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Georg Götz

Global Change

Interviews with Leading Climate Scientists

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ISSN 2191-589X
e-ISSN 2191-5903
ISBN 978-3-642-23443-9
e-ISBN 978-3-642-23444-6
DOI 10.1007/978-3-642-23444-6
Springer Heidelberg Dordrecht London New York

Library of Congress Control Number: 2011942410

Printed on acid-free paper

Springer is part of Springer Science+Business Media (www.springer.com)

Preface

What are climate scientists doing exactly? Which methods are they using for their research? What is the current level of understanding about climate change and global warming? What is still uncertain or unknown?

These questions are not only interesting for the scientific community itself but also draw an increasing attention from a more general public. Terms like “climate change”, “global warming” or “the greenhouse effect” have become part of common speech during the past decade. Sometimes, it seems important to recall that it is still the scientific research, that forms the basis for all the statements and opinions, which are debated so widely today. The scientific results dealing with global warming and the impacts of a changing climate are discussed extensively and often controversial. This has lead to an increased interest in the methods which are used to obtain these scientific results and conclusions. But the techniques and concepts used in climate science may not be easy to understand immediately by everyone. It seems evident that it is very difficult to obtain a “complete” description of the earth’s climate, as soon as one realizes the complexity of the problem and the endless number of aspects involved, as well as all the possible interactions between various mechanisms that can influence the climate and the extremely long timescales involved.

Nevertheless, great progress has been made during the past decades on many aspects: understanding the climate of the past, identifying and quantifying key processes that influence the earth’s climate, recognizing the way climate and biosphere influence each other, the human impact on the climate and many more. At the same time, many questions are still subject of ongoing research, such as the exact role of clouds for the climate system or the rate at which the Greenland and Antarctic ice sheets are melting. Computer models that allow to simulate the evolution of the climate under different scenarios are extremely important and widely used in climate science. There are few scientific disciplines that use such large and complex computer models. An important question is how to verify these models and quantify their uncertainty since the involved timescales are that long that is is difficult to check these models is situ. But also field measurements are still important and instructive: obtaining information about the past climate or the interactions between climate and biosphere from so called proxydata is a widely used concept.

During the last years, the field of climate science has grown explosively. Not only regarding the number of researchers participating and scientific papers published, but also regarding the number and variety of scientific disciplines involved: mathematics, physics, geology, biology or econometrics, just to name a few. This makes climate science a truly multidisciplinary field and it is interesting to see how results from the different sub-fields contribute to a large picture. Today, not only climate change itself is a subject of research; also the impacts of a changing climate on, for example, ecosystems, the human society or economy are investigated scientifically.

Coming from a different scientific field, I was always interested to learn more about the scientific methods of climate science. Following the huge public debate about global warming and its impacts on environment and society, I realized soon that I did not know much about the scientific background or the “daily research work” in climate science. And I noticed that this experience was shared by many other people. For this reason, I found that a book where different scientists explain their own field of research could be an interesting addition to the existing literature about climate science.

This book consists of interviews with leading climate scientists from many different fields. The covered subjects include the greenhouse effect, glaciers, sea-level rise, computer models of the earth’s climate and the role of the oceans for the climate system. The last few interviews in this book deal with impact assessment and the historical, political and economic dimensions of climate change. Each expert is interviewed mainly about his own area of research but also about his views on the ongoing public debate and interdisciplinary questions. Very few areas of science are subject of such a wide—and sometimes also heavy—public and political discussion as it is the case for climate science and it might be interesting to hear how this fact is experienced by different scientists and how it influences their research work.

As mentioned above, this book is intended to present opinions from experts in some of the many different areas associated with climate science and motivate the readers to go into more detail of what they find interesting. Neither the selection of subjects nor the interviews itself are meant to present a complete picture—there are many more topics that could have a place in a book like this. But I hope the reader will get some insight into the present status of climate science and the views of some of the experts on both past and possible future developments in their field.

Finally, I would like to express my gratitude to all the scientists that contributed to this book by talking to me about their area of research and also their personal views on this exciting and important area of science.

Georg Götz

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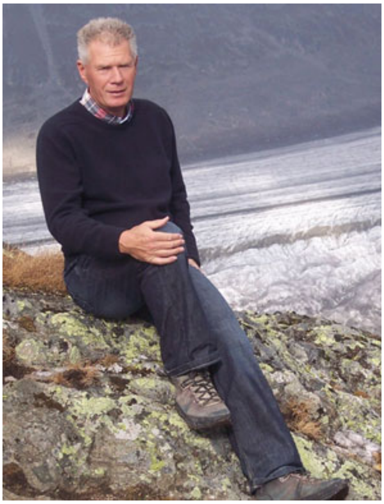
Chapter 1
Understanding Does Not Imply Predictive Power



Hans Oerlemans is professor of meteorology at Utrecht University in the Netherlands. His main research interests are glaciers and sea-level change. He served as a lead author for the first three IPCC assessment reports. In 2001 he was awarded the *Spinoza prize*, the highest scientific award in the Netherlands; since 2007 he is a honorary doctor of Stockholm University.

Prof. Oerlemans, during the history of the earth, there have been Ice ages (the Glacials) and the warmer periods (the Interglacials). In what kind of period are we currently?

I often use a scale from 0 to 1, where 1 means that the complete earth is covered with ice (the "snow-ball earth") and 0 means no ice at all. On this scale, we are currently at 0.25. The last few million years we had the glacial cycles, corresponding to oscillations between 0.25 and 0.5. During the ice age periods, ice sheets have grown in Eurasia and North America; this would then be 0.5 on my

G. Götz, *Global Change*, SpringerBriefs in Earth System Sciences,
DOI: 10.1007/978-3-642-23444-6_1, © The Author(s) 2012

scale. Even longer time ago, there was no ice at all on the earth, this was around 40 million years ago. And before that, the earth was at some point probably totally covered with ice.

The earth was completely covered with ice?

That is the so called “snowball earth” theory, which is supported by more and more evidence. The last time this happened was probably around 700–800 million years ago. Before the development of higher life. So, everything is possible. But most of the time during the history of the earth, there was no ice at all—it was much warmer than today.

One of your main interests are glaciers. Why are glaciers so interesting, if you want to learn about the earth climate?

Well, they are not particularly more interesting than forests or lakes. But for me the link between glaciers and the climate and global warming is the sea level change. Changes in sea level are very directly connected to changes in ice volume, that is how my interest grew.

But apart from that, glaciers are also very good thermometers: First of all, they can be found on all continents, from the tropics to the polar regions. Also they integrate (*average out*) shorter fluctuations, so you get rid of the noise already in a natural way and also they are relatively simple physical systems (as compared to biogenic indicators like tree-rings). Another feature of glaciers is the fact that many of them are at high altitude, relatively far away from cities. So they are at least not directly influenced by local pollution.

An essential information, we get from glaciers is the climatic record in the ice cores (vertical samples of ice, recovered from a depth of down to several km). There is a lot of information stored in the ice itself: if you retrieve an ice core, you get a chronology. And you can measure a lot of parameters in these ice cores: chemical composition, structure of the ice, composition of the air bubbles enclosed in the ice and so on.

What can you reconstruct from that—the past temperatures?

You can reconstruct temperatures, yes. You also find sea-salt in the cores, this might give an indication of how far open water was. There is a lot of dust in the ice, maybe you can find out where the dust comes from which then again tells you about atmospheric circulation. So there is a whole spectrum of information you can obtain.

But one of the most interesting things are the air bubbles which are essentially very small copies of the past atmosphere that you can analyze. So you learn about the climate change and also about the forcing—at least the forcing that is related to changes in the atmosphere. This makes ice cores unique. Of course other objects also have their own advantages, for example the deep-sea cores go further back in time. But in terms of quality, ice cores are unsurpassed.

When studying a glacier, what are the typical measurements and experiments you do?

My group has specialized on the interactions between the glaciers and the climate: We study the micro-climate of glaciers, we have built weather stations right on top of the glaciers because in the past very little has been done on this. But these are automatic stations, we go there and install them, from time to time we service them and do some additional measurements. Really huge measurement campaigns, as we used to do it 20 years ago when we went for a whole summer onto a glacier, have become rare. Instrumentation, especially battery technology, has improved enormously. Today, we have stations in Antarctica, the Alps, Norway, Iceland, so quite a few.

You mentioned to reconstruct the climate of the past from information you get from glaciers. How exactly can you do that? What is the typical margin of error?

We study how a change in climate affects the geometry of a glacier. It is know that—on the global scale—it are mainly temperature changes that drive changes of the glaciers. Of course, the further you go back in time the more qualitative it gets. But if you restrict yourself to a couple of centuries then you can really quantify changes. We have studied how the geometry of glaciers affects its sensitivity to climate changes and the response time. Here is a complication: you remove noise when you study glaciers but they have a response time, which depends on the geometry of the glacier (size, slope but also the climate setting: is it in a wet or dry climate?). But these are things we know by now. Given a glacier, we can estimate fairly well what the response time is and the sensitivity to temperature changes.

I assume, the response time is the time it takes the glacier to adjust to a temperature change. What is this response time for the glaciers in Europe typically? And what what about the ice in Antarctica?

For glaciers in Europe, the typical response time is between 10 and 100 years. You can estimate the response time: take the total precipitation on a ice sheet and the divide the volume by this total precipitation—this gives you a timescale. If you do this for Antarctica you get 50,000 years, for Greenland 5,000 years and for a glacier 100 years. I think that the Greenland ice sheet and in particular the ice on Antarctica are not in equilibrium with the current climate because there are climate changes that happen on much faster timescales that these 5000–50,000 years. For example the last huge climate change was the transition from the last ice age to the current inter-glacial about 15,000 years ago. And Antarctica is still adjusting to that. So even if the climate would stay constant now, you would not expect the ice sheets to stay like they are now, they probably would get smaller.

Actually, by how much would the sea level rise if all the ice on earth would melt?

That is simple, you only have to estimate the total volume of the ice. We then talk about 60–70 m of sea level rise. But this is very unlikely to happen. There was no ice-free earth for at least the last 43 million years. Even if it gets much warmer, a rise of a few meters would take a few 1,000 years.

You talked about the micro-climate of glaciers. From analyzing this micro-climate, can you also obtain information how to model the climate of the entire earth? Are there similar model used?

Well, it is the same equations, the Navier–Stokes equations of hydrodynamics, that govern what happens on the glacier. But on glaciers, there are very huge gradients, e.g. on a warm summer day, the temperature 3 m above the glacier is 15°C but at the surface 0°C. This is a huge stratification that usually is not encountered. The micro-climate of glaciers is persistent but small scale. That makes it special.

You already mentioned the sea level. Let me first ask a basic question: the sea level e.g. in the Netherlands is rising while in other places it is falling (for example in east Scandinavia or the Maledives). What exactly is the "Global sea level", this one number you always hear about?

The global sea level is very difficult to define. There are so many factors that play a role and you have very few absolute references. But normally, the sea level is defined as follows: if you would have no motion in the oceans, the sea level would follow an equipotential level (a geoid) of the gravitational field of the earth. But of course, then you have the dynamics which cause a gradient in the slope of the ocean surface. Furthermore, there are differences in atmospheric pressure, the wind set-up in the shallow coastal regions etc. And then there are tectonic movements and isostatic adjustments. That is what dominates in Scandinavia. But also the salinity and the temperature of the ocean play a role, since they can change the water density. So its very complicated. But what we now normally define as the global sea level is an average of many satellite measurements. This is still a relative number, but that is not a real problem because you are interested in changes. When you use the same source and do the data handling always in the same way, you get reliable information about the changes.

Let me ask a more general question about climate science. About 20 years ago, Chaos theory was very popular when talking about complex, non-linear systems such as the climate. One essential result of Chaos theory is the "Butterfly effect", which means that the time evolution of a complex, non-linear system can change drastically for very small changes of the initial or boundary conditions. This strongly limits the predictability of systems like the climate.

On the other hand, if you follow today's discussion in the media, you sometimes get the impression that the climate evolves in a rather simple way: You often her about monotonous, even linear dependencies. Are these two incompatible vies of how climate science works?

I was trained as a physicist and meteorologist when these theories were popular. The work of Edward Lorenz, where he looked at how non-linearites limit the predictability of non-linear systems, was a standard work for any meteorologist. I think this has been a bit forgotten nowadays—wrongly I think. This is also a bit part of my personal struggle. In many lectures, I say that we have to study the climate but we must realize that predictability is limited, especially when you go to more regional scales, where atmospheric dynamics play a more important role.

In general, people do not like this—especially politicians. People, not specialized in climate processes do not like the message that things are not that predictable. Take the people making impact studies: non-predictability limits the relevance of their work. But I am convinced that there is quite some limit to the predictability.

If I understand you right, there is really a theoretical limit of how precisely we can predict the climate, even if you improve the network of weather stations and the computer models etc.?

Yes, a fundamental limit. And that is of course fully acknowledged when it comes to weather predictions but not so much for the climate. The problem is also that in some stages, the climate may be quite predictable but when the climate system is close to so-called "critical points", it is hardly predictable at all. This variability causes discussion. I gave a public lecture in front of the Dutch academy of science a couple of years ago, where I said that understanding does not automatically imply predictive power. This is the crucial point. But sometimes it is very hard to explain to people that are not familiar with dynamics.

Can you illustrate this non-predictability a bit?

It translates into practice in the way that we get surprises that our models not predict. The huge decrease of sea ice in the Arctic is an example. Or the heat wave that we saw 2010 in Russia, the heat wave in central Europe 2003. Afterwards, you often find an explanation. And when the models are calibrated they can do it. But these events are almost never predicted, this is what I mean. Surprises will keep appearing—not because we are stupid but because there are feedbacks, non-linearities in the system, that suddenly work together.

And you cannot explain these events only with statistics. During the heat wave in 2003, the mean temperature in Switzerland was more than 5 standard deviations above the average. In terms of probability, you would expect such an event once in 1 million years (if you assume a Gaussian distribution). Therefore, it is very unlikely that this was just a statistical fluctuation. I believe these events are rather the results of complicated non-linear processes.

That fact that climate science has become extremely popular during the last 10 years–did this have a positive or a negative effect for you as a researcher?

It is of course always nice when your topic get more attention. But in terms of funding and the way we can work, I cannot say it is positive. The positive effect has been earlier—20–30 years ago when people first became interested in the climate question. But during the last 10 years, research has become more and more scheduled, even from the science foundations. There is less room for fundamental research—but this is not only the case for climate science. Compared to 30 years ago, there is more mistrust from the side of politics, that scientists would not do things that are useful for society. The views, the way society looks at science in general, have really narrowed (this holds for culture as well, by the way).

During the last years, climate research has moved extremely into the focus of politics. Decisions of huge impact are made. Nicholas Stern (London School of Economics) has estimated that reaching even moderate goals for the reduction of CO_2 emission will cost at least 1% of the global Gross National Product.

How does the transfer from knowledge and assessments from science to politics happen? What is your experience?

My personal experience is not too good. I was in fact involved in the IPCC assessments from the beginning, I participated in the first three reports (1990–2001). It started with fairly direct connections from scientists to politicians, but then there came what I call "Climatogracy": There are large groups, even at universities, that specialize in translating the scientific results of climate research for the policy makers. And then they summarize and they summarize the summaries. I thinks this happens in a time in which we anyhow have got a "consulting culture" in politics. When politicians need advice on some topic, they go to a consulting bureau and pay them a lot of money.

And is it worth the money? Are your results transferred correctly to the politicians?

No, not at all. I think the IPCC is still the best we have. It has been criticized a lot, sometimes with reason. But there is no organization like that, that could work without criticism. The IPCC is still fairly unique. But all the initiatives on the more national and local scales are completely useless. We have some groups here in the Netherlands that claim the IPCC reports are too conservative about the sea level rise, a statement based on only a few publications on glaciers and Greenland. So they say: well, lets do it our self and they established this Delta commission and so on. This is useless, rubbish.

Many people are involved, it a huge machine. But the number of scientists that look at fundamental processes in the climate system is very small compared to the number of people that do something with their results. Its a pyramid upside down. Once, I talked about that to our minister and said: *Why are you asking these consulting bureaus for advice about climate problems? If you are worried about the Greenland ice sheet—we are working there, we have a good track record. If you need information, phone me and within a week I will write down in 2 pages, what I think is the state of scientific knowledge. And you do not even have to pay me since you are already paying me.*

Would you go as far as saying that politicians are receiving wrong information about climate science?

It is not a question of right or wrong, it is the color. Once, I had people here from the USA, asking for scientific results, I call that "shopping". The asked me for data about glaciers. I gave them results on 200 glaciers and they selected the 20 glaciers that did not retreat and made a story out of that. They knew from the beginning what they wanted, they were just collecting suitable arguments.

All these groups from the advisory and consulting business have a lot of influence. But they do not produce anything useful. It would be the responsibility of politics to recognize this and go back to the more direct lines. I think on the global scale this is the central problem of climate science. Science has a minor role, the consulting bureaus select results the like.

But I do not fight against this anymore, you cannot win and it takes all your time. I try to write decent scientific publications. When politics do not check the quality of the people they ask for advice—then it is over.

In your research field, what are the most crucial open questions to look at in the near future?

I would say, it is still the diagnosis of the ice sheets: What would they do without climate change? That remains a central question. I think we can make good progress there by using satellite measurements or investigating ice cores. But there are incredibly many processes that contribute to the memory of the ice sheets. One example: We are currently studying darker zones on the Greenland ice sheet, that are directly related to dust that emerges from the deeper layers to the melting zones. This has a huge impact on the melting rates because darker ice absorbs more solar radiation.

And there are many other things that we still need to understand, so I would say the diagnosis of the current ice sheets is still the central issue.

My final question: could you give a prediction what the temperature in central Europe will be like in 100 years?

Well, this prediction is the outcome of a chain of assumptions: what will happen to society? how do we generate our energy? What will be the level of Greenhouse gas emission? But I think the temperature here will be a couple of degrees higher, maybe 2 or 3 °C. Also the see level will rise, maybe by 50 cm, but surely not by meters. But the error-bars of such predictions are large.

Prof. Oerlemans, thank you for the interesting discussion.

Chapter 2
We Always Make Models for Something, Not of Something

Hans von Storch is director of the *Institute for Coastal Research* at the *Helmholz Center* in *Geesthacht* (HZG) and professor of meteorology at the University of Hamburg in Germany. His research interests include coastal climate and methodical questions of climate science. He was a lead author of the third IPCC assessment report, and now again, of the fifth assessment report. He has published several books on climate science and the social and cultural aspects of climate. He is a doctor h.c. of the University of Göteborg.

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G. Götz, *Global Change*, SpringerBriefs in Earth System Sciences,
DOI: 10.1007/978-3-642-23444-6_2, © The Author(s) 2012

Prof. von Storch, you are working a lot with mathematical models that describe the earth's climate. Are all scientists using the same models or are there major differences?

There are very different types of models, we usually talk about hierarchies of models of different complexities. That goes from maximally simple models to maximally complex models. Maximally complex models, the global climate models (GCMs), try to describe the climate as a whole and they are all quite similar to each other. Some may be spectral models while others use finite differences. But in principle they are all built on the same ideas. Maybe one should point out that the concept of a "model" is not an easy one, simply because different scientific disciplines mean something different when they talk about models.

Could you explain how the models that you are using work?

First of all, it is important to realize that we always make models *for* something and not *of* something. They are made for a certain purpose but they are not models of something per se. Take the maximally simple models—they are made to describe a fundamental connection in a simple way. You cannot expect that they reproduce the evolution of the climate system in detail, but their purpose is to generate understanding of key mechanisms. One example is the model of the geostrophic wind: This is a wind that is characterized by a flow direction parallel to the isobars (and not from high pressure regions towards lower pressure regions, as one might naively expect). Here we have a simple model already. With the simple models, I can generate understanding but rarely (meaningful) accurate numbers. But it is these types of models that we have in mind, when we say "I understand a mechanism".

In the GCMs, on the other hand, I try to include as many processes as possible-of course preferably those ones that I expect to be crucial for the problem I am investigating. That are not only first order processes but one also tries to include higher order processes—second order, maybe even third order. The limits are only set by the available computer-power. But these models do not generate understanding, they provide an experimental platform, where we can change or adjust parameters. For example, I can try out what happens to the climate when I remove Australia from my model world. But this does not imply that I understand why things are changing then, for this I again need the simple models. They can help me to understand the mechanisms.

But I assume some understanding is necessary to set up these GCMs…

…that is true, but I do not directly generate new knowledge about the world, I only generate numbers, and possibly knowledge about the model. It is an engineering challenge to create such a simulation, a "machine", that behaves somewhat analogous to the reality. How this is done? We have the advantage to have a few fundamental equations. We can write down those—fine. But when we want to implement these equations into a computer model, we have to decide which length scale we want to resolve. We have to set the resolution somehow and this leads to problems. When I look at the Navier–Stokes equations (the fundamental equations of hydrodynamics) and go to small length scales, I will get turbulence. But I cannot describe this turbulence explicitly in my GCM. Without turbulence, however, I will get wrong results. So the solution is to modify the equations. I do this by choosing which length scales I still resolve and which length scales I do not resolve any more. Then I have to investigate what effect the unresolved length scales will have on the resolved, the large length scales. I have to describe turbulence somehow implicitly, in order to get correct results for the large length scales, I am interested in. This technique is called *parametrization* and it has much of an art.

How can you validate a climate model that you maybe would like to use to investigate a certain effect?

Generally speaking, you cannot validate a climate model, you can only validate a model with respect to a certain purpose. I can look at diagrams I am interested in, relatively simple diagrams: for example, is the temperature distribution of the earth reproduced correctly? The trick is now that we have the parametrization: terms, which depend on the resolution, are added to the equations. If I use a resolution of 1 km, I might already get some convective clouds explicitly, but if I use a resolution of 20 km I do not have a single convective cloud. Then I have to describe clouds by parametrization.

The often asked question: "What are the equations, you are using?" cannot be answered because there is nothing like "the equations". Given the resolution, one can make suggestions how the equations might look for this very resolution. For example, if you look at the first theorem of thermodynamics (energy conservation), you will find source terms in your climate models which are looking strange at first glance. But then you realize that they are related to condensation taking place on very small scales. You are back to the parametrization again.

We have a lot of these micro-physical scales, which we have to parametrize and that is, as I mentioned already, an art. This is something that might not always be understood from the general public. Parametrization is also the aspect where the different GCMs might deviate from each other.

If the models are different in terms of the parametrization—are they nevertheless giving the same predictions about the climate?

In principle, yes. Predictions in climate science are mostly conditioned predictions, scenarios. The emission of greenhouse gases, for example, is something you cannot predict but it is a crucial parameter for the evolution of the climate. You can make an assumption about the level of emissions and then calculate how the climate evolves under that assumption.

But taking this into account, all the models yield quite similar results. There might be small differences, maybe the equilibrium temperature for a doubling of the CO_2 concentration in the atmosphere is 3°C for one model and 4°C for another model. But the general trends are reproduced with all models. Of course that is something, one could also be critical about. The scientists making these models know each other more or less. And if somebody then finds very unusual results, he might become shaky and say: Well, maybe my model is not as good as the other 17 models that are around. And then he tries to adjust his model to agree with the other 17 models. There is also a social process that leads to the agreement between all the different climate models.

But in general, the question about the quality of these models is not easy to answer. There is a huge difference to weather predictions, where you can permanently validate and adjust your models since you basically see within three days whether your prediction was good or not. You can try out much more, that is something you unfortunately cannot do for climate models.

At the *Helmholtz Zentrum Geesthacht* you are also investigating regional aspects of climate change. Is this not even more difficult? I could imagine, you can average much less when looking at smaller scales, compared to global scenarios.

I do not agree. Maybe it is a mathematically less well defined problem compared to the global climate. But what we do in practice is to set the global-scale climate: the temperature, wind direction etc. And then we determine what happens on a regional scale—that works quite well.

How large are regional fluctuations for a given global evolution of the climate? Is it possible that the climate in northern Europe develops very differently than in the Mediterranean?

Temperatures are rising everywhere and there are no huge differences between neighboring regions. But if you look at precipitation, regional differences can be significant and one has to check carefully whether different models give the same results. We have recently made a survey for the Baltic Sea region and we find that it gets more humid in the entire area, especially in the north and during winters. Around the Mediterranean sea, however, we expect it to get dryer. You see, here we have very different regional manifestations of the same emission scenario.

Is this a general feature—that predictions about precipitation are much harder to make than about temperature?

Absolutely. Precipitation is a quantity that you can only represent by parametrization. There are no raindrops falling in your climate models. It is rather the warming which would result from precipitation that you specify in the models. The model does not care about rain, it cares about the release of heat which happens through condensation. Afterwards, you can convert this into precipitation—but inside the model there is no explicit rain. There are also models that determine the amount of water stored in clouds—this a bit more explicit but still not what we would call "rain" in our daily life.

Extreme weather events have been widely discussed during the past years. I think of the heat waves in Russia 2010 and central Europe 2003 or also the strong winter 2009/2010. Are that statistical fluctuations or do you have to take complicated mechanisms into account to explain these events?

You can view the weather system as a random generator and the climate is the statistics of this random process. If you average a certain variable—let us say temperature—over a certain period then you will get one number for the last decade and a different number for the decade before. But the emergence of a difference does not imply that there is a reason for this change, it might just be a result of the randomness, smoke without a fire. The weather is a complex, non-linear system that also has some intrinsic inertia. It is very well possible that the weather system remembers: I was warm last summer, now I will be warm this summer again. But this does not mean that the long-term statistics are changing.

The question: "Is the climate changing?" should rather read "Are the long-term statistics different?". Only if you need different statistics to explain the changes, you can talk about climate change. One example: We have measured global temperatures for the past 126 years. The probability that the 13 hottest years of this period all occurred within the last 16 years (these are real numbers, by the way) is very small, about 1/1000. This is something we have calculated (taking also this memory effect into account, which I mentioned earlier). That means, we have with a very high probability a change of the statistics—the climate gets warmer. From just two warm summers, you cannot draw such a conclusion.

A few years ago, there was a controversy concerning the so-called *hockeystick graph*, that illustrates the strong global warming during the last century as compared to the past 1000 years. You have criticized the underlying mathematical methods, leading to this *hockeystick*. Could you summarize what the controversy was about?

The hockeystick graph has the following background: If you want to reconstruct the temperature record of the past 1000 years, you are facing the problem that there are no measured temperature records for most of this period. Therefore, people have analyzed data which is believed to contain climate information (so called *proxydata*, e.g. data from tree rings) and tried to reconstruct temperatures from that with statistical methods. This is an appealing idea and it resulted in the hockeystick.

We have then tested the underlying statistical method, by using data from a regular climate model which also was run over a period of 1,000 years. We used a simulation which gave us some significant changes in temperature because we varied not only the CO_2 content of the atmosphere. As a result, we got a temperature record which was comparable to the real one. Now we created artificial proxydata using simulated local temperatures to which we added some noise, at first we used white noise. We took this artificial climate data and applied the hockeystick—method to it. Now you would expect to find back something comparable to the original data from our simulation. But what we got was a temperature record, where the low frequency components where heavily damped, that means the slow changes were not reproduced correctly.

We then did some additional checks to make sure we really investigated a situation equivalent to the original hockeystick problem—with the same result. So we concluded that the low frequency components of the temperature change were not represented correctly—that is something you do not want when investigating this kind of problem since it means the shaft of the hockeystick is not reproduced correctly.

In my eyes, the controversy about the hockeystick was not that significant from a scientific point of view. But the topic has great political potential because the hockeystick has become an icon. Its purpose is to show in a simple way and even to the most stubborn skeptics that climate change is really taking place and a threat. However, you certainly score an own goal if it turns out that the scientific method you used has some serious methodological issues. But in science, I would say, this was one controversy among many others.

You certainly appreciate the attention, your field of research is receiving these days. Has this enormous public and political interest also a positive effect on your working conditions as a scientist?

The attention is certainly helping to raise funding for climate science. But this also has a drawback which I use to describe with the term "postnormal science": In climate science, we have an inherent uncertainty and we cannot run away from that. We can only sit it out (which, however, takes much longer than our lifespan) until it becomes clear whether our models are good or not. And then there are the enormous implications for political and economic decisions. This leads to a situation where all kind of groups are joining the discussion process, including the research work. Science itself has become heavily politicized and this makes science not better but worse because now results are not only judged upon whether they are scientifically solid and methodically correct but also whether they are of political or economic use. That leads to damages inside the scientific community and also harms the reputation of science itself. Sometimes you get the impression of science serving various interest groups.

Would you say that politicians are correctly informed about the state of current knowledge and results in climate science?

I cannot talk about politicians in general, there are many different ones. I think, many if not most politicians pick the results which suits them and their views the best, as you would expect it for any interest driven institution. At the end, politicians represent certain interests. I see politicians, who are concerned about the environment, concentrating on rather pessimistic and alarmistic results. And politicians, who have a preference for a strong economy, are citing other scientists. Politicians are most often looking for those scientific results that support their program—that is something we have to recognize.

I sometimes get the impression that climate science is an extraordinary controversial field. Is that true or is that an exaggerated picture we get from the media?

The media try to make things controversial because it is more entertaining then. I would not condemn that, controversies are what the people like to hear about.

But there are also scientific controversies, that is for sure. At the end, we as scientists are elements of the culture we live in and that surely affects our analytical abilities. The decision about which questions are appealing to us, what we want to research—that is something that has to do with our views and values. And also, which answers you are willing to accept: if you find something plausible, you do not need much to be convinced. If, however, you are reluctant, you will need to see much more evidence before you are willing to accept a certain result.

That is understandable. But are there also disagreements between scientists that are investigating the same problem but come to different conclusions?

The question of global warming is not very controversial, as you also can see from the last IPCC assessment. Almost all agree that there is a global warming at the moment and that a significant part of it is caused by humans. There is also consensus that this will go on for a while unless there is a fundamental cut of greenhouse gas emissions.

But there are also more controversial problems, for example about the future of the ice sheets on Greenland and Antarctica. Or sea level rise. These are topics where we need to do more research and where we may see some surprising results in the future. And under these circumstances, it is natural to have scientific debates. One mistake of the IPCC was not to point out where there are disagreements. Instead, we created the idea that we agree on everything. That is not the case.

Also clouds and radiation are topics where we know that we do not know enough. Or issues like dissipation of energy in the oceans and the CO_2—budget; these were unsolved problems for a long time. Actually, I do not know whether they have been completely solved by now. But these are all good and interesting research topics that can keep us scientists busy, and, of course, we argue about them.

Let us talk about your research field. What are the big, unsolved problems that you want to tackle the coming years?

That is maybe not as spectacular as you would think. As a director of an institute, I am not completely free in my choice what to do. There is also some long term strategy, I am part of. But this is fine, I am responsible for a lot of people and I think their work should be coordinated in a neat way. But in the coming time, I want to concentrate on mainly two topics.

The first one is a scientific one. I want to investigate how different kind of storms develop in different regions. How did they develop in the past? What is their impact on the coastal waters in different sea areas? What can we predict for the future?

The storms I have in mind are our local storms here, plus polar lows, typhoons and mesoscale storms in the Mediterranean. I decided not to include hurricanes since this topic is so controversial from a political point of view that you cannot work on it without being heavily involved in these political aspects. You know, there is this strange correlation: if you claim that the number of hurricanes is not increasing you automatically support the war in Iraq and vice versa. This is this "postnormal science". Hurricanes play a key role in convincing the people in the US that climate change and global warming are real and that something needs to be done about that.

We investigate typhoons in Asia instead, this topic is less preoccupied. But: by no means less relevant—maybe you remember the flood in Myanmar 2008: 100,000 victims of a storm that even was predicted. There are huge risks related to these typhoons which are maybe not always recognized here.

And you want to develop a prediction system or are you planning to do fundamental research?

We want to investigate which changes there were during the last decades, concerning these storms. Were there changes at all? Are the changes within the natural statistical fluctuations or do we need to take climate change into account for a proper description? Are the developments consistent with the scenarios of plausible future developments we get from our models?

What I can already say: For our regions here in Europe, there are no systematical changes (although everybody thinks there are). By the way, this is consistent with our models which envisage only weak changes in regional storm activity until the end of this century—despite the ongoing climate change. Our goal is to extend this type of analysis to other regions of the world.

Something which we also work on is the effect of storms on the marine environment. Which other factors have an influence on the marine environment? The storm surges in Hamburg, for example, have gotten higher and higher during the last 50 years. But this is not because there would be more or heavier storms but it is mostly because of the deepening of the river Elbe up to Hamburg and because of the improved coastal protections. For a wave, like the tide or a storm surge, it is much easier to travel up the river today. So we are also taking these alternative explanations into account—anthropogenic factors that might increase the risk. On the other hand, such modifications of the river's geometry could also be used to decrease the risk. If we can redesign the environment of the river to make it a bit harder for waves to travel up the river, then this could reduce the impact of storm surges.

The second topic, I am interested in, has to do with this "postnormal science", we already talked about. In my field, we are working in a strongly politicized environment. This means, I need to reflect on the cultural, social and political boundary conditions that are present.

Already our grandmothers said that storms are getting heavier and heavier. So it seems this is a pattern of thought that is somehow inside our heads. And it also influences us as researchers in one or the other way. This is something, I find extremely interesting. I think, you cannot do research on the climate without caring about these social and cultural aspects. Someone like me, who also talks to politicians and the media, has to reflect on his working conditions and how he can maintain his scientific independence.

Is there a central problem in climate science which has not been solved at all up to now? Something you would concentrate on, if you had all freedom and possibilities?

Maybe I am a bit unimaginative in that respect. But if I could initiate a research program—let us say I am 35 years old, I get my own institute and can do what I like during the coming 30 years—then I would like to simulate the period from the last interglacial until today with a global climate model. Starting 100,000 years ago and including all details, the growing and melting of the glaciers and ice sheets for example. There are so many interesting scientific problems connected with that and today we might have enough computer-power to do these kind of things. But this would be huge project—you would need a lot of time and manpower. And I am no longer 25 years old.

Prof. von Storch, thank you for the interesting discussion.

Chapter 3
Time Means Are More Predictable Than the Instantaneous State

Erland Källén has worked as a professor of Meteorology at Stockholm University from 1996–2009. Since 2009, he is director of research at the *European Centre for Medium-Range Weather Forecasts* (ECMWF) in England. His main research interests are climate modeling and weather prediction systems.

Prof. Källén, around 1900, the Swedish scientist Svante Arrhenius investigated the role of CO_2 for the global climate. He concluded that a doubling of the CO_2 level in the atmosphere would lead to a temperature increase of 4–5°C. This is a bit higher than today's estimates but nevertheless a remarkable achievement. How did he come to this result?

This was before anybody knew about quantum absorption of radiation, spectral lines and things like that. But he was basing his research on results which, among

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DOI: 10.1007/978-3-642-23444-6_3, © The Author(s) 2012

others, a researcher named Langley had obtained earlier, doing some very smart measurements on the radiative fluxes, both from the earth's surface and also from the clear sky under dark conditions with moonlight. This, he then put together with some physical reasoning where these radiative fluxes could originate from.

He knew from experiments, that CO_2 was absorbing heat radiation but also emitting. He also knew the thermodynamics of water, in particular the relation between temperature and the concentration of water vapor in the atmosphere. He combined all these ingredients in a very simple energy balance model and then he made some *back of the envelope* calculations, as we would say today, of what the result of increasing CO_2 in the atmosphere would be, taking the feedback with water vapor into account. This is, how he got to his results.

Arrhenius was really before his time in doing that. His motivation was not so much the fear that men would start burning coal and oil but he wanted to understand the mechanisms for ice ages. By the way, he did not take any effects related to circulation in the atmosphere into account—he did a thermal equilibrium calculation.

You have been working a lot on climate models and now you are dealing with weather prediction systems. If you compare a computer model that is used to do whether prediction with one that simulates the climate over longer periods: what are the main differences?

Conceptually, they are very similar. They are both based on the laws of physics, on conservation laws: conservation of momentum, conservation of energy etc. They are also based on radiative fluxes in the same way and they use the same formulations for phase transitions between clouds and water vapor. The big difference between weather models and climate models is the spatial resolution and the time resolution. Obviously, if you want to run a model over several 100 years, you cannot afford to have the same resolution as if you run it over 10 days.

What is the typical time resolution, a step in time, you use in a climate model?

In a model, you have to divide the earth in segments. And then you calculate how the weather variables of these segments evolve in time. Also time is discretized in steps.

In a climate models a step in time is on the order of one hour, whereas in a weather prediction model such a time step would be 5–10 min. But the big difference is in the horizontal resolution. Today, the most advanced climate models have grid resolutions of around 200 km. In weather prediction, we are doing global modeling down to 15 km resolution.

An important point is also the validation of models. This is probably much more difficult for climate models?

Weather prediction models are validated against reality every day. But there is an important distinction between climate models and weather models, which has to do with the chaotic behavior of the atmosphere. Because the atmosphere is a chaotic physical system, the result of a forecast is very much initial value dependent. For a forecast beyond a certain time range, it is impossible to say what the weather will be at a certain place and at a certain time with any accuracy. This time range limitation is on the order of two weeks. We know that from theoretical research but also from practical experience. That means for weather prediction, that our models will never be very useful beyond 14 days. Presently our forecast systems give useful information out to about one week, so we still have some research and development work to do until we reach the theoretical limit.

When we go to longer timescales and investigate the climate, then it is not so much the initial state dependence of the atmosphere that matters. Important are now the oceans and the land surface. And these systems have a much larger inertia, a longer memory. Furthermore, if we talk about climate we do not mean the weather at a certain place and a certain time, but we are looking at time means. For example the mean temperature or average precipitation. And time means are more predictable than the instantaneous state.

About this limit of 14 days: You say, there is a general limit of how far one can predict the weather. And this limit is given by the weather system itself and cannot be improved by using more complex models or better computers?

Exactly. You can look at this from a theoretical point of view: If you have a system, where the small scale motions seriously affect the larger scale motions, there is a timescale, how long it takes a small scale perturbation to completely saturate the error on the large scale. And this is an intrinsic property of the system. This is, how the dynamics of the atmosphere work and it has been investigated by turbulence theory.

But nevertheless, there are aspects that are quite predictable over longer timescales: on average it is warmer during the summer than during the winter—that is very predictable. But now we talk about time means again. In climate modeling, we investigate how these time means change when the total energy balance of the system changes. To do this, one has to understand all the feedbacks within the system.

Not only the complexity of the models has developed during the past decades but also the amount and quality of the available data has increased. I think of satellite measurements or a more dense network of weather stations. In which way does that improve our understanding of climate processes?

This climate data is extremely important to determine the fit parameters of your model. All models have a limited resolution. Even a weather prediction model with a resolution of 15 km can only fully resolve phenomena of maybe around 50 km. But we have phenomena that have much smaller length scales than that: clouds or turbulence, for example. Those are essential for the energy balance of the model and have to be included parametrically. And this is where the difficulty lies—to obtain and validate these parametric descriptions. Usually, when we want to parametrize a certain process, cloud formation for example, we start from some basic physical equations, make a parametrization scheme and then compare the results to the reality. Here, it is very important to have precise and complete measurements—to be able to make an accurate comparison between model and reality.

You just mentioned cloud formation. If I am correct, the formation of clouds is a process, that climate models still have problems to describe correctly. What is the current status here?

Clouds generally form, because air cools as it rises. When the air gets colder, at some point the saturation humidity is reached, water condensates and clouds are formed. Cloud formation depends on the vertical motion field in the atmosphere. But the vertical motion field is much weaker than the horizontal motion field. The reason for that is that the vertical motion field is determined by slight imbalances in the horizontal motion field. The average wind speed at 5 km height is maybe about 15–20 meters per second. And we can measure this with an accuracy of a couple of meters per second. But to determine the up-and downward motion, we must have a description of the wind which is more accurate than the current observations. So it is really the model dynamics that determine where we have these ups and downs in the motion field. And that determines where the clouds form. Furthermore, when the clouds form, they have all kinds of shapes. The effect on radiation is very sensitive on whether clouds contain water drops or ice crystals. And this point is really the big challenge today—and very difficult: To describe properly where we have water clouds and where we have ice clouds. We know from basic thermodynamics, that ice particles start to form at temperatures below 0°C. Around temperatures of $-15°$ C, we have a maximal efficiency in the conversion from water to ice. And as we get to temperatures of $-40°$ C to $-50°$ C, everything is ice. But in between, there is a mixture of water and ice. It is very tricky to describe this properly. Surely, this is one of the big questions in climate research today. The parametrization of this process is one of the major uncertainties in determining the cloud feedbacks and sensitivity of climate models to changes in radiative forcing or changes in CO_2 concentrations.

And what exactly is the different effect of ice clouds and water clouds on the climate system?

Clouds can reflect the visible radiation, here water clouds are more effective. But clouds also have a greenhouse effect. They trap long wave radiation: they reflect back a substantial amount of the outgoing heat radiation, which then leads to warming at the surface. Water clouds and ice clouds have very different properties in trapping the heat radiation. Also the specific composition of the ice particles has a very large effect on this radiation trapping. And this is one of the big questions—how to describe and model this in an accurate way. Already to get precise observations is difficult, since you have so many different possibilities in the atmosphere: different layers of clouds, overlapping clouds etc. Almost infinitely many cloud configurations can occur.

Is the cloud structure not something that one can look at with satellite measurements very precisely?

Satellite measurements are important, but not enough. Especially not, when you have layered clouds, since you only see the top layer. You can use radar measurements, but radar has a limited range and sometimes, it is not that easy to interpret what you see on a radar reflection measurement. A very interesting technique to investigate clouds is Lidar *(light detection and ranging; similar to radar, but using light instead of microwaves)*. There are even satellites equipped with Lidar systems. This has really enhanced our understanding of the cloud dynamics in the past few years.

You work on weather prediction models. Weather prediction is not only about telling people whether it will rain tomorrow but also about sending out warnings for heavy weather events in time. I think of storm surges or windstorms, that can be potentially very dangerous and lead to huge casualties.

One of the main goals, we are pursuing at the *European weather centre* is to improve short and medium term predictions of what we call "severe weather events". Today we can give reliable predictions of such events on rather short timescales. One to two days, I would say. Our goal for the next 10 years is to improve the quality of these predictions on a timescale of two to four days. We are quite confident that we can do this if we get an increased funding for our computer resources and also for maintaining and developing the research we are doing. We need to continuously improve the models and tools—both with respect to forecast models but also to use the observations that we have in the best possible way. We have to be able to describe the current state of the weather system very accurately to make good quality predictions. This aspect takes more than 50% of our efforts. Because the weather system developments are highly initial state dependent, it is absolutely crucial to get the initial state right.

You have done research on Arctic warming, as well. I heard that warming in the Arctic region is almost twice as large as the global average. Why?

This is correct, if you talk about the warming at the surface. There are a number of different factors which can explain this. One factor is the sea ice. When you have warmer temperatures in general, then the sea ice melts. As the sea ice melts, you get less reflection of visible radiation during summer time. That means now, that more heat is stored in the Arctic during summer time. And this heat is released in the dark part of the year, which means that the winter temperature does not go as far down as would be the case otherwise. This is a feedback process, which enhances warming. Another factor is the higher temperature impact of greenhouse gas warming in the Arctic. As we usually have shallow inversion layers in the Arctic a certain increase in the greenhouse gas forcing gives a larger surface temperature response in the Arctic than it does in the tropics.

But this alone cannot explain all of the enhanced warming in the Arctic, there must be other factors. One specific factor which we have done research on is the possible feedback from both the atmospheric heat transport into the Arctic and the transport of water vapor into the Arctic. When you increase the CO_2 concentration in the atmosphere, you can see from simulations that also the transport of heat and humidity changes. And this also seems to give a contribution to this increased warming in the Arctic. So there are different factors that contribute, but we still do not have a complete understanding why the warming in the Arctic is twice as large as the global average. I strongly suspect that the missing pieces of this puzzle are again connected to the clouds.

Has this enhanced warming in the Arctic region also some influence on global climate phenomena?

That might be the case, but it is hard to give a definite answer. The enhanced warming of the Arctic could have an effect on the so called thermohaline circulation of the ocean. But this is difficult to say at the moment. For the ocean system, there is still a lack of observations. We need more measurements of the ocean circulation and the ocean temperature distribution. This is necessary to investigate a number of mechanisms that are theoretically proposed. Until now, less resources have been put into deep ocean measurements than for example into atmospheric measurements. This is partially driven by the need for weather predictions but also by technology advances. Considerable resources have been put into the industry that build and develop satellite technology.

In the media, climate research or global warming are often discussed in a very simplified way. It seems, people are looking for short and simple answers, even if the subject is highly complex. How do you as a researcher experience this issue?

Well, since I work here at the *European Weather Centre*, I concentrate on weather predictions and do not take part in the climate debate. But from the time when I worked as a climate researcher in Sweden, I have some experience with the media and public discussions about climate. I think an important point to convey, is the message that climate research is based on fundamental physics. We have the laws of physics and use those to describe the dynamics of the climate system: atmosphere, ocean, ice and so on. It is not a purely statistical description, in the sense that we would use data from the past to make extrapolations into the future by statistical models. If you can convey that in 30 s during a television interview, then you are really good. But yes, sometimes you have to use simplified arguments to make a point clear.

Prof. Källén, thank you for the interesting discussion.

Chapter 4
The Large Scale Dynamics Are Reasonably Well Understood, Un-Certainty Lies in the Parametrization of Small-Scale Processes

Andrew J. Weaver is professor and Canada Research Chair in the *School of Earth and Ocean Sciences* at the University of Victoria/Canada. His main research interests are climate dynamics, the role of the oceans in the climate system, and Earth system modeling. He has served as a lead author for the 2nd, 3rd, 4th and 5th IPCC Working Group I reports. He has published numerous articles and several books on climate change. Andrew Weaver is a Guggenheim Fellow, a Fellow of the Canadian Meteorological and Oceanographic Society, a Fellow of the American Meteorological Society and a Fellow of the Royal Society of Canada.

Prof. Weaver, it is widely known that ocean currents have an effect on the climate. The Gulf Stream, for example, influences the climate in western Europe. What about the opposite effect? How do changes in the climate influence the oceans?

In fact, the ocean and the atmosphere are really a coupled system. It is not so much one influencing the other, as them influencing each other simultaneously. On the timescale of days to weeks, the atmosphere interacts mainly with the ocean's sea surface temperatures. But on longer timescales, you begin to involve the upper layer of the ocean; on very long timescales you also affect the deeper

G. Götz, *Global Change*, SpringerBriefs in Earth System Sciences,
DOI: 10.1007/978-3-642-23444-6_4, © The Author(s) 2012

ocean. You probably know about El Niño—this is a fully coupled phenomenon that involves an interaction between the upper ocean and the atmosphere. On the decadal to century timescales the thermohaline circulation (or overturning circulation) interacts with the atmosphere. But it is hard to actually say one would be driving the other as they are fully coupled.

The oceans form a very complex system, with their temperature distribution and all their currents and circulations etc. How stable is the current state of this system? Are there instabilities, where small changes in the boundary conditions can lead to dramatic change in the system?

Over the last decades, the community has come to understand quite well the actual mechanism behind how a large scale instability of the sinking in the North Atlantic can occur—not the precise amount of fresh water, required to cause it, but the mechanism itself. The mechanism is the following: You have surface water that is relatively salty in the North Atlantic. But should that surface water become very fresh then it can become lighter than the deeper water and therefore it can stop sinking. And that has a feedback on the transport of heat from lower to higher latitudes. So this instability is reasonably well understood.

The question is: To what extent do we understand the thresholds beyond which this effect is triggered? And of course there is a lot of variation between model projections, with different parametrizations of small scale mixing processes showing different results. But the main statement is that the large models as a collective really do not show this as something that is in the cards over the next century or two. This is simply because the perturbation on the fresh water is too small.

But as I said, there is much evidence that this has happened many times in the paleo-record over the last glacial cycle. The difference between then and now is that there were vast quantities of ice on land which provided a much greater potential for fresh water sources than we have today. We also know that the threshold depends on the mean state itself. Colder climates are inherently more unstable than warmer climates.

Is there any characteristic phenomenon you would expect to happen with the ocean currents as the global temperatures raise?

Historically, when we look at the paleoclimate record, there is an awful lot of evidence that the sinking or the thermohaline circulation in the North Atlantic has been variable. And this has led to somewhat dramatic shifts and reorganizations of ocean circulations in the Atlantic. What is thought is that much of this was driven by changes in the amount of fresh water entering the North Atlantic from the melting of various ice sheets.

There is clear evidence from climate models that one can expect an increase of precipitation at higher latitudes as we warm. At the same time there is also evidence in the observational record that this is occurring. Many have asked how this may affect the overturning. It turns out that as we have learned more and more about the thermohaline circulation we have become confident in the finding that you would expect a slight weakening of this so-called overturning and a weakening of the transport of heat from lower to higher latitudes to take place over the next century. But we do not expect a rapid transition or shutdown of the overturning that has been seen in the paleo-record as a consequence of warming. The reason why: there is not enough fresh water from the atmosphere, so it would require a rather dramatic melting from the Greenland ice sheet.

Can you give an indication over which timescales ocean currents typically change?

There are several key timescales in the ocean. There are very long timescales, associated with slow diffusive properties. There are centennial timescales that are associated with the overturning circulation. There are decadal timescales associated with gyre circulation. And there are very short timescales associated with convection. If you are asking how long it took abrupt changes in the past to occur: There is a lot of evidence that the transitioning from on to off states or on to weaker states of the north Atlantic overturning took place over a few decades—quite rapid. In the paleorecord, this was probably very common.

You already mentioned climate models. You have developed and used numerical models to investigate the oceans for quite some time. What is the state of the art with these models? What can you model well and what is still uncertain or unknown?

I think the situation is very similar to atmospheric models. The large-scale dynamics are reasonably well understood. These involve the equations governing the motion of the fluid itself—the Navier–Stokes equations. The processes involving the large-scale transport of heat and tracers (so-called conservation equations) are also well known.

Where we have uncertainty is in the parametrization of the small-scale processes that are potentially playing an important role in the kind of instability thresholds we just talked about. For example: convective processes happen at very small scales in the real ocean. In global ocean models, we have to somehow represent this on the scale of grid-cells that are approaching 50 × 50 km. In an ideal world we would be able to integrate high resolution, fully three-dimensional, non hydrostatic models, for many centuries. This would allow us to capture these convective processes. But we are not there yet. I am also thinking of parametrizations of other mixing processes: there are internal waves that propagate and break and when they break they actually mix water properties. These are again very small-scale processes. We have internal waves in our models, but their properties are much more sluggish. These are really the biggest uncertainties in the ocean models: the very small scale mixing processes that are parametrized.

But one remark: While you can have great uncertainty in a particular small-scale process it doesn't mean that reducing this uncertainty by improving our understanding of its physics will lead to different large-scale results. That is, the existing parametrizations may be doing a very fine job. As our knowledge increases we may find that improvements to the parametrizations have little effect. This is something that is not always clear to everyone.

How do you want to access these smaller scales with your models? Is it mainly about computer power or is there still much conceptual work to be done?

There are experimental field programs, making microstructure measurements and trying to understand the basic physics of small scale mixing processes, such as the interaction of circulation with topography. So there is clearly a lot of basic research that needs to be done. This problem will never go away completely. We have tried to close turbulence theory for quite some time and we will keep doing that for quite some time more. We are still building a knowledge base of how these small-scale processes work, how they affect the diffusion and transport of heat, fresh water and salt in the oceans.

When you use a model to investigate a certain climate effect, how do you validate this model? What tells you that you can trust your model?

If you are incorporating a new process, you first must be able to reproduce the current climate. I have noticed that a lot of people think that we inject data into a model. We don't. The starting point is the governing equations: Equations that govern the motion of the fluids; equations for the exchange of momentum, heat, fresh water, between various components of the climate system; equations that govern the uptake and release of various gases; equations that govern radiation, formation of clouds, evaporation etc. So you have all these governing equations that you have to link together under conservation of various properties, such as the energy. The next step is to drive all these equations by energy from the sun. So you turn on the sun and then you have to get a climate that looks like a real climate. And then you also have to observe its seasonal variation and its internal variability.

If you can reproduce the current state of the climate, you have to go back and look at the temporal evolution of the climate. So you are going to start at, say, 1850 and run a transient simulation through to where we are today. When you get comfortable with the comparison between the transient evolution of the model and the 20th century climate, you can look at small perturbations around the present climate. You go back to some paleo-examples; maybe the last glacial maximum. And now you observe what happens to the model if you change the way the earth is tilted etc. Perhaps you change the level of CO_2 and you look what happens then and you compare with some paleo proxy records.

I like to think of it as a three-step evaluation process: one is the contemporary climate; two is the transient climate; three is paleoclimate. And only then, if you feel comfortable with reproducing what we know, can you start undertaking projections with the model.

This process probably takes some time….

Oh, sure. My group started in the early 1990s and now it is 2011. And we are still adding new components to our model and testing things.

And is it still the same model that you are continuously improving?

Yes, by now our model has more than 100,000 lines of source code. For example: Suppose you want to include an ice sheet subcomponent that allows ice to grow on land. We must remember that we have already included terrestrial vegetation dynamics (vegetation that grows and can interact with climate) and a carbon cycle. We have to ensure that the various modules interact because if ice grows where trees were: What happens to the carbon? Does it go to the atmosphere? Does it get stored in the soils? You see, as you add more and more complicated processes, you also have to worry about their interaction with existing processes. And when you do that you have to go right back to the drawing board again.

When people want to know about the past state of the oceans, which sources of information can be used? For investigating past temperatures on land, people make use of tree rings and other proxydata that are believed to contain temperature information. What can you use for the oceans?

There are a number of techniques that are used. There are tiny critters (e.g. foraminifera) that live in the upper ocean and other different levels of the water column. And as these creatures grow, they take into their shells the chemical properties of the environment around them. When they die they end up in the sediments of the deep ocean. Year after year more material is added to the sediments. So you have the remains of these little critters in the sediments and you can measure the chemical properties of their shells. From that you can infer the conditions at the time when they lived.

For example, oxygen isotopes are a classic indicator: Calcium carbonate is the building block of the shells of many critters. There is a fractionation that occurs when the shells grow. What this means is that depending on the temperature, the amount of ^{18}O vs. ^{16}O that is taken up during the formation of the calcium carbonate shells changes. This can give you information about past temperatures of the ocean. And there are other examples of these isotopes that are used to infer past environmental properties.

So the main information you can get from this analysis are the temperature at earlier times?

You can also look at proxies for the amount of ice on land. If you have a lot of ice on land you have very ^{18}O—enriched water left in the sea itself so the sediment should be enriched in ^{18}O. Another tracer that can be detected is salinity. So you can infer various things in different regions.

Let us talk about the carbon cycle. There is large amount of CO_2 stored in the oceans. How large is this amount, compared to the CO_2 in the atmosphere or on land?

There is the CO_2 on land (I will use gigatons (Gt) of carbon as unit), which is about 2,300 Gt. Then you have the carbon stored in all the fossil fuels we know about today; that would be 3,700 Gt. The intermediate and deep ocean contain about 37,000 Gt. And the surface sediments have another 150 Gt in them. The ocean surface contains another 900 Gt. When you compare the atmosphere that hosts about 595 Gt—that is about two orders of magnitude less than the ocean.

In rough numbers, the ocean has about 40,000 Gt of carbon and the atmosphere has about 600 Gt.

What is known about the conditions under which the uptake of CO_2 by the ocean is increased or the conditions that would maybe lead to a release of CO_2 from the ocean?

The uptake of carbon by the ocean is strongly dependent on temperature. As you warm, the efficiency of uptake becomes weaker.

If you look at outgassing, the single most effective means of outgassing is volcanic emission. On very long timescales, the carbon from the ocean went into the sediments. The sediments were eventually subducted under continental plates through tectonic processes. The carbon would eventually go back into the atmosphere again via volcanic emissions. This a very long timescale carbon cycle (many millions of years).

Nowadays, the concern is not so much getting it out. Rather, the oceanic buffer is reducing its efficiency. As we put more CO_2 into the atmosphere, the ocean is less and less effective in absorbing what exists already. There are other processes: if you get CO_2 enriched water and bring it to the surface you can outgas instead of uptake. But there is an awful lot of deep ocean that can still take up carbon. The key is really that the surface ocean becomes less effective as it warms, which means more CO_2 in the atmosphere.

And this temperature dependence is very strong—already sensitive to a change of a few degrees?

Oh yes, this is a considerable effect. We know that if we warm by a couple of degrees the efficiency of uptake becomes very slow. This is why the residence time of CO_2 in the atmosphere is very long. If we burn existing fossil fuels, CO_2 in the atmosphere will have a residence time of thousands of years.

Let me ask you about a quite different topic. Climate science it not only a scientific discipline but has also become a very political and social subject during the last years. How has this affected your work as a scientist?

My background is physics; I did my PhD in applied mathematics. The reason why I went into climate science is that I was interested in physics and mathematics applied to real world problems.—things that make a difference. It is surely very important to study particle physics or astronomy, but I personally found these much less relevant to our daily lives and that is how I got into physics of the environment.

Back in the late 1980s, early 1990s, we did our work, published papers, made our contributions and life went on. At some point, governments became aware of our results and said, well, maybe we should start to do something about that. But as more and more people realized that this is an issue and developed a sense of fear that they did not want to deal with this issue, it became really a bit of a "shoot the messenger" approach that was taken. There is a group out there that think that we as climate scientists are taking part in some global conspiracy to create a global government or something similar. This is absolutely absurd.

What we really are trying to do is to convey the science to the people who fund it. I have my individual opinions on policy, just like anybody does. But in terms of science, having published more than 200 papers in the field, I believe I have some kind of expertise in this area. And it is also important for us to communicate our work to the world out there because they are funding it. We do science to inform people. The reason why governments fund science is because science creates knowledge and knowledge is useful for society. And we have a responsible to communicate that knowledge.

It has been very frustrating and it is frustrating on many levels because you begin to see really how bad the scientific literacy is within the general public, and particularly within the media and the political structures that we have. And in Canada, I think, we take it to a completely new level compared to Europe. The level of scientific illiteracy—at least inside the government—is just mind-blowing.

Are there also positive effects of the public discussion? Better funding or many talented people that want to start a scientific career in climate science?

Funding? This might be the case in Europe, but surely not in Canada. We are in a time of draconian cuts to climate science now. We get the feeling that our research funding is drying up because the government approach right now is to simply take whatever global warming hits us with. It seems that our government has decided that their task is to ensure that we produce oil from the Alberta oil sands and export it to the USA. It's not a very pleasant time to be a climate scientist in Canada.

In terms of students and postdocs, there is positive effect, absolutely. There are many people like me, having physics or mathematics degrees, that want to do something useful; apply their skills to physics of the environment.

There is a lot of controversy around climate science and its results these days. From a scientific point of view: what is really controversial at the moment and what are well-established facts?

I do not think there is a lot of controversy about the big picture. The ocean's role in climate is pretty well understood on the large scale. There are still questions about the effect of clouds on climate. There is some consensus emerging on the effect of global warming on tropical cyclones. I guess people are now interested in understanding the timescales involved with the melting of the Greenland ice sheet, for example. Not the question if it will melt but how fast. This is not really a controversy but rather a really exciting area of research.

Same thing on land: There are open questions about biochemical cycles in high latitude regions, where you have vast amounts of organic matter stored in frozen soils that may or may not be exposed to the atmosphere. What I mean by this is: as the surface layers thaw, more wetlands could be present. This would mean that the decomposition of organic matter underneath the wetlands produces methane instead of CO_2.

And what are the questions, you want to investigate during the coming years?

One key area we are trying to look at now is carbon cycle feedbacks on the long timescale, associated with things like the organic matter being exposed after the thawing of permafrost. Increasing temperatures at high latitudes can expose that previously frozen soil to the atmosphere and produce CO_2 or methane. Looking at biochemical feedbacks associated with increasing temperatures is really a fun area to work on. Also, we are looking at the stability of West Antarctic and Greenland ice sheets; that also is a quite exciting area of research.

During the last years, more and more scientific disciplines became part of climate science: not only physics or geology but also biology, econometrics and many others. What does it mean to include all these different aspects? How is your experience with this kind of interdisciplinary work?

I view the role of people that develop and use climate models as integrators. I am not involved in field measurements myself but I will have to somehow incorporate these observations indirectly when I develop climate models. Even if I am not collecting the data, I have to understand the language of the people doing that. So there is a lot of learning in my business. I have to read a lot of papers from a lot of different areas. And: I come from a physics background, so when I start to discuss biology, chemistry or economics there are cultural differences—and I do not mean this negatively—there are simply cultural differences between different fields. One has to respect this in interactions with other disciplines.

Biologists or chemists are very influential in building the representations and parametrizations that we incorporate in our climate models. There is no magic recipe on how to engage others in different disciplines. If I need to parametrize a certain feedback (e.g. clouds), I had better go and talk to people who work in cloud physics. I inform them what I would like to do and then they suggest some approach. They might even say my approach doesn't make sense. Hopefully through discussions we will be able to figure out a way to proceed. Finding the right person to work with is by far the most important aspect of any collaboration.

Prof. Weaver, thank you for the interesting discussion.

Chapter 5
It is the Poor And the Marginalized That Are Hit Hardest By Climate Change

Martin Parry is a former director of the Environmental Change Institute at Oxford and is currently a visiting Professor at the Centre for Environmental Policy at Imperial College London/England. His main research interests are the various impacts of climate change. He has served as a co-chair of the IPCC Working Group II in its Fourth Assessment, and as a lead author in the first three IPCC assessment reports. Among several other awards, he won the World Meteorological Organisation's Gerbier-Mumm International Award (1993) for his contributions to research on climate change.

Prof. Parry, you are doing research on the impacts of climate change. For example, you have investigated how global warming increases the number of people suffering from hunger. What are the methods, the scientific tools, you use to tackle such kinds of questions?

In impact assessment, we take a range of scenarios of likely climate change and use these as inputs to impact models. If investigating impacts on agricultural production, we would run crop models for different places to look at changes in crop yield of different crops, how they may vary for different climate scenarios.

G. Götz, *Global Change*, SpringerBriefs in Earth System Sciences,
DOI: 10.1007/978-3-642-23444-6_5, © The Author(s) 2012

Then we might modify fertilizer application and irrigation, just as possible forms of adaptation, to see how much flexibility there is in possible management response. This gives us an idea of yield changes due to climate change, which we can then translate into estimates of altered production potential for a region. And from there, using UN projections for increasing demand (due to the growing number of people and demand per capita), we can estimate the effect on risk of hunger. So we are building a chain of models, connecting them but also being aware of the uncertainty that increases as you go down that chain.

Are you starting from biological models for crop growth under different climate conditions or are you using statistical models?

Although statistical models were used about 20 years ago, we now use almost only biological models, which are basically mathematical equations that represent certain parameters of plant growth and how these are affected by temperature, available moisture, solar radiation, etc. We also might specify soil conditions or type of land management. These models are called crop growth models, and essentially were developed about 20 years ago to help improve management, allowing farmers to specify weather and soil conditions and optimize management. In our research, we reverse the question in the model: specifying the management and varying the weather to look at the yields of crops.

What is the typical uncertainty for assessments like the additional number of people suffering from hunger due to climate change?

We have not been very good at specifying uncertainty ranges, up to now. But we have done sensitivity analyses around some of the key assumptions, and then ask how they affect uncertainty.

For example, on the demand side, key assumptions which affect food demand are projections of population growth and income growth, which can be more important than changes in climate in affecting future risk of hunger. Climate change is a relatively minor component of the total food demand projection, adding something like 20–30% to the price increases that would otherwise occur as a result of demand change.

This example gives you an idea of the uncertainty derived from an uncertain future. There is another range of uncertainty that originates from the modeling itself and we have been less good at specifying this. In the next round of work now planned, inter-model comparisons will be made to improve descriptions of this kind of uncertainty.

You have investigated many different kinds of impacts of climate change. We talked already about crop production, water shortage is another example. Which of these impacts are comparably easy to investigate quantitatively and which ones are difficult?

I think one can treat each sector—water availability, risk of flooding, impacts on human health or impacts on food supply—at different levels of simplicity or complexity. Probably it would be wrong to say that some sets of impacts are easier to simulate than others.

For example in risk of coastal flooding, one can make simple calculations (e.g. assuming a given sea level rise of 20 cm by 2080), and estimate the number of people within that zone who might be affected. But then you can overlay more complexity on that estimate, adding changes in tropical cyclones as a result of climate change. And then we might allow for migration to coastal areas. As we add complexity we can evaluate how impacts alter according to different assumptions which might be alterable through adaptation. For example, how much is the number of people at risk of flooding in the future reduced by assuming fewer poor people?

What emerges is that—not surprisingly—if you allow for projections of the world with fewer poor people, you basically have much less people at the risk of climate change. It is the poor that generally are at most risk. Knowing more about this relationship between poverty and risk of impact from climate change can help us explore the most effective forms of adaptation, which almost certainly include poverty reduction by economic development.

Probably it is also interesting to look at the past. How did climate changes in the past affect ecosystems and human societies? I am thinking for example of the warm period during the middle ages. What is known about that?

Actually, that is how I got interested myself in climate change. I was looking at the effect of climate variability on marginal agriculture in Britain and Scotland in upland areas. In these marginal areas, and in conditions of poverty, you undoubtedly had greater exposure to weather variability. When the weather was adverse, people suffered.

Now, the warm period in the Early Middle Ages may have encouraged farmers to move uphill, but I suspect that they were more driven there by population pressure in the lowlands; and, when adverse weather did set in (and it seems to have become more frequent in the Later Middle Ages) then these marginal upland farmers suffered. So, the vulnerability of these people seems to have been due both the socio-economic as well as to climatic factors; and this is almost certainly also true of people and climate change in the future.

So, while conditions were different in the past, it was the mix of vulnerability and weather which created the adversity?

Yes. The lessons from these historical case studies are about exposure. If sections of the population are especially exposed—the young, the elderly, the poor, the politically and socially marginalized—then this is where impacts are most likely.

You pointed out that the poor are the most vulnerable to negative effects of climate change. Are there also regions, groups or sectors that would benefit from global warming?

Again, you need to specify the conditions carefully. To benefit implies being able to make a potential opportunity work for you. As a generality, in northern Europe north of a line from Paris to Warsaw, climate change will probably bring more opportunities than dis-benefits when seen in the global context. Agricultural potential will, for example, likely increase northern Europe's competitive position agriculturally. But this would only be the case for locations where rainfall is expected to increase in Europe, and we do not yet have an accurate projection of that.

In Britain we have actually seen new crops appearing in the past 20 years, such as sunflowers and soya. Similarly in Germany, temperature increases may generally bring increases in agricultural potential, but the more detailed regional pattern will be affected by how rainfall alters.

However, what may benefit agriculture may be a disadvantage for the natural environment. Let me take a more severe case: Iceland. It is just about possible to grow wheat in Iceland now, and it will be more practicable to do so in a warmer future. But what would that mean for Iceland's soils and its wild grasslands? Climate change will bring 'pluses' and 'minuses' in a very mixed-up arrangement.

A widely discussed question is: Under which circumstances would it be preferable to adapt to climate change rather than trying to mitigate. What is your view on this problem?

It is clear that we need to find a balance between reducing emissions and adaptation. That balance may vary from place to place. Mitigation may require local action but has global consequences because we all contribute to global emissions. Adaptation will require local action but has a local effect, here and now.

We lost about 10 years by not realizing the importance of adaptation. During the years following Kyoto, the effort was almost wholly on mitigation. Only during the last 5 years have we started to consider adaptation seriously. We thought we could mitigate completely, but in fact we have done very little yet in reducing emissions. And now we realize that, even if we mitigate to the fullest possible extent, we still need to adapt to probably 2.5°C of global warming, which will be extremely challenging.

So, now there is a realization that we have to act both on adaptation as well as mitigation. The best mix of these two will likely vary from place to place. In some countries which are most vulnerable, especially in tropical regions, there will certainly need to be more adaptation. Whereas in developed countries, which are major GHG emitters, there will need to be much more effort towards reducing emissions.

There is a huge public discussion about keeping temperature increase below some critical magnitude. You often hear about this 2°C limit, it is stated that it still would be possible to adapt to that amount of warming. Is this limit coming from scientific studies or is it a political number?

There is evidence that we can adapt to 1°C without much difficulty—we actually have experienced 0.7°C of warming already. Beyond 1°C we will see some important adaptation costs; and beyond 3°C some adaptation will not be possible. So, 2°C is a reasonable adaptation limit, based on limited evidence. But it also happens to be the case that 2°C is approximately the amount of warming that will occur even with the fullest mitigative efforts we can manage. Consequently, there is a pragmatic element here: We will need to adapt to at least 2°C of warming.

If you look at your field of research: what are the questions where there is a broad consensus in the scientific community and what are the controversial issues?

There are major uncertainties about water availability, and how it will alter around the world. It potentially effects on every human sector: human health, waste disposal, agriculture etc. We have an emerging picture of where the major decreases will be—around the Mediterranean, for example. But changes in the seasonality of rainfall patterns, for example in the Tropics (including monsoon rainfall), are not yet very well understood.

And then there are the extreme weather events, such as tropical storms. They will govern the frequency of storm surges and substantially enhance the effects of sea level rise.

You have been involved in several IPCC assessment reports. There has been a lot of criticism of the IPCC recently. What is your comment on that?

There was one significant mistake that was made. This related to the rate of retreat of the Himalayan glaciers. But it was not an error that was carried through into the policy summary of the IPCC assessment. Yet it resonated heavily because it came at a time when there were criticisms of research on climate change, for example concerning that at the University of East Anglia, UK. The climate-change 'deniers' highlighted the issue and the IPCC was to slow to respond because it was not 'media-savvy'. It was a single mistake, one amongst millions of 'facts' that the IPCC reports. It had no effect on policy and it has now been corrected. But it is important, nevertheless, that the IPCC has tightened up on its checking procedures because a mistake like that should not have slipped through the net.

I think that there is an emerging view that the IPCC needs to evolve with the demands on it. When it was set up 20 years ago there was no international pressure on it for speed of reporting. The sheer amount of information is now probably 100 times more than it was then. About 20 years ago, you could have put all the books on climate change impacts on two shelves. Now you would need an entire library. I think the IPCC will re-think its ways of working.

In your field of research: What are the big scientific questions that should be tackled the coming years?

An important issue is the funding the cost of adaptation. Up to now this has been very limited, but at the Cancun CoP of the UNFCCC in December 2010 governments agreed very substantial sums to be made available for adaptation. The challenge now is to develop the scientific knowledge about the most cost-effective adaptive actions that should be taken, and where and when.

Prof. Parry, thank you for the interesting discussion.

Chapter 6
When Deciding About Long-Term Strategies, Irreversibility is a Key Point

Stéphane Hallegatte is a researcher with Météo-France and at the CIRED institute in Paris/France. His main interests are modeling the economic costs of climate change and methodological questions concerning the interactions between environment and society. He serves as a lead author for the IPCC special report on M*anaging the Risks of Extreme Events and Disasters to Advance Climate Change Adaption* (SREX), as well as for the fifth IPCC assessment report.

Dr. Hallegatte, you have estimated the feedback time between the climate system and the economic system to be on the order of 50–100 years. What is this feedback time exactly?

My starting point was the idea that climate change can reduce economic growth and activity and therefore also reduces the emission of greenhouse gases. This has

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DOI: 10.1007/978-3-642-23444-6_6, © The Author(s) 2012

a back action on climate change itself again. In my view, this is an important point. In most of the research work, we start from scenarios of economic growth and emissions of greenhouse gases, then look at the consequences for the climate and finally at the impacts. But the impacts do not feed back on the emissions we started with.

The question for me was now: Is this a methodological issue or is it fine to do that? As a result, I found that the duration of this feedback is larger than 50 or 60 years—regardless of the amplitude of the feedback, which is also a subject of discussion. So there might be an issue if we look at the 22nd century—but for the rest of this century, this feedback does not seem to play an important role. This result also shows how all decisions we make now can have only long term consequences. When we are working on reducing emissions, we work for the time after 2060 but not for the near future.

Can you explain this feedback mechanism a bit further? Why are the time scales that long?

In fact, in the coupled system of the societies, economies, climate and ecosystems, there are stocks everywhere. What changes the climate is not the emission of greenhouse gases but the stock of greenhouse gases in the atmosphere. This introduces a delay: if we stop emitting, the stock is still there for more 100 years. And it is similar with the economy: if you have impacts, one thing you can do is reducing your investments to maintain your level of consumptions. Then you can have a smoothing effect thanks to the capital or the equipment you have installed in your factories. That means some of the consequences are instantaneous and some are long term.

This climate inertia together with the socio-economic inertia make the entire system very slow in responding to perturbations. Imagine—as a thought experiment—a sudden reduction of emission, a shock. The reduction of emissions would lead to a change of the CO_2 concentration, then effect the climate, then affect the impacts and finally change the emissions again. And this process would take about 60 years.

It might be interesting to hear more about the models you are using. What are the main ideas behind them? How are these models built up, which are used to to study the interactions between the climate and the economy?

This coupled models of climate and economy are built on some very simple assumptions. If you look at climate models—they are very complicated, with a huge amount of variables. But what they show as a result in the end—at least if you look at the averages—is just thermodynamics. What they provide in addition to that is information how the climate may change locally and also some more complex feedback mechanisms etc. But the idea that CO_2 is warming up the atmosphere, raising the global mean temperature—that is just basic physics, nothing complicated. In my model, I use this main mechanism without looking into the details and I assume that impacts from climate change on the economy are driven by changes in the global mean temperature. This is obviously a simplification because global temperature is just an index that we invented to measure climate change. It does not exist in reality, nobody feels the global temperature. This is the type of simplification, I built the model on. The idea is that, for looking at the inertia, you do not need to be precise about the amplitude of all the changes. At the end it can be done in a very simple way. The economic part, on the other hand, is based on an economic growth model from Solow (1956), so again a very old school model. The last ingredient is the coupling, which has been investigated in the 1970s. What is new, is that instead of focusing on the amplitude of the change, I looked at the inertias, the timescales.

Is there a good way to verify these models? Probably you have tried different kind of approaches.

In this type of work, there are generally two types of approaches: One is to build a model of the real world, this is basically what the climatologists are doing when they develop global climate models. These are very big models, they provide detailed results but they are very difficult to use and not adaequate for interdisciplinary work. Just because they are so heavy to use.

But there is another approach that is becoming popular nowadays in the field of environmental science: We make models of models. We are building models which are simple but can reproduce what complex models are able to do. These simplified forms are simple enough to be coupled together, so we can look at the interactions. The idea is: if you want to look at the interactions between the climate and e.g. the economy, you want to work at the interface but you do not need the full modeling of the two subsystems. I admit, in a way this could be seen as a trick. But the small models used for interdisciplinary studies are validated against the big models in each of the disciplines, and they are able to include all progresses made by disciplinary researchers.

I assume, uncertainty is quite large when you get these long timescales of more than 50 years—about the climate but also on the economy side. What does this uncertainty mean for implementing strategies to adapt to climate change or fighting it?

My positions is that the problem of climate change lies rather in the uncertainty it creates for the world than in the direct impacts. In other terms: If we knew exactly what climate change would do to all the ecosystems, we could implement strategies to adapt. This would be much easier to manage than the current situation. But because climate change is uncertain, we do not know how to build bridges, cities or buildings. So there are two dimensions in the impacts of climate change. The real physical impacts, that we will know about in 50 years, and the impacts of uncertainty that we feel right now: how do you make decisions when climate is uncertain? And to be complete on that: uncertainty is nothing new. All governments and businesses are used to make decisions in uncertain situations. So it is not that we need to reinvent all decision making processes.

But global warming creates an additional uncertainty, a quite dramatic one. It influences almost all choices, that is something we notice when discussing with users of climate information. What they are typically used to, is to have historical data series and then doing statistical analysis, assuming it is stationary and then taking decisions on the basis of this statistical analysis. Now they realize: this does not work because you cannot pretend that the data series is stationary. You cannot simply continue the trends because we are in a changing world and we do not know about future emissions. So they realize, they need numerical models; this is a big change in practices.

The next step is then to throw away the historical data series and use models instead. But many people just take one model or one series and act as if this was a historical data series (which somebody would have sent us from 100 years ahead). And by doing this they totally neglect the uncertainty. One thing we can be scared of of, is that all these people makings decisions based on model results, assuming that these models are correct, even though all climate models disagree to some extend. And the situation is complex because this uncertainty arises from three sources:

The first one is the one we talk about all the time: The fact that there are scientific uncertainties; we do not know how the climate system is working in all details. This uncertainty is just insufficient knowledge and we can work on it.

The second one is that there is some randomness in the climate system, so there is an uncertainty that cannot be reduced, simply because there is some intrinsic chaotic behavior in the climate system.

The third uncertainty arises from human behavior: Will we reduce greenhouse gas emissions? Will we change land use patterns? Will we keep pumping ground water? All this has a real influence on the climate and its impacts. And this is something than cannot be predicted since we are free to do what we want. We can guess, how we will implement climate policies in the future and whether we will change water use etc. But it remains just a guess. We know very well that one natural disaster can completely change the direction of politics in a country. So this uncertainty, much like the randomness of the climate system, is an uncertainty that cannot be reduced. And that we have to manage. I think that all climate user communities have to learn how work with this new information which has a lower quality than historical data series.

What about the long time scales? I assume there are also much shorter timescales that play an important role in economics. Does this fact favor certain strategies of adaption or mitigation?

Many countries are building their national adaptation plans and are considering what they are willing to accept in terms of consumption reduction for the sake of mitigating emissions. I think a good starting point for this question is to look at irreversibility. When I do not know how the future looks like, a good way to begin is to decide which situations I want to avoid at any costs. And I think that this is really the basis of the 2°C target of the European Union. We do not know whether 2°C or 3°C of global warming would create huge damages to ecosystems. It is very difficult to predict whether an ecosystem will be able to adapt or not.

Let us assume that in 2070 temperature has increased by 3°C and we realize that our socio-ecosystem cannot adapt. What will we do? Well, it is too late, we cannot do anything. If, however, we aim at the 2°C target and realize in 2070 that ecosystems are able to adapt much better than we assumed originally, we would have made big efforts to reduce emissions for nothing. But then we can decide to release the 2°C constraint overnight. By aiming at the most ambitious target, there is much less irreversibility, although we might make unnecessary sacrifices. If we are over-pessimistic, we sacrifice something only for a given period of time; if we are over-optimistic, it is almost irreversible—at least over human timescales. In these types of situations, the idea of irreversibility is extremely important and should be included in decision-making.

It is similar for adaptation processes. Land use planers ask us what they should change in their planning and my first answer is again: avoid irreversibility! One example: There is a low lying area in a coastal zone which you can protect if sea-level rise is small but you cannot protect if sea-level rise becomes very large (or you can protect it, but at an extremely high cost). Today we do not know whether sea level rise will be 20 cm or 120 cm. If you are optimistic and you develop this area, then, if you realize in 30 years that pessimists are right on sea level rise, half of your people are now living there. And we all know how difficult it is to retreat from these zones: you need to relocate people, destroy their homes, etc. Politically, this is just a mess and it is also very expensive. The only alternative would be to protect this area at any costs and then you end up with 6 m high dikes or something like that—at some points it makes no sense any more.

If you decide, not to develop the area and you realize in 20 years that you could have done so, well, you just do it then. This, you can decide over night. Of course, this does not mean that you never develop an area if you have any doubt—then you would end up doing nothing. But if you have two alternatives, one of them is reversible and the other one not, then there is a clear preference for the strategy that can be reversed if you are wrong. I think, if the future of the world is totally unknown, flexibility is what you should look for in terms of strategies.

You already mentioned natural disasters. What is the role of serious natural disasters with huge damages in triggering mitigation or adaption?

It is quite unfortunate, but if you look at investments in mitigation or adaption, you find that they always follow natural disasters. Even the implication of the USA in international negotiations was related to the drought in the central US in the 1980s. Disasters are striking events, everybody realizes his own vulnerability, this helps making decisions. But there are two problems and I will discuss the one related to adaptation first.

If you want to adapt to long term trends, you cannot work only with single-shot investments and then do nothing until the problem returns. Take a look at New Orleans: They had four heavy hurricanes with huge damages during the last 100 years and each time they agreed that this would have been the last time and decided to invest a lot in sea defenses. And each time, they actually started to invest and the first few years they were really motivated and spent the necessary money. But building an efficient protection system takes 10–15 years and after a few years you meet increase difficulties, such as budgetary constraints. And a dike can easily be postponed in case of budgetary problem. You think, that is fine because it is just for one year. But then it gets postponed forever. That is exactly how the protection system that was planned after the hurricane Betsy in 1965 never got finished. And this was one reason explaining the heavy consequences of hurricane Katrina in 2005.

I am talking about New Orleans because it is a very good example, with the 4 heavy hurricanes. But there are many similar cases, for instance in France. We had a big storm in February 2010, with tens of casualties, followed by an announcement of the president, promising to rebuild all dikes and remove all houses from places that are too risky. But six month later, we see political leadership already disappearing because people resist—what a surprise—against having their homes destroyed and also because it is more expensive than expected to rebuild the dikes. Furthermore, the attention is shifting away. The memory of the disaster is still there with the people directly affected, but the rest of the country, that only saw the disaster on TV, was conscious of it for only a few month.

We realize that this reactive way of managing risks, acting only after disasters, is not very efficient. I think the Netherlands are a good example. The reaction to the flood of 1953 was not primarily to build dikes. More important were changes of institutions and laws to make sure that what was decided at that time had a long term impact. I think that after a disaster you should use the momentum not to find money for building dikes but to change the laws and to change institutions. In a way this is more difficult because after a disaster you want to show that you are doing real things and people tend not to regard institutions and laws as real things. But it is quite the opposite. What can change a situation in a lasting way are laws and institutions.

The second point is more related to climate change impacts and the need to mitigation. If natural hazards change everywhere on the planet in response to climate change, you can basically think of two scenarios. One scenario is the most optimistic: we anticipate the changes in climate and hazards, and we change the way we manage risks to limit disasters. The second scenario, the worse one, is that we do not anticipate but act only after disasters. In that scenario we would need one disaster in every location to prompt action, just to help people realize that risks have changed and actions are necessary.

So here is a very important criterion of what future climate change impacts will be like: will we adapt only after disasters or will we be able to anticipate? Will we be stupid or smart? I was talking about sources of uncertainty at the beginning. Who can guess whether we will be stupid or smart in the future? We have plenty of examples of both. The answer will depend on education, training, media, institutions, politics—it is very complex.

Depending on whether we will be smart or not, climate change impacts will be very different. So, when someone asks me about climate change impacts—whether they will be large or small—I usually say: it depends on whether we will be stupid or smart! Of course, the need for mitigation is different depending on your ideas on this point: if you are pessimistic on how societies will be able to adapt to climate change, then you need to invest much more in mitigation.

You talked already about the two possibilities: fighting climate change or adapting to it. It seems to me that fighting climate change only works on a global scale while adaption can take place very locally. Does this fact not cause people to adapt rather than trying to mitigate?

The first thing is: you do not need an international agreement to do adaptation. But you need an international agreement to do mitigation. However, I think the difference is sometimes overstated. For example when you have a drought in Russia and you loose yields, Russia is closing its borders for the export of wheat. And then you get a hike in wheat prices that will be felt across the globe. When Katrina hit the oil-producing system in the gulf, we got an increase in energy prices everywhere. We have worldwide markets for food and energy, migration make regions even more dependent on each other, much more than in the past. In our world, as soon as one country or region is not able to adapt, everybody will feel the aftershock of this problem. So adaptation also needs some international strategies.

This is especially true for impacts that are already global. Today, natural disasters are globalized through the re-insurance market, and trends in one place of the world matter for everyone. We have India and China and all the emerging economies that start to become insured. This is an additional demand for the re-insurance market and nobody knows if (and at what cost) the global market will be able to absorb this additional demand. This question exists even without climate change. If you add climate change on top of that, the question is even more difficult to answer.

If we take a look at environmental problems in the past and the way they have been tackled—I am thinking of the Ozone depletion problem for example—can we learn something about how to deal with the climate problem, which is much larger and more complex?

It is a bit provocative, but I think we were lucky to have the Ozone issue before global warming. It shows that we can solve global problems. It is also good that it went in that order because the Ozone issue was much easier to solve than the carbon issue, mainly for two reasons. The first is that it just affected a few industrial sectors. We were not talking about economy-wide issues. The second thing is: technological solutions (for example alternatives for CFCs) existed already when we started to look at the Ozone problem.

For the carbon problem it is very different. First, it is not a localized issue at all. The whole economy is affected. And we are talking about mitigation costs that are of the order of a few percent of the GDP—that is really huge. Furthermore, we do not posses magic technologies to solve the problem. It is not just only a bit more expensive—that would be doable. It also involves changes in lifestyle, changes in competitiveness of different countries. Some exporters will be affected in a terrible way, I am thinking of the oil exporting countries. We cannot just bring a few new technologies and pay for it. Solving the problem creates side effects for which we cannot simply just pay. If you remove the income from oil sales for the oil exporting countries, nobody really knows how that can be compensated. Also, you do not just want to give money to a country to make it stop producing oil, that does not really make sense. So the amplitude of the problem is much larger and it goes beyond today's technological possibilities. This is already complicated, but technological issues are still simple compared to lifestyle issues.

The Ozone problem was a training round—and it worked really well. That is also why I think an international agreement is possible. I think the Kyoto process followed a little bit the Ozone process but maybe underestimating the difficulties. Furthermore, it was probably easier to solve the problem in the 1990s, in which you had rich countries on one side, concerned about the Ozone, and developing countries on the other side. Now, we have three types of countries: OECD countries, developing countries and emerging countries. These emerging economies like China, India, or Brazil are very reluctant to accept any international agreement: they are becoming important nations with a voice in the international community and they do not like the idea of having someone in the UN deciding about penalties for not following an agreement. There is also this geopolitical side, which is very recent. 10 years have changed a lot and maybe in another 10 years from now it will be much easier again to get China at the table to solve the problem. But now—it is just wrong timing. The emerging economies really do not like the idea of someone restricting their actions and this is understandable.

What are currently the most interesting questions in your area of research? What do you want to investigate during the coming years?

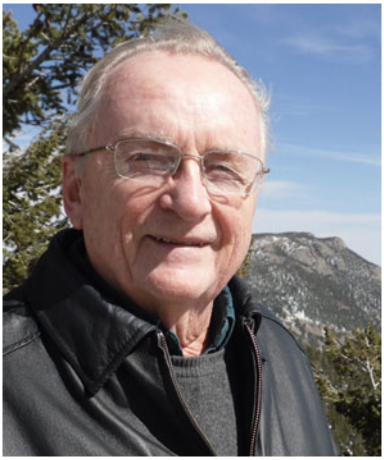
I am sure, I could make a list of thousand interesting problems, but I will just discuss two. The first one is decision making and the idea that we—as experts on a certain domain-cannot just provide information and expect users to manage what they can do with that. We are the ones who know how this information has been produced, we know their limits, we know what could be done and what could not be done with our data. So we have to be in a close relation with the users and this a huge challenge—not only in our field, it is the same for medical studies or biology. We start realizing that science should get closer to decision making. But this is difficult: we want to get closer to decision making but the only ones who are legitimated to make decisions are politicians and we should not try to make decisions instead of them just because we are experts. We need to be in close contact to politics while remaining independent. But the closer you work with politicians, the more difficult it is to remain independent. I think this is really about research strategy: How do we manage our relationship with society and decision making? I suggest, we need to go a step back and do research on that point itself.

The second topic, I am interested in is from the field of economics. Most of the models, used today to assess the costs of mitigating climate change, are based on very simplified worlds. In most of these models, you have no unemployment. In most of the models, developing countries are just treated as developed countries with a lower productivity, without taking into account the institutional situation etc. I think the fact that these models are that simple, explains why decision makers remain in doubt when specialists repeat all the time that the costs will be limited. They are concerned about all the cost we maybe do not see with our models. Again, I think we need to take into account the fact that the world is not perfect, we know that economic models are still very limited. They demonstrated their failure several times in the past, most recently with the economic crisis. So we have to be modest and avoid providing only numbers to decision makers. We cannot completely trust these numbers, our models are too simple. We need to understand better, what the mechanisms are at play and how to use this information to make decisions. We should not only say: The costs are only 0.5% of GDP, so lets go and do it! We need to do better than that.

To finish with something positive: we have a lot of problems in our countries, in our societies, in our economies. Unemployment, poverty, inequalities, just to name a few. I think that by bringing these problems into the context of environmental considerations, we could develop policies that help not only to solve environmental issues but also bring benefits in terms of employment or poverty reduction. We need to broaden the analysis and take also the other problems on board and try to find integrated solutions.

Dr. Hallegatte, thank you for the interesting discussion.

Chapter 7
The Montreal Agreement is a Good Example of How to Deal With a Global Environmental Problem



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Prof. Hughes, you have proposed that the discussion about climate change and global warming should be divided into three periods. What are these three periods?

The tenor of scientific discourse about climate change in the decades from the second quarter of the nineteenth century to the present has changed considerably, and exhibits three major periods. Each of these periods is characterized by one of three phases of scientific effort and its relationship to society. Briefly put, they are a period of hypothesis (up to about 1945), a period of gathering evidence and testing hypotheses (roughly 1945–1975), and a period of controversy over the

G. Götz, *Global Change*, SpringerBriefs in Earth System Sciences,
DOI: 10.1007/978-3-642-23444-6_7, © The Author(s) 2012

application of apparent scientific consensus (from perhaps 1975 to present). Of course, all three of these aspects of science continued throughout recent history: theories have been developed and tested all along, but these periods do seem to follow an emergent dialectic, and the public debate on the relationship between science and society has unmistakably intensified in the most recent decades.

Sometimes, it might seem that global warming is a rather recent topic. But many of the underlying scientific fundamentals were discovered quite some time ago. For example, the basic physical mechanism of the greenhouse effect was already discussed by Fourier in the 1820s. When did people begin to recognize man-made climate change as a possible threat?

Governments granted money for studies beginning with the International Geophysical Year of 1957–1958. Roger Revelle and Hans Suess, attempting to establish the role of oceans in absorbing carbon dioxide, inspired Charles Keeling to establish remote stations on Mauna Loa in Hawai′i and in Antarctica to monitor the concentration of that gas in the atmosphere, resulting in the discovery of the "Keeling Curve," which showed that it is steadily increasing. Revelle and Suess made an ominous conclusion: "Human beings are now carrying out a large scale geophysical experiment of a kind that could not have happened in the past nor be reproduced in the future."

Directly measured temperature records only exist for at most the past 150 years. So historians have a key role in obtaining climate information about earlier periods. What kind of sources can be used? What about the accuracy of the obtained data?

Proxy data have been found, increasingly. Climate scientists braving difficult weather drilled ice cores in Greenland and Antarctica, revealing evidence of past concentrations of atmospheric gases, temperatures, dust, pollutants, and radioisotopes. For example, it became possible to plot long-term temperature variations and changes in carbon dioxide concentration on the same time scale, and to note that they follow patterns that are remarkably similar. In a chart of these two curves, a relatively regular, approximately 100,000-year cycle is prominent, although it also appears that the climate has occasionally changed rapidly in very short time periods. The most recent decades have shown a divergence in which the level of carbon dioxide has risen more rapidly than temperature. These data require refinements, but they are extremely accurate.

To these ice core archives were added sea and lake bottom cores: Harold Urey and Cesare Emiliani made seabed discoveries that revealed ancient climate changes. Many studies of coral reef stratigraphy, malacology, dendrochronology (perhaps less accurate than ice core analysis), and other long-term archives also represented extended natural cycles, rapid climate variations, and the apparent recent influence of human-induced change. Generally speaking, these archives reinforce each other.

Mitigating global warming by reducing greenhouse gas emissions can probably only be done on a global basis. Is this something unique compared to other environmental problems that we have come across in the past?

The Montreal process of global agreement on reduction/elimination of production of chemicals such as chlorofluorocarbons (CFCs) that deplete the ozone layer is a precedent worth considering.

The Montreal protocol to protect the Ozone layer seems indeed exceptional: I think it took only a few years of scientific and political discussion until it was ratified worldwide. And by now, only 20 years later, the increase of ozone-depleting substances in the atmosphere has stopped, even a decrease has been reported recently. In your view, why was it possible to solve this problem that fast on a global scale? Were there any specific conditions or strategies that enabled this success?

Scientists took an active part in the negotiations, and even though there were scientific uncertainties, important political leaders involved decided to take the side of caution. There was strong support from NGOs and public opinion, especially in the US, reflected in declining sales of spray cans. The US banned spray cans with CFCs in 1978. The US provided leadership in the negotiations (including, surprisingly, President Ronald Reagan though not the ideologues in his administration), while Canada and some European nations gave support. The European Community, a major producer, was opposed but eventually Germany and the UK (PM Margaret Thatcher) brought it around. The Montreal meeting was small enough to reach consensus, and was mercifully short. Throughout, the United Nations Environment Programme and its head, Mustafa Tolba, actively worked for the agreement. The agreement took account of the special problems in North/South inequalities, and included reasonable positive and negative market incentives. Although industry producers fought against regulation almost every step of the way, they finally decided to phase out CFCs and seek substitutes.

The climate has always been changing during history of the earth. Can we learn something from the way past societies adapted to these climate changes? Maybe this information can even help to estimate future social and economic impacts of adaption to global warming?

Yes, we should study such former periods as, for example, the ancient Climatic Optimum, the medieval warm period, and the Little Ice Age. It should be noted, however, that the present rate and scale of warming is unprecedented.

What are in your view environmental problems that have been solved well in the past? And what would be an example of how not to approach an environmental issue?

Again, the Montreal Agreement is such an example. I think also of the conservation measures undertaken by the Franklin Roosevelt administration to mitigate the conditions of the Dust Bowl.

How not to approach an environmental issue? The way many of my fellow Americans are doing now: (1) Deny that the problem exists; (2) Make it a partisan political issue where even environmentalism becomes a pejorative word; (3) Put all the emphasis on the economic side while not recognizing the importance of the environment to the economy; (4) Oppose environmental regulations as a form of restraint on business.

Is there a correlation between the structure of a society (the political, economic or cultural structure) and the way this society deals with environmental issues?

Most nations dealing well with environmental issues today are socially aware democracies. Those doing nothing are most often authoritarian states. There have been exceptions in history, however. See Jared Diamond's comparison of Haiti and the Dominican Republic.

The concentration of wealth in a few hands looms large as a negative factor, since large corporations tend to treat the environment as a money-making resource for themselves, rather than a trust for societal benefit.

Climate change is often discussed by referring to its economic consequences. For example, the costs of reducing emissions are compared to the costs of adapting to higher temperatures etc. How has the relation between economic and environmental developments evolved during time? Are the major differences between different cultures?

The logic of capitalism and the world market economy is to measure everything in terms of financial cost or benefit. The cultures that did not do so also probably did not possess a primarily money economy. If you know Daniel Quinn's *Ishmael*, they were the "leavers," not the "takers" of today's world culture. Herman Daly, in *The Economics of Sustainable Development*, and other economists envision a "steady state" economy that would operate within the constraints set by the biophysical environment. This would mean that the size of the human population would stabilize, perhaps at a level lower than at present. Use of non-renewable resources would slow and eventually depend entirely on recycling, while use of renewable resources would remain below the replacement level. At present, these ecological economists, how discerning they may be, have little influence on the course of the world market economy. Still, even some of those friendly to expanding markets have been forced to admit that there is not much point in growth that completely lays waste to the environment.

Finally, what is the relation between the scientific discussion and the political discussion about climate change? How has this relation developed in your view?

The history of the recognition of global warming and its implications for human society is an illustrative case of the interaction between the growth of scientific knowledge and the interests of political and economic entities. Science can tell us of the relative (not absolute) certainty of continued human-induced global warming and its probable effects. It can evaluate possible measures intended to forestall negative changes, or at least to lessen their magnitude, as well as courses of action intended to enable human societies to cope with the negative effects of the likely changes. But the decisions as to what measures will be implemented will be made by governments and corporations that historically have demonstrated a pattern of acting in accord with what they see as their own interests in the relatively short term. The common good of humans and the Earth in the long term has received less consideration.

Prof. Hughes, thank you for the interesting discussion.