Geert Jan van Oldenborgh – approaching truth in a sea of noise and data deficiencies

An interview prepared by Hans von Storch and Sjoukje Philip in December 2020, with a foreword by Myles Allan

Editor’s Preface

With changing partners, I, Hans von Storch, have carrying out interviews with respected scientists for more than twenty-five years. Hans Hinzpeter in 1996 was my first interviewee. This interview is number twelve in this series of snapshots of our joint scientific history.

Nearly all interviewees have been geoscientists who can look back at long and impressive scientific careers. They are all witness to events that often lie far in the past but heavily influence our present time. These “testimonies” alone are already valuable in that they allow younger generations to understand why conditions and knowledge are the way they are today, that science is not a collection of truths, but is, as George Philander describes, a social process: *”Science is organized skepticism, a strange religion that demands of its congregation a firm commitment to the continual testing of all observational and theoretical results. The tests permit no compromises, are not democratic – if 99% of all scientists are in agreement, then they are not necessarily right – and produce results that, in the long run, are objective, independent of ethnicity, race, gender, religion, values etc*.”

These interviews follow the same premise – it’s not about truth, but the perception of our witnesses; it's therefore only to a certain degree about how matters were, but about how the witnesses had experienced these matters. It is naturally possible that others understood the events and conditions differently. Our partners in dialogue have control of what finally ends up in the interview – all statements were checked and approved by our interview partners.

This time I asked **Geert Jan van Oldenborgh** to carry out his retrospective together with Geert Jan’s former PhD student **Sjoukje Philip** as my interview partner. We were also able to have Prof. **Myles Allan** writing a tribute to Geert Jan von Oldenborgh in the form of a foreword.

The interview was conducted in English and was carried as an online-exchange. A Dutch translation will follow. The subject of the interview was on science and its dynamics, but also its conditions under changing attention milieu; the subject is not the interviewed individual’s personal background.

January 16, 2021, Hamburg – Hans von Storch

Foreword

It's an honour and a pleasure to contribute a personal introduction to this interview, as a friend and scientific collaborator, of course, but also in my capacity as Geert Jan's nominator to his Visiting Professorship in the Department of Physics, University of Oxford.

Like Geert Jan, I have always felt faintly uncomfortable with being introduced as a "climate scientist" – as if climate science was somehow a particular kind of science, with its own rules and ways of doing things. Geert Jan is, self-evidently, a physicist who happens to study the climate system, bringing to it the same intellectual rigour and unflinching commitment to "saying what can be said (and no more)" that he developed in fundamental particle physics.

Many in our generation have made a similar transition, as climate science grew exponentially over recent decades, entraining researchers from all kinds of fields. Part of the fun of the discipline has been the different perspectives that everyone brings to this virtual Ellis Island, and it always seems to me that the "refugees" from fundamental physics stand out. They seem faintly uncomfortable in this messy new world they have moved to, perhaps unconsciously missing a field in which greater precision can be bought with longer data acquisition or (at an eye-watering price) an even-larger accelerator.

Geert Jan hints that he regrets not making this transition earlier, but I think having established himself as a physicist before moving into climate science must have contributed to his no-nonsense style and focus on real-world observations (being Dutch probably also helped). It is revealing that I instinctively added the "real-world" qualifier: being stuck for the foreseeable future with only one Earth-like planet to observe, much of our field, in pursuit of repeatable experiments, tends to focus on observing and explaining the behaviour of numerical models.

Geert Jan has not been shy of analysing climate models: indeed, his Climate Explorer is probably, for many undergraduates and graduate students, their first introduction to the world of numerical modelling. But what was for many years the Climate Explorer's great "unique selling point" was the ease with which it encouraged the user to relate these simulated data with actual climate observations.

One of the longest-standing problems of our field has been the uncomfortable co-existence of distinct observational and modelling communities, always slightly disdainful and suspicious of each other. Everyone acknowledges that we need both observations and modelling, supported of course by underlying theory, to make sense of the world – but everyone tends to owe their primary allegiance to one community or the other. Arguably, it took the IPCC 30 years and 5 assessment cycles before they broke down the divide between the first (observational) and subsequent (modelling and projections) chapters of Working Group 1 reports.

Geert Jan is one of the few scientists I know who, from the start, could not be clearly pigeonholed into either the "observations" or "modelling" communities. Perhaps that is because so much of his work has been around testing models against observations – "validation", as it is often called, or perhaps more accurately, "invalidation". And it is not always about invalidating models. Because he understands where observations come from, Geert Jan has never been shy of criticising our data records: always shorter than we would like, subject to unknown confounding influences, and riddled with gaps and errors.

Geert Jan has, I believe more than anyone else, helped the attribution of extreme weather events to graduate from an eccentric and controversial idea largely confined to a single university into a credible field of science. With the benefit of hindsight, it seems like he was uniquely qualified to do so. First of all, Geert Jan brought his deep understanding of climate observations and their limitations – the care that needs to be taken in defining what the meteorological event is we are talking about when often, in the immediate aftermath of an extreme event, understanding of the immediate causes may be largely anecdotal. Second, of course, he brought an entirely new approach to quantifying the human contribution to changing weather risks. Unlike previous studies based almost exclusively on numerical simulation with general circulation models, his approach was based on fitting statistical models, informed by our understanding of the timing of large-scale climate drivers, to observed timeseries.

Of course, he would be quick to point out, both approaches have their drawbacks. But given we have no way of directly validating an attribution claim for an extreme weather event (how can we "confirm" that a particular flood was a 1-in-100-year event in today's climate, never mind confirm that it would have been a 1-in-300 years event in a pre-industrial world?), corroborating lines of independent evidence is all we have to go on, so bringing in a new and independent approach was transformational (even though I do have to confess I found it mildly annoying, having laboured away for years running massive model ensembles, for Geert Jan to pump out his relative-risk estimates with a few clicks on Climate Explorer!).

Finally, of course, there was the credibility Geert Jan brought to this field, both within and beyond the scientific community. His background in fundamental physics and the study of natural climate variability and predictability, coupled with reputation and position in the KNMI, made it clear he was no headline-seeking activist. I'm sure it helped that he came in from outside the "attribution community" (which was, it must be admitted, becoming a little incestuous at the time), so it was clear to our colleagues that he saw the point of what we were trying to do rather than simply promoting the idea for its own sake. As he made clear from the start, he was not shy of publishing negative results (or criticising others) if the evidence for a role for human influence in a high-profile weather event failed to stand up. And his experience of communicating with politicians, the media and the public was invaluable in providing a completely credible voice to convince everyone that we were just trying to answer the exam-question, not push a political agenda.

I believe the fact that event attribution has become accepted as a central part of Europe's Copernicus Climate Services is largely Geert Jan's achievement, working of course with his long-standing collaborators Fredi Otto and Robert Vautard. As questions about the harm caused by human influence on climate and how we can plan for changing risks of extreme weather become ever more urgent, we need this information to be communicated to everyone, as a routine part of meteorological services, in measured, careful and dispassionate tones, by a voice like Geert Jan van Oldenborgh's.

21 January 2021, Oxford - Myles Allan,

Interview

Geert Jan, please let us have a rough overview, how your path from school to visiting professorship in Oxford developed.

Born in 1961 in Rotterdam in view of the Holland America Line in its last years, I went to school in Hendrik Ido Ambacht, Dordrecht and an international school near Victoria, BC, Canada 1978–1980. I studied theoretical physics in Leiden and continued with a Ph.D. in Amsterdam. After three post-doc positions in that field I switched to climate research in 1996 and worked at KNMI since then. In 2019 I became a visiting professor in Oxford.

- 1978-1980: I won a scholarship to attend the Lester B. Pearson College of the Pacific near Victoria, BC, Canada. This is an international school where 200 students from all over the world are brought together on scholarships to study, live, discuss together to promote international understanding and foster global networks to make the world a better place.

Pearson College 1979

Student in Leiden 1983

- 1980-1986: M.Sc Theoretical Physics at Leiden University, the Netherlands, with minors of Mathematics and Modern Chinese (which I had already studied at Pearson College).
- 1986-1990 Ph.D. at the University of Amsterdam, in practice at the Netherlands Institute or Nuclear and High Energy Physics (NIKHEF). My Supervisor was Jos Vermaseren, the smartest person I have ever met. The first part was the implementation of a set of Fortran routines to compute highly numerically unstable functions needed to compute experimentally verifiable results from theories of elementary particles, the last part was using these to compute an example.
- 1990-1992 Assistant at the Ludwig Maximilian University in München, learning to be an independent researcher, and teaching (in German).
- 1992-1994 Post-doc at the Paul Scherrer Institute in Switzerland, still in the phenomenology of elementary particles, computing cross sections.

- 1994-1996 Post-doc at Leiden University, still in the phenomenology of elementary particles.
- 1996-1997 Post-doc at the Royal Netherlands Meteorological Institute (KNMI) in data assimilation to improve forecasts of El Niño with Gerrit Burgers, who I knew from my M.Sc. time in Leiden and had made the same switch from elementary particle physics a few years earlier. Climate research turned out to be much more suited to my personality and offer more possibilities, as it was a newer field and hence it was simpler to make significant contributions. It was also much easier to explain to the public and the answers more relevant for society.
- 1997-now researcher at KNMI, first in El Niño research, soon including seasonal forecasting, next verification of regional trends in climate models, the physics behind these trends, and finally extreme event attribution. In 2009-2013 I participated in writing the IPCC Fifth Assessment Report part 1, The Physical Science Basis.
- 2019-now also visiting professor at the physics department of the University of Oxford, the university I collaborate with most in our attribution studies.

As an outgrowth of the 1996/97 El Niño I developed a web site "KNMI Climate Explorer" that contains a lot of climate data and statistical tools to analyse these data. For a long time, it was the main reason I was known in the research community. It was partly inspired by my time at Pearson College, as it enables researchers everywhere in the world to analyse data that they may not be able to study at home.

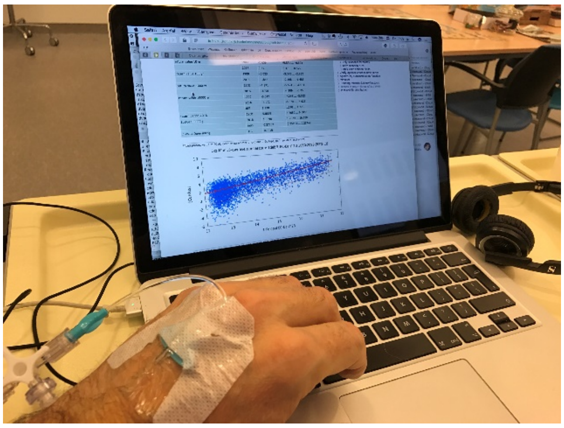
Climate workshop Barcelona 2007
with Noel Keenlyside

Formal dinner at Merton College, Oxford
with Friederike Otto and Valerie Masson,
6 March 2020

In July 2013, you were diagnosed with cancer. What did that mean for your scientific life?

Yes, unfortunately, I was diagnosed with an incurable cancer, Multiple Myeloma, also known as Kahler's Disease. I had walked around with it for at least 1½ years before that, including at the IPCC AR5 meetings in Marrakesh, Morocco and Hobart, Australia, where it had been very clear something was very wrong. The last decade treatments for this cancer have improved greatly. Survival times depend very strongly on the type of cancer, I appear to be somewhere near the middle with a fairly aggressive form. Fortunately, my life was saved three times by then-new treatments, so I am very grateful to medical research. I managed to keep up my normal life, including working full-time in climate science about two-thirds of the time, being a patient the remaining one-third.

For my work it means I cannot do as much as I want to. In fact, I have always wanted to do about ten times more than I can do, but the illness takes away 1/3 of my time and energy up to now, a fraction that is increasing the last year. (I should maybe mention that when I was down 2/3 due to chemotherapy and complaining about it, colleagues on the corridor told me that from their perspective my usual level used to be about three times the average person's, so I now did as much work as most 'normal' researchers.)



With reduced intensity and speed, but not giving up scientific work while having to spend time at the hospital.

The other way around, my work means I can spend time thinking about other problems, be distracted from the cancer. It also means I can feel that I am still making a positive contribution to society, which helps me a lot in going through these times. For instance, looking back at the last seven years that I lived with it, I can feel proud of what I have achieved and cherish that modern medicine has given me that life extension as an antidote for sometimes feeling depressed by the lack of much future to look forward to. More practically, it means being part of a community of supportive people (although more virtual in Corona times), it is amazing how colleagues turn out to be friends in times of trouble.

The last year it also forced me to take scenario planning seriously. I always claim that in climate science there are problems where you can quantify the uncertainties, so you can make a probability forecast (like in seasonal forecasting), but also areas where you do not know the uncertainties well enough so you have to switch to scenarios: likely outcomes without probabilities attached, used to try to make decisions that are robust under the different outcomes. Applying the same logic to my disease, I decided to concentrate more on teaching and disseminating the knowledge I have gathered over the years, and less on writing papers and improving my H-index (although I am still proud of it). Hence my priorities in becoming a visiting professor in Oxford, giving seminars at universities and research institutions, and also media work.

I would also like to do some original research again, but I am unsure whether I can still concentrate enough for it and have the time left to do it, both short-term next to all other obligations and medium term in life expectancy.

From 1978-1980, you went to Canada to conclude your school education. How did this come about?

After growing up in a boring suburb of Rotterdam, I wanted some adventure. My father had heard about Atlantic College in southern Wales from a friend and wondered whether I would be interested. I applied, but the selection committee told me they only had a place at the sister school in western Canada. It looked even more interesting than Atlantic College, so I went there.

You began learning Chinese there, and continued so while studying in Leiden - what did you expect you would be able to use it for? How fluent did you become in thus language?

It was more an intellectual challenge than useful. I was intrigued by the language, philosophy and culture, and it was offered next to more traditional alternatives (English, German, French, Spanish). I got to exercise my brain in a new way—learning hundreds of characters—but also avoided having to read books and write essays, which I was not very good at. After my final exam I went to a 6-week summer school in 北京 (by train via Moscow). At that time, I could manage a simple half-hour conversation. Back in Holland, I took modern Chinese as a minor next to theoretical physics and mathematics, but that was only reading and writing, with the old characters rather than the simplified I had learned before. So a lot of learning. I reached about 2000 characters, but about the only use I ever got out of it (besides volunteer work teaching Dutch to Chinese immigrants) was ordering beers for some British colleagues at a bar in Kunming (昆明) where the first IPCC WG1 AR5 meeting was held in 2010. This did earn me their eternal gratitude.

After schooltime, you entered Theoretical Physics - what did you expect to learn and be able to expand knowledge there?

In Holland I had always thought I would like to study mathematics, but at Pearson college I had a very good physics teacher and a bad mathematics one, so my allegiance switched to physics. With hindsight, it may have attracted me more because of its stronger connection with reality. I sailed through my undergraduate studies with ease, obtaining top grades with little effort, so I took what looked like the most ambitious subfield, theoretical physics.

During this education in Leiden, also as PhD student in Amsterdam, have you - except for Chinese - been taught on issues that were not related to pure physics, like philosophy of science or STS (Science and Technology studies)?

No, neither the Leiden curriculum nor the Amsterdam Ph.D. programme encouraged that. Neither did they offer courses in other useful skills like scientific writing, which still hinders me in composing papers to this day. I did spend quite some time learning computer skills, due to the people surrounding me in Amsterdam and my own curiosity. I got my first personal computer in 1987, an Atari ST1040, which was the standard computer in our group. With the help of my supervisor, Jos Vermaseren, I wrote some software on it. The main scientific software was a library that I wrote for particle physics and is described in the first half of my Ph.D. thesis. The underlying article[1] still is my best-cited first-author paper and the library was in active use until a few years ago. I also wrote some system software to improve the performance of the Fortran compiler on that computer.

I did read some books on the philosophy of science for my own education, but did not get much further than a simplistic Popperian view. It did give a fascination for

[1] van Oldenborgh and Vermaseren, 1990

the interplay between theory and experiment as the most interesting part of physics: where the rubber hits the road, i.e., where our ideas how nature should behave are tested against how nature actually behaves.

You went to KNMI in 1996 with a for you totally new field. Your focus was first on El Niño. What would you consider a major achievement in this (ENSO-focused) time?

The first was a direct product of the post-doc project I was hired on, which was to finish an adjoint (derivative) ocean model and use it for data assimilation. I realised it could be used to investigate the causes of the unexpectedly strong onset of the very strong 1997/98 El Niño event in a linear approximation[2].

The second was a very straightforward but new couple of papers on the verification of the ECMWF seasonal forecast model in forecasting El Niño and the resulting seasonal forecasts in collaboration with the ECMWF seasonal forecasting group[3]. The many questions we received on the effects of El Niño on the weather world-wide led me to set up a simple tool to make correlations of station time series with ENSO indices, which grew into a local web site and afterwards the public web site KNMI Climate Explorer. (David Stephenson suggested I make it public and coined the name.) I reinstated the earliest (1999) home page from the wayback archive at https://climexp.knmi.nl/history/.

Finally, a Ph.D. student at that time, Sjoukje Philip[4], and I did a series of papers on the effects of climate change on ENSO using both the theory of ENSO and the experience we had gained in regional climate change verification[5].

Later the focus of the field went totally on anthropogenic climate change and its impacts. How did you perceive this change; how did the challenges change - El Niño was a relatively esoterical issue, with little interest in the general public, while anthropogenic climate change became a dominant issue of public and policy debate.

El Niño was indeed free of policy debates, but hardly an esoteric subject. During the strong 1997/98 El Niño, which started half a year after I had started work in my new research field, it garnered a lot of publicity. At first that was all handled by my supervisor, Gerrit Burgers, but when he was at a workshop in the U.S. I was invited for breakfast TV and afterwards had regular interviews for TV, radio and newspapers. I gained a reputation for being able to talk about it in an understandable way and enjoyed the challenge of bridging the gap between science and the public without sacrificing accuracy. A fixture in the Netherlands are the winter forecasts, which have a lot of interest due to the skating possibilities. As the official seasonal forecaster at KNMI, every year I had to defend the thesis that I had no idea what the winter would be like, and this was better knowledge than the people who invariably claimed it would be a very harsh winter. The reputation of KNMI was such that I always succeeded.

On the other hand, as civil servants we were not supposed to enter the policy debate on climate change, so we could only report on relatively dry facts on how climate change was progressing but not on what could be done about it. The PR department also quickly realised that my debating skills were not sufficient to score points against climate change deniers, who had a lot of political influence around 2010.

[2] van Oldenborgh et al, 1999

[3] van Oldenborgh et al, J. Clim, 2005a,b

[4] Who acts as co-interviewer here.

[5] van Oldenborgh et al, 2005; Philip & van Oldenborgh, 2006

The work I did was very similar to what I had done in seasonal forecasting: verification of past climate model results against observations, so I do not see it as a break in my work.

We would like you to tell us something about your most noteworthy work-related achievements and disappointments. What would you say is an absolute highlight in your work?

In my career I have only had two moments in which I had the really nice feeling I had really discovered something new, both in a discussion with someone else in which we could not reconstruct who thought of it. The first was during my Ph.D. with my supervisor Jos Vermaseren, the idea that we could reformulate notoriously numerically unstable expressions as determinants of matrices, which enabled us to apply well-known ways to circumvent these instabilities[6]. The second one was after a climate diagnostic presentation, when I was brainstorming with a KNMI colleague Richard Bintanja why the land ice on Antarctica could be melting whereas sea ice was increasing. We figured that the meltwater from the land ice could form a fresher surface layer in the Southern Ocean that prevented the warming of the deeper ocean to reach the surface[7]

For the rest, my work seems more incremental, with results building upon previous results, progressing from simple seasonal forecast verification to applying these techniques to climate change and combining these into extreme event attribution. The latter relatively new subfield is concerned with answering the question how anthropogenic climate change has altered the probability and severity of extreme weather and climate events, often raised after an event that just occurred. Over the last decades methods have been developed to give scientifically defensible answers to these questions[8], although given the hypothetical nature of the problem these do depend on how the question is framed exactly. From a science point of view, including observed trends in the analysis implies that the trends in extremes in climate models are verified against the observed trends.

Because I am much better at computing things than at writing, the resulting articles are more often results-focused than methods-focused. For instance, the paper on the attribution of Hurricane Harvey[9] is just a repeat of the analysis of extreme rainfall in Louisiana the year before[10] for a slightly different event. I am working on overview papers on our methods and problems we encountered.

My way of working has always been to add any new datasets and tools I needed to the Climate Explorer at the same time as using them myself for analyses. That only takes a bit more work but enables everybody to do similar analyses[11]. This way it has become a useful site for several groups of people: for instance, paleo-climatologists who upload their series to correlate with various aspects of the climate, educators who want to teach students without requiring them to learn an analysis language yet, consulting firms making projections based on CMIP model output and lately the extreme event attribution community. In all, thousands of people from all over the world now use it.

Did you discover your highest strength and if so, could you tell us about that skill?

6 van Oldenborgh and Vermaseren, 1990

7 Bintanja et al, 2013.

8 Allen, 2003, van Oldenborgh, 2007

9 van Oldenborgh et al, 2017

10 van der Wiel et al, 2017

11 Share and enjoy!

I think my first strength is seeing that there is a gap in our knowledge that I can fill with my experience, skills and tools. I have collected a fair amount of climate data and tools to analyse these data (almost completely also publicly available on the Climate Explorer) to tackle these problems, often in formally different subfields of climate science. Finally, I think I have learned over the years how to make almost sure that the answers I get are robust and not due to faulty data, model deficiencies or assumptions in the statistical analyses.

What new expertise or competence did you have to develop most over the past decade?

Collaborating more closely with a diverse group of people, e.g., in the World Weather Attribution consortium, in which every study is a true team effort.

Do you also have disappointments, pathways that in retrospect you would not take again?

My biggest error was pursuing the one-loop correction to W pair production after my Ph.D., which is a project that was far too large for me alone and which I could not solve in the time I thought it could be solved. I did learn to avoid these kinds of projects in climate science.

Weaknesses that have prevented me from doing the work that I would like to do also include until recently uncertainty about whether I really could contribute new results to the field and hence stress when reading the literature. This also leads to too much stress dealing with reviews of papers.

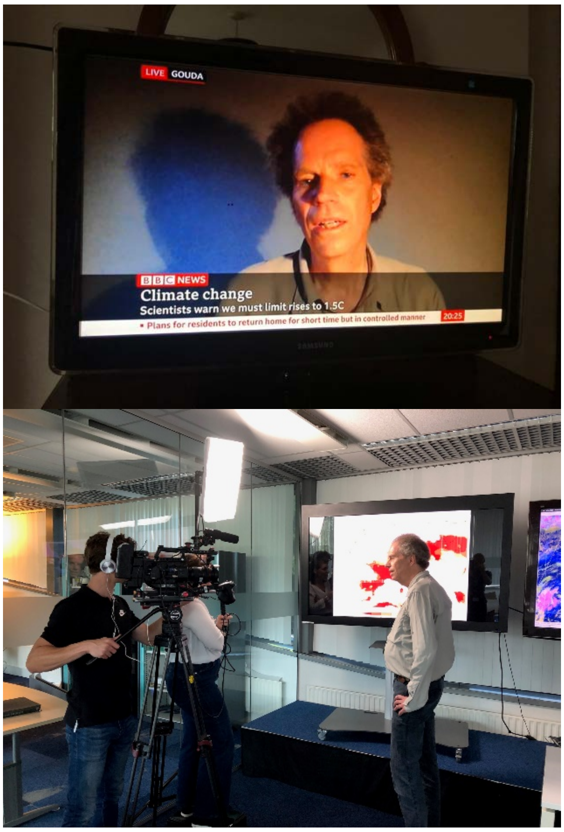
Climate science is unusual in how politicised it has become. Sensational results can garner a lot of attention in the media. A negative consequence of this is that it is much harder to have an open debate about the quality of the science as climate change deniers use these discussions to sow doubt on the basic science. Coupled with a stronger emphasis on the quality of story-telling than the quality of the numbers underlying these stories in peer review for high-impact journals, I believe this has had a bad effect of the quality of the results the field has produced. There are many peer-reviewed papers that claim often sensational results that simply do not follow from the observations or modelling due to errors in logic or overgeneralisations.

However, there are also corrective mechanisms. To my positive surprise, when I participated in the IPCC Working Group 1 AR5 effort in 2010–2013 I noticed that virtually all these wrong results were filtered out of the report.

Let us speak a little about your experiences with media, policymakers and societal and economic stakeholders.

When did you have your first contacts of this sort?

Let me first make my position as a scientist and as a civil servant clearer. As a scientist, I moved from elementary particle physics to climate science, but entered the field in the non-political subfield of ocean data assimilation to make better El Niño predictions and seasonal forecasts. I saw my main task making good forecasts by establishing their skill and reliability, and if possible, improving them. Due to the large 1997/98 El Niño I had frequent interviews with media, but purely fact-based. I kept the same attitude when I moved to climate change: establishing how good the projections had been and trying to improve them, and communicating the results. This also fitted into the KNMI emphasis on adaptation rather than mitigation.



Geert Jan in action with the media.

As a civil servant I have strict rules and unwritten habits about how I can communicate. We are not allowed to speak to politicians directly, and do not have many direct contacts to other stakeholders or activist groups. Contacts with the media have to go through the PR department. This sometimes worked well, but sometimes it made that part of my job unworkable. It is a curious situation, in which everything I say automatically becomes the KNMI message and hence the government message, so it can only work starting from mutual trust. The disadvantage is that I have to be very careful what I say, the advantage is that my words carry a lot more weight and have much more impact than if I would only have been a lecturer at a university.

Talking about science to people outside of science is often a challenge, not only because of the different language, but also since the attempted transfer of knowledge is often hampered by the fact that the audience holds its own views about of climate, climate and impact.

In my experience, the problem of the different languages can be solved with some effort and often multiple takes or a good journalist. How well you manage to communicate after that depends on how much trust the audience has in you. Apart from a few years after “climategate”, I have not noticed problems in that respect. Both KNMI as an institution and myself as scientist and messenger seem to command a fair amount of trust outside the small climate-denying echo hole. I hope I have contributed to that by communicating solid information and refraining from mixing in opinions or prescriptions, acting as an honest broker in Pielke Jr’s terminology. More controversially, I also advocate being open about mistakes, as a scientist and in contrast to most government PR departments I think that at least in science communication, the short-term damage of admitting mistakes is more than countered by the long-term increase in trust by being seen as honest and therefore trustworthy.

You have been an active person with the IPCC, thus you know the process of building the assessment reports. Could you describe it a little, and tell us, how much authority such products have?

To my surprise I was invited to participate as Lead Author of Chapter 11 of the Working Group 1 AR5 report (published at the end of 2013), after publishing a few articles that were critical of the skill of then-current climate models in reproducing observed climate trends. I am afraid I saw my role less in determining the main arguments and more in making sure the details of the report were correct and all statements in our chapter referred to the real world and not to climate model output. The main story was in any case borrowed from the previous report and not discussed much at all after the scoping meeting in which a fixed lay-out was presented.

The significance of the Fifth Assessment Report was in my view that it confirmed almost all findings of the fourth one by a group of authors that consisted in majority of scientists from outside the traditional IPCC community. I was quite surprised by how thorough this re-evaluation was and also how much weight the final report had in the world, as basis for the Paris accord.

Due to my illness, I have not been able to participate in the sixth assessment report.

The IPCC provides global reports; have there been national efforts, to cover knowledge on the reginal specifics of climate, climate change and impact in the Netherlands?

At KNMI we provided the KNMI'14 scenarios[12] on the basis of the IPCC AR5 report, following on the KNMI'06 scenarios that were based on AR4. An up to now unsolved problem is how to reconcile the observed trends with the climate model results in our part of the world: observations show much stronger trends in some variables than the models, so that the scenarios seem biased low compared to the observations[13]. This is strongest in heat waves, which have increased almost four degrees (3 to 5 degrees) in the observations in the Netherlands since about 1900, but only half that in the climate models that are used for constructing the scenarios. Unfortunately, I fell ill in 2013 when I planned to address this for the KNMI'14 scenarios, and I do not know whether my health will allow me to attack this problem for the next KNMI scenarios due in 2023. The aftermath of a budgetary squeeze in the intervening years at KNMI means there is virtually no other manpower available for this.

May we ask you to tell us your view about the relationship of "truth" and "science"?

In my, maybe simplistic, philosophy of science the truth is the way the world really operates. The scientific method prescribes a way to approximate this truth. Although it can never fully describe it, it can come closer, although usually not in a linear way. As far as I know there is no reason that the scientific method should work, the only support for it is that in evaluation of the results it appears to give on average better results over time, in contrast to other methods. Observational fields like climate science have the problem that one cannot do an experiment to falsify a theory, so a theory or model can only be verified against past observations or new ones as they slowly come in year by year. This requires a high degree of self-criticism to make sure that the model is not optimized on the same observations that are used for verifying it. It also implies a serious risk of overfitting or triggering on coincidences in this fairly small set of observations. Physical modelling, the third method of investigation next to theory and observations, therefore has to play a large role in winnowing the coincidences from true connections.

A second major complication in climate science is the interconnectedness of our study object. Many discussions in the field can be traced back to different definitions of the word 'causality' in a highly coupled system.

Note that extreme event attribution is not scientific in the Popperian definition as it cannot be falsified against observations,

[12] www.climatescenarios.nl

[13] (see, e.g., van Oldenborgh et al, 2009, van Haren et al, 2012, Vautard et al, 2020

only against model results. This implies that extra care has to be taken that individual steps do agree with reality.

The European Community finances since about 40 years massively science projects, also in the field of climate science. The funding had a strong effect on the way, European science interacts and focusses research. Could you comment on the beneficial effects of this EU policy? Are the adverse effects?

As far as I can see, the main beneficial effect is that it forces researchers in the field to collaborate, share results, and build upon each other's results. In theoretical physics, the main problem is very smart people having to solve very difficult problems. In contrast, climate science solves complex problems in which the main factor holding back progress is getting people who have solved relatively easy subproblems to communicate better with each other and integrate their partial results into a better solution to the whole problem. Climate models are one way to collect all these partial solutions, but EU consortia encourages this communication among groups of researchers from different countries, preventing them from going down their own rabbit holes.

The disadvantage is that more often than not they involve what we call "sausage-making": applying well-known techniques in a systematic way, maybe giving results that stakeholders have asked for but not advancing the state of science in the field by improving the techniques or our knowledge of how good they actually are. This is more engineering than science and at best is a waste of resources to let good scientists do this kind of work. At worst it gives piles of results of unknown reliability and questionable utility.

The underlying problem is often that the users of these results do not want to hear to what extent they are reliable, they just want numbers to make deterministic projections of impacts of climate change. Solving this disconnect requires good science to establish the reliability of the results and strong two-way interaction with the users to make sure they get the intersection of what they really need and what the science can deliver at the moment. This interaction with the people using our results is an important part of our attribution work and of the KNMI scenario effort.

In climate science, what does the difference between "policy prescriptive" and "policy relevant" stands for?

The KNMI scenarios, which are our main results, describe a set of possible future developments of the physical climate. How these are used for adaptation of society, in our country mainly in water management but also in city planning and other fields, is the task of other government agencies and other actors. Mitigation policies are not based on KNMI products, so we have hardly any role to play in setting climate policies except raising awareness of the problem.

I was taken aback by the role of extreme event attribution in these processes. From a purely logical point of view these results are superfluous: decisions on adaptation and mitigation should be based on projections and not on the random process of extreme events happening now, events that normally trigger attribution studies. However, in practice they are seen as very important in non-logical aspects of decision-making, raising awareness of the problems that may arise from climate change for adaptation and mitigation. These studies also build or decrease trust in projections from the extreme event trend verification that is implicit in extreme event attribution.

As I emphasized before, successful communication of extreme event attribution results depends crucially on the trust that the practitioner has been able to build, based on factors like also publishing negative results and not appearing to have an agenda other than trying to figure out how the probability or characteristics of extreme events has changed due to anthropogenic climate change as well as possible. The scientific integrity I have always been aiming for in my career.

Education

1978–1980 **International Baccalaureate**, *Lester B. Pearson UWC College of the Pacific*, Metchosin, B.C., Canada.

1980–1986 **M.Sc. Theoretical Physics**, *Leiden University*, Netherlands.

1986–1990 **Ph.D. Theoretical Physics**, *University of Amsterdam*, Netherlands, One-loop calculations with massive particles. Supervisor: Jos A. M. Vermaseren.

Employment history

1986–1990 **Ph. D. student**, *National Institute for High–Energy and Nuclear Physics*, Amsterdam, Netherlands. Particle physics, teaching.

1990–1992 **Assistent**, *Ludwig-Maximilian Universität*, München, Germany. Particle physics, teaching.

1992–1994 **Post-doc**, *Paul Scherrer Institute*, Villigen, Swizterland. Particle physics.

1994–1996 **Post-doc**, *Leiden University*, Leiden, Netherlands. Particle physics.

1996–1997 **Post-doc**, *Royal Netherlands Meteorological Institute*, De Bilt, Netherlands. Ocean data assimilation, physics of El Niño.

1997–now **Senior researcher**, *Royal Netherlands Meteorological Institute*, De Bilt, Netherlands.

Selected publications

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