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**Q: 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.

**Q: 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.

**Q: But I assume some understanding is necessary to set up these GCMs ...**

...that is true, but I do not directly generate new knowledeg about the world, I only generate numbers, and possibly knowledge abouot 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.

**Q: 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 1km, I might already get some convective clouds explicitly, but if I use a resolution of 20km 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.

**Q: 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 scientist 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.

**Q: 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.

**Q: 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.

**Q: 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.

**Q: 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.

**Q: 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 1000 years. We used a simulation which gave us some significant changes in temperature because we varied non 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.

**Q: 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.

**Q: 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.

**Q: 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.

**Q: 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. Thre 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.

**Q: 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.

**Q: 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 stormactivity 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.

**Q: 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 icesheets 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 35 years old.