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.

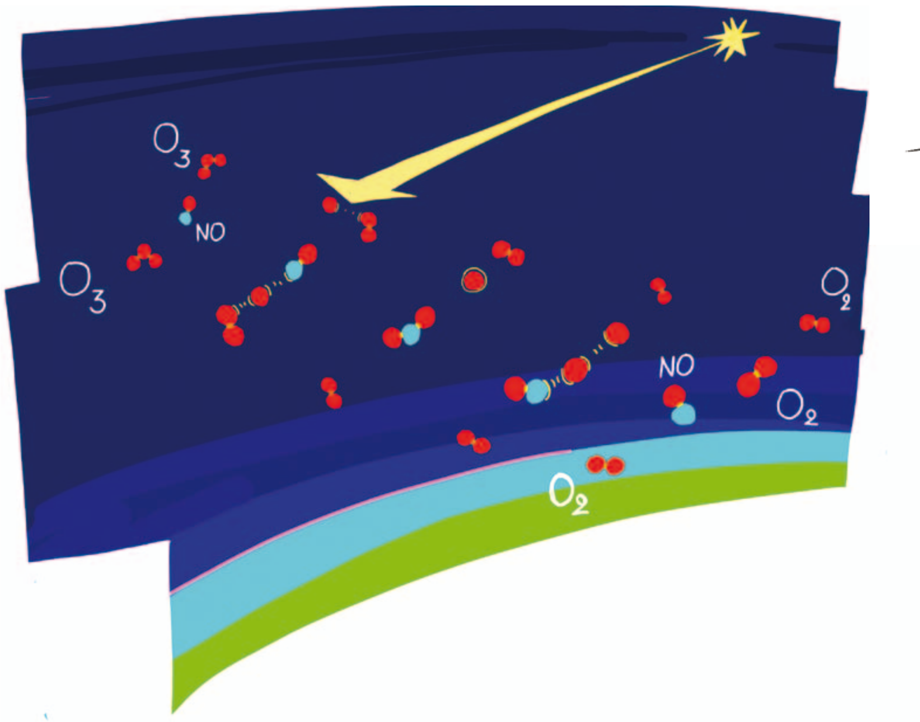
Quarterly Journal of the Royal Meteorological Society, Vol. 96, 1970, pp. 320-325.

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.



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

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.

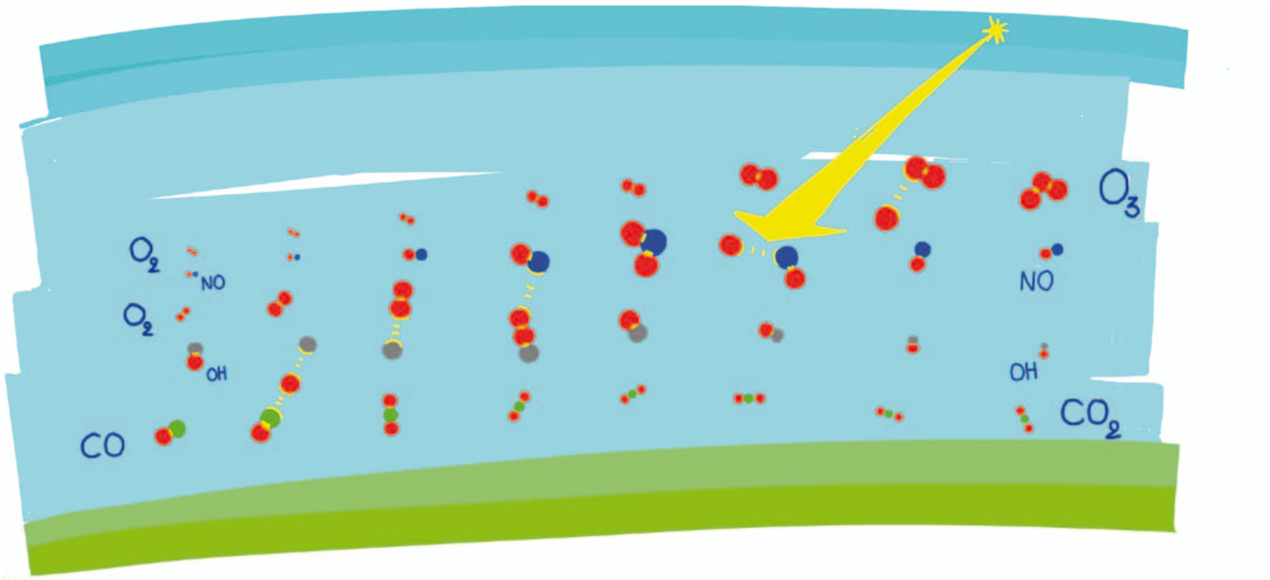
$$CO + OH \rightarrow CO_2 + H$$
$$H + O_2 \rightarrow HO_2$$
$$HO_2 + NO \rightarrow OH + NO_2$$
$$NO_2 + h\nu \rightarrow NO + O$$
$$O + O_2 \rightarrow O_3$$
$$\overline{CO + 2O_2 + h\nu \rightarrow CO_2 + O_3}$$

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.

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?

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

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)