Introduction of person

I, Hans von Storch, have been actively involved in climate science since the early 1980s. I have held positions with the Meteorological Institute of the University of Hamburg and at the Max-Planck Institute for Meteorology in Hamburg. At the present time, I am a director of the Institute for Coastal Research of the GKSS Research Center in Germany. I have co-authored more than 120 peer-reviewed articles on various issues of climate dynamics, climate statistics, climate change and climate impact as well as the textbook "Statistical Analysis in Climate Research" (together with Francis Zwiers) published by Cambridge University Press. I was a lead author of Chapter 10 of the Third Assessment Report of the IPCC, but I am not involved in the Fourth Assessment Report of the IPCC.

Based on the scientific evidence, I am convinced that we are facing anthropogenic climate change brought about by the emission of greenhouse gases into the atmosphere.

For further personal details please refer to my web-page: http://w3g.gkss.de/staff/storch.

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Outline

I briefly address three aspects of the hockey-stick issue, namely

1. Scientific aspects:
   - How valid are the regression-type methodologies for reconstruction historical climates?
   - How relevant are these reconstructions for claims that we presently experience a climate change outside the range of what we consider as "normal" (no human interference).
2. The process of achieving success of a scientific knowledge claims in the climate science community:
   - Independence of the review process or presence of gatekeepers.
   - Reproducibility
   - Selection process by Nature & Science.
   - Acceptance by IPCC assessment process.
3. The social conditioning of climate science:
   - The history of perceived anthropogenic climate changes.
   - Post-normal science.

On the basis of my analysis I draw a couple of conclusions, chief being that the process of climate science must be organized in a *sustainable* manner. This means that climate science should be conducted with a low sense of subjective passion; that climate science provides "if-then" answers to questions society poses; that it presents to the society a broad range of possible policy responses and does not restrict the range of policy options to a small corridor that appeals to certain value-driven agendas.

The conditioning of science by the culture of its actors and society is unavoidable. However, the scientists can attempt to make such influences explicit by acknowledging and explicitly reflecting on such influences, especially by engaging social scientists in the process of critical self-

reflection. The Wegman-report claims that a major problem in studies such as MBH would be an insufficient engagement by mainstream statisticians. I think a major problem with this study and its transformation into a policy-relevant issue is an insufficient comprehension of the social dynamics of the post-normal process of (not only) climate science.

There are three appendices to this document:

1. My responses to the "Boehlert"-questions given at the NRC hearing on March 2, 2006 in Washington.
2. A contribution to the debate about the "Barton-letters" on the "Prometheus"-weblog http://sciencepolicy.colorado.edu/prometheus/ dated July 8, 2005 (http://sciencepolicy.colorado.edu/prometheus/archives/climate_change/000486hans_von_storch_on_b.html)
3. An English translation of an article published in the German weekly "DER SPIEGEL" (4/2005): von Storch and Stehr: A climate of staged angst. (http://sciencepolicy.colorado.edu/prometheus/archives/climate_change/000343a_climate_of_staged_.html)

Scientific aspects

How valid are the regression-type methodologies for reconstruction historical climates?

The key statistical assumption of any of such methods is the uniformity of informational content in the proxies which are regressed on the climate variables (mostly temperature). In other words, are these data influenced by non-climatic variable factors (inhomogeneity), is the transfer function linking proxies and temperature constant in time (stationarity)? Likely, most if not all proxy data (tree rings, coral rings, vine harvests) suffer from some inhomogeneities and instationarities. This is unavoidable and has to be dealt with by using additional insight into the system, e.g. by data assimilation approaches combining limited theoretical (models) and empirical knowledge (uncertain data).

Regression-type models are designed so that they return only part of the full variability of the variable of interest, namely that part which can be traced back to the proxies. Not all of the variability can be accounted for in this way. The difference in variability of temperature and of proxy-derived temperature is dealt with by "scaling", i.e., by applying a suitable normalization. If "scaling" is used, then the basic principle of regression is violated, as the part of variability in the predictand (temperature), which can not statistically traced back to the predictor (proxy), is nevertheless related to predictor-variability. Scaling is useful, when the transfer function is not regression (screening of co-variability of two variables) but based on physical arguments.

Nevertheless, attempts like those by MBH are useful and should be explored. They may provide useful estimates. The problem with MBH was that the result was presented by the IPCC and others in a manner so that one could believe a realistic description of historical temperature variations had successfully been achieved. The NRC report published in June 2006 has made clear that such a belief was incorrect.

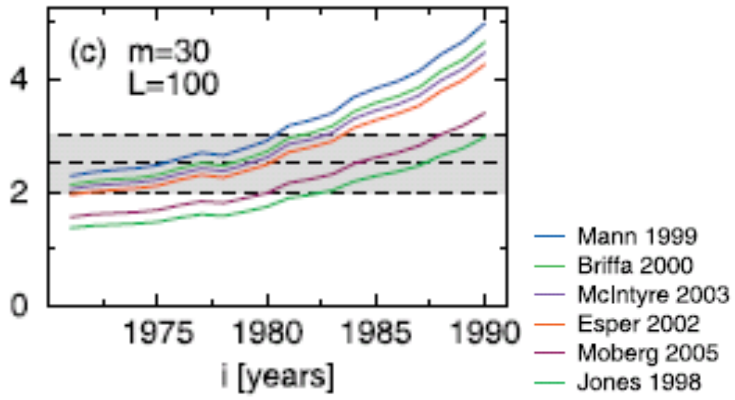
How relevant are these reconstructions for claims that we presently experience a climate change outside the range of what we consider as "normal"

Whether the present climate is influenced by non-natural factors is answered through "detection" studies. Such studies are based on the insight that the predicted signal of human-caused climate change should emerge in most recent times from the natural variability. Second, one would expect it to manifest itself with a higher "than normal" *rate of change*. Thus, the signal is expected to be a rapid warming in the most recent past. The method to test this hypothesis is to find out if we have a "steeper-than-normal" recent upward temperature trend. The hypothesis is not "we have a period which is warmer than ever in historical times". In that sense the claim whether the last decade is the warmest of the past millennium is not relevant to detection; the question is whether the recent rate of warming is markedly stronger than what has happened in the past.

The hypothesis is tested by framing the problem as a statistical test of a null hypothesis. The null hypothesis reads "the present trend is of natural origin". Then, one determines the range of trends consistent with natural variability – and rejects the null hypothesis (and accepts the hypothesis that the trends is not of natural origins) if the present trend is larger than, say, 97.5% of trends originating entirely from natural variability.

The crux of this approach is of course the determination of the range of trends which are observable under natural conditions. To do so, one may rely only on the instrumental period, which is contaminated by the expected signal and rather short, on multi-century reconstructions as MBH and on extended model simulations of undisturbed conditions. Obviously the determination of the range of "normal" trends is uncertain and absolute certainty can not be attained within a reasonable time.

We[1] have examined which range the different historical reconstructions suggest. To do so, the time series of reconstructions have been "modelled" as a long-memory process, and standard deviations of trends are derived. Here, the trend is defined as the difference of two 30 years means 100 years apart. Then these trends are determined from the instrumental record as given as multiples of the standard deviations derived from the different reconstructions.



The result is given in the diagram; the curves are all the same, but they differ in scale because of the unit of different standard deviations derived from the reconstructions given at the figure caption. The horizontal dashed lines mark 2, 2.5 and 3 standard deviations. Two standard deviations correspond to a risk of false rejection of the null hypothesis of 2.5%.

Obviously, in all cases, the critical 2-standard deviation mark is passed sometimes in the past decades; in case of MBH this happens very early, while in Moberg's more variable reconstruction at about 1980.

I conclude that the claim of "detection of anthropogenic climate change" is valid independently of which historical temperature reconstruction one chooses to believe in.

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[1] Rybski, D., A. Bunde, S. Havlin,and H. von Storch, 2006: Long-term persistence in climate and the detection problem. Geophys. Res. Lett. 33, L06718, doi:10.1029/2005GL025591

It should also been taken notice that the claims of successful detection on non-natural warming trends and its attribution to chiefly elevated greenhouse gas concentrations in the atmosphere in the Third Assessment report were not based on the historical reconstructions but on the analysis of the instrumental temperature record as well as on numerical experiments with climate models.

The process of achieving success of a scientific knowledge claim in the climate science community

A normal condition in the progress of science is that knowledge claims are accepted only after a *"peer-review"* process. The peer-review process attempts to assure that knowledge claims are consistent with the empirical evidence, and properly related to contemporary accepted knowledge claims, and that the methods are sound and are reproducibly described. The "peer-review" process does not eliminate the possibility that new ideas are rejected since they may contradict contemporary, powerful but possibly false knowledge claims (see Ludwik Fleck's seminal book on "Generation of a Scientific Fact"). In order to minimize such a danger, the verdict of peer-reviewers should, to first order approximation, be independent of the persons involved in the review process. Nonetheless, the danger is that a few scholars may become powerful *gatekeepers*, for example as reviewers who are regularly called upon or as editors of scientific journals. The primary goal of such gatekeepers is to fend off publications which may contradict their own thinking, and not to ensure that only internally consistent and plausible publications reach the market of knowledge claims (i.e. scientific journals). Unfortunately this seems to have happened in the field of historical global climate reconstructions, where a small group of scientists has exerted an undue control of the entire field.

Usually, a further mechanism more closely tied to the substance of research is used to quality-control scientific knowledge claims, namely *reproducibility*. This mechanism has ceased to operate in some quarters of paleo-climate science, since some scientists consider "their" data as their personal property and not that of the scientific community, so that others are unable to challenge conclusions drawn from these data by analysing the raw data in their own manner. Although such secrecy is a very human trait it violates the norms of science. Even hostile competitors should have an opportunity to independently re-examine the empirical evidence for conclusions drawn by others, in particular when they become relevant for the policy domain. Data must be become public; the methods employed must be described in algorithmic detail.

Another relevant aspect is the functioning of the two prestigious journals "*Science*" and "*Nature*". The journals enjoy high esteem within and outside of the scientific community as having the highest scientific standards, which is not always the case. The contents of Nature and Science also receive exceptional attention in the media world-wide. However, different from "normal" scientific journals, the editorial decision to accept a scientist's contribution to Science or Nature is also based on the newsworthiness of the research contribution. The presented results must not only be valid and innovative but must also be of interest for a wider community of readers. Such a criterion is reasonable from a economic point-of-view, but it clearly introduces a filter in what is reaching the public is not solely based on the scientific merit of research. Research results with stronger media appeal fare better in this competition of scientific findings; results biased towards higher sensitivity to human interference are more interesting to a broad audience than findings that report low sensitivities. In addition, there may also be a bias towards certain authors, who are well known, because they enjoy public visibility, or command appealing writing skills, "sell" well. Sometimes such contributions are invited.

Another problem with the same journals is that their articles must be relatively short so that technical aspects cannot be described in any detail; indeed, the MBH publication was cursory on the methodical side – thus the statistical method, the validation and the reproducibility, have not been seriously subject to the review process. Ironically, after publication in "Nature" the method was considered "peer-reviewed" and thus valid. However, this was not the case, as the method had not been properly described.

The *IPCC* has different levels of operation – the generation of the technical chapters, which is done by a group of "lead authors", headed by "convening lead authors", and the process of arriving at a SPM (Summary for policymakers) and other overall assessment documents, which is done by the convening lead authors and representatives of the countries.

How the selection process of lead and convening lead authors is done, I do not know – but it is clear that the "lead authors" are supposed to be *experts* in the field. This leads to the situation that the IPCC chapters are dominated by the authors of the most influential articles in their respective

fields of research. Participation as a lead or convening lead author has the advantage that one can make sure that one's own work is positively covered in the IPCC report. However, most lead and convening lead author excel as honest brokers, but some level of gatekeeping may prevail. Indeed, the reputation of the IPCC among scientist has increased to very high levels in the past years.

The IPCC procedure differs markedly from the procedure adopted by the National Research Council assessment. In that case, a group of eminent scientists was chosen, who have contributed to the issue only little or not at all, but have a god understanding of the field as a whole. These scientists then invited a group of experts to present the different angles and knowledge claims. I consider the NRC procedure better in assessing the field of knowledge than the IPCC approach. It may be, however, that the NRC approach can not be used for such a complex and large field, which the IPCC is covering.

In case of the MBH temperature reconstruction one should note that in the technical chapter of the TAR different reconstructions had been presented; it was the SPM and the synthesis report, where the range of reconstructions was reduced to just one, the MBH. It would be interesting to learn how this could have happened.

The social conditioning of climate science

Science is a social process, which, as all social processes, is conditioned by the culture of the actors. This does not mean that scientists would do their analysis irrationally or in a biased manner, but it means that our questioning may by guided by culturally constructed concerns and interests. Also, we may be convinced of the validity of some findings more easily if these findings are consistent with our prior lay-knowledge.

The history of perceived anthropogenic climate changes

It has often been claimed that anthropogenic climate change is a recent concept. This is incorrect. In the history of ideas of the past 1000 years, we[2] have found a number of occasions when (western) people have used the concept to explain observed changes:

> "During the last 20 years the concept of anthropogenic climate change has left academic circles and become a major public concern. Some people consider 'global warming' as the major environmental threat to the planet. Even though mostly considered a novel threat, a look into history tells us that claims of humans deliberately or unintentionally changing climate is a frequent phenomenon in Western culture. Climate change, due to natural and anthropogenic causes, has often been discussed since classical times. Environmental change including climate change was seen by some as a biblical mandate, to 'complete the Creation'. In line with this view, the prospect of climate change was considered as a promising challenge in more modern times. Only since the middle of the 20th century, has anthropogenic climate change become a menacing prospect. The concept of anthropogenic climate change seems to be deeply embedded in popular thinking, at least in Europe, which resurfaces every now and then after scientific discoveries. Also, extreme

[2] von Storch, H., and N. Stehr, 2000: Climate change in perspective. Our concerns about global warming have an age-old resonance. nature 405, 615

> weather phenomena have in the past often been explained by adverse human interference."[3]

This finding is insofar relevant as it points out that we, as members of the western culture, are somehow prepared to accept "anthropogenic influence" as an explanation for otherwise unexplainable events, such as a cluster of extreme events. Our common understanding is that such a human influence would be associated with negative impacts. This pre-conditioning may influence our process of drawing conclusions, in particular when we (scientists) deal with the problem of transferring scientific findings into the political arena.

Post-normal science.

Most of environmental science is what sociologists call "post-normal", i.e., loaded with high uncertainty on an issue of great practical importance. Climate change science is an example of such post-normal science.[4]

A characteristic of post-normal science is that the boundaries between science and value-driven agendas get blurred; that representatives of NGOs are considered to know better about the functioning and dynamics of systems than scientists; that parliamentarian committees delve into the technicalities of science; that amateurs engage in the technical debate: and that some scientist try to force "solutions" upon policymakers and the public. In such a situation it becomes entirely

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[3] von Storch, H. and N. Stehr, 2006: Anthropogenic climate change - a reason for concern since the 18th century and earlier. Geogr. Ann., 88 A (2): 107–113.

[4] Bray, D. and H. von Storch, 1999: Climate Science. An empirical example of postnormal science. Bull. Amer. Met. Soc. 80: 439-456

possible that individual scientists emphasize those insights which are assumed to influence certain policy decisions more forcefully, while downplaying others.

Typical for such a post-normal situation is the flooding of the media with books and movies which dramatize the issue. Recent examples include: The Day After Tomorrow, State of Fear, Satanic Gases, The Revenge of Gaia, and An Inconvenient Truth.

In this situation we need a discussion, not only among scientists about the role of science for the public, which must be the provisions of options for policy, not the narrowing of the range of options to satisfy different worldviews. To limit the influence of non- or pre-scientific knowledge claims, social and policy scientists need to analyse the different processes in climate science, and the interdependence of culture, policy, politics, media and climate science. Even if science can never be fully "objective", it may nevertheless be possible to make climate science a considerably more objective practice than what we have in these days.

Appendices

(a) My answers to Chairman Boehlerts questions, given at the NRC hearing

What is the current scientific consensus on the temperature record of the last 1,000 or 2,000 years? What are the main areas of uncertainty and how significant are they?

- There is consensus on the "blade", but the claimed smoothness of the shaft is likely false.
- The main problem is the loss of information encoded in the proxy data and the shortness of the instrumental record for training the statistical models.

What is the current scientific consensus on the conclusions reached by Drs. Mann, Bradley and Hughes? What are principal scientific criticism of their work and how significant are they? Has the information needed to replicate their work been available? Have other scientists been able to replicate their work?

- There is no consensus on the claims (which?) made by MBH. The main critique is that the method is suffering from a too large loss of variability on long time scales.
- No, the information required for replication was not made available in a suitable manner. The original publication in "nature" did not provide this information and was obviously published without careful review of the methodology.
- Yes, the details of the method were finally determined, among others by Bürger et al., who checked a wide range of combinations of details – which all gave widely different results.

How central is the debate over the paleoclimate temperature record to the overall consensus on global climate change? How central is the work of Drs. Mann, Bradley and Hughes to the consensus on the temperature record?

- The main conclusions about "detection and attribution" are drawn from the instrumental record and models; the different reconstructions do not contradict "detection".
- The MBH work is widely accepted as truth outside of people directly engaged in the issue, because of a less than satisfactory marketing by the IPCC.

(b) My posting on weblog "Prometheus" July 08, 2005 on the "Barton letters"

My reaction to Rep. Barton's requests is split. In his five letters, he is asking for information from two different groups, namely institutions with reviewing responsibilities (IPCC, NSF) and individuals with scientific responsibilities (M, B and H). I find his inquiry of the performance of the institutions IPCC and NSF valid, but the interrogative questioning of the individual scientists is inadequate.

a) Scientists. The scientists have the task to be innovative, creative, to try new avenues of analysis and the like. They have the right to err, the right to suggest explanations and interpretations which may need to be revised at a later time. They should document what they have done, so that others can replicate.

However, this documentation often can not take the form of keeping runnable old codes of the applied algorithms, simply because the software is no longer consistent with quickly replaced hardware. For instance, most of the state-of-the-art coupled AOGCMs used in the mid 1990s are simply no longer available and running at, for instance, the German Climate Computer Center. After replacing a high performance computer with a new system, the standard model codes, including community models, need to be adapted to the requirements and possibilities of the new system, and the old code will often no longer run. This has nothing to do with the norms of the community but simply with technological progress. Also specific commercial libraries of specialized algorithms may no longer be accessible. Data and codes written on old magnetic tapes or even floppies are usually no longer readable.

Therefore the documentation must take the form of a mathematical description of the algorithms used. This is in many if not most cases sufficient for replication. Also, the intention of replicability is not to exactly redo somebody's simulation and analysis, but to find the same result with a similar code and different but statistical equivalent samples. The problem is usually not that the codes contain errors (even if many of the more complex ones likely contain minor, mostly insignificant errors), but that specific elements of implementation and specific aspects of the considered sample of evidence will lead to conclusions, which do not hold if another sample is considered or a different but equally good algorithm is employed. The reason is that we want to learn about the dynamics of the real world, and these insights should not depend on random choices in sampling and implementation. We generally do not expect scientists to manufacture results, or that unintended but significant errors will affect the eventually published conclusions.

Having this situation in mind, I consider Rep. Barton's requests to the three scientists as inadequate and out-of-scale. However, the language used by Rep. Barton makes me perceiving this request as aggressive and on the verge of threatening.

The situation is different with the second groups of recipients, the:

b) "Reviewers". Reviewers have a different role, namely they shall make sure that the standards of scientific reporting are held up. They have to ensure that the proposed explanations are considered by independent experts as to whether the presented analysis seems valid and in principle reproducible. "Independent" means that the reviewers have no vested interests for or against the case presented. In the conventional set-up these interests usually refer to academic schools of thought, but in the unfortunate, post-normal case of climate science independence from the political utility of the case should be established.

In this case, I find the inquiry of Rep. Barton to be valid. The IPCC has failed to ensure that the assessment reports, which shall review the existing published knowledge and knowledge claims, should have been prepared by scientists not significantly involved in the research themselves. Instead, the IPCC has chosen to invite scientists, who dominate the debate about the considered

issues, to participate in the assessment. This was already in the Second Assessment Report a contested problem, and the IPCC would have done better in inviting other, considerably more independent scientists for this task. Instead, the IPCC has asked scientists like Professor Mann to review his own work. This does not represent an "independent" review.

The NSF seems to have failed to ensure that sufficient information is provided about work done under its auspices.

Rep. Barton should also have asked the editors of "Nature", why the original manuscript was accepted for publication even though the key aspect of replicability was obviously not met by the MBH manuscript. Actually, MBH could not meet this condition because of the strict length limitation of that journal (nowadays one would ask for extensive Supplementary Online Material). One should ask why the manuscript was accepted nevertheless - and not, as in many other cases, the manuscript was recommended to be published in a "normal" journal without the severe length limitations. I believe the reasons for Nature were the journalistic reasons - namely the expected broad interest in the subject. One should also ask why after the critique von McIntyre and McKitrik only MBH got the opportunity for a correction of his paper, whereas the short manuscript of their opponents was rejected.

To conclude - the requests to M, B and H are not fair but may unfortunately lead to a repressive atmosphere within climate science; the requests to NSF and the IPCC, however, are appropriate, as these institutions may have failed in a primary task, namely to guarantee an open scientific discourse. And, Rep. Barton should have included the editors of Nature in his analysis.

A Climate of Staged Angst

By Hans von Storch and Nico Stehr

The days are gone when climate researchers sat in their ivory towers packed to the rafters with supercomputers. Nowadays their field has become the stuff of thrillers, and they themselves have risen to take on the leading roles. The topic is so hotly contested, the prognoses so spectacular, that they are no longer merely the subject of media reports; now the specialists in staged apocalypse have moved in. Last year Roland Emmerich depicted a climatic collapse provoked by humankind in his film "The Day After Tomorrow." Since last week the belletristic counterpart has been available in German bookstores: the novel "State of Fear," by the best-selling author Michael Crichton.

The thriller is about the violent conflict between sober environmental realists and radical environmental idealists. For the idealists, the organized fear of abrupt climate change serves as a handy weapon. They interpret every somehow unusual weather event as proof of anthropogenic global warming. "You have to structure your information so that it's always confirmed, no matter what kind of weather we have," the P.R. consultant for the environmentalist organization advises. The realists, who protest that the evidence that human activity has increased meteorological extremes is thin, are fighting a losing battle. Their dry scientific arguments are unable to gain any ground against the colorful, horrific visions of the climate idealists.

Film and novel have certain aspects in common. Where Emmerich holds out the prospect of a threatening climate catastrophe, the book prophesies an economic collapse. In both cases, greenhouse gases produced by humankind are the culprit – in the film, because the emissions themselves are too much; in the book, because the fear of them is. The idealists are so obsessed with their mission that ultimately, in order to rouse the public, they themselves bring about the foretold catastrophes.

Despite a good deal of factually untrue – and thus all the more striking – compression, Crichton has quite correctly observed the dynamic of the paths of communication among scientists, environmentalist organizations, the state and the civilian population. For there is indeed a serious problem for the natural sciences: namely, the public depiction and perception of climate change. Research has landed in a crisis because its public actors assert themselves on the saturated market of discussion by overselling the topic.

Climate change of man-made origin is an important subject. But is it truly the "most important problem on the planet," as an American senator claims? Are world peace, or the conquest of poverty, not similarly daunting challenges? And what about population growth, demographic change or quite normal natural disasters?

In the U.S., only a very few remain interested in the greenhouse effect. At the end of the 1980s, the situation was still different. That was the era of the great drought of 1988, the Mississippi flood of 1993, and the climate capers ought by rights to have taken off in earnest from that point. But that never happened in the U.S., and interest petered out. According to a survey by the CBS television network in May 2003, environmental problems were no longer ranked among the six most important subjects; and even within environmental problems, the topic of climate came in only in seventh place. In Germany, so far, things are still seen differently. But for how much longer?

In order to keep the topic of "climate catastrophe" – a concept nonexistent outside the German-speaking world, by the way – continually in the public eye, the media feel obligated, exactly like the protagonists in Crichton's thriller, to keep framing the topic "a bit more attractively." At the beginning of the 1990s – severe storms had just swept through the country – one could read and hear in the German media that storms were due to become ever more severe. Since then, storms have become rarer in northern Europe. But no notice is taken of this. The fact that barometric fluctuations in Stockholm have shown no systematic change in the frequency and severity of storms since Napoleon's time is passed over in silence. Instead, there is now talk of heat waves and floods. Very much in the style of Crichton's instigators of fear, the story is now that all

manner of extreme events are on the increase. Thus even drought in Brandenburg and deluge on the Oder fit the picture without apparent contradiction.

Add to this – besides normal floods and storms – other, more dramatically threatening, scenarios: the reversal of the Gulf Stream and the resultant cooling of large areas of Europe, for instance, or even the rapid melting of the Greenland ice pack. The question has already been publicly raised whether perhaps even the Asian tsunami can be attributed to the disastrous effects of human activity.

This will not be able to hold the public's attention for long. Soon people will have become accustomed to these warnings, and will return to the topics of the day: unemployment and Hartz IV, Turkey's entry to the E.U. or whether Borussia Dortmund can avert disaster on the soccer field and in the boardroom. Thus we will see firsthand how the prophets of doom will draw the climatic dangers in even more garish colours. The terrifying visions to haunt the future can already be guessed at: the breakup of the west Antarctic shelf ice, which will cause the water level to rise much more rapidly, and after a few decades of uncontrolled carbon dioxide emissions, an abrupt rise in temperatures, giving us a deadly atmosphere like that of Venus. Prospects such as these have long been in the public eye; can they not compete effortlessly with Emmerich's Hollywood images?

The costs of stirring up fear are high. It sacrifices the otherwise so highly valued principle of sustainability. A scarce resource – public attention and trust in the reliability of science – is used up without being renewed by the practice of positive examples.

But what do climate researchers themselves think, how do they deal with the media and the population?

Public statements by noted German climate researchers give the impression that the scientific bases of the climate problem have essentially been solved. Thus science has provided the prerequisites for us now to react appropriately to the goal; meaning, in this case, to reduce greenhouse gas emissions as much as possible.

This does not at all reflect the situation in the scientific community. A considerable number of climatologists are still by no means convinced that the fundamental questions have been adequately dealt with. Thus, in the last year a survey among climate researchers throughout the world found that a quarter of the respondents still harbor doubts about the human origin of the most recent climatic changes.

The majority of researchers are indeed of the opinion that global climate change caused by human activity is occurring, that it will accelerate in the future, and that it will thus become more readily apparent. This change will be accompanied by warmer temperatures and a higher water level. In the more distant future, that is, in about 100 years, a considerable increase of atmospheric greenhouse gases is foreseen, together with an increase in heavy precipitation in our latitudes; in some regions there could be more powerful storms, in others weaker ones.

But again and again, there are scientists to whom, true to the alarmists' maxim in Crichton's book, this does not sound dramatic enough. Thus, more and more often they connect current extreme weather events with anthropogenic climate change. To be sure, this is usually carefully formulated; interviews sound something like this: "Is the flooding of the Elbe, the hurricane in Florida, this year's mild winter evidence for the climate catastrophe?" Answer: "That's scientifically unproven. But many people see it that way." Neither of these statements is false. In combination, however, they suggest the conclusion: Of course these weather events are evidence. Only no one dares to say this explicitly either.

The pattern is always the same: the significance of individual events is processed to suit the media and cleverly dramatized; when prognoses for the future are cited, among all the possible scenarios it is regularly the one with the highest rates of increase in greenhouse gas emissions – and thus with the most drastic climatic consequences – that is chosen; equally plausible variations with significantly lower emission increases go unmentioned.

Whom does this serve? It is assumed that fear can motivate listeners, but it is forgotten that it mobilizes them only in the short term. Climatic changes, however, demand long-term reactions. The effect on public opinion in the short view may indeed be "better," and thus may also have a positive effect on reputation and research funding. But in order for this to function in the long

run, each most recent claim about the future of the climate and of the planet must be ever more dramatic than the previous one. Once apocalyptic heat waves have been predicted, the climate-based extinction of animal species no longer attracts attention. Time to move on to the reversal of the Gulf Stream. Thus there arises a spiral of exaggeration. Each individual step may appear to be harmless; in total, however, the knowledge about climate, climate fluctuations, climate change and climatic effects that is transferred to the public becomes dramatically distorted.

Sadly, the mechanisms for correction within science itself have failed. Within the sciences, openly expressed doubts about the current evidence for climatic catastrophe are often seen as inconvenient, because they damage the "good cause," particularly since they could be "misused by skeptics." The incremental dramatization comes to be accepted, while any correction of the exaggeration is regarded as dangerous, because it is politically inopportune. Doubts are not made public; rather, people are led to believe in a solid edifice of knowledge that needs only to be completed at the outer edges.

The result of this self-censorship in scientists' minds is a deaf ear for new and surprising ideas that compete with or even contradict conventional patterns of explanation; science degenerates into being a repair shop for popular, politically opportune claims to knowledge. Thus it not only becomes sterile; it also loses its ability to advise the public objectively.

One example of this is the discussion of the so-called "hockey stick," a temperature curve that allegedly depicts the development over the last 1000 years, and whose shape resembles that of a hockey stick. In 2001 the Intergovernmental Panel on Climate Change, the committee of climate researchers appointed by UNO, rashly institutionalized this curve as the iconic symbol for anthropogenic climate change: At the end of a centuries-long period of stable temperatures, the upward-bent blade of the hockey stick represents the human influence.

In October 2004, we were able to demonstrate in the scientific journal "Science" that the methodological bases that led to this hockey-stick curve are mistaken. We wanted to reverse the spiral of exaggeration somewhat, without also relativizing the central message – that climate change caused by human activity does indeed exist. Prominent representatives of climate

research, however, did not respond by taking issue with the facts. Instead, they worried that the noble cause of protecting the climate might have been done harm.

Other scientists lapse into a zeal reminiscent of nothing so much as the McCarthy era. For them, methodological criticism is the spawn of "conservative think tanks and propagandists for the oil and coal lobby," which they believe they must expose; dramatizing climate change, on the other hand, is defended as a sensible means of educating society.

What is true for other sciences should also hold for climate research: Dissent is the motor of further development, Differences of opinion are not an unpleasant family affair. The concealment of dissent and uncertainty in favor of a politically good cause takes its toll on credibility, for the public is more intelligent than is usually assumed. In the long term, these allegedly so helpful dramatizations achieve the opposite of that which they wish to achieve.

By doing so, however, both science and society will have wasted an opportunity.

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