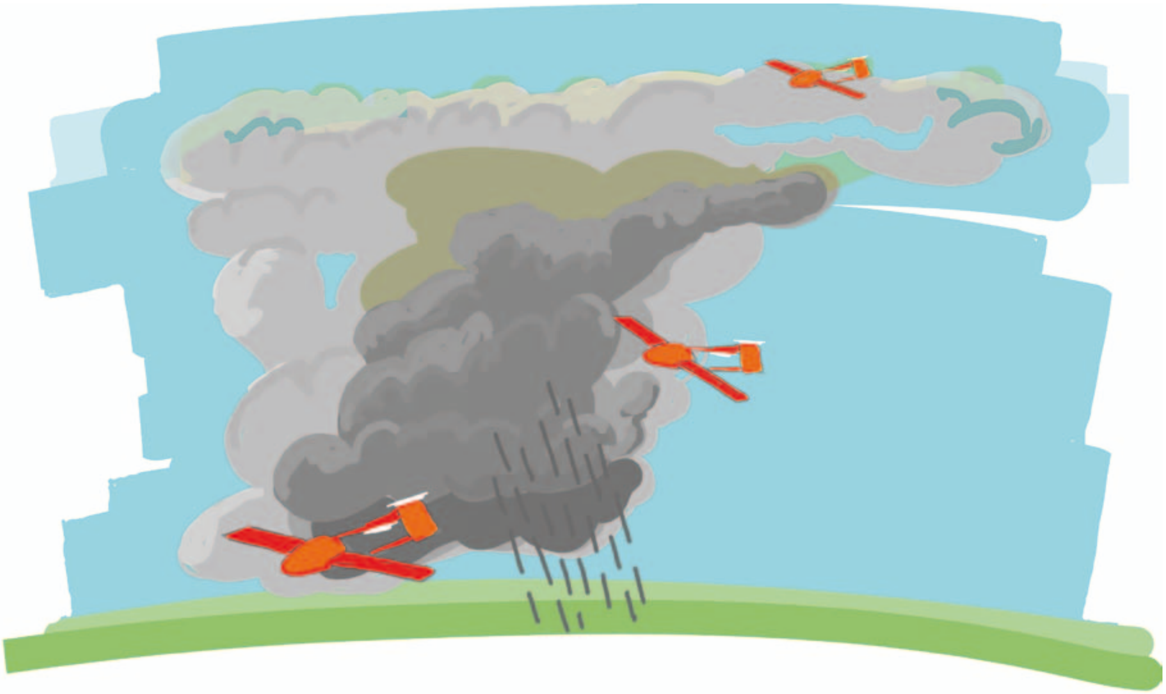
Indians had started to engage in more experimental work. Building that interferometer for my masters thesis was rather unusual, because it was not in the Indian culture to build things. This had also to do with poverty; there were simply no means available. By the time of INDOEX there was more money, but still, at least in the atmospheric science community, Indians would not build their own instruments. And the tendency is still a bit like that. I kind of feel sad about it. In science, you come up with a problem and you build an instrument to test your ideas. That culture is still not there in India. I think it is because of our tradition. We are not tool builders. In India, you still see women carrying bricks on their heads; they don't use wheelbarrows. The way of thinking is that if something works, leave it alone, don't try to improve it. You see, that is the difference between Eastern and Western culture; our American colleagues are never happy with what they have, they are constantly changing and improving their instruments.

Anyway, I knew INDOEX was going to be an expensive experiment, and I could not ask the US government to sponsor it for millions again. So, I was eager to find ways of doing things more cheaply. That is how I came to start working with small unmanned and remotely controlled aircraft, to replace the US research aircraft that cost ten thousand dollars an hour to fly. It took me seven years to install the appropriate equipment on these small airplanes, which have a wing span of only about four metres, and make it work. Luckily, my engineering skills hadn't left me.

I was eager to find ways to do things more cheaply, to replace the US research aircraft that cost ten thousand dollars an hour to fly. It took me 7 years.

I can well imagine that you took a childlike pleasure from flying these airplanes in formation in and out of clouds.

That is absolutely true. That work was among the most exciting things I ever did. These are fantastic toys to play with. I must mention the help of some talented experimental scientists in my group: Greg Roberts, Craig Corrigan and Muvva Ramana. Let me also add that INDOEX changed Indian atmospheric research tremendously. It opened the doors, and now India has some of the world's strongest teams in aerosol science.



Studying clouds with remotely controlled aircraft: a childlike pleasure.

Following INDOEX, you and Paul Crutzen set up what you first called the Asian Brown Cloud project. Because of that name you got into quite a lot of trouble with Indian politicians.

We hadn't immediately seen the problem. In one of my papers I had called it the Great Indo-Asian Haze, but Asian Brown Cloud gave a much nicer acronym (laughs). UNEP, who sponsors the project, had made a very successful press release about it which was picked up by Indian and Chinese politicians. They were worried that we were pointing the finger at them. As you know, under the Kyoto Protocol these countries are not asked to reduce their greenhouse gas emissions, and even today they are very fearful of having emission reduction targets. India still contributes only 5% of global greenhouse gas emissions, so the country is not contributing significantly to global warming. With the ABC project, we seemed to be saying that they did contribute through their air pollution. That was the issue.

Did the relationships improve afterwards?

We changed the name into 'Atmospheric Brown Clouds'. That helped and allowed us to keep our acronym. But more fundamentally, we showed with satellite data that large brown clouds were present in many parts of the planet, not just in Asia. So the argument with the politicians was put to rest. I am now part of the advisory committee of the Indian environment minister. The Surya project is of course also a way to set up relationships with the Indian government. At the moment they don't fund the project, but we keep them informed about our results.

You and others sometimes say that, in the face of the climate change problem, we cannot wait for the politicians. But how do you imagine that what you do today in one village can be implemented in the other 800 000 villages in India, without political support and a governmental programme?

You see, I first have to convince myself before I can convince others. We are getting our first year of data from that village. We find ten times more black carbon than what the models predict. So I feel I am on the right track: the signs are there and cutting emissions will bring about visible effects. We also work with medical doctors to look at the effects on people's health. We are now starting to work in an area of 100 square kilometres with a population of 50 000 people, so that we can see the effects of stove switching from space. I want to create a black carbon hole and show it to the world. Fortunately it will not take 10-15 years; we will finish it in two years. That is the beauty of black carbon; you see the effects of emission control measures immediately. I need to get such results first, and then we can go to the governments to convince them. So we have a plan, but it is a step-by-step process.

I first have to convince myself before I can convince others. I want to create a black carbon hole and show it to the world.

You know that one of the Millennium Development Goals is to reduce poverty by half between 2000 and 2015. We are in 2011 and we are nowhere near that target. I believe that the climate change issue and its link to local air pollution can change that. We have a fantastic opportunity to show that it can. We can measure the emissions of black carbon, the heating of the atmosphere, and show that this has an impact on regional and global climate, and on other environmental issues such as the melting of glaciers and the provision of drinking water. In the meantime we work together with the people who are affected, and we bring workable technology to the poorest. So far our experience with the involvement of local people has been nothing but positive. With all that information and expertise we can approach the governments. I hope that the results of the Surya project will convince others to set up similar projects in other parts of the world; Africa for instance.

Will this keep you busy for the rest of your life, or do you still have other ideas to pursue?

Demonstrating this in India will take another two or three years. I will be spending the rest of my life on actions that mitigate climate change; trying to find out what works and trying to get the message across through groups like yours.

What keeps you going from one project to the other?

Ideas keep coming. Most of the science I have done is not because I read books. I think hard about certain problems and suddenly an idea appears about how to solve it, and I go after it. As long as my mind is active, I won't quit. If I am not creating original ideas any longer then I will start reading books, I mean popular books. So what keeps me going is the excitement of coming up with a new idea.

Are you not afraid that you will run out of ideas?

My meditation helps me a lot. A lot of people think meditation is some sort of religious chanting. No, true meditation is about being quiet in the mind. The mind never rests, right? Even when you are sleeping, you are dreaming. I meditate almost every day, twenty minutes, not too long. Even for me it is impossible to sit quiet for longer! One way of meditating is to watch your mind, observe your thoughts, being conscious of the trivial thoughts up to the point of not generating any new ones. In my case I close my eyes. Immediately my mind starts racing, thinking about what I need to do tomorrow, the post-doc student I need to see, the guy I am upset with, etc., all these things come crowding in. The key thing is to be aware of them, and know that by getting stressed about them you are not solving anything, you are just making it worse. When you then continue the meditation, and that takes some practice, the thinking process stops, and what remains is the awareness, the state of being by which you look at yourself. It is a pleasant feeling; you are absolutely quiet within yourself. It is difficult to say how it works, but in my case it helps to sort things out, to separate the rational from the irrational. I still do irrational things, but at least I am aware. It helps me to see solutions from a distant perspective. It doesn't mean I have the solution, no, that still requires hard work afterwards.

Mm…

Shall we go back to the meeting, and finish some of that work?

(July, 2010, Ispra and San Diego; January, 2011, Geneva)

James Hansen

In January 2010 Jim was on his way to Italy where he would make a number of appearances to present the Italian edition of his book 'Storms of my Grandchildren'. On Monday he made a stop in the UK, to be a defense witness for climate change activists charged with trespassing on a coal mine near Nottingham. Their plan was to shut it down for a week and avoid the emissions of 15 000 tonnes of CO_2 *into the atmosphere. Jim told the court, 'the fact that we continue to burn more coal and build more coal plants shows that governments are not telling the truth. If they are saying they understand the climate problem but continue to burn coal, it is easy for me to understand that young people get upset, because they know governments are lying or kidding themselves'. He left the UK early on Tuesday morning, escaping a snowstorm, and flew to Milan. From there he was driven north of the Alps to the small town of Pontresina in the Swiss Engadin, where he was to give a talk in the evening. That same night, between Tuesday and Wednesday, he and his driver tried to cross the Alps back into Italy, but they got stuck in the same snowstorm that Jim had escaped in the UK, which was now sweeping across central Europe. They found a hotel along the road and checked in for the night.*
The next morning they continued to Ispra, north of Milan, where the Joint Research Centre is located. We had lunch together with several other JRC researchers and had a broad discussion about the scientific, moral and legal issues linked to climate change. After a strong double espresso, he gave his talk to some 150 scientists at the Centre. More than in Pontresina, Jim's driver told me, where only five people had turned up. After his talk I joined him in the car to Milan. Jim was tired, and he still had to give a radio interview that same night.

James Hansen (born 1941) is Director of NASA's Goddard Institute for Space Studies, New York, since 1981.

In 2009 he published the book 'Storms of My Grandchildren: The Truth About the Coming Climate Catastrophe and Our Last Chance to Save Humanity'.

WHY are you doing this, Jim?

You know, I have only been doing this for the past two to three years. When you introduced me to the audience this afternoon, you said that ever since I made my first climate change predictions, I was at the forefront of making the climate change problem clear to the greater public and policy makers. That is not totally true. For most of the past 30 years I have actually been getting on with science and staying out of the public sphere.

What happened three years ago?

A fundamental change occurred, beginning in 2006, when NASA tried to stop me from communicating to the public. I needed to react and become more vocal about the whole issue. In those days I was already asked why I was doing this. I did not immediately have an answer myself. But one day, when walking to an interview with '60 Minutes', I decided that the reason was that I didn't want my grandchildren to say that Opa (Granddad) knew what was happening but he didn't make it clear. But that was only the first step; it was something within the family, between me and my grandchildren. At that time I was still not thinking about the intergenerational injustice that human-induced climate change brought with it.
Everything became clearer when I started visiting other countries. The first was the United Kingdom, where I thought Tony Blair understood the climate change problem. He also had obvious contacts with George Bush, because he had supported George Bush in the Middle East. I was asked to give a public talk after which a dinner was held with a lot of powerful people. They wanted to know what they could do. I argued that they should try to convince the UK to phase out coal. This has obviously to be done on a global basis, but if it is not done in the UK, why would it be done anywhere else in the world? Over the years, the UK has produced more CO_2 per capita than any other country, with the US coming second and Germany third. So these people decided to write a letter to Tony Blair, and I helped them to write it, in which they opposed the building of a new coal power plant at Kingsnorth in Kent. I wasn't completely happy with the final draft of the letter because it didn't explain the situation as clearly as it could. So I wrote another letter to the Prime Minister myself and got an answer from the Environment Department. But it was the usual greenwash.

What do you mean by 'greenwash'?

It's this attitude of showing concern about climate change or the environment, and not taking the concrete measures needed to stabilise climate or protect the environment. It was and still is an attitude very much present in the US, but also in other countries, including the greenest among them.

I went to some five or six other countries. Each time I went optimistically but retuned disappointed. Except for China, which I visited just two weeks ago. There the decision makers listen to science, even when it is inconvenient. In the other countries I realised that the governments were not telling the truth. And they are still speaking greenwash. Their actions are not going to solve the climate change problem, because they are not phasing out coal. And that was when I started to become angry. The governments are cheating their people, especially their young people. That is when this idea of intergenerational justice got a hold of me.

Then I felt like I had to do something. Because once you become aware of that injustice, then unfortunately it is difficult to decline certain requests. Young people had been blocking the operations at Kingsnorth, and were being threatened with being sent to prison, and two years ago I had to go and testify on their behalf. On my way here to Italy I did the same. Because here are a hundred young people protesting against another coal power plant and basically standing up for their future, and they are being threatened with being sent to prison!

The transformation of my perspective on that whole issue was quite slow. All this should have happened much earlier, but it only came about in the past two or three years. I am afflicted with a lot of patience in dealing with this, but it is patience that is being consumed.

The climate issue is not properly understood: the difference between weather and climate, the inertia of the climate system which hides the warming to come, the existence of tipping points which means that one day we might lose control. At least the young generation must understand this!

You now want to take it a step further and sue the governments for not doing their job in protecting the people and the planet. Aren't you concerned that, given the uncertainties that still exist, the legal system will even further polarise the existing knowledge about climate change? It will no longer be about sceptics and scientists, but about defense and prosecution. Will people not become more confused than they already are today?

Legal action will not solve the problem but it will raise public awareness. It will force the courts and possibly even the Supreme Court to involve scientists with proven expertise; the National Academy of Sciences and so on. The courts are much less politicised than Congress, and I believe they can easily make a distinction between how the scientific community is building its knowledge base and how others are doing so.

It looks like there will be a high level of uncertainty in climate sciences for the next decade or so. It will therefore be difficult to make decisions, even for the Supreme Court.

But we continue to reduce that uncertainty! We know from empirical evidence, looking at the differences in climate during ice ages and interglacials, that climate sensitivity is such that a doubling of CO_2 leads to an increase in temperature in the range of 2.5 to 3.5 degrees Celsius.

Why is the IPCC still talking about a range of 2 to 4.5 degrees?

They base that range on general circulation models, which have a hard time dealing with the many feedback processes that link a temperature change back to its forcing and make it weaker or stronger; processes related to water vapour, clouds, snow and ice. These are all very difficult to describe, and there is still a need for dedicated research and observation programmes to get a better understanding of each of them. The climate sensitivity derived only from models remains quite uncertain. On the contrary, the empirical determination includes all these processes. I don't know why the IPCC still accepts such large uncertainty. I am not part of the IPCC (smiles).

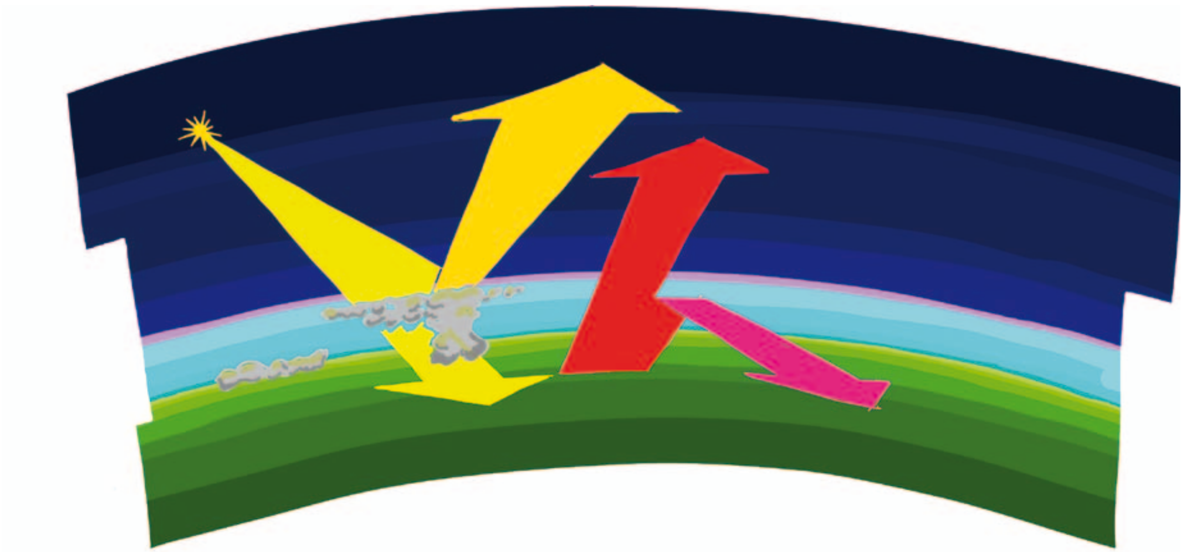
You started as a planetary scientist, studying Venus. Then you switched to studying the climate of the Earth. Did you have an intuition that something could go wrong with our planet?

Planetary science was in fact very exciting in the seventies. The US had just started to send missions to various planets. As a young scientist I could even propose to NASA that they undertake a mission to Venus! That proposal was accepted, but it took five years for some aerospace company in California to build the instrument. So while waiting for that, I got interested in that emerging realisation that man-made gases emitted into the atmosphere could affect the Earth's ozone layer. I proposed to NASA, in 1976, that they start a modelling programme and study the possible relationships between the composition of the atmosphere and climate. I hired a young chemist, Y. L. Yung, who suggested we should look at greenhouse gases other than CO_2, such as NO_2 and methane. Ramanathan had already discovered the role of CFCs. We added them all up and wrote a paper in 1976* saying that the greenhouse effect of the sum of these

*Wang W.C. et al., 'Greenhouse effects due to man-made perturbations of trace gases', *Science*, Vol. 194, 1976, 685-690.

I realised that a planet whose atmosphere is changing before our eyes is in many ways more exciting than a planet that, although somewhat mysterious, is millions of miles away.

other gases was comparable to that of CO_2. That's when I really became interested, because I realised that a planet whose atmosphere is changing right before our eyes is in many ways more exciting than a planet that, although somewhat mysterious, is millions of miles away.



The Earth's radiation balance.

We started to build a climate model, starting from the weather model developed at the University of California, Los Angeles. UCLA was different from the two other big groups – NASA's Geophysical Fluid Dynamics Laboratory and the National Center for Atmospheric Research. UCLA was just a university, they didn't have the computing capacity for large scale modelling and so they were very generous in sharing their computer codes. We imported that model into our lab and I and Andy Lacis supplied the radiation code. My interest was not in the weather, but in climate, so I decided to take the model and make it faster, so that we could run it for a hundred years. I asked NASA for the support to make a model with a coarser geographical resolution but including the necessary physics that were not included in the weather model. That was in 1976, and by 1979 we were already in a position to contribute to Jule Charney's famous study on climate sensitivity. Suki Manabe had already run studies to look into the effect of doubling CO_2, and we were able to do the same. Manabe was predicting that a 2-degree temperature increase would result from a doubling of CO_2 but we found that this would lead to a temperature increase of four degrees. So Charney concluded that the best estimate was three degrees (laughs). Our present estimate is still in very good agreement with that!

Did you immediately see the social implication of such a large sensitivity of the climate system?

That didn't take long. While we were developing the general circulation model, I became very eager to get answers, and I could not wait for the general circulation model be able to run scenarios. So I started building a simple one-dimensional radiative transfer model which I used for our 1981 paper in *Science*, in which we presented our first climate predictions.

That paper reads like a small IPCC study 'avant la lettre', written not by two thousand scientists but by just seven!

Yeah! In that paper we really looked at the whole problem, including the implications, and concluded that we would have to phase out coal! (Laughs). The global warming that we calculated with our simple model was consistent with what had been observed so far. In fact, I had to put together a global dataset of temperature measurements for the past century. Most papers then had used only measurements in the northern hemisphere, but I knew from my planetary experience that the few measurement records in the southern hemisphere would be enough to calculate a meaningful measure of *global* temperature change on decadal time scales. That also went into that paper.

How did you deal with the many uncertainties involved?

The model we used included a climate sensitivity of 2.8 degrees Celsius for a doubling of CO_2. We realised we didn't have a very accurate knowledge of climate sensitivity, so we performed calculations with the sensitivity half as large and twice as large. In addition, there was uncertainty about how rapidly heat was mixing into the ocean, so we used a range of values for this mixing rate. We also didn't have very accurate figures about how much carbon there was in the coal, oil and gas reservoirs, so we also used a range for that. But with those ranges and assumptions we were able to project what the global temperature response would be for certain emission scenarios. We examined a 'coal phase out' compared to using all the coal, and, even when considering all the uncertainties, these two scenarios predicted very distinct climates. So

Climate Impact of Increasing Atmospheric Carbon Dioxide

J. Hansen, D. Johnson, A. Lacis, S. Lebedeff
P. Lee, D. Rind, G. Russell

Summary. The global temperature rose by 0.2°C between the middle 1960's and 1980, yielding a warming of 0.4°C in the past century. This temperature increase is consistent with the calculated greenhouse effect due to measured increases of atmospheric carbon dioxide. Variations of volcanic aerosols and possibly solar luminosity appear to be primary causes of observed fluctuations about the mean trend of increasing temperature. It is shown that the anthropogenic carbon dioxide warming should emerge from the noise level of natural climate variability by the end of the century, and there is a high probability of warming in the 1980's. Potential effects on climate in the 21st century include the creation of drought-prone regions in North America and central Asia as part of a shifting of climatic zones, erosion of the West Antarctic ice sheet with a consequent worldwide rise in sea level, and opening of the fabled Northwest Passage.

Atmospheric CO_2 increased from 280 to 300 parts per million in 1880 to 335 to 340 ppm in 1980 (*1*, *2*), mainly due to burning of fossil fuels. Deforestation and changes in biosphere growth may also

The major difficulty in accepting this theory has been the absence of observed warming coincident with the historic CO_2 increase. In fact, the temperature in the Northern Hemisphere decreased by

$$\pi R^2(1 - A)S_0 = 4\pi R^2\sigma T_e \quad (1)$$

or

$$T_e = [S_0(1 - A)/4\sigma]^{1/4} \quad (2)$$

where R is the radius of the earth, A the albedo of the earth, S_0 the flux of solar radiation, and σ the Stefan-Boltzmann constant. For $A \sim 0.3$ and $S_0 = 1367$ watts per square meter, this yields $T_e \sim 255$ K.

The mean surface temperature is $T_s \sim 288$ K. The excess, $T_s - T_e$, is the greenhouse effect of gases and clouds, which cause the mean radiating level to be above the surface. An estimate of the greenhouse warming is

$$T_s \sim T_e + \Gamma H \quad (3)$$

where H is the flux-weighted mean altitude of the emission to space and Γ is the mean temperature gradient (lapse rate) between the surface and H. The earth's troposphere is sufficiently opaque in the infrared that the purely radiative vertical temperature gradient is convectively unstable, giving rise to atmospheric motions that contribute to vertical transport of heat and result in $\Gamma \sim 5°$ to 6°C per kilometer. The mean lapse rate is less than the dry adiabatic value because of

Science, Vol. 213, 1981, pp. 957-966.

already we were looking at policy implications. Reading back on what we said thirty years ago … well, it was fairly comprehensive.

You mention in your book, 'Storms of My Grandchildren*', that you had thought of providing scientific results to policy makers so that they could make decisions on a rational basis, but that this approach had been disastrous. Nowadays, people would say that if you deal with a problem which has high stakes but is surrounded by uncertainties, you have to involve policy makers and stakeholders in general from the beginning. In this way you might come to an outcome which is both scientifically and socially robust. Do you agree with that? Would you carry out your research and your interactions with stakeholders any differently?

*James Hansen, *Storms of my Grandchildren: the truth about the coming climate catastrophe and our last chance to save humanity,* Bloomsbury USA, 2009.

I don't think I would have done it very differently, given the circumstances of that time. Take the example of the stratospheric ozone problem. Governments did a very good job of responding

Governments didn't act simply on the basis of scientific information; sometimes they simply didn't want to know.

to the scientific information available at the time. All that scientists did was explain the consequences of alternative policies to deal with the problem. The governments implemented the right ones and phased out CFCs. That doesn't mean that the research on stratospheric ozone was carried out in a smooth way. There were in fact two intense periods of research in the US. First, when it turned out that supersonic aircraft could affect stratospheric ozone, and second, when it was discovered that CFCs were affecting ozone. Some top scientists argued that we really should not carry out research to respond to problems. We need to invest in basic knowledge so that we can provide information to the government when it is needed. One of these scientists was Don Hunten, who had a big influence on me, and I respected his opinion. So that became my way of thinking and operating: keeping the focus on getting a better understanding of the world, with the presumption that governments would use that information wisely. I didn't question that at the time, but five years later I already had some reasons for doubt, because after writing our 1981 paper we lost the financial support of the Department of Energy. At that time the US administration had become conservative and did not want the business community to be adversely affected by any policy. I was a junior researcher and was not in a position to determine the overall science policy. It was not until the past five years, when I tried to inform governments, that I realised that they didn't act simply on the basis of scientific information, but that sometimes they simply didn't *want* to know.

Despite this, or maybe because of this, you testified before US Congress in 1988 and again in 1989.

Yes, and I got quite some publicity. But in 1988 I hadn't been totally clear about the climate change issue. I left the impression that it was all a matter of heat waves and droughts, while I actually wanted to explain that through global warming there also would be an intensification of the hydrological cycle, and that extremes, droughts *and* heavy rainfall would get even worse. But that message also got lost in my 1989 testimony, organised by Al Gore. The fact that the summer of 1988 had been extremely dry undoubtedly played a role in what the public wanted to hear. Heavy rains and floods occurred in the Midwest only four years later. Anyway, I decided to leave the public speaking to others, and for 15 years I was not involved in the public aspects of the debate. Even though I started showing a photo of my first grandchild in my scientific lectures, it didn't have any impact on how I was proceeding: I stayed focused on the research. It was not until 2004, before the elections, that I decided to give a public talk again. I was careful not to be associated to a political party, but I still wanted to give a public talk because I had become aware that there was a big gap between what science related about climate change and what the public knew about it. I wanted to make clear that the matter was urgent and that the Bush Administration was not addressing it. Frank Loy, chief climate negotiator under the Clinton-Gore administration, said he would arrange for an opportunity for me to convey my message. If I would prepare a careful talk then he would host it at an event of an organisation of which he was a board member, and make sure I got television and press coverage. I spent several months preparing this talk and the scientific papers to back it up. The most important of these was the 'Earth's energy imbalance' paper that we eventually published in *Science* in 2005*; it sort of proves that greenhouse gases play the major role in global warming. But when I called Frank and told him I was ready, his organisation had changed its mind. They said the talk had to be after the elections (laughs). As a last resort I called Professor Van Allen, my former professor and supervisor at the University of Iowa, who arranged for a so-called 'distinguished lecture'. But being held at the University of Iowa, it didn't have a lot of impact.

*Hansen J. et. al., 'Earth's energy imbalance: confirmation and implication', *Science*, Vol. 308, 2005, pp. 1431-1435.

You went on giving public talks; some very visible ones at the conferences of the American Geophysical Union. You became a public figure.

And that is when NASA tried to stop my outreach initiatives. But, you know, I have always thought that when you have relevant science you should not be shy about making it available to the public. However, I didn't pursue that on a regular daily or yearly basis, like Steve Schneider for instance. I was not interested in doing that since it requires a significant part of your time. I preferred to do science. Anyway, already in 1981 when I wrote that *Science* paper, I sent a pre-print to the science writer for the *New York Times*, Walter Sullivan, and he ended up writing an article about it on the front page. After the *New York Times* article, Rafe Pomerance, an environmentalist, arranged for me to testify before Congress in 1988. That is how it went. I even called up Rafe myself, the day before my testimony, saying that I would make some strong statements and that he should arrange for some media coverage (laughs). So it's not that I thought such things shouldn't be done. Only that I was maybe not the best person to do so.

What has been your most pleasant experience as a public figure?

The nicest experience is when what you say gets reported well and seems to have some relevance. Walter Sullivan was an excellent science writer, and his article in the *New York Times* was really well written. Several days later there were lead editorials in the *New York Times* and the *Washington Post*, both of which said the climate issue needed to be considered when we talk about energy policies; it is no longer a speculation, this is serious business. What may have annoyed some people is that *NASA scientists* were saying this: that gave the whole issue some credibility.

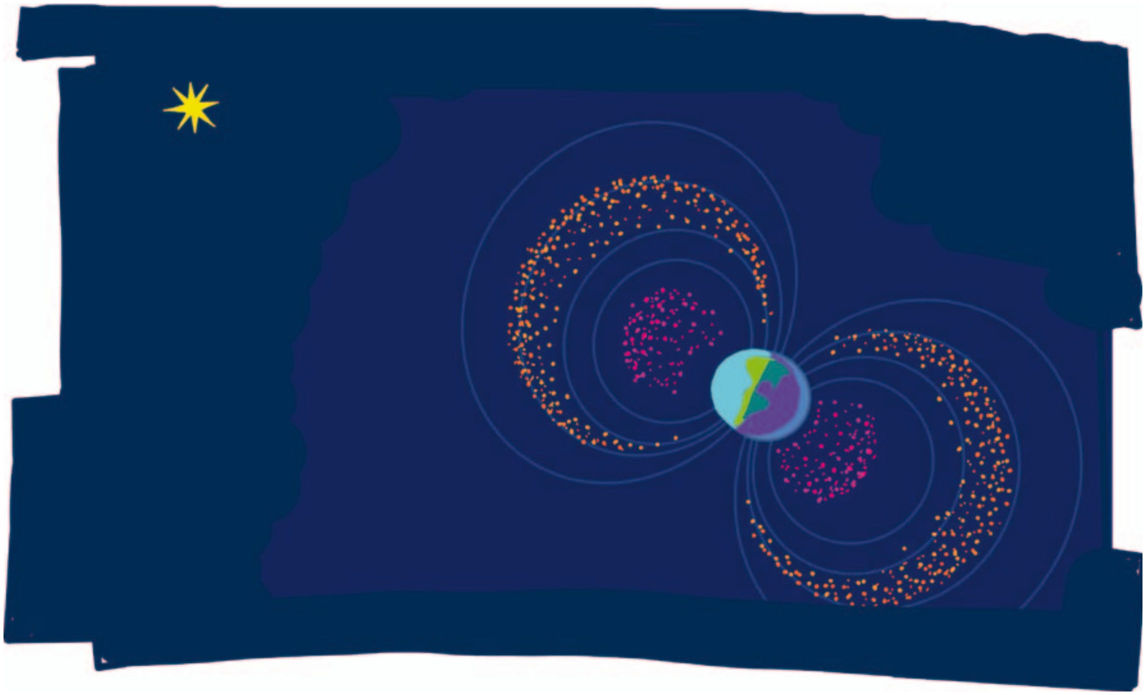
The worst experience?

(Thinks for a while.) The worst experience is of course the complete misunderstanding by the public. The fact that things are really going backwards. Thanks to the efforts of those who prefer to continue business as usual, a significant fraction of the public became convinced that scientists are not to be trusted. This is really extremely disappointing. Because, first of all, the public generally has great respect for scientists; the polls would show that. And of course, what the deniers were saying was such complete nonsense. They argued that scientists were fabricating the issue of climate change in order to get more research money. Well, my case was a good example of how completely nonsensical that was! Once the Department of Energy came under a Republican administration, they cut off our funding for carbon dioxide and climate research.

Did you always want to become a scientist when you were a kid?

No, not when I was a kid. I did not grow up in an academic or intellectual environment; I grew up on a farm. We moved into town when I was four years old. My father became a bartender and later a janitor. My parents separated when I was ten. So I was pretty much on my own. I was not a particularly good student, I would rather play baseball and basketball. But I always got a high score in mathematics and physics in the state examinations. As a kid I delivered newspapers and then I became a newspaper distributor where I had newspaper boys working for me. I saved money so that I could go to college. My great good fortune was that I was in a place where the state supported public education, so it was possible to go to college even if you didn't have a lot of money. And, by a stroke of luck, I went to the university where the physics and astronomy department was headed by James Van Allen. He had just discovered the radiation belts around the Earth that were later named after him. There was a wonderful research

The Van Allen belts of charged particles. The inner belt extends from as low as 200 km up to 15 000 km above the Earth's surface. It is caused by energetic solar radiation that ionises the molecules in the uppermost layer of the atmosphere, and the trapping of these ions in the Earth's magnetic field.



environment in his department; they were building instruments in the basement to measure properties of the Earth's upper atmosphere, that they would then put on rockets and the first US satellites. I got totally wrapped up in this environment.

Could you have become something else other than a scientist?

Well, I majored in mathematics and physics as undergraduate, but I didn't know what this really meant for a future career. So it was all rather open, until I got involved in Van Allen's department.

May I ask again what motivates you in your work?

What drives me is what drove Richard Feynman, namely the pleasure of finding things out. Science is really interesting, and that is something I didn't realise as a student. The fact that I could look from Earth at the dust particles in the atmosphere of Venus and derive their refractive index, their size and shape; that was remarkable. Studying and trying to get good grades is one thing, but realising that you can find things out that nobody knew before is amazing. Science allows you to do that!

Feynman talks a lot about scientific integrity. Is it this integrity that you now bring to your public activities?

The moral aspects came much later. In the beginning it was just the pleasure of finding things out. And even today I am fascinated by the paleoclimatologic records, for instance. I know there is more information there and I still hope to look into it because I have some ideas on how to model certain things.

Thanks a lot Jim. Have a good night.

(December, 2010, Ispra – Milan)

Mario Molina

For the past 20 years, the Massachusetts Institute of Technology regularly brings together representatives from science, government and industry that have an interest in climate change. The discussions held are open and frank, and are useful for understanding the different perspectives on climate change that exist in the various quarters of society. They follow the 'Chatham House Rule', which states that one can quote what is said at the meeting without mentioning who said it. In what follows I might break this rule slightly, but hope to be forgiven.

At the 32nd of these meetings, Mario Molina gave the keynote lecture at the opening dinner. I met him the evening before in the lobby of our hotel, where he was using the hotel's PC and internet connection. I introduced myself, saying that we had met, fifteen years ago, at a Gordon Conference. Professor Molina kindly listened when I told him about the interviews I was doing regarding science and policy, and he agreed to have a conversation about his experience. We tried to find a time in his busy agenda, and settled on lunchtime of the last day. He had to catch a flight to Mexico that afternoon, so it didn't look very likely that it would work out.

Professor Molina's keynote lecture was more or less a repeat of his opening speech at the UNFCCC climate talks in Cancun. It conveyed his belief in the value of scientific research, the importance of universal ethical values, and his optimism, which he called a long term optimism, that mankind would eventually be able to control climate change.

Our lunch conversation came after a session in which two American analysts, one a former CIA Director, explained the state of affairs in US climate change politics. 'There is so little willingness in the US to reduce the emissions of greenhouse gases and mitigate climate change, that I now focus my research on adaptation and geo-engineering', one said.

Mario Molina (born 1943) received, together with Paul Crutzen and Sherwood Rowland, the Nobel Prize for Chemistry in 1995.

He has been a professor at the University of California, San Diego since 2004.

THAT was a daunting session, wasn't it?

Well, the mood about whether or not something can be done about climate change is like a pendulum. We are now at an extreme position of the pendulum, and there is no other way to go than to swing back again.

How long will it take for the pendulum to swing back?

We might not have to wait the usual five or ten years if we can point to the things that are slowing us down. One is the denial of the climate change problem by the Republicans in the US Congress. They are denying the underlying science of the problem, and that is in fact their weak point. Climate science is based on the laws of physics, discovered by people like Planck, Einstein and Boltzmann. There is no way you can dismiss these fundamental laws, and I believe that we can be clever enough to make that clear. But yes, I have to admit that there is a fine line between such optimism and wishful thinking.

You are usually portrayed as a gentle and calm man. Still, when I read your Nobel autobiography, I get the impression that you are also a restless man.

That's right!

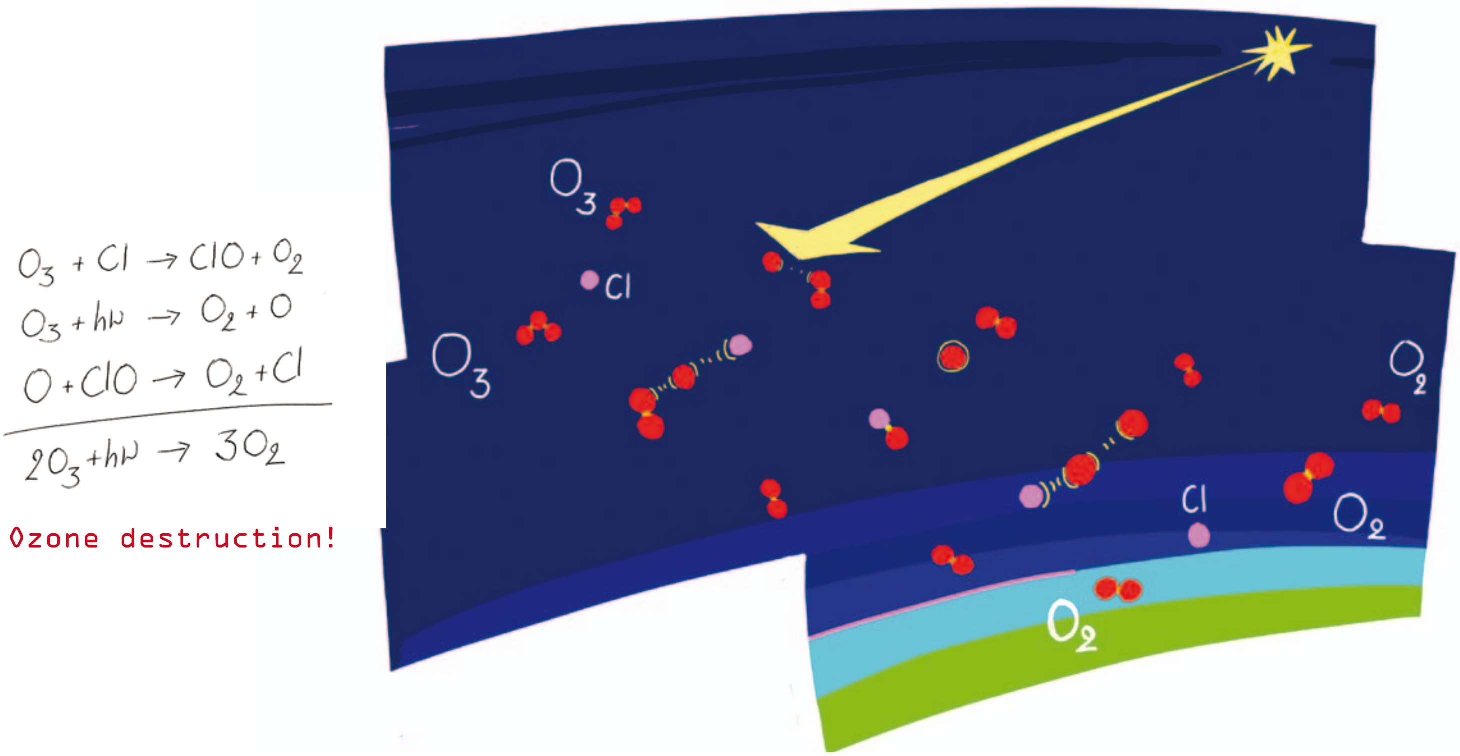
You always wanted to become a chemist, but it came about in a rather unconventional way: you attended various schools throughout the world, and you dropped out of your PhD research in Germany to go back and study the basics in Berkeley. You interrupted an academic career in Irvine to go back to the lab at NASA's Jet Propulsion Laboratory, and came back later to teach at the Massachusetts Institute of Technology. What motivation was driving you?

I haven't really analysed that. That's just how it came about. I always wanted to do something that would have a positive impact. There was always some mix of wanting to move science forward combined with a sense of social responsibility. I felt that adding value would be rewarding, even though you might have a hard time getting there, and that you had to be patient and persistent. The social responsibility component was enhanced by winning the Nobel Prize, which confers more power to convince people. You have to make good use of such power.

Paul Crutzen mentions that he picked stratospheric ozone as his PhD subject 'without the slightest idea of what lay ahead'*. The awareness of social and policy implications came later. Was it the same for you?

*see p 28.

Paul started working on natural processes that regulated stratospheric ozone: the role of nitrogen oxides. We were looking at the effect of man-made CFCs that were being emitted into the atmosphere. We looked at the purely scientific question, out of curiosity so to speak, as to what the fate of these CFCs would be. But from the very beginning we also realised, even if rather vaguely, that humans were doing something to the atmosphere and it was normal for us to try to understand what the possible consequences could be and whether they could be significant. In fact, there were other groups at that time, sponsored by industry, which had studied the same problem and had come to the conclusion that these emissions didn't matter at all.



The realisation that chlorine chemistry acts much faster than nitric oxide chemistry showed that we were possibly dealing with a major human disturbance of the stratospheric ozone layer.

What made the difference in your work?

We really focused on the basics. We extended measurements of the absorption spectra of some of the CFCs in the deep ultraviolet spectral region. We needed these data to calculate the efficiency with which such UV radiation, present only in the stratosphere and above, could split a chlorine atom from a CFC molecule. Certain catalytic reactions led to one such Cl atom destroying many ozone molecules. We carefully considered the chemical reactions involved. It was important to integrate such fundamental data with what was known about the industrial production rates of CFCs. In this way we estimated the global production rate of Cl atoms, and found that it was of the same order as the estimated natural production of nitric oxide molecules in the stratosphere. This, together with the realisation that chlorine chemistry acts much faster than nitric oxide chemistry, showed that we were possibly dealing with a major human disturbance of the stratospheric ozone layer.

You and Sherry Rowland also communicated the CFC-Ozone theory to policy makers. Did you like doing that?

I felt uncomfortable about it in the beginning. In those days you could easily become discredited by your colleagues if the only goal you were aiming for was to be newsworthy. There had been examples of scientists presenting their results to the New York Times before sending them to a scientific journal, and that was not at all acceptable. So we realised that we ran the risk of being seen as trouble makers in the community. But somehow we felt that this was an important issue. The way Sherry and I put it was that if we don't tell people about it, who will? If not now, when? In those days there were no environmental pressure groups that would take that job over from you, because not many environmental problems had been identified yet.

Apparently there was less of a communication problem than there is today concerning climate change. Was that because the problem was scientifically simpler and the solutions easier to find?

No! But that is often how people tell it. It took a lot of time and effort to make the science understandable. There have been very frustrating moments. For at least a decade it looked like nothing was going to be done by the decision makers. We had not anticipated all the barriers that were placed in our way. There were moments of despair but we still kept working. Sherry and I spent a lot of time with writers and people from the news media to make the story clear. It

Stratospheric sink for chlorofluoromethanes: chlorine atomc-atalysed destruction of ozone

Mario J. Molina & F. S. Rowland

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Chlorofluoromethanes are being added to the environment in steadily increasing amounts. These compounds are chemically inert and may remain in the atmosphere for 40–150 years, and concentrations can be expected to reach 10 to 30 times present levels. Photodissociation of the chlorofluoromethanes in the stratosphere produces significant amounts of chlorine atoms, and leads to the destruction of atmospheric ozone.

photolytic dissociation to $CFCl_2 + Cl$ and to $CF_2Cl + Cl$, respectively, at altitudes of 20–40 km. Each of the reactions creates two odd-electron species—one Cl atom and one free radical. The dissociated chlorofluoromethanes can be traced to their ultimate sinks. An extensive catalytic chain reaction leading to the net destruction of O_3 and O occurs in the stratosphere:

$$Cl + O_3 \rightarrow ClO + O_2 \quad (1)$$

$$ClO + O \rightarrow Cl + O_2 \quad (2)$$

This has important chemical consequences. Under most conditions in the Earth's atmospheric ozone layer, (2) is the slower of the reactions because there is a much lower concen-

Nature, Vol. 249, 1974, pp. 810-812.

all looks very simple now, but think about it; how can you explain in a credible way that invisible gases are attacking an invisible layer high up in the atmosphere that shields us from invisible rays, and why should that matter, even decades from now. It is not very different from explaining how emissions of carbon dioxide will have impacts on climate twenty or fifty years from now.

Was the status of science different from what it is today?

At least there was no fight *against* the science, like there is now with climate change. And because of that, it was easier to talk to decision makers. In the beginning we had to seek them out ourselves, but later decision makers would come to us.

Today, many people in the US Congress do not even want to listen to scientific arguments. Research shows that only 12% of Americans would advocate against climate policies, but in Congress more than 50% will! It is true that Congressmen are elected officials, but they are elected for their views on employment and security, not on climate change. The status of science must certainly be restored amongst those people.

When did you start thinking about the air pollution problem in megacities? Moving from the stratosphere to the streets of Mexico City seems like a big jump.

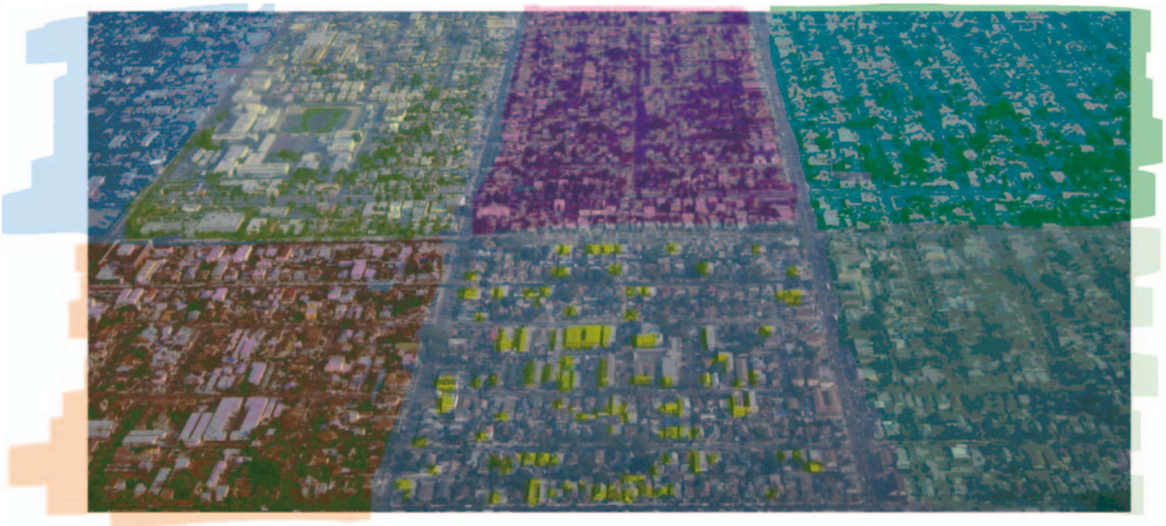
Initially my research group carried out fundamental work on atmospheric chemistry. We engaged in a lot of gas phase kinetics, and we also started to look at the role of aerosol particles. There were a lot of very interesting scientific questions, and many of these were related to substances emitted by man. So looking at air pollution was a natural application of the basic work we were doing. At some point, after being awarded the Nobel Prize, I and a group of MIT colleagues decided to focus on a single problem that required an interdisciplinary approach. I chose air pollution in Mexico City as a typical example of a large city in the developing world. During the nineties Mexico City had become extremely polluted. It was a repeat of what had happened in Los Angeles during the sixties. The air pollution was intolerable. Catalytic convertors were introduced as a result of public pressure. However, the levels of pollution kept increasing because of the increasing number of cars. That was the situation when we came in.

What were the challenges and benefits of your interdisciplinary approach?

From the very beginning our work was organised in such a way that we broke out of our silos in order to work together. Atmospheric chemistry dominated initially but we readily expanded to focus on, for instance, health effects, bringing in people from Harvard, and all the way to technical solutions and public policy. We had the example of the loss of stratospheric ozone. If we could explain the air pollution problem *and* the solutions to overcome it, we would have access to relevant decision makers. And we were quite successful. It helped of course that I am Mexican and knew a lot of people in Mexico, and had won the Nobel Prize. The integrated nature of the project forced the scientists to talk across the barriers of their discipline and also to communicate with non-scientists such as policy makers. We were also very aware of the importance of including Mexican students and scientists and eventually the local legislators.

One might wonder why you had to set up an expensive study in order to solve air pollution in Mexico City. Couldn't you just learn from the Los Angeles case?

Of course, it was straightforward to identify where 80% of the pollution came from, but it was not immediately clear how to tackle the remaining 20%, which is still a lot of pollution! We carried out two major field campaigns involving scientists from the US and Europe. Mexico City, with its pollution, offered a nice natural laboratory in which we could learn about atmospheric



Mexico City or Los Angeles?

chemistry in extreme conditions, and a lot of scientists came for that. But the results were also useful for designing better policies. In the end you have to understand the relationships between emissions of pollutants such as nitrogen oxides and the levels of ozone. We talk about tropospheric ozone now. This relationship is very non-linear and depends on the local conditions. You can have a situation in which you reduce nitrogen oxide emissions and initially *increase* the ozone production rate! In order to avoid that, you should also reduce emissions of organic gaseous substances which are not solely related to traffic. So you need to do some work locally to understand the particular situation you are dealing with. Initially the focus was on gas phase chemistry leading to ozone formation, but during the years of measurements in Mexico City scientists learned more about particulate matter (PM) which, according to our Harvard colleagues, is even more damaging to health. So we started looking for the sources of PM and how it could be controlled.

How much of the effort went into atmospheric chemistry research and how much into socio-economic research?

The socio-economic aspects, the technical solutions and so on, were funded by Mexico. For the field campaigns we had some funding from Mexico for their own scientists, but most of them were funded by the international research community. We just invited people that already had their own research funds, and it quickly became a multimillion dollar effort. We coordinated all this and convened meetings to interact with the policy makers and in the end we made policy recommendations. It was pretty much a seamless transition from the scientific findings to making policy recommendations.

Are there similarities between tackling the global climate problem and tackling a local air pollution problem? I mean when it comes to engaging with policy makers and politicians?

In both cases you have to identify and convey the risks involved. This is built into the work of the health research community, the epidemiologists. They have a well defined methodology for calculating the statistics and the probability of increased mortality given a certain increase in the levels of particulate matter, for instance. We have to expand these methods to climate change. Of course we cannot carry out an epidemiological study of the Earth, there is only one after all. But we can come up with a range of scenarios and identify the risks related to each.

Identifying risks goes one step further than assigning probabilities. What is defined as a risk depends on your value system; it becomes political. Have you, as a scientist, had any problems in taking that extra step?

You need to clearly communicate to the decision makers what is known. This is different from what we do at our scientific meetings where we focus on what we don't know.

No. We know what the role of science is, but we should not wait until the science is totally unambiguous. You need to clearly communicate to the decision makers the nature of the science and especially what is known. This is different from what we do at our scientific meetings where we focus on the uncertainties, on what we don't know, because what we know is taken for granted. So far, climate scientists have failed to communicate the issue of risk related to global warming, especially the risk of extreme and high impact events, even if the probability of their occurrence is low. Talking about risk involves making a value judgment, and hence it becomes advocacy. Scientists have to take that step, but also need to be open about their value systems.

I was surprised when you said in your talk last night that we should not mix the climate change issue with other issues such as food security, biodiversity, poverty, etc.

Let me clarify that. Of course we have to worry about all these problems. That was the point of the meeting we recently had in Stockholm with about twenty Nobel Prize winners. We wanted to communicate the fact that we are at risk because of the many things we are doing to the planet, and that society has to change in order to leave a world that future generations can live in. To communicate this, you need to talk about all the problems and how they are interlinked. But that complexity should not stand in the way of doing something concrete. My point yesterday was that climate change is a reasonably well defined problem in itself. It is a huge problem but it can be handled in stages. We can start by dealing with carbon dioxide on its own. We know how to reduce its emissions by putting a price on them. So this is a step we should take.

Reducing CO_2 emissions would also help to solve these other problems, not just climate change.

Of course, and you can make that part of your strategy in order to get things done. But the focus of the strategy should remain on climate. It is certainly OK to bring in the non-CO_2 climate forcers such as methane and black carbon aerosols and talk about the impacts of their reductions on air quality and human health. But I doubt whether it is a good idea to go to the other extreme and link climate change to such an important but poorly defined problem as poverty.

What do you think of the UN Framework Convention on Climate Change as a vehicle for solving the climate change problem?

It is a very inefficient and poorly designed process, because it has allowed all sorts of discussions to take place: development, food security, technology, etc. You cannot possibly solve all of these issues with the whole world sitting around the negotiation table. The strategy should be to split the problem of sustainable development into parts, to separate even the climate change problem into stages and start doing something about them one by one.

We just heard that more science will not improve policy making. Do you agree?

More science is indeed no guarantee of increasing people's sensibility regarding climate change. The reduction of uncertainties should in principle make decision making easier, but there will still be uncertainties after many years of further research. My point is that we have come to a point today where we need to better communicate to key decision makers that the science is not as uncertain as they think. These people are biased by the normal scientific reasoning that you have to go through the numbers for years before you can say something with certainty, or they are biased by the very effective publicity by lobbying groups that have an interest in questioning climate science. At the furthest extreme, some people in the US Congress even question the very essence of science! So we scientists have to speak up. I would even go as far as suggesting that we should employ public relations people. That is what the other side has done forcefully and with success.

We scientists have to speak up. I would even suggest that we should employ public relations people. That is what the other side has done with success.

Doing it this way, isn't there the risk that the scientific community would be seen as just another stakeholder in the decision process? Scientists have already been accused of exaggerating the problem in order to get more funding.

No. This is a basic misconception, which is in fact fuel for the deniers, namely that scientists are motivated to act in the same way as anybody else. But scientists cannot freely motivate their research, nor can they have opinions about how the world functions. Whether the law of gravity is true or not does not depend on whether you want it to be true or not in order to enhance your chances for funding. The law of gravity is just there! There are sociologists who question the certainty in science, because, they say, it all depends on your perception, or on context, or whatever else. You can say that science makes an assumption that the laws of physics will remain the same forever, but that assumption is continuously tested and confirmed by the scientific method that looks for evidence. Therefore, science and its evidence have nothing to do with opinions.

What should a scientist be aware of when he presents his evidence to non-scientists?

Although it advances human knowledge, research should have a lesser priority if it does not have relevance for society in terms of advancing the quality of people's lives.

My advice would be for all science students to be more aware of the possible implications of science for society. These can be through science's role in technology or through its role in decision making. Our students should be a little bit more broadminded and be able to discuss other disciplines, and to bridge the hard sciences and social sciences. That is something we need to improve in our education. In the end, our students should be informed and responsible citizens, knowing that what they do in their specific field might be relevant far beyond that field. I understand that not every student wants to become active in the science-policy debate, and that is OK. But those who do should be informed about how to communicate. It involves using a different language, a different perception; it is about solutions rather than problems. This is not something we are very good at, again because of the lack of training. Consequently research, even though it advances human knowledge, is given a lesser priority if it has no relevance for society in terms of advancing the quality of people's lives. But of course it is not easy to know where to draw the line.

Mexico is one of the emerging countries. How is the climate change problem perceived there?

There are two perspectives on this. People in the street are not that involved yet, most do not even care about air pollution. They are not aware that their children and grandchildren might be at risk. They have other priorities, particularly those who are on lower incomes. So it is really a matter of identifying people with a longer term vision who can see the problems and the need for solutions. Fortunately, we have some politicians in Mexico who are involved in this and with whom we can communicate.

Taking a broader perspective, our politicians still want to address the facts that the developed countries are responsible for the climate change damage incurred to date, and that they owe us something to help us develop. However, some of the developing countries keep asking for more, and the developed countries are saying that they themselves are in trouble, they are in a financial crisis, and there are limits to how much money they can put on the table. This problem can be circumvented through proper negotiation processes, and that is where the United Nations is important. However, negotiations seem to be stuck on the Kyoto Protocol, which allows the developing world to tell the developed world that they have signed up to binding commitments, and so they must deliver on them first. That is clearly a very simplistic perspective because very significant emissions are coming from the emerging economies and they need to start implementing emission reduction measures themselves. The present policy makes things difficult. We should start from scratch, based on an analysis of the weak points and the bottlenecks in the process.

How do you see European climate change policy making from your US and Mexican perspective? Does Europe still count on the international scene?

Europe has shown that the commitments under the Kyoto Protocol can be achieved. Furthermore, it is an economic power and hence very important as a leader in the negotiations. Europe's role will remain important as long as we have a majority of deniers in the US Congress.

Is there more you would like to say about climate policy in the US?

There is extreme irrationality in the US Congress, which is unacceptable and which we as scientists need to fight. It is hard work but at least we have reason on our side. But there is also more. Consider nuclear energy. Given the scale of the climate problem, nuclear energy is potentially one of the technological solutions. However, in the US the regulatory process to build a reactor takes five to eight years. It takes so long because the bureaucracy has become that complicated, and that is not acceptable! It is simply inefficient government and it has little to do with the need to be very careful with nuclear installations, which of course you have to be, no doubt about that.

We just had the nuclear accident in Fukushima. The German government reacted to this by not prolonging their nuclear energy programme. In a referendum in Italy people voted against starting a nuclear programme.

I don't think that will last long. People have short memories. The Fukushima reactors were obsolete reactors, they were about to be closed. We now know how to make safer reactors. For the absolutely safe ones we will have to wait for Generation IV, but in the meantime we should not just do nothing.

The climate change discussion is very much held in terms of physics, engineering and economics. Is there a role for softer approaches?

We should not say, ok, the science doesn't work, so let's work around it and try something else. I don't think we scientists have tried hard enough to communicate what we know.

We should not say, ok, the science doesn't work, so let's work around it and try something else. I don't think we scientists have tried hard enough to communicate what we know.

Which talents and skills did you find to be particularly useful in your career?

I guess you have to be very passionate about your work and do it as well as you can. You have to enjoy what you do, it shouldn't be a sacrifice. You have to be patient and persistent, because things don't always work out the way you want them to. But if you have a clear goal and know where you want to get, obstacles are usually overcome. Try to have an overall picture and see how it all fits together. That is a matter of intuition and, in the case of my policy work, of experience and practice. But it is important not to lose sight of the big picture and not to become isolated in some corner.

Is that big picture something you have for yourself, or is it something you work on with others?

Both. It is about what makes your life agreeable, but also about what makes it useful. That big picture is needed to know where to focus your efforts in order to have some impact. It is not only made of scientific facts but also of values. I have my values: I want my children and their children and those of my friends to have a better future. You get an enormous sense of satisfaction when you succeed. But these things remain outside the realm of science, and you have to be clear about this.

Apart from the frustrations with policy makers, you seem to be a happy man. What have been your happiest moments?

There were many happy moments. I remember, first as a child, discovering how interesting science can be. I very much enjoyed the world of books and so on. A highlight was carrying out original scientific work for my PhD. It was very academic science, but just realising that you were observing or interpreting things in a way nobody else had done before was thrilling. My PhD subject might not have been terribly important, but I felt I was contributing to human knowledge, and that it would be part of scientific articles and text books. Even then I was convinced that it was science that raised our standards of living. Getting recognition for your work is of course also very satisfying.

Winning the Nobel Prize is certainly a blessing. Is it also a curse?

I wouldn't call it a curse, but it certainly becomes a social responsibility. You are under pressure to make an impact, and you are definitely less free to do as you please. You can compensate for that pressure if what you do is still enjoyable. I stopped working on creative science with my team, but that is compensated by having a number of students who became professors themselves! What I am doing now is still very challenging on a personal level and leads to a very different type of satisfaction. The satisfaction that you are doing something that is in line with your values, which you think are very well justified and which you can share with, maybe not all of humanity, but at least with part that is advancing civilisation. We do see a net advancement of civilisation, with lots of ups and downs, but overall there is an improvement. Contributing to that gives tremendous satisfaction, and that satisfaction keeps me going.

Thank you for this conversation.

I am glad it worked out, and now you should eat something!

(June, 2011, Cambridge (MA))

C.S. Kiang

For a long time C.S. Kiang had been part of a virtual world of colleagues whose names and scientific work I was familiar with, but whom I had never met. He burst into my life in 2002 at a meeting organised by Jim Hansen on the interlinkages between air pollution and climate change. During a coffee break, I received a pat on the shoulder, 'Hi Frank, I am C.S.!' I was startled. 'C.S. Kiang', he repeated, 'Chicken Soup Kiang!'. 'Kiang from the Kiang and Staufer papers?', I ventured. 'Yes!', and we enthusiastically shook hands and started talking about our research of the previous fifteen to twenty years.

That meeting was followed, in 2002 and 2003, by two visits to Peking University where C.S. had become Dean of the College of Environmental Sciences. These were my first visits to China. We were put up in the 'Friendship Hotel', a hotel known to many visitors of Beijing, as for a long time it was the only hotel for foreigners. Initially, these had been foreign diplomats; hence its somewhat Orwellian name. The hotel was located on a six-lane street. During the day, the street was filled with bicycles and tricycles going in and out of the centre: newspaper vendors, farmers carrying vegetables, others with towering cardboard boxes on their luggage carriers. In each direction only one of the three lanes, sometimes one and a half, was being used by cars.

C.S. organised these meetings that gathered speakers from Europe and the US – former colleagues, friends from the business sector, urbanists and policy makers who were invited to inspire the young Chinese students. When we came into the lecture hall, all of the students rose to their feet and bowed to welcome us. That had never happened to me before; the habit of greeting professors in this way had actually disappeared during my time as a university student. It felt nice of course. The seminars were accompanied by daily lunches and dinners that were held around large round tables on which meat, fish, duck, vegetables, rice, milk, wine and beer were constantly appearing and disappearing. One evening I was sitting beside a professor called Mao, who was one of the few that had been at Peking University since the sixties. He talked about the isolation of Chinese research in his early days. When I asked what was the biggest difference between now and then, he paused for a while and then, with a shy smile, replied, 'People are less friendly now'.

C.S. Kiang (born 1939) founded and was Dean of the College of Environmental Sciences at Peking University from 2002 until 2006.

He is presently the CEO of Sustainable Development Technologies and a member of the World Future Council.

The conversations and discussions with this colourful group of guests and locals were an eye opener. I realised that as a scientist I could have access to, and in fact was already involved in, the many aspects of society worldwide. There were more things on Heaven and Earth than I had ever dreamt of in my philosophy (PhD stands for Doctor of Philosophy). After the last dinner, before returning to Europe, I said to C.S., 'It is maybe an odd thing to say, but I think you have changed my life.' 'I hope for the better!', he joked.
The following coversation with C.S. took place on the fringes of a meeting in Beijing, which was related to that first meeting Jim Hansen had organised nearly ten years before, on how one could mitigate global warming by reducing air pollutants such as black carbon aerosols and ground-level ozone. We had done some important work on this at the Joint Research Centre. I was in Beijing for the fifth time. The city had changed again; it had more of an international flair. This was after the 2008 Olympics, and many of the road signs were translated into English. Beijing now had the first seven star hotel in the world, and the architecture of the new buildings was more European. The Olympic Stadium, called the Bird's Nest, the National Opera, called the Bird's Egg, and the new building of the China Central Television, the Bird's Leg, were all designed by European architects. Only the magnificent Bird's Nest had used input from the local star artist Ai Weiwei. The street markets had given way to huge supermarkets. The Panjiayuan flea market, which ten years ago only featured antiques, was now largely taken over by cheap 'chinoiseries', basically rubbish. Most noticeably, all traffic lanes were now occupied by cars. There was an enormous heap covered by a fabric at the south gate of Tsingua University. When I lifted the fabric I discovered thousands of abandoned bicycles. All of this was a result of China's incredible economic growth over the previous decade.

WHERE will the economic growth of China lead?

It cannot possibly go on as it has done up until now. For 50 years, every five-year plan only focused on economic growth. Then in 2003 the SARS epidemic hit. That was the wake-up call for the Chinese government. It realised that the epidemic could well be just the tip of the iceberg, and that the quality of life of its people was coming under a real threat. The 11th five-year plan, which began in 2006, was the first to clearly address environmental issues. This plan refers to a 'harmonious society', where economy, environment and equity are equally important. It also very much refers to scientific research as providing the guidance for moving towards such a society.
People often think of the Chinese central government as a closed group of people who make decisions. The government actually relies on a very broad basis of knowledge. Take the climate change problem, for instance. For a long time, the Chinese government considered the climate change debate to be a western conspiracy designed to curtail Chinese economic growth, and in particular to boost the EU economy, which had taken the lead in green technologies. As scientists, we were able to explain to the government the real danger of climate change, and also the business opportunities that would be created in tackling it. They understood, and now they are pushing very hard for renewable energies, for batteries, the grid, etc. They want to lead the world in this. They have turned the game on its head.

Let's start from the beginning. What got you into physics and atmospheric research?

I was in fact more inclined to philosophy. When I was young I was very much against western thinking, but at some point I realised that I could not be against something without really knowing it. I mean knowing it profoundly. Physics was for me the best expression of western thinking, so I studied that. For my PhD thesis, I chose processes at the interface of the solid and the liquid phase, of liquid and vapour. There were two reasons for that. As a child I was fascinated by the boiling process of water. I would watch the bubbles form and rise in the water and burst at the surface. I asked my father, 'why does water boil?', and he answered, 'It is a natural phenomenon.' That is a very Chinese answer; if things just happen there is no need to explain them! The second reason is that I liked to think of the liquid phase as representing the western, capitalistic system: if one molecule changes other molecules can change as well. The immobile solid phase was communism. I studied the behaviour of substances at the critical point, that is the condition where the solid, liquid and gas phase co-exist. You see?

The integration of Western and Eastern thinking.

I am still driven by the idea that *this* is what is needed.

But first, I thought we knew why water boils?!

It boils when its vapour pressure becomes equal to that of the ambient atmospheric pressure. That is, when the pressure of the atmosphere is no longer able to suppress the tendency of water to evaporate. That is what thermodynamics tells us. But, as you know, thermodynamics explains things only in terms of variables that we can experience in the macroscopic world: temperature, pressure, etc. We still have to explain what temperature and pressure and also the boiling process are in terms of the underlying microscopic processes. In the case of the boiling process, we need to explain why and when individual molecules of water in the liquid phase tend to separate and become gas molecules.

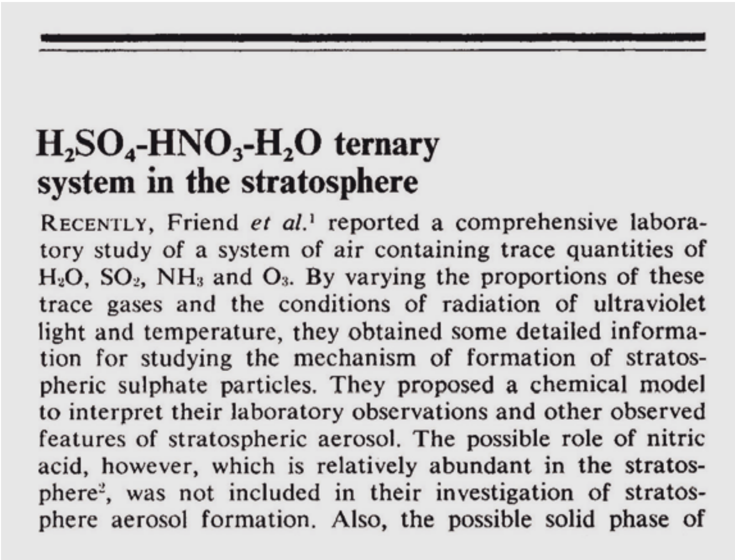
At some stage during my PhD research I was invited to the Max Planck Institute for Physics and Astrophysics in Munich. Werner Heisenberg was still its Director, the Heisenberg of quantum mechanics and the uncertainty principle. One day I asked him, 'Why does water boil?', and he answered. 'You don't know that? I got the Nobel Prize for explaining that!' He had indeed explained, using quantum physics, which was then in its infancy, how the hydrogen atom could exist on its own, or bind with another hydrogen atom to form the hydrogen molecule, or with two to form tritium. These three types of hydrogen can be considered three phases, and the transformation of one into another as a phase transition. That was very fundamental but still a long way from explaining the boiling of water. So I said to Heisenberg, 'You have a Nobel Prize and you still cannot tell me why water boils!'

Did you eventually find out why water boils?

Not in the fundamental way that I would have liked. It remained an explanation based on thermodynamics. So I wrote on the cover of my PhD thesis, 'It's a natural phenomenon' and I sent it to my father.

What brought you to atmospheric research then?

The boiling process creates bubbles of vapour within the liquid. Condensation, the reverse, creates droplets within a vapour. The physics is the same. Droplets in the atmosphere, clouds, are obviously very important. But cloud droplets form by the condensation of water vapour on the surface of even smaller aerosol droplets that float around in the atmosphere. When the conditions are right, certain mixtures of molecules are indeed able to come together to form such aerosol droplets: sulphuric acid and water, for example. That is called nucleation. But you know these things! I became involved in studies that sought to explain the existence of a layer of sulphate particles in the stratosphere, discovered by Christian Junge in the fifties. I calculated that a mixture of sulphuric acid, nitric acid and water vapour could form aerosol particles in the cold conditions of the stratosphere, and published an article on this in *Nature*.



H_2SO_4-HNO_3-H_2O ternary system in the stratosphere

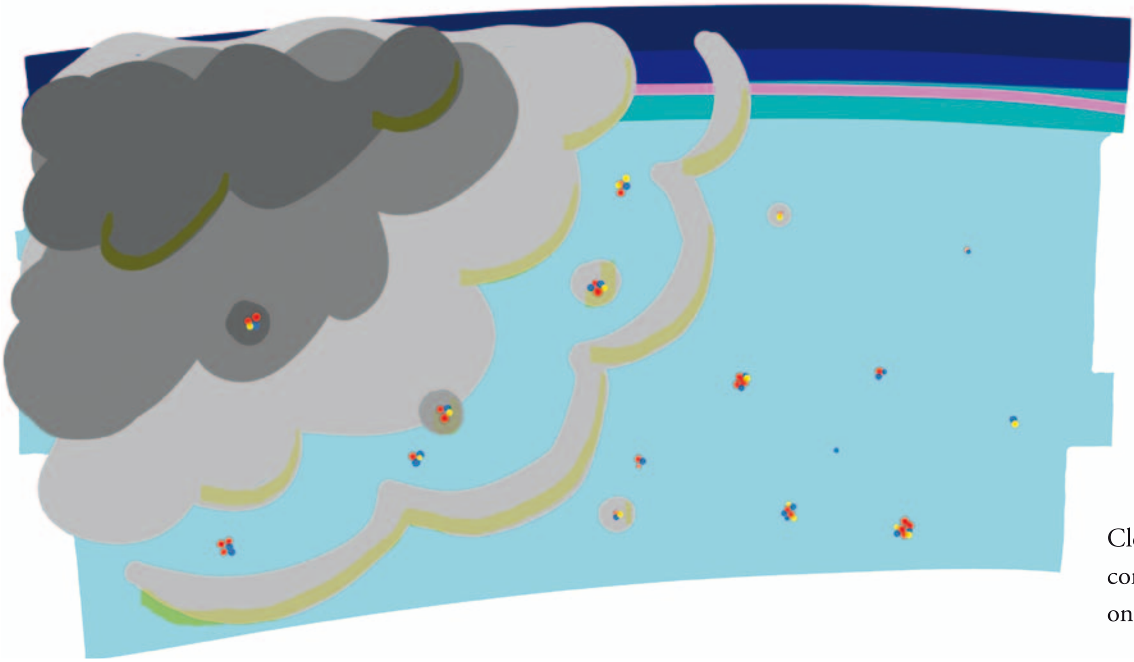
RECENTLY, Friend *et al.*[1] reported a comprehensive laboratory study of a system of air containing trace quantities of H_2O, SO_2, NH_3 and O_3. By varying the proportions of these trace gases and the conditions of radiation of ultraviolet light and temperature, they obtained some detailed information for studying the mechanism of formation of stratospheric sulphate particles. They proposed a chemical model to interpret their laboratory observations and other observed features of stratospheric aerosol. The possible role of nitric acid, however, which is relatively abundant in the stratosphere[2], was not included in their investigation of stratosphere aerosol formation. Also, the possible solid phase of

Kiang C.S. and P. Hamill, *Nature*, Vol. 250, 1974, pp. 401-402.

I read probably not all, but certainly many of your papers.

And I read yours (laughs); you would later apply these theories to explain the existence of sulphate particles above the oceans. I had a student working on that same issue, but you were ahead of us!

One question was where did the sulphuric acid and nitric acid molecules come from? They could only come from the chemical transformation of gaseous sulphur and nitrogen compounds. SO_2, for instance, could enter the stratosphere via volcanic eruptions. SO_2 is a gas but when it reacts with hydroxyl radicals (OH) it becomes sulphuric acid, which will nucleate together with water molecules to form aerosol droplets. The problem was that, when I was calculating these things in the seventies, there were hardly any thermodynamic data that could be used to pin down what really happened. Ten years later, when the ozone hole was discovered over Antarctica, these aerosol particles were implicated in the chemical destruction of stratospheric ozone. Not Junge's sulphate particles, but droplets of nitric acid and water that could form at the very cold temperatures of the polar night. Paul Crutzen had figured that out, back in 1986, and submitted a paper on it to *Nature*. My former colleague Patrick Hamill had submitted the same findings to *Geophysical Research Letters* a week before. Research was very intense in this area.



Cloud droplets forming by condensation of water vapour on aerosol particles.

But all that is, let's say, the technical explanation of how I got involved in atmospheric physics. Behind this I also had an interest in the atmosphere, because it is a system (draws a full circle in the air with both hands). It was not enough to just talk about chemical reactions, you had to include radiation, thermodynamics and atmospheric transport, which together would create the conditions for a phenomenon like, for instance, Junge's sulphate layer, or the ozone hole. That systems approach is very much a characteristic of the way we think in the East; holistically (draws a circle again) rather than analytically (draws a triangle).

I pursued these studies mainly at NCAR, the National Center for Atmospheric Research, where I had become the head of the aerosol group. NCAR was a great place in the seventies: the importance of atmospheric chemistry was being revealed. I hired Doug Davies to start measuring OH radicals in the atmosphere. This was extremely challenging, but to me it was important to understand the link between gas phase chemistry and aerosols. Paul Crutzen became the director of the atmospheric chemistry division. In fact, he was first a visiting scientist at NOAA, in downtown Boulder, and I worked hard to get him to come over to NCAR, which he did!

You then moved back to Atlanta to set up the atmospheric sciences programme at the Georgia Institute of Technology. There you set up the Southern Oxidant Study; the first multi-disciplinary, multi-agency, multi-actor programme to study air pollution. Was this one step further in holistic thinking?

It started with a simple workshop at Georgia Tech, in 1988, to discuss air quality in the southern states of the US. Despite a decade of reducing emissions of volatile organic compounds, one of the compounds you need to produce ozone, the amount of ozone at ground level had not fallen. In fact, several of the southern states were not complying with the regulations in those days. Clearly something was not working, but there was not enough chemical or meteorological data to figure out what this was. We recommended that the Environmental Protection Agency should set up a long term research programme.

Were the regulations in place at that time not based on research?

Research had been carried out, but it had had a rather narrow focus. The paradigm was that first you do your research, then legislation follows, and that's it. Since the legislators often had short deadlines, the research period was short as well; it was terminated once the political decisions had been made. Experience showed that this sort of linear connection between science and policy didn't work for a complex environmental problem like ground-level ozone. It was simply impossible to understand, through a narrowly focused study, all of the scientific and socio-economic issues involved. Even if it were possible, the changes in socio-economic

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conditions would rapidly make the findings outdated. So we proposed a different approach, a different paradigm. We thought that an iterative approach would be more effective. The goal of this approach was long term management and mitigation of the problem, rather than the elimination of the problem using an one-off regulatory fix. The iteration was between research and policy making, so that regulations could be periodically fine-tuned or altered as new technical data became available. This approach required a longer term commitment, both from scientists and from policy makers, to work together and improve the situation. We got the support of the EPA, and in 1991 the Southern Oxidant Study officially got underway. I was the founding Director, but Bill Chameides, who I had hired at Georgia Tech, soon became the leading figure in the study.

The iterative process meant that nobody was really in charge: neither science nor the EPA?

That's right, but it was very much a university-based research programme. Its goal was to improve knowledge that was policy relevant. Government officials from all levels – local, state, and federal – and scientists and administrators from the private sector were included in the selection of the research priorities.

Was the programme and its new paradigm successful?

The paradigm was certainly successful. The need to *manage* a problem, rather than looking for a *quick-fix* solution, has become accepted elsewhere; climate change is probably the most notable example. The ozone problem has not been solved yet. Atlanta is still not complying with the standards, but at least its ozone concentrations are no longer increasing.

In 2002 you were called back to China to found the College of Environmental Sciences at Peking University. In what condition did you find Chinese research?

I had gone back to China many times before 2002 of course, as guest professor or serving on various panels and advisory boards. But now I had the chance to really set something up there. It was clear that China was still suffering from the legacy of Mao's cultural revolution. That had led to many intellectuals and young students being sent back to the countryside to learn (makes two quotes in the air) from farmers and workers. They became what is referred to as the lost generation. So, at the university I had to start with the younger generation. The brightest of these were sent to the US and Europe to get a degree. As you have seen today, they are now back and hold professor positions. But initially they were inexperienced and not well connected in the international research community. For instance, they didn't publish in international peer reviewed journals, and one of the things I insisted on was that they do just that. Peking University was already becoming recognised as one of the world's biggest universities; the Harvards and the Oxfords of the world. My goal was to raise its environmental research to that same level.

Peking University to become Harvard, and Tsingua University, across the road from here, to become MIT?

No (laughs), Tsinghua already claims to be MIT and Harvard combined!

It was around that time that we met for the first time, at a meeting Jim Hansen had organised in Hawaii about air quality and climate change. I still remember you giving a passionate speech about collaboration.

In hindsight, that is probably what I have been doing throughout my career, bringing people together to collaborate on interesting problems. China clearly had some interesting environmental problems. GDP was growing by 10% a year, but some people, like Lester Brown, claimed that the cost to the environment – water, air, soil – was also 10% of GDP. That meant that China was not growing at all, and that its green GDP was zero! The Chinese government could not stay insensitive to that. So my idea was to team up my young Chinese scientists with top scientists from across the world in a small focused project. A sort of demo project that would then attract others and grow to address air pollution in China in a real way. The project was to study the Pearl River Delta. If China is the sweatshop of the world, then the Pearl River Delta is the sweatshop of China. It was a special economic zone where the government was experimenting with capitalism. Population, industry and pollution levels were rapidly increasing. We had satellite data showing how, year after year, the blob of dirty air over the region was intensifying and spreading. It was easy to convince American and European colleagues to join our study, because we could promise them record-breaking levels of air pollution. Every measurement would be worth a scientific paper! And in the end that turned out to be the case. Once again, the project was not just about the physical and chemical aspects of air pollution. It had also a strong governance aspect. Hong Kong had just become part of China again. That gave us two socio-economic systems that had to co-exist in one state, which required taking approaches to solve conflicts that we were not used to. I always looked at the European Union to see how you tackled the problem of managing one state and 27 systems!

Through a lot of talking, I can tell you!

The 2008 Olympic Games in Beijing was another event around which you created collaborations.

All institutes and universities that dealt in some way with air pollution treated the Olympic Games as an opportunity to understand air pollution processes in Beijing. Everybody also helped to formulate air pollution control strategies. My colleague Tong Zhu organised most of the studies at Peking University. Systematic air pollution control had already started in 1998, three years before Beijing was officially selected to host the Games. There was a strong political will to do something about the problem. The air pollution levels in the city were slowly coming down due the closure or relocation of dirty industries, replacing coal with natural gas, controlling dust from construction sites, and phasing out old cars and buses. You will remember that

Beijing.

before the Games there were two types of taxis in Beijing, the more expensive black limousine types, and the cheaper but outdated Volkswagen Jettas – there were about 12 000 of these older models. They have now all been replaced by modern Hyundais.

Did air pollution eventually reach an acceptable level during the Games?

During the first two days of the Games the concentration of $PM_{2.5}$, the fine particles that penetrate deep into the lungs, were around 150 µg/m^3. On the second day it started to rain, the winds turned and $PM_{2.5}$ levels dropped sharply and then hovered around 50 µg/m^3, which is still double the WHO limit value of 25 µg/m^3.

Two weeks before the start of the Olympics the city implemented extra emission control measures in the Beijing metropolitan area, closing all construction sites and certain industries, setting alternate day driving rules, etc. How much did that help?

That was of course the key question for legislators and scientists alike. You need to run an air quality model to distinguish between the effect of emission controls and that of a changing meteorology. For $PM_{2.5}$, about 60% of the measured reductions in the concentrations were due to the extra control measures. The effect of NO_x and VOC emission reductions on ground-level ozone was more difficult to ascertain.

Were weather modification techniques used, for example producing artificial rain to clean the air?

Such techniques were used to prevent it from raining during the opening ceremony of the Games. That seems to have been the main concern of the government. It did not rain that evening, but it is impossible to prove that it was because of the attempts to control the weather. Maybe it was just the lucky number (laughs) – 8th of August 2008; as you know, 8 is a lucky number in China.

Did the measures taken during the Olympics have a lasting effect on the air pollution in Beijing?

They did. Some of the measures, such as modernising the taxi fleet or closing dirty factories, have of course had a lasting effect. The experience of the Olympics has also made the public more aware of the air pollution problem. Newspapers now talk about it and that puts the government under some pressure, and rightly so. But traffic in Beijing is soaring; we now have about 5 million cars and each of them is driven on average twice the distance of a car in Tokyo. There is also the problem of the importation of pollution from growing industrial areas outside the Beijing area. Control measures risk being undermined by continuing economic growth. But the experiences of the Olympics fostered confidence and cooperation between the authorities and the scientific community. It became clear that, as we said before, air pollution will be not solved by a one-off action, but that it requires a long term and systematic effort. The problem I see at the moment is that, because of the great interest in climate change, air pollution is a bit forgotten.

You are retired from Peking University. What are you doing now?

We need to use nature, but we also need to understand nature in order to use it in a better way; that is also Eastern wisdom. The Western use of nature has led to its destruction.

I have set up the Sustainable Development Technology Foundation to radically look at how we can solve the big needs of humanity: health, food and shelter. I can now really try to help implement sustainable development by merging the holistic and cyclic thinking of the East with the analytical thinking of the West.

The Foundation taps the financial resources of the corporate world and uses them to develop zero-emission communities. Initially I didn't know how to get money out of the corporate world, so I had to go to meetings and learn about this. Through the World Future Council, of which I am a member, we like to promote investment in zero-emission communities all around the world. Zero emissions will be the common thread in all communities. Everything else – culture, politics, religion, etc. – can be different. In this way we can learn about what is working and why. It is the same approach as always: start small with a good idea, and let it attract people and grow by itself. At this stage the investment in these communities is in the order of tens of billions of dollars. It is big.

We think about solutions that don't destroy nature but that are also economically sustainable. This is the only way for them to succeed. We need to use nature, but we also need to understand nature in order to use it in a better way; that is also Eastern wisdom. The Western use of nature has led to its destruction.

Can you give an example of how you use nature without destroying it, without returning to the practices of the Middle Ages?

Take photosynthesis for example. It is a fundamental process for life on Earth, but it is a very inefficient one. Of all the light that plants receive from the sun, they only use the light of a few precise wavelengths in the red and the blue spectrum. That of course has to do with the chemistry that occurs within the plants. So what we propose is to capture the full spectrum of sunlight with photovoltaic cells, and use the electricity generated to produce light with LEDs that are tuned to the wavelengths that photosynthesis needs. That will boost the light use efficiency of plants by a factor of ten and will allow us to really move towards vertical agriculture within cities – the vertically stacked, multilevel greenhouses where plants receive the water and the minerals they need. These arrangements require artificial sunlight. With LEDs we'll provide the light needed and significantly reduce energy consumption.

Beautiful! An awkward question to finish: can you really do as you please in China? For example, is there freedom to carry out scientific research in China?

Before you ask this question, you have to ask first whether you in your country are free in the research you carry out. Who decides the priorities? Is it a transparent process?

What about the crackdown on students in Tienanmen Square?

Of course I will not justify what happened there, but let me ask you the following: when did it happen?

In 1989.

Right. China only started its process of economic liberalisation in 1978, and a process of democratisation is slowly following in the wake of that reform. China is definitely moving away from the cold war mentality. That move is slow and fast at the same time. The economic growth of the past thirty years was truly miraculous. First and foremost because it has brought 300 million people out of extreme poverty. That is more than what all the other countries did together. *That* is important! The middle class is growing and becoming more outspoken. But the Chinese government is reluctant, at this point in time, to give it more freedom, because they don't know what it might lead to. The Party, being the only party, cannot afford to promise something on which they cannot deliver. Keeping control is a difficult undertaking for the government. It still controls Google and YouTube and Facebook, but it had to give up on microblogs, the weibos, that run on cell phones. Even some Party officials are using weibos nowadays!
China is a big county with a lot of people. One has to be patient with China and its government!

The Party, being the only party, cannot afford to promise something on which they cannot deliver. One has to be patient with China and its government!

OK, I will not ask about the problems with Ai Weiwei then. When we last met, you brought me to an art district, the first one in Beijing.

The 798 Art District in Dashanzi.

You told me then that this district was tolerated by the government because the city needed some art to show to its international visitors. This was before the Olympic Games. How is art doing today?

It is booming. It is out of control. Ai Weiwei is so popular, in and outside of the country, that the government cannot stop him by the usual means, by repeatedly putting him under house arrest. Just some months ago it tried to stop him by fining him one million dollars for tax evasion, but the Beijing people went to his house and threw money into his garden to help him pay. He is getting more publicity and selling more of his works. The government understands that it is counterproductive to suppress people. The art scene is of great social importance. My wife, an art gallery owner in Atlanta, displays the work of Chinese photographers; they all capture moments of change. Art is also creating diversity in Chinese society.

Will China ever become a democratic country?

Democratic with a Chinese characteristic. That is, it will also pay attention to the community rather than just the individual.

(November, 2011, Beijing)

The atmosphere is just one part of the **Earth System**, which also includes land, oceans, ice caps, the biosphere and humans. Climate is largely governed by the composition of the atmosphere, but that composition is in itself controlled by continuous exchanges of chemical substances between various parts of the Earth System and the atmosphere. For example, the concentration of CO_2 in the atmosphere is largely governed by exchanges with the oceans (degassing/dissolution) and with the biosphere (photosynthesis/respiration).

Many of the important chemical elements (carbon, oxygen, sulphur, nitrogen, phosphorus, iron, etc.) cycle through the various parts of the Earth System. Their presence in one part can last from a week, as in the case of sulphur in the atmosphere for example, to millions of years, as in the case of carbon stored in deep ocean sediments. These cycles are impacted by climate, for example ocean temperature determines the degassing and dissolution of CO_2 from and into the oceans, and precipitation removes sulphate from the atmosphere. The cycles themselves influence climate, as many of the gases and particles cycling through the atmosphere have an impact on the Earth's radiation balance.

Through all these interconnections, the Earth behaves as a System. A change in one part of this System will have consequences for another, which in turn might have impacts on the first. Such feedback processes can be positive or negative, i.e. they can amplify or dampen the initial change. The natural regular cycling of the concentration of CO_2 in the atmosphere between 0.020 and 0.027% between ice ages and interglacial periods is a result of feedbacks involving the atmosphere, plant life, oceans and ice caps.

Humans are now significantly changing these natural cycles, with notable impacts on the atmosphere, plants, animals, the oceans and climate. Humans can reflect on and adapt their actions and hence create feedbacks in the System. Realising that the burning of fossil fuel creates well-being in the short term, they may burn more of it. Realising that fossil fuel use has negative impacts on atmosphere and climate, they may burn less of it. It remains to be seen whether human intelligence will act as a positive or negative feedback; whether it will lead to an unsustainable or sustainable development of the Earth System.

Epilogue

On page 128 there is a picture of the new headquarters of the China Central Television in Beijing, a building designed by Rem Koolhaas. Apart from being a star architect, Koolhaas is also a witty commentator on his profession. In the essay 'Bigness, and the problem of Large'* he describes the problems and opportunities of architecture at a very large physical scale: airports, shopping malls, Beaubourg… He describes Bigness as an architecture that breaks with the usual scale, tradition and ethics. It is an architecture that, on the one hand, surrenders to engineers, contractors, manufacturers, i.e. an architecture without architects, while on the other hand *'Bigness has the potential to reconstruct the whole, reinvent the collective, reclaim maximum possibility'*.

*Koolhaas R., 'Bigness and the problem of Large', in *S,L,M,XL* by O.M.A., Rem Koolhaas and Bruce Mau, Monacelli Press, 1995.

Air, Climate & the Earth System

From small to big science

The scientists who contributed to the conversations all started out in their careers focusing on a well-defined problem, such as the role of catalytic reactions in destroying stratospheric ozone, the lifetime of sulphur dioxide in the atmosphere, the absorption of infrared radiation by particular molecules in the air, etc. Putting the answers together they have created, in collaboration with many colleagues, a coherent understanding of atmospheric chemistry and climate interactions. They were engaged in what was very traditional science, carried out on a small scale by individuals. The romantic view emerges from the conversations of the lonely scientist working in the laboratory basement, in a Parisian café, at home late at night. However, in order to find solutions to the problems they had uncovered, these same scientists had to venture into other areas of human knowledge where it was less clear how and if the scientific method could be applied. Paul Crutzen, who 40 years ago wanted to work on his own and often spoke up to defend grassroots research, is now working with a broad range of scientists to show how mankind has made an epochal, lasting impact on the Earth, and how to deal with it. Atmospheric chemistry and climate sciences are now part of a more encompassing Earth System Science. John Schellnhuber, in his groundbreaking essay, called it Earth System *Analysis*, indicating that in trying to understand the interaction of humans with the Earth System one must certainly include the hard sciences of mathematics and physics, but also some softer analysis techniques that address uncertainty, human perception, behaviour, etc.

The more holisitic approach of Earth System Science (ESS) is seen as being necessary to steer the world within tolerable boundaries towards a sustainable future. In order to be useful, ESS has to consider the natural, economic and social systems as being interconnected and interdependent. ESS must therefore *'integrate across existing research programmes and disciplines, across all domains of research as well as local knowledge systems, across the North and South, and must be co-designed and implemented with input from governments, civil society, research funders, and the private sector.'**

*Brito L. and M. Stafford Smith, 'State of the Planet Declaration, Key messages emerging from the proceedings of the Planet Under Pressure Conference', London, 2012. *http://www.essp.org/fileadmin/redakteure/pdf/others/PUP_declaration.pdf*

Earth System Science has clearly become 'Big', i.e. large in scale, broad in scope and of great significance. Using Koolhaas' essay as a template, one can argue that beyond a certain scale, beyond a certain degree of integration of disciplines, science acquires different properties. We need to understand the problems and opportunities that these new properties may present.

The concept of 'Big science' has been used to describe endeavours such as the Manhattan Project during WWII, which led to the first atomic bomb, the Apollo Project in the sixties, which put the first man on the moon, and more recently the Human Genome Project, which unveiled the sequence of the billions of basic building blocks that make up human DNA. In these big scientific undertakings, the role of the individual scientists was subordinate to that of the engineers who built the big infrastructures needed, the managers in charge of spending big budgets, and the politicians who made the budgets available. In order to deal with the climate problem, some have argued that we need another Manhattan or an Apollo Project that will push things forward, for example by speeding up the development of highly efficient renewable energy systems. However, others have pointed out that the Earth System Science approach is very different, more decentralised and vastly more multi-disciplinary than, for example, the physics and engineering approach of the Manhattan Project.

A problem of such decentralisation of Earth System Science means that its analyses and results do not have a real owner. Ownership can at most be claimed by a broad community of researchers. In principle this is a good thing, as the results will be broadly supported and therefore more socially robust. However, the lack of a more identifiable ownership together with the fact that results will remain shrouded in large uncertainties, might also lead to a systematically biased use of the results by some stakeholders. ESS might lose its autonomy and become an instrument of other forces. Sir John Houghton talked about the difficulty of maintaining scientific rigour in a process (in his case the functioning of the IPCC) that is also open to non-scientists. Complexity and complication result in uncertainty and make it more difficult to access a single solution or truth. Results of a fully integrated ESS cannot simply be validated against observations. Eventually they will have to be judged on the quality of the science; through documentation, reviews, and assessments by the full breadth of stakeholders involved. Again, this will make the results more socially robust but it comes with the price of linking ESS to quality control issues, which will make it slow, inflexible, and possibly less appealing to grassroots scientists.

Given the global problems at hand, we cannot *not* engage in Earth System Science. Its promoters (see reference above) talk about a '*new contract between science and society, in recognition that science must inform policy to make more wise and timely decisions*'. However, the Bigness of ESS must be addressed in order the see whether it can live up to its claims. The Bigness of ESS should be a domain of research in itself. ESS will eventually be the result of the interactions of thousands of scientists and agents from different disciplines, from which new insights, possibilities and powers might emerge in a rather unpredictable and chaotic way, in the same way as the economy behaves rather unpredictably as a result of the dealings of millions of buyers and sellers. If this is the case, how can ESS be guaranteed to inform policy in a timely manner?

On the positive side, it is difficult to dismiss the fact that ESS has the potential '*to reconstruct the whole, reinvent the collective, reclaim maximum possibility*'. After centuries of analytical research, ESS draws attention to the collective, the common, and holds the potential for redefining and reorganising society. Some people claim that this is indeed a prerequisite for sustainable development. The programme of ESS is definitely richer and more stimulating than that of the disciplinary sciences. It might therefore attract the best minds from the scientific and non-scientific communities. Earth system scientists will be less 'pure' but will still be 'honest' scientists, where honesty is not so much related to sticking to a truth or a method, but to using one's talents in a responsible way.

Responsibility is also linked to another question: what are the limits of ESS? If it is to be the basis for wise decision making, what must it encompass and what must it leave to political discourse and democracy? The answer to this question will undoubtedly develop with time, as the boundaries of ESS will shift. But one could argue that ESS could limit its ambition and refrain from trying to understand 'everything'. More pragmatically, it could (1) focus on the many areas where the scientific method, the corroboration of theory by repeated observation, is applicable, and (2) focus on communicating its achievements within the political discourse on which democratic decision making is based. Investing in communication channels and creating a sociology* of individual areas of human endeavour might be as important as trying to understand these areas within an all-encompassing integrated system.

*Latour, B., 'A plea for earthly sciences'. Keynote lecture for the annual meeting of the British Sociological Association, East London, 2007. *http://www.bruno-latour.fr/sites/default/files/102-BSA-GB_0.pdf*

A strategic position for Earth System Science might therefore be obtained through humbleness rather that bold and unrealistic ambition and, in Koolhaas' polemic language, ESS could *'yield the rest of a contested territory to enemy forces'*, alluding to the fact that in democratic decision making, 'battles' are necessarily being fought.

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Frank Raes

Frank Raes is presently Head of the Climate Risk Management Unit at the JRC. He obtained his PhD in at the University of Ghent, Belgium. His scientific background is in atmospheric chemistry, specifically in climate interactions. He lectures at the Bocconi University, Milan, Italy and has been visiting professor at the California Institute of Technology, Pasadena, United States of America. Between 1999 and 2011, he led the Air and Climate Unit of the JRC.

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Abstract
This book is a collection of conversations with some of the ‘fathers’ of air pollution and climate change science. They talk about the origins and drivers of their scientific work, as well as the many ways in which science can interact with society. The conversations took place between 2009 and 2011, when policy makers and scientists were struggling to raise awareness among politicians and the general public about the severity of the climate change problem. This was also the period during which scientific research fully embraced the inter-connectedness between many environmental, economic and social problems, leading to the merging of the individual sciences into a coherent Earth System Science. These dynamics are reflected in the frustration and hope that emerge from the conversations. Frustration about the difficulty in bringing scientific evidence to the forefront, but also hope and trust that the scientific method, i.e. critical thinking without ever being complacent, does play a unique role in guiding the world towards a sustainable future.

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