4

Personal Accounts by Colleagues and Co-workers

During his long career, Klaus Hasselmann has been a boss and teacher but also a colleague to many people. Therefore, we have asked quite a few of these people about how they remember their time with him. Specifically, we asked:

- How did you meet Klaus?
- What is the legacy of Klaus' scientific work in your field?
- Is there a personal advice from Klaus that helped you in your career?
- How did Klaus' thinking influence your scientific work?

We left it open to the addressees of our survey as to whether they would prefer to answer these questions or if they would like to discuss their experience in a different way. The people whom we approached and who gave us a wealth of answers were:

- **Susanne Hasselmann**, Klaus' wife and research partner during his scientific work on ocean waves and particles,
- **Dirk Olbers, Jürgen Willebrand, Peter Müller,** and **Peter Lemke**—first generation of co-workers during the early years of Hasselmann's move into the field of climate science,
- A second generation of co-workers at the Max Planck Institute that included **Martin Heimann, Christoph Heinze, Mojib Latif, Hans Graf,**

Compiled by Martin Heimann and Hans von Storch

H. von Storch, *From Decoding Turbulence to Unveiling the Fingerprint of Climate Change*,
https://doi.org/10.1007/978-3-030-91716-6_4

Gabriele Hegerl, Jin-Song von Storch, Hans von Storch, Patrick Heimbach Jörg Wolff, Ben Santer, Ulrich Cubasch, Achim Stössel, Robert Sausen, Dmitry V. Kovalevsky, Carola Kauhs,

- Colleagues who shared his interest in ocean waves and remote sensing: **Gerbrand Komen, Luigi Cavaleri, Kristina Katsaros, Peter Janssen,** and **Ola M. Johannessen**,
- Colleagues, who assisted Hasselmann in constructing the network of competence: **Lennart Bengtsson, Jürgen Sündermann, Klaus Fraedrich, Udo Simonis, and Hartmut Graßl**.

Placing the various characters into these categories is not always perfect; indeed, in many cases, people would fit in several categories rather than just one. However, this placing them in these categories is sufficient to provide a rough overview.

We allow these people to speak their minds in the following sections and, as the reader will soon learn, discussions with Hasselmann could sometimes be stormy, but were always honest, and constructive, so that the overarching conclusion is: **respect for a great scientist and a great person**.

4.1 Susanne Hasselmann: Klaus—Scientist, Husband, Father, Grandfather, Great-Grandfather

We met in 1955 in Hamburg. Klaus had just finished his diploma in physics and started his Ph.D. work in Göttingen. I was a student of mathematics and physics in Hamburg. I was fascinated by the intensity with which his mind constantly worked. Any problem was trivial for him and could be solved in two or three lines of formulas. He was full of humor and very fond of sports. All in all, a very attractive young man.

We married in 1957, because a little apartment had been offered to us. One has to keep in mind that it was only 10 years after the war and Hamburg had been bombed immensely. So a two room (14 m^2 and 16 m^2) was divine for us. Within the span of one month, Klaus finished his Ph.D., started a position as an assistant at the Institute for Shipbuilding in Hamburg, and got married. Our plan was that I would finish my diploma. However, times were different then. Only three girls from my school started university after the High School Exam. Women got married and had children.

Therefore, when our daughter was born, I stayed home. However, I could take part in Klaus' work. We were happy. For instance, when he thought he

had solved the Turbulence Problem, even if the next day showed an error in the computations. Or after long walks in the park, he announced that he would have to go one order higher in the computations. And out came the wave-wave interaction theory.

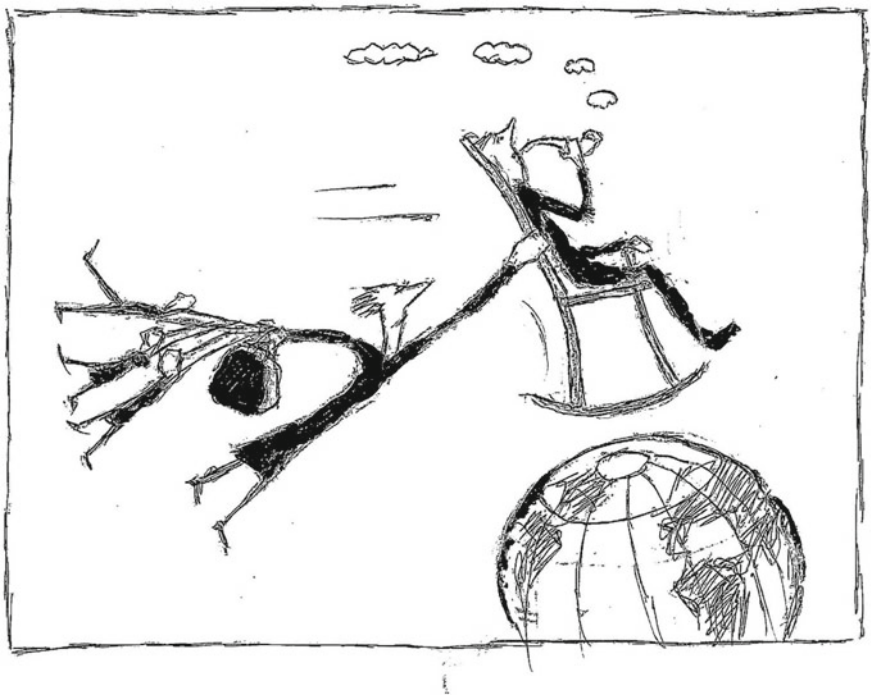
He was invited to a conference in Easton, Maryland, on wave dynamics in 1961. There he was able to offer the link that scientists had been looking for years. So he was invited to several places in the US and was offered jobs. Meanwhile, I was at home with two little kids, one newborn and the other with a very bad case of the measles. However, he was so happy on the phone about the sun and the blooming bougainvillea in California and the lively science there that I could only prepare myself and the family for years of packing and travelling. One has to keep in mind that science in Germany at that time was underdeveloped and the scientific community here was generally old and stuffy. For a young man at that time, gaining entry into a lively scientific atmosphere was just wonderful.

However, for the children it was not easy, especially for our oldest daughter. Three and a half years of California, back to Hamburg, then six months in Cambridge, England, followed by two years in Woods Hole, where the JONSWAP data were worked on, because there were no efficient computer facilities in Hamburg.

Much later, we were invited to a party at a friend's house in Hamburg. Every guest was asked to introduce him/herself with a picture on a black board. Klaus drew himself sitting on a rocking chair, smoking a pipe and flying over the globe. I added myself to the same picture, gripping with one hand the rocking chair and holding suitcases and three children in the other.

However, to see the first curve of the wave-wave interaction in the JONSWAP spectrum and seeing Klaus' theory verified was an experience that we enjoyed immensely. And taking part in all this was worth the inconveniences for the family.

His work on the stochastic nonlinear interaction on ocean waves and other wave phenomena in geophysics in the 1960s, for which he used Feynman diagrams, led to ideas of a new Elementary Particle theory, which he followed up with deep interest on the side. However, I saw how much this theory worked in him. Therefore, when the directorship of the Max Planck Institute for Meteorology (really for Climate Research) was offered to him, I was against accepting the offer. However, he knew that this would give him complete freedom for research and he accepted the position. For the inauguration he quickly developed a stochastic climate model, which he was able to present.



In the mean time I had finished my Mathematics Diploma and was thinking of my future career. For Klaus it was clear: I would work with him. I could follow up the wave-wave interaction and develop a global wave model. However, working at his institute would mean seeing more of him, which was, of course, a good thing. Money for my salary came from ONR. It certainly was an experiment for a woman to work in the institute that her husband was directing. However, the colleagues were very friendly and even found advantages to this arrangement. For example, if you had any problem that needed to be conveyed directly to the top, just mention it if Mrs Hasselmann happens to be in the room. Or, people would call me to say that they had sent Klaus a message weeks earlier and that they needed a response. Etc.

Another question was, how does that work to be his wife and his coworker? Is she only his programmer? We are different. We complemented one another. He presented me with a new theory and I did the untying of the knots, which means that I corrected his mathematics and formed it into something that could be programmed for the computer. For example, the eight-fold integral of the nonlinear interactions. To compute one spectrum cost lots of computer time. The coupling coefficients had to be separated from the integration. Then the integration had to be reduced to the main contributions, etc.

If he had a new idea, he asked me to try it out. After that he followed it up with other coworkers or myself.

The longest and most difficult job was the Metron Theory. It took almost 20 years of my retirement time. It is disappointing, that physicists refused to even think about it.

The title above was: Klaus, Scientist, husband, father, grandfather and great grandfather. Therefore, I have to say something about the family. Most people live a life period first for the family, then profession, then grandchildren and if they live long enough great grandchildren. When we lived in Hamburg in the 1950s, Klaus was very close to his daughter. She adored him (today daddy goes to the institute, tomorrow Meike goes to the institute). In the evenings, he played a puppet show for her. She had admired him all her life and became a very successful scientist herself. She was three years old, when we moved to California in 1961 and was losing him. This was hard, however understandable from both sides and I had to make the best of it. When we had almost lost her, Klaus finally made the decision for the family to move back to Germany. The years to come were travelling years. He tried his best besides Science to be a father and bravely chauffeured the family every Saturday from Woods Hole to the New England Conservatory in Boston. He cuddled with his youngest daughter, enjoyed his son's musical talent. Best was when he could have long discussions with his oldest daughter. When she reached puberty, he managed many occasions with his humor. She was a little talking waterfall. At one dinner, she asked, "What would you do if I would not talk to you anymore?" And he answered, "We would take you to the psychiatrist and ask how we could keep this status." Everyone laughed and the situation again was under control. His humor spread in the family and his fondness of discussion was transferred to his children, too. The older they became the more he could take part in their lives. And he was happy when his son, who never was interested in school much, later after becoming a professional musician, taught himself science to perfection.

It was fun later on to also have professional contact with our children. Our older daughter told me about her research into gene manipulation in the fight against AIDS, and I told her we could put this problem in a system of linear differential equations and compute her free parameters on the computer. We published two papers together on the topic [163, 164].

And with our younger daughter, who creates exhibits about nature and the environment, we could work on climate change or on ocean wave development.

With music we had a problem. Klaus played the flute. However, he thought he did not have to practice. The better the children became on their instruments, the more this became a problem: so they sent him off to practice.

When we had grandchildren, he became a storyteller. He created the character "Little Joe," an angel, after a Christmas show one year. Little Joe always wanted to help but somehow managed to completely mess everything he got involved in. The kids loved it and remembered every subject. Klaus had to create new stories every time.

In 2021, his real family time is now as a great grandfather. He enjoys those little ones enormously, and they adore him. They play together for hours. "Where is grandpa," are their first words when they come to visit.

It is now 64 years that we have been married. It was not always easy, but with a husband, a father, a grandfather and great grandfather like Klaus, it was the richest life one could possibly have dreamt of.

4.2 Dirk Olbers: How to Cook an Ostrich Egg

My first contact with Klaus was in 1968/69 when he dropped into Wolfgang Kundt's seminar on statistical physics at the University of Hamburg, which we attended to find topics for our respective diploma theses. Peter Müller was in that group as well as Hans Juranek, Hajo Leschke and Arne Richter. As I remember it, we had all been searching for a couple of years and we were all well educated in the techniques and concepts of statistical physics but had no idea what to do with it. Klaus hijacked almost all of Wolfgang's students, we all had research topics immediately, and writing our theses took a matter of months. My topic was on plasma physics because Klaus had a proposal on interplanetary space physics which, however, was not approved, and so we (Peter and myself) were suddenly oceanographers (not real ones, this took probably more than a decade) working on JONSWAP and all kind of waves.

Our internal wave research began in 1971 at the Sonderforschungsbereich 94, which Klaus had created and of which he was the head spokesman. Of course, he spoke but our supervisor also vanished immediately to Woods Hole for two years and the best we could do was to follow him. My time at WHOI was full of new experiences, work, learning, and enjoyment. Wednesday dinners at Susanne and Klaus' home, where we worked on weak-interaction theory and the JONSWAP data, were outstanding (Thursdays at Bob Long's). I don't remember what I actually did for the latter except for carrying magnetic tapes, punching cards and fitting the spectral shape to the

data measured at Sylt and the profile of Klaus' nose. The JONSWAP paper—which was co-authored by 16 researchers (I am number 13)—is my most cited paper (having been read over 50,000 times on ResearchGate). Another vivid memory of that time is the MODE workshop in Boulder. Klaus was invited, but didn't go himself, instead sending Peter and myself "to tell the people what to do". The people in question were the top theoreticians and observers in the field of US American ocean science with whom we now shared student housing for 6 weeks. So, every day one could find two innocent German diploma physicists (my contract with WHOI referred to me as a "diplomatic physicist") sitting by the pool with Walter Munk, Henk Stommel, Pierre Welander, Carl Wunsch, Francis Bretherton, Kirk Bryan, Peter Rhines, Jim McWilliams, and a dozen famous others. I think that we didn't contribute much to MODE but started learning oceanography instead.

Another experiment to which we made a major contribution was IWEX, the internal wave experiment in the Sargasso Sea, which was originated by Klaus during his stay at WHOI, and performed by Mel Briscoe and Terry Joyce of the WHOI in 1973, and then evaluated by Jürgen Willebrand, Peter and me in Kiel and Hamburg over the following years. Another matter of note for my career is that the Garrett-Munk model of the internal wave spectrum was first introduced at the WHOI in 1971 in the form of a preprint and a lecture from Walter, the result of which for me was that I found in it a foundation and question for my Ph.D. thesis: what is the role of the wave field in the ocean interior for dynamics and mixing? This a problem that still keeps me busy even today.

Peter and I had a joint Ph.D. viva in Klaus' office in Hamburg in 1973 and one of Klaus' questions was how deep the temperature signal of daily insolation would go? No idea! We certainly could write down the solution of the diffusion equation with a delta-function initial condition, but a number, and from what? The simple dimensional argument later led me to my most popular exam question (lectures at Bremen University): how long would you cook an ostrich egg if you knew to cook a hen's egg?

One of the most influential meetings Klaus took me to was the conference on oceans and climate in Helsinki in 1975. Manabe 's talk on CO_2-doubling was disturbing, and we thought it was clear to do, we thought. It was my first contact with the climate problem. The MPI was founded that same year.

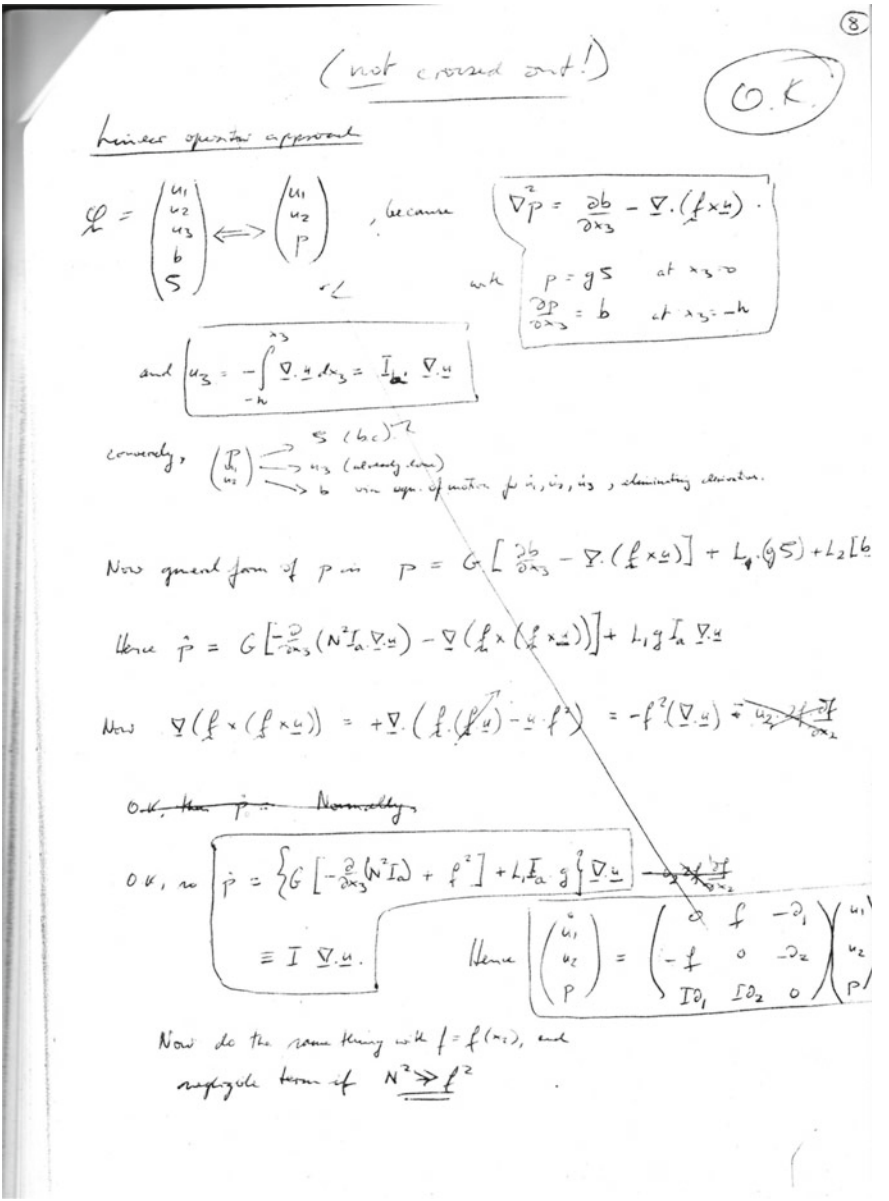
In 1979 I went to Kiel to follow Fritz Schott as lecturer in physical oceanography; it was a move in which Klaus played no part (I think; other than my later move to AWI in 1985). I was not happy in Kiel; my friend Jürgen was still in Princeton, and I continued to live in Hamburg for personal reasons. It was a relief when Klaus called me shortly before I had to take the

train and offered me a position at the MPI. The negotiation took 3 min, I had to catch the train. I spent another few years working in Klaus' sphere of influence and could follow the early development of the MPI. Klaus also advised me during my habilitation (1981) telling me that "you must be convinced that you're right, not the committee".

Tim Barnett was visiting the MPI around 1984 and brought 14 years of wind field data over the equatorial Pacific with him. Mojib Latif and I had the idea of inputting this data into an existing ocean model to see whether El Niño would pop up. We took the idea to Klaus and were harshly dismissed. But Ernst reached into his desk drawer and pulled out a couple of punching cards, an equatorial circulation model—and El Niño did appear, which launched the career of a promising young scientist.

Most of what I learned from Klaus was communicated in seminars. Parallel to the statistical physics seminar, we attended the plasma physics seminar with Gerd Wibberenz in Kiel. Later, in 1970s, when interest had shifted to oceanography, we had the 'Hamburg-Kiel-Seminar' (which our Kiel colleagues called 'Kiel-Hamburg-Seminar'). I remember that one time Klaus was supposed to give a lecture in Kiel but did not appear. He had forgotten to change trains in Hamburg and ended up at the end of the line terminal—"Abstellgleis". Except for this occasion, Klaus dominated the discussion in the seminars, so much so that we invented the '2 min-seminar' at the MPI: Klaus was forbidden to say a word during the first two minutes.

An outstanding event for me was the meeting in Rissen to mark Klaus' 60th birthday in 1991 at which he presented his metron model for the first time. We all saw a glimpse of the great unified theory of physics and the next Nobel prize. I remember many later boring administrative meetings on computer resources where Klaus sat scribbling metron equations under the desk. And then there was Klaus' 80 birthday celebration in 2011. I tried to give an overview of the first Hamburg ocean model, a multiregional construction that Klaus had created back in 1981 during a summer school in Alpbach. Jürgen and I backed up his lectures (Jochem Marotzke and Robert Sausen were there as students). The idea of the ocean model was to couple the different ocean regions together (which differed in terms of their physical properties) to form one dynamical system. Jürgen was to do the western boundary currents, Peter Lemke the mixed layer, Ernst Maier-Reimer the ocean interior (boring) and I the equatorial currents (complicated). I went to Hawaii for a year to carry out local studies and when I returned, Ernst had already done the whole thing and Klaus had published his work on oceans and climate [68]—the foundation of the celebrated Hamburg LSG model—back in 1982, which was an important reference paper for my own work.



Klaus' lecture from 1970. Not crossed out!

Jürgen Willebrand, Klaus, and Dirk pondering about ocean dynamics in Alpbach in 1981

We (Jürgen, Carsten Eden, and I) also finished our book on ocean dynamics in 2011 and I asked Klaus to write a foreword. "I can't do that," he said, "I don't know anything about ocean physics". I had thought I had learned everything I knew from Klaus! He never liked and rarely gave student lectures. I think that a counter example (from September 1970) explained all I know about internal gravity and other waves. The foreword to the book was very favourable and I well remember celebrating the book's publishing in 2012 with Susanne and Klaus in our garden in Fischerhude.

The temperature signal has reached 18 m deep (by molecular heat diffusion) since 1973. Who cares? The ostrich egg must be cooked $(6\ \text{cm}/1.5\ \text{cm})^2 = 16$ times as long as the hen's egg.

From left: Carsten Eden, Christoph Völker, Klaus, Dirk, Susanne, Peter Lemke, Christine Klaas, Dieter Wolf-Gladrow and Jürgen 2012 in Fischerhude on occasion of the publication of 'Ocean Dynamics', the book by Olbers, Willebrand and Eden

4.3 Peter Müller

I met Klaus back in 1968/69 when I and Dirk Olbers were working on our 'diplom' thesis in physics at the University of Hamburg. Klaus joined a weekly seminar on statistical mechanics that his friend and our thesis advisor Wolfgang Kundt had organized. Klaus shook up the orderly conduct of the seminar quite a bit with his distinctive interpretation of a scientific discussion, lots of questions, lots of diversions, but in the end usually some profound insights. On completion of our diplom thesis Klaus offered us to do a Ph.D. thesis with him, gladly accepted; he also offered us a well-paid position as a 'scientific employee' that allowed me to rent a one-bedroom apartment and marry my long-time girl friend. Life in this apartment was cut short because we joined Klaus and many others at the Woods Hole Oceanographic Institution on Cape Cod to analyze the JONSWAP data. My task was to parametrize the spectra, a simple curve-fitting exercise. The year at WHOI was a transformative and happy time in my life: immersed in an exciting research program, being exposed to a new culture and the stimulating intellectual life in Woods Hole and Cambridge, being nurtured by weekly dinners

at Klaus and Susanne's house; the only problem was Klaus' German shepherd Shiva who stubbornly occupied the front passenger seat in the car, but who would argue with a German shepherd, of his thesis adviser. Despite being hard pressed to complete the JONSWAP analysis Klaus found the time and energy to conceive and secure funding for the Internal Wave Experiment IWEX. Funding for the experiment required a trip to the Applied Physics Lab at the Johns Hopkins University where it took all of Klaus' persuasive skills to get two German grad students (Dirk and me) without passports and other legal documentation past the security guards. It should be mentioned that IWEX resulted in many publications, none of them carried Klaus' name. This was his gift to us, providing the basic idea (and funding) and let us run with it. My Ph.D. thesis worked the same way. I got two hand-written pages from him with some formulas and arrows and some crossed-out parts and was then on my own. Klaus also managed to made me a co-principal investigator of the then emerging MODE project. I still remember the expression on Dennis Moore's face when he realized that this young German scientist sitting at the table with all the esteemed East-Coast oceanographers had not gotten his Ph.D. yet.

During this year in Woods Hole I made my way into the scientific world, with Klaus guidance, help and patronage. I realized that not every scientist is as gifted as Klaus and not every curve-fitting exercise is a contribution to a seminal paper. It took me much longer, 25 years to be exact, to realize that after all that Klaus had done for me, I could do something, whatever so slightly, for him. So I invited Klaus and Susanne to one of my Hawaiian Winter workshops, with some 'relaxation and recreation' added.

In summary, my rewarding personal and professional life would not have been possible without Klaus.

4.4 Jürgen-Willebrand: Kiel-Hamburg Oscillations

The way I came into contact with Klaus was perhaps a bit unusual. In 1970 I joined the theoretical oceanography department at IfM Kiel headed by Wolfgang Krauss, with a diploma in physics and some initial exposure to ocean surface waves, a field in which Klaus already was recognized as the leading authority. Sometime earlier, Klaus and Wolfgang had agreed to intensify the exchange of information between their departments, by having a scientist from Hamburg working for half a year in Kiel and then one from Kiel working in Hamburg. At the time when I arrived, Heinz-Hermann Essen

from Hamburg was just completing his term in Kiel. However, the scientist who had been designated to work in Hamburg meanwhile had left the Institute. Now someone from Kiel was "owed" to Klaus' group, and instead of strolling to the Institute in Kiel each day I spent the next half year commuting to the Institute for Theoretical Geophysics at Schlüterstraße, Hamburg. It was a time when I learned a lot without contributing much.

When in Woods Hole, Klaus had conceived the tri-moored Internal Wave Experiment which was carried out in 1973 by Briscoe and Joyce at the WHOI. Together with Fritz Schott, I had just completed an analysis of internal wave spectra from another experiment and felt well prepared to join Dirk Olbers and Peter Müller in analysing the IWEX data. We collaborated very closely over the following two years. Frequent trips between Kiel and Hamburg were necessary to keep up the communications. At times, the travel frequency was so high that my director in Kiel concluded that I must have a girlfriend in Hamburg. In 1976, I was looking for a postdoc position in the USA and received an offer from the Oceanographic Institution in Woods Hole. That offer was appealing, because I had maintained close contacts with the WHOI colleagues during my time at the IWEX, and I knew roughly what to expect there. Klaus, on the other hand, suggested that working at GFDL Princeton—a place about which I knew nothing—would be more attractive. In the end, I took his advice and never regretted it. I came in contact with large-scale ocean circulation modelling in Princeton and had the opportunity to work with George Philander and Kirk Bryan. This was also where Bryan and Manabe developed the first coupled atmosphere–ocean GCMs, which, among other things, enabled the first 3-d simulations of the effect of a rising CO_2 concentration on climate.

I was again back in Kiel in 1980, holding a tenured position and working on my habilitation. The process of building a home for my family had just begun when, quite unexpectedly, Klaus called to ask if I wanted to work in his group at the MPI? There really could be no question, and after a brief consultation with my family, it was decided that the house plan had to be stopped. A few truly exciting years at the MPI followed before I finally oscillated back to Kiel. My interactions with Klaus on many further occasions, such as the memorable workshop in Alpbach which resulted in Jochem Marotzke getting involved in oceanography, are nicely described in Dirk Olbers' contribution to this volume, and there is no need to add to this here.

When Klaus became the founding director of the MPI in Hamburg, I recall that in the morning of the opening ceremony (which was attended by officials and dignitaries), he highlighted the importance of climate change prediction. In the afternoon (when only scientists were present) he discussed

his new linear statistical climate model from which it follows that climate variations are not predictable. Initially, Klaus had been somewhat skeptical regarding GCMs, which at that time were indeed rather far from representing ocean–atmosphere dynamics in a valid manner. Over time, of course, GCM-based climate modelling at the MPI has achieved an international level of excellence. Klaus has been a leading light within our field of science for many decades. The most amazing thing is that he has also been able to contribute to a completely different field with his metron model. And last but not least, the community (and I personally) have also benefitted from his leadership in national and international climate research programmes.

4.5 Peter Lemke: A Stochastic Decision?

My first encounter with Klaus was indeed of a stochastic nature. It was in late June 1975, after I had submitted my Diploma Thesis in Theoretical Solid-State Physics about plasmons in quasi-one-dimensional metals and the question, why a disordering of the atoms in the metal chain destroyed the beginning of superconductivity. One evening, I rushed down the stairs of the Physics building at the University of Hamburg and Wolfgang Kundt was standing halfway down the stairs talking to a colleague. While passing them, I overheard one sentence of their conversation: "Klaus Hasselmann is looking for physicists for climate research." This sentence stuck in my head the whole evening, even though I had settled on the idea of becoming a high-school teacher in mathematics and physics in Hamburg, which I was going to start on the 1st of August. Klaus was looking for physicists doing climate research in the newly established Max-Plank-Institut für Meteorologie (MPI-M). This sounded so interesting that I suddenly could no longer envisage the "secure" job as a high-school teacher, which I already had in hand. I first went to see Wolfgang Kundt, then I called Klaus, got an appointment and, following an interesting discussion on what was expected, i.e., "the application of stochastic methods to the climate system", I expressed my strong interest. I called Klaus again after my final oral exam, and he offered me a job whereby I could work on my Ph.D. whilst I was also expected to help him to write research proposals and reports. This took up some of my working time, but for me it was an excellent learning period on how to write research proposals with a high probability of obtaining funding.

Yet, the first test in my new job was not of a scientific nature. The official opening of the Institute was scheduled for the 5th of December 1975. I was responsible for the technical appliances during the opening ceremony

in the big lecture hall in the basement of the Geomatikum, where the Institute was located. This high-rise university building had just been built, but it was far from being finished. Nothing really worked perfectly. Each one of us got stuck in the elevator several times per month. After learning a bit about stochastic models, the Geomatikum—in my mind—was already displaying many similarities with some sort of stochastic creature with on–off functions following a random process. Consequently, as a theoretical physicist with responsibility for the technical infrastructure, I was really worried about the potential malfunction of all the devices. Fortunately, the lecture hall was in a deterministic phase during the time of the ceremony. After a deep sigh of relief, my heart slowed down and I enjoyed the reception afterwards.

Klaus' attitude with respect to his Ph.D. students was to give them a long leash provided that they made successful progress by themselves. I handed a draft of my first paper "Stochastic climate models, part 3. Application to zonally averaged energy models"[1] to Klaus after a year. A few days later he gave it back to me "with a few editorial remarks". Needless to say, being the optimistic student, these remarks were certainly more than just editorial; instead, they provided clear guidance on how the paper should present the basics, the logic of the model and the results. This was another excellent learning process for me.

The Institute's Science Advisory Board met in 1978 and, following my presentation, Joe Smagorinsky, Director of the Geophysical Fluid Dynamics Laboratory in Princeton, invited me to apply for a Postdoc position in the Atmosphere–Ocean Programme at Princeton University after finishing my Ph.D.. Klaus supported my application, which was eventually approved. He also supported my application for the Woods Hole Summer Study Programme on Polar Oceanography in 1979. This was a marvellous opportunity to talk to eminent scientists next door in Walsh Cottage, where the summer programme took place. Knut Aagaard, Kirk Bryan, Adrian Gill, Peter Killworth, Peter Rhines, Melvin Stern, George Veronis, and Pierre Welander were sitting in the various rooms ready for an intense discussion. During this time, I started my work on "A model for the seasonal variation of the mixed layer in the Arctic Ocean", supported by many valuable suggestions by Ken Hunkins, Peter Killworth, and Adrian Gill.

Very early on, Klaus sent me to several international meetings on his behalf, all of which presented wonderful opportunities to meet with established climate scientists—and for earning a bit of recognition for myself.

[1] Lemke, P., 1977: Stochastic climate models. Part 3. Application to zonally averaged energy models. Tellus 29, 385–392.

This was the manner in which Klaus provided me with an excellent springboard for my international work in World Climate Research Programme and Intergovernmental Panel on Climate Change in later years.

After completing my Ph.D. with a dissertation on the "Stochastic dynamic analysis of polar sea ice variability", I spent two exciting years at Princeton University, a personally and scientifically rich period, where Kirk Bryan, Suki Manabe, Isaac Held and several others from GFDL provided me with valuable guidance. After my return to the MPI-M in 1983, I continued working towards my habilitation "On the interaction of sea ice with the atmosphere and ocean", again accompanied by Klaus' advice on how to follow the right path.

Following my habilitation in 1988, Ernst Augstein, a former colleague at the MPI-M, asked me to apply for an Associated Professorship at the University of Bremen and the Alfred Wegener Institute for Polar and Marine Research, where Dirk Olbers had taken up a professorship a few years earlier. My application was successful, and my transition to Bremen was planned for November 1989.

However, in June 1989, Rainer Roth, Professor of Meteorology in Hannover, had to decline his planned participation in the upcoming Winter Weddell Gyre Study on board the research icebreaker *Polarstern* which was scheduled for the 6th of September to the 30th of October, and I was asked to replace him as leader of the Hannover Meteorology Group at short notice. When asked Klaus whether I—a climate modeller still employed at the MPI-M—should participate in an Antarctic Winter Expedition, he said: "If I were you, I wouldn't think twice. It's a great opportunity to learn, how observations are made and how they should be interpreted."

This expedition marked a transition point for me, in several ways. It represented a transition from Hamburg to Bremen, from climate modelling to polar climate observations, from giving lectures occasionally to teaching regular courses at the University, from concentrating on my own scientific research to supervising Ph.D. Students. This transition also meant leaving Klaus' sphere of influence. I left the MPI-M with rich memories and a valuable basis of scientific insight provided by Klaus and our colleagues.

Now, while putting these memories to paper, I found myself musing about what would have happened, had I rushed down the stairs of the physics building on that June evening in 1975 just a few minutes earlier or later. What would have been the outcome for me had I not caught the invaluable information which was to determine my scientific career to such a large degree. Was my decision to join Klaus a living model example of a stochastic

process? It definitely manifested as a short-term forcing causing a long-term response involving a memory-term.

This decision was not based on pure chance, but it proved to be an excellent opportunity which led to an interesting and fulfilling scientific career.

Klaus was not only the head of our scientific alma mater the MPI-M, but also the Captain of the Institute's soccer team (around 1980, Klaus: top row, 3rd from left; P.L.: bottom row, 2nd from left).[2]

4.6 Martin Heimann

Encountering Klaus

First time I came across Klaus was in the late 1970s when he gave a seminar on stochastic climate models at the physics institute of the University of Bern in Hans Oeschger's department. The department was very much involved in reconstructing climate from paleorecords (radiocarbon, isotopes) and was developing methods to determine past greenhouse gas concentrations from ice cores. The prevailing paradigm within this community at the time was

[2] Susanne Hasselmann commented on the photo: "die Fußballmannschaft war ne lustige Angelegenheit, besonders wenn sie gegen die Hamburger Müllabfuhr (harte Burschen) spielten. Am Anfang, erinnere ich noch, dass Klaus nach dem Training mit unserem VW Bus zu Hause vorfuhr, dann passierte erstmal gar nichts, dann ging ganz langsam die Tür auf und ganz langsam kam ein Bein heraus, dann ganz langsam das andere Bein usw. Er hatte so einen Muskelkater".

that variations detected in climate records must be caused by external factors. I am not sure if Klaus' intriguing concept of stochastically driven climate variations were taken very seriously by the departmental scientists. For us students, however, the concept sounded fascinating even though it lacked practical implications, as the department's main research focus was on climate history and budgeting the global carbon cycle but not climate dynamics.

When I was a postdoc in the USA, I came across some fascinating papers by Klaus, which were published as book chapters in the late 1970s early '80s. In these, Klaus envisaged the construction of comprehensive earth models in which biogeochemistry would also play an important role: "…This requires the development of a detailed climate model, which takes account of the oceanic circulation at the global level as well as biological and chemical cycles …" [56]. This visionary research agenda was the critical incentive for me to apply to the Hamburg group later in 1985.

Working at the MPI for meteorology in the late 1980s and early 1990s was a fantastic experience. Klaus' style of running the Institute was much more inspiring, relaxed, and friendly than anything I had experienced before, even in other Max-Planck-Institutes. As a "biogeochemical" outsider I could profit greatly from the frontier Earth System science analysis methods and modelling tools developed by my colleagues and could sometimes apply them successfully to my own work.

Beyond science, life on the 17th and 12th floors of the Geomatikum and later in the "Pavilion" (Klaus: "we don't call this a barrack") was also a lot of fun. Annual highlights included retreats in Salzau, summer excursions, and the Christmas party. And of course, the seminars within the Institute, especially after 1989.

After Perestroika, a seemingly endless stream of eminent Russian turbulence theory scientists visited the MPI for Meteorology to present their research to Klaus. Whilst turbulence theory in the west had already moved to explicit large-scale computer-based numerical modelling, our Russian colleagues still used pencil and paper to calculate smart second, third or higher order turbulence closure schemes. These visits led to very tough seminar experiences. Typically, Klaus would round up the entire Institute department staff in the seminar room at 11am. There we were exposed for up to two hours, well into lunch time, to huge stacks of tightly hand-written transparencies full of equations, often with Cyrillic letters, presented by researchers who were usually not very fluent in English. And all of this related to a topic, that I didn't understand at all. Once, halfway through, a slide had accidentally been copied on paper instead of a transparency. Klaus immediately offered to go to the copying machine to make a transparency.

While we waited, we heard his footsteps from the ceiling of the seminar room in the pavilion where he had his office. Then Klaus came back and gave the transparency to the presenter. But he also brought a thick stack of proposal documents, which he started to read during the second half of the presentation. Nevertheless, in the questions and answering section, Klaus jumped up and posed several sharp Belgrano questions. An amazing feat in multitasking.

What is the legacy of Klaus' scientific work in our field?

Prior to the mid-1980s, the global carbon cycle had been viewed in Earth System science as just a series of passively connected reservoirs. It had been thought that any atmospheric carbon dioxide variation would simply be damped by the redistribution of carbon among these reservoirs. The concept of potentially significant carbon cycle—climate feedbacks was still in its infancy. When the first greenhouse gas concentration records of the last glacial cycle from ice cores became available, this view changed dramatically. One tool used to understand the interplay between climate and biogeochemistry are Earth System models that describe both spheres in spatial and temporal detail in a coupled, physically consistent way. Klaus' research agenda from the late 1970s outlined the way forward for the development of such coupled models. And indeed, Klaus' team in Hamburg took the lead: Ernst Maier-Reimer built the very first dynamic three-dimensional ocean carbon cycle model coupled to a global ocean general circulation model. For the land side Klaus fostered a collaboration with Gerd Esser, a former student of Helmuth Lieth of the University of Osnabrück, yielding the first spatially resolved global terrestrial biosphere model for carbon cycle studies. Unfortunately, however, this model was in many ways too simple to be coupled into a global climate model in a meaningful way. Eventually, despite these pioneering achievements, it took another decade until the very first coupled global carbon cycle climate model was ultimately realised by a group working at the Hadley Centre.

A personal lesson from Klaus that helped me in my career

During my time in Hamburg, I learned two important things from Klaus, which were very helpful when I got into a driving seat later in Jena:

Decide. Klaus was very fast in deciding, mostly on the spot. For example, he offered me a job right after I gave a presentation as an unknown postdoc visiting the institute in 1984. And when I referred back to this offer in a letter in the following year, I received an answer with a prepared contract within

3 working days (which is completely impossible these days, even for a Max-Planck director). Whether right or wrong, Klaus made a decision. In the rare instance when one perhaps had a better alternative, one could challenge the decision and Klaus would be open to revisions. But any alternative really had to be convincing.

The tropical greenhouse. Klaus mentioned once that he envisages the Institute and its inhabitants as a tropical greenhouse full of fast-growing plants, and his own role was the gardener, who simply has to put some fertiliser here, some water there, and perhaps cut a branch or two over there. But the plants are allowed to flourish themselves. This metaphor reflects nicely the inspiring and very free environment we scientists experienced in order to pursue our ideas.

Hamburg, 1990

4.7 Christoph Heinze

A personal memory of Klaus

After completing my diploma in oceanography, I enquired at the Max Planck Institute of Meteorology about potential Ph.D. opportunities. I was invited to an interview with Klaus Hasselmann.

It was a very pleasant and thorough interview at the end of which Klaus told me that I could start if I would like to. I was pleasantly surprised. He also

told me that I would be free to knock on the doors of all MPI-researchers to find out more about what it would be like to work at the MPI. Open doors, total trust, and freedom—I found all of that extremely attractive.

What is the legacy of Klaus' scientific work in my field?

In terms of marine biogeochemistry in a climate context, Klaus Hasselmann promoted research on all aspects of ocean carbon cycling of relevance to shaping and changing the Earth's climate. The fruitful collaboration between Klaus and Ernst Maier-Reimer resulted in the first global simulation of the inorganic and organic carbon cycles including a simple prognostic atmospheric reservoir. Ernst and Klaus' respective publication in 1987 [84] includes all key features of modern marine Earth system modelling. It nicely lays out the importance of inorganic carbon chemistry and transport with the ocean currents for uptake of anthropogenic carbon dioxide from the atmosphere. At the same time, the publication documents the fact that an ocean model based solely on the inorganic carbon cycle can never be validated against oceanic measurements because the internal structures of carbon and alkalinity (in contrast to the carbon uptake) are dominated by the organic (i.e., biologically driven) carbon cycle. Therefore, biological processes had also been included in the model to show that it worked. The 1987 paper also covers the relationship between different carbon dioxide emission scenarios, the corresponding uptake by the oceans, and the atmospheric retention over time. Thus the paper anticipates the issue illustrated lateron by the IPCC SRES, RCP, and SSP scenarios and related projections: reducing carbon dioxide emissions effectively helps the ocean to buffer the excess carbon dioxide whilst strong peak emissions of carbon dioxide lead to high atmospheric carbon dioxide concentrations because ocean mixing is kinetically incapable of buffering the emissions to a sufficient degree.

Enabling and furthering the development of the Hamburg ocean carbon cycle model (HAMOCC) still influences modern Earth system models with their inclusion of an interactive carbon cycle to provide quantitatively adequate climate projections. Among other things, the development of the ocean carbon cycle model was possible because of the visionary development of a fast physical prognostic ocean water mass model known as the dynamical Large Scale Geostrophic ocean general circulation model (the "LSG"). In the late 1980s and early 1990s this model was among the very few (if not the only one) dynamical ocean grid point models that could be integrated into full quasi-equilibrium models over at least 2000 years of model time. Combining LSG and HAMOCC was an unbeatable model combination at the time because they allowed drift-free extremely long-term integrations

whilst rendering the key features of ocean physics and biogeochemistry in an astonishingly good way. This modelling work was far ahead of its time.

Personal advice from Klaus that helped me in my career

Klaus gave me a lot of advice throughout my time as a Ph.D. student and researcher. For my Ph.D. studies, Klaus had a fantastic idea on how to combine paleoclimatic archives and the results of sensitivity studies with the HAMOCC model on parameter changes and resulting marine tracer changes as well the related shift in atmospheric carbon dioxide concentration to arrive at an estimate of the maximum likelihood for the various hypotheses for what caused the glacial drawdown of atmospheric carbon dioxide. This advice—laid out on a piece of paper in the lobby of a conference that we attended—proved to be a true treasure and a cornerstone for my further Ph.D. work.

How did Klaus' thinking influence my scientific work?

In addition to thinking more in a multivariate and probabilistic way rather than in terms of simple cause-effect relationships, a general attitude towards scientific collaboration comes to mind. I remember the glass cuboid on Klaus' desk that documented an award he received for unselfish collaboration. I thought: "well, this is a really nice award and is characteristic of Klaus' way of handling the scientific process". Creating something together with others without focusing on one's own losses or gains—that helps one to focus on the good, true, and beautiful aspects of scientific work, especially when things may sometimes get difficult in the course of one's daily work.

4.8 Mojib Latif

A personal memory of Klaus

Klaus supported me throughout my career, and I could not be more grateful to him. For example, he thoroughly edited the first draft of my early research papers, and that is how I learned to write scientific papers. Klaus also taught me how to write grant proposals, which also helped me a lot during my later scientific career. Most importantly, however, from the very beginning when I was still a graduate student, he always took great care of me, knowing that I had serious health problems. Klaus even contacted a doctor and made

an appointment for me, which demonstrates how much he cared about my health. Suffice it to say that Klaus was my mentor in every respect.

What is the legacy of Klaus' scientific work in your field?

To me, the explanation of climate variability based on the concept of the stochastic climate model is *the* most important scientific achievement Klaus ever made in my field of climate variability. When published in the mid-1970s, the stochastic climate model revolutionised the field of climate variability. The stochastic climate model provides an elegant framework in which climate variability, which is one of the salient features of the climate, can be understood through the interactions between climate subsystems exhibiting vastly different internal timescales. The stochastic climate model concept can be applied to climate variability over a wide range of timescales, from seasonal to multimillennial. Nowadays, complex Earth system models can be integrated for many millennia to investigate such things as the climate-system dynamics during and after the last ice age. These models, which, among other things, include interactive ice sheet dynamics very much support the stochastic nature of climate variability on timescales up to the multimillennial. In comparison to the spectra obtained from standard climate models (simulating the atmosphere–ocean-sea ice system), the spectra obtained from the Earth system models are much "redder", i.e., the variability keeps increasing beyond centennial timescales.

Personal advice from Klaus that helped me in my career

Good work prevails. Colleagues will recognise and acknowledge high-quality research.

How did Klaus' thinking influence your scientific work?

I try to understand climate variability and climate predictability from a stochastic perspective. Klaus also taught me to put things into a wider context.

4.9 Hans Graf

This is a very personal view of what happened two to three decades ago. Since I never kept a diary, some of the details may be less accurate than a historian might hope for.

My first encounter with Klaus was during a seminar talk I gave in the spring of 1987 at the University of Hamburg as a guest speaker from behind the Iron Curtain (that was already beginning to show signs of rust). I had been researching processes that could potentially result in the El Niño phenomenon. Because of the lack of data available to me at Humboldt University in East Berlin, the talk was based on conceptual ideas and hypotheses. Shortly after I began to elaborate on my ideas, Klaus, who was sitting centre front, seemingly started to nod off. "That's it …", I thought "… it's boring." But, to my surprise, after I finished my talk and was basking in some polite desk-knocking, Klaus' hand rose, and he began to bombard me with detailed questions. He understood what I had meant in my doggerel English. And, most importantly, he was supportive and to my very great surprise and satisfaction, invited me to accept a fixed-term five-year position with MPI whenever it suited me. Although I was not able (or allowed) to accept his invitation right away, it strengthened my backbone and let me grow a few centimetres. I finally came back to this offer in 1990 after two extended visits to the Max-Planck-Institute for Meteorology in 1988 and 1989. By that time it was possible for me to move to Hamburg with my family: Germany had finally been re-united. Meanwhile I had incorporated Klaus' PIP and POP concepts into my research and was studying the interaction between tropospheric and stratospheric circulation, a process that would later become very important for the interpretation of continental winter warming following major volcanic eruptions.

When I began my five-year contract in January 1991, Klaus initially suggested that I might be interested in enabling the then active climate model ECHAM2 to be used for paleoclimate studies. This was always typical of his manner—to make suggestions rather than issuing orders.

External events soon put an end to my coding efforts. First, the media reacted to the burning oil wells in Kuwait during the Gulf war with stories of global apocalyptic consequences similar to a nuclear winter. We had long and controversial discussions about these rather extreme visions in the tearoom in which I stated that the relevant effects would only be local during the winter due to the prevailing very strong inversion layer over the Middle East. Klaus joined in and at the end of the day he suggested an effort involving the whole Institute. I received his long leather whip. All work on the climate was put on hold for several weeks, at least for a dozen or so people, and a paper was written and readily accepted and published in *Nature* in which it was proposed that the effects from the soot belching oil wells would only be local. We did not include the summer simulations. This was my first in-depth contact with aerosol science.

When Mt. Pinatubo erupted in June 1991, I asked Klaus if he would be in favour of a study of the effects of the massive volcanic eruption. He suggested writing a proposal to BMBF (the Federal Ministry of Education and Science), which was accepted within two months. I received my first funding for a project of my own, which marked the start of my scientific independence. I also got my own research group.

About a year later, when our research into Pinatubo was well under way, Klaus called me into his office where he talked to me about his wish to include atmospheric chemistry in climate research. It seemed to me that our institute should mark its leading role as the MPI for Chemistry became quite strong in atmospheric research. Finally, Klaus offered me a permanent position on the proviso that I would concentrate on atmospheric chemistry. A PERMANENT POSITION at MPI-M! Since I had no idea of chemistry beyond what I had learned at school, I asked for a day to consider the offer, which he allowed me, saying that: "You can do anything if you are intelligent!". The next day I suggested concentrating on aerosols as a combination of physics and chemistry, which would give me more confidence. I guess that was an intelligent idea. Klaus accepted my proposal, and I began my period of aerosol research, which culminated in the BMBF-funded National Aerosol Research Programme, which eventually resulted in many invaluable contacts including a very close collaboration with the MPI for Chemistry in Mainz.

The last great piece of advice that Klaus gave me in 2002 was to accept the offer to take on a newly installed Chair and Professorship on Environmental Systems Analysis at the University of Cambridge. He dismissed my reservations about missing links and the lack of climate research activity at Cambridge pointing out that: "Once you're there, you'll be able to do whatever you want!".

He was right … again.

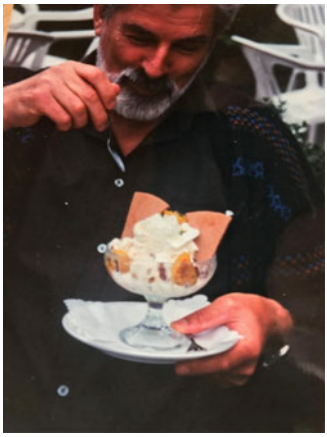
4.10 Gabriele Hegerl: Der Alte

I first met Klaus when I came to Hamburg for a job interview as postdoc—I was clearly considered slightly unusual by Hans von Storch and Klaus, and not only because of my very strong Bavarian accent. Klaus found my interest in climate, my weird Ph.D. topic and my language amusing and they hired me to work on a hugely exciting topic, namely the detection of climate change. I followed in Ben Santer's very large footsteps, and worked directly with Klaus and Hans, or rather mostly with Hans at first, as Klaus was too busy and may also have been considered too intimidating. His input also

needed translation for a climate science novice like me. It was interesting to see what a huge presence Klaus was within the MPI at that time, and probably even now! The only other time I encountered this level of admiration was when I went out with the Munich philharmonic players after a concert and noticed how they spoke about their then conductor. In both cases, it had to do with people's admiration for someone who had mastered his field and was able to do things we could only dream of. For us, he was the boss, the guru, the man who sets the topics, mentors our work, sees the flaw in our scientific arguments but also has some amazing ideas how to fix them, the decision maker, the person who knows where to go. Presenting results in front of Klaus at the annual retreat in Salzau was an amazing opportunity but also quite terrifying. If there was any flaw in one's work—and surely there was bound to be—then Klaus would be sure to spot it. Everybody had seen him happily dozing through seminars only to wake up and ask THE QUESTION—the one question that really picked on the deep issue somewhere in that problem, the unjustified assumption, the pedestrian approach, or the core of the problem. I know very few scientists who are able to do this—to spot the big issue and latch onto it –so Klaus is a rare and truly outstanding scientist. It was no wonder that the survivors of this opportunity and ordeal would take part in a lot of relaxing activities after the Salzau presentations.

Klaus has very much shaped the field of climate science, and the two pieces of his work that I admire most are his stochastic climate models [38] and signal detection methods [54, 110, 129]. His work is still frequently cited, and his way of thinking about the problem has shaped the field. We no longer search for a deterministic response that can be linked very simply to chains of argument. Instead, we look for the climate system integrating weather phenomena and other noise and resonating in response. This has also worked for me in relation to the role of volcanic activity in the last millennium, where short sharp shocks lead to low frequency variability such as the Little Ice Age. Of course, I am very partial to signal detection—Klaus' 1979 paper [54] was the first to set out a framework for how to achieve this and, whilst the idea has been reshaped by the community, it has survived and his relevant papers still cited frequently. By the way, my Ph.D. student has recently rediscovered principal oscillation patterns [86] and Klaus' arguments against discounting future damage to the climate in integrated assessment modelling has taught us how to think about climate change and the benefit of mitigation. But, I still find stochastic climate models the most beautiful of all Klaus' ideas.

Post meeting relaxation with some beverages (with Ernst Maier-Reimer and Joerg Wolff)



While Klaus tucks into his reward

In addition to teaching me a lot about climate science, Klaus also taught me three essential practical things: the first is that it is worth spending a lot of time polishing papers and getting them just right. I will never forget that long evening when I needed to submit our attribution paper [135] prior to leaving on some trip or other. I thought it was ready. Klaus thought it wasn't. So, throughout the day I got lots of scribbled corrections handed

down from the upper floor pavilion to my office. At around 9 pm he ordered pizza and handed over the final corrections. I completed the paper, printed it, and boxed it into a courier box (those were the days). Of course, his corrections were almost illegible but if I didn't manage to decipher them this time, they would come back exactly the same the next time. Very predictable! Even more challenges in reading his handwriting arose when he faxed equations from Sylt. We once had a debate about how to optimise the fingerprint in practice—it's quite hard to decipher faxed scribbled equations and when successful, this is followed by the even harder task of understanding them.

Klaus taught me two other practical things:

To do important things really well. It is fine to focus on the thing one is most dedicated to and to put less effort into less important things. I keep repeating this to myself as a mantra—you want to do research to perfection in midterm project reports or committee reports is not needed.

Research and life are full of opportunities. There is no need to continue doggedly with what one is currently doing. If the topic in question is too busy and the good stuff has already been published then move on and look for new questions. Try something crazy. Don't get stuck on the same thing. I try to do that too and sometimes it works. Following Klaus' advice, at least sometimes, has opened up many really interesting research opportunities. And his optimism also extends to politics: people, he says, will understand that climate change is important and that it needs to be addressed. We will solve this. I certainly hope we do and that the future proves him right!

4.11 Jin-Song von Storch

I belong to the younger generation and got to know Klaus as a generous director interested in science and only in science, although it took me some time to realise this. I started my Ph.D. at the MPI with Hans in 1987, working on predicting ENSO using POPs, and had little idea about Klaus. I remember that at some point I needed to talk to him. I went to see him and was nervous, but found only Elsa Radmann: "Mr. Hasselmann is in a meeting", she snapped. So that was my early impression of Klaus; a director who is not easily reachable.

Like many young scientists, I was keen to present my results. My problem at that time was that I was unsure about how to get Klaus' attention. There were so many great scientists at the Institute such as Mojib who became famous within the TOGA community overnight. And some of them, such

as Hans, have always been loud. So, the only way for me to get Klaus' attention was to give a talk. But most talks did not go well, because Klaus usually started asking questions after two minutes and then dominated the entire discussion. One needs to do something about it. I discovered he likes sweets so, when it was my turn again to give a talk, I brought a box of Toffiffee. I was able to keep him busy chewing, but not for too long.

I learned more about Klaus from elder colleagues such as Peter Müller and Dirk Olbers. Actually, I (as a meteorologist) learned physical oceanography from Peter and Dirk (more precisely from their books). I was struck by the rigor and the precision of their theories, both in terms of the fundamental equations and the various approximations derived from these equations. I had a hunch that the way Peter and Dirk work very probably had something to do with their mentor, Klaus. Peter described his experience after he left Klaus' group. He was surprised to learn that Klaus was an exception: when you leave the MPI, you get to know normal people.

The most unforgettable picture I got of Klaus was the one I formed of him at the colloquium held on his 60th birthday shortly after completing my Ph.D. The colloquium was attended by some world-renowned people and Klaus gave his famous talk on his metron theory. Like many others, I didn't understand a word of it, but I do remember him saying: "you can ask me, but you cannot stop me". I witnessed a real scientist talking!

4.12 Hans Von Storch

Encountering Klaus

When I first got into the field of meteorology as a recently graduated mathematician working in the Günter Fischer group, I worked 2 floors below Klaus in the Geomatikum. I was aware that there was a Max-Planck Institute—two friends of mine had done their Ph.D. studies there—but I had no real idea about what they did, and who Klaus was. I didn't meet him until about 1982 or '83, when Klaus organised what we foot soldiers called the "Lütjenseer Wendeparteitag", to which our university group was invited. That was because Klaus had determined that the work being carried out at his Institute had matured to the point that quasi-realistic modelling capacity had to be installed at the MPI, and that our group, in particular Erich Roeckner and Ulrich Schlese, would be useful for this purpose. I had to give a talk, and I decided to talk about the statistical comparison of ensembles of model simulations and went on to discuss non-parametric methods. Klaus did not like it and tried to teach me the significance of red and white

noise about which I had no idea. He was probably right, but I didn't want to accept that. It all ended in an unconstructive heated debate. Later Ernst Maier-Reimer comforted me with a good beer. I got my "habilitation" the following year, thanks to Günter Fischer, who then suggested to Klaus that he should hire me. When we met for a chat, I referred to our "discussion" in Lütjensee, but Klaus just waved it away as irrelevant. I found out that he had also had a similar experience, when Reimar Lüst offered him the MPI. Lüst had attended Klaus' now infamous presentation at the Atlantic Hotel, at which he overlayed so many slides on the overhead projector that the screen was just black—an event that Klaus remembers with a certain amount of embarrassment.

I was hired and entered a new scientific world. Eventually I read Jenkins & Watts, learned about red and white noise, about detection and attribution and all that, wrote the book "Statistical Analysis in Climate Science" with Francis Zwiers, and had many more beers with the unforgotten and much missed Ernst Maier-Reimer. What a privilege!

As for Klaus himself, he was always generous, mostly charming and humorous, but strict and impatient when it came to the science. One problem is that his pronunciation is often difficult to understand—but now I know why: his speech is often not a communication to others, but rather the sound that accompanies his thinking. Whenever he mumbles, then he is still doing his intellectual analysis.

What is the legacy of Klaus' scientific work in your field?

Klaus contributed to many scientific challenges, but I only grasped, and perhaps incompletely, his achievements in the field of stochastic framing of the climate system, in ocean wave modelling, and in linking climate and society. Whilst the first two have obviously been enormously successful, I do have certain reservations about the latter.

The really significant part was his statistical thinking, the concept later encapsulated in the concept of "Principal Interaction Patterns", according to which the full phase space of a system is divided into two parts, a small, low dimensional part, where the key dynamics takes place, and the remainder with very high dimensions, which is mostly a slave of the first part and feeds back into the dynamical core through conditional statistical models (commonly named parametrisations). This concept was already encapsulated in his first strike—the stochastic climate model, which predicted that long-term variations would emerge in the climate system, without a forcing acting on these time scales: "smoke without fire". This "noise" was not just a nuisance when it comes to identifying the dynamics and interlinkages

but was a generic part of the dynamics. His second strike in 1979 was the discrimination between this unavoidable unprovoked variability, the noise, and any signal reflecting the presence of external forcing. The detection-and-attribution concept developed from this, which justified the assertion that the ongoing emissions of greenhouse gases is changing the climate of Earth. His strike 3a was the formulation of the Principal Interaction Patterns in 1988. He had already developed an early version, which he named Principal Oscillation Patterns, which he asked me to breathe practical life into. I did so—but at the cost of simplification, of vulgarisation. He invented PIPs in response to this. This was his strike 3b. His first two achievements changed climate science and the role it plays in the global economy and policy making. As an abstract concept, the third shaped my thinking.

Klaus attempted to expand these ides to include society as a component within the climate system. However, the basic assumption, namely the persistent existence of a low-dimensional subspace with a dominant dynamic, is questionable in relation to societal dynamics. I do not believe that such a subspace could exist for a sufficiently long time and think that it would be conditioned by a variety of inhomogeneous cultural configurations.

How did Klaus' thinking influence your scientific work?

His thinking guided me—in conceptualising the climate system in the spirit of PIPs, with the detection and attribution being carried out in an appropriate low dimensional subspace of dominant dynamics. He convinced me that noise is ubiquitous in the climate system, on global and regional scales, in the atmosphere and the ocean. My latest research interest was, and continues to be, the emergence of such (hydrodynamical) noise in marginal seas and its scale-dependency.

His thinking also influenced me to ask whatever and whoever: why? Where is the evidence? What are the hidden tacit assumptions?

What piece of personal advice from Klaus has helped you in your career?

The only piece of advice I remember is "don't worry, when time is ripe, a door will open for you. When you are good at something, and it is of interest, an opportunity for an application or for a job will emerge". I now give this same advice to my own Ph.D. students and co-workers. It works.

Whilst not taking the form of explicit advice, his management approach has informed my own. Never look for finance planning details (Hinzpeter's dogma in this context: "Eine Zahl ist keine Zahl") but base your decision solely on a consideration of the relevance for the work and the issue, whilst

keeping the personal implications in mind; decide immediately in most cases. In and of itself, increasing the number of co-workers and of the influx of money is not a legitimate goal when it comes to running a research institute.

4.13 Patrick Heimbach: Interactions with Klaus' Sphere of Influence

It was Spring 1993; I had recently completed my Dipl. Phys. in Bonn and was keen to change my subject of study to climate research. A first application for a Ph.D. position at the University of Hamburg had failed, leaving me deeply disappointed. This was the backdrop for my interview at the MPI, where I first met Klaus and Susanne. At the end of a day's visit, Klaus explained to me that there was no current opening in the climate dynamics division, but that they were looking for a student in the "Seegangsgruppe". I didn't quite know what that was all about, but immediately accepted the offer nonetheless, having been deeply impressed by the person and the interactions I had had just on that day. Two initial personal lessons I learned were: (i) more often than not it is good to follow your instincts; (ii) sometimes an initial rejection opens the door to a much brighter sequel.

Thus, in the summer of 1993 I began work in remote sensing and modelling of ocean surface waves. The context was the recent launch of the first European Remote Sensing satellite ERS-1, which opened up the prospect of being able to observe ocean surface waves on a global, quasi-routine basis, and the ability to perform detailed validation of the third generation Wave Model (WAM). One specific scientific question concerned swell propagation over long distances, and the process of dissipation. The fact that we were picking up a classic field experiment that had been conducted by Walter Munk and colleagues—including Klaus—in the early 1960s, following swell propagation across the Pacific along a great circle [18], but now using remote sensing, offered me a wide range perspective for studying the problem as well as giving me an insight into Klaus' early work.[3]

More happy surprises awaited me soon after starting my work: within days of starting my job, a group of researchers from the USA visited the MPI, and Klaus invited me along to their meetings, despite my almost complete ignorance of the subject. Within months, I took my first trip to Utrecht (KNMI) to celebrate the publication of the now classic book on *Dynamics*

[3] There is a beautiful, 30-min documentary about the experiment, which I regard as a must-watch when teaching about ocean surface waves, narrated by Walter Munk, and in which Klaus has several brief appearances: https://www.youtube.com/watch?v=MX5cKoOm6Pk.

and Modelling of Ocean Waves [244]. These are but a few examples of Klaus' trust in people and his ability to develop the deep sense of community that makes research teams successful.

More lessons were learnt along the way, e.g., (iii) that what I had studied in theoretical physics about particles and fields could also be applied in oceanography, as pioneered by Klaus, and (iv) that—arguably—ocean wave research provides the basic training for climate science ("Seegang, die Grundschule der Klimaforschung"); at least this was one (of several) way(s) in which the small "Seegangsgruppe" justified its *raison d'être* within a large climate research institute, sometimes in a slightly tongue-in-cheek manner. Nevertheless, the current renewed interest in the subject provides some vindication (e.g., Villa Bôas et al. 2019).[4]

I was the third "Rheinländer" in the small group, next to Renate Brokopf and Georg Barzel. Renate's cookie box (always filled with "Prinzenrolle") ensured that we'd get regular visits from Klaus. Over the years I would come to represent the group in a variety of project meetings and symposia—earning me the title of "Reisedoktorand" (the travelling doctoral candidate).[5]

The time came for me to produce scientific results. "Schon sehr schön" is what I would get to hear a lot. "Very nice for a start" might be a *precise* translation, but an *accurate* one would emphasise the fact that *lots* of work remained to be done. Those words are telling of Klaus' deeply human approach to mentoring. Always encouraging, setting a positive tone, but just as clearly conveying to the mentee the many ways in which the work he or she presented remained insufficient. Frequently overwhelmed by the deep insights of the mentor, the mentee would walk away from a meeting, wondering how he or she could ever move beyond "Schon sehr schön". What may have saved me was the privilege of being exposed to a rich spectacle of perspectives that Klaus weaved together into a complex story of the climate system, from its physical machinations to its societal interactions.

It is difficult to choose among the many lasting impacts that Klaus has had on the field. Others who have contributed to this volume have provided accounts in the context of surface wave modelling and remote sensing (and see a recent review by Klaus himself [176]), so I will highlight work not done by Klaus himself, but which he had the vision and foresight to support, and which would prove important to my work as a postdoc with Carl Wunsch

[4] Bôas, A. B. V., et al. (2019). Integrated Observations of Global Surface Winds, Currents, and Waves: Requirements and Challenges for the Next Decade. *Frontiers in Marine Science*, *6*, 2219–2234. https://doi.org/10.3389/fmars.2019.00425.

[5] Among the noteworthy places and people that left an impact on me were encounters with Bertrand Chapron and Harald Krogstad at Ifremer, and David Halpern at the World Expo'98 in Lisbon, Portugal.

at MIT. This was about developing a software tool, initially developed at the MPI by Ralf Giering and Thomas Kaminski and later matured at MIT, that could "differentiate" a model code, i.e., generate code that represents the derivative of some model output with respect to some inputs by means of "automatic differentiation".[6] This tool would prove essential in NASA's ocean data assimilation consortium "Estimating the Circulation and Climate of the Ocean" (ECCO), which now has a 20-year legacy, involving various former and present members of the MPIMet (Marotzke et al. 1999, Stammer et al. 2002, Heimbach et al. 2019).[7]

My personal, deep, and lasting impression is that of an extraordinary individual, not only intellectually, but as a human-being, generous, caring, free of allures, and with a rich sense of humour. The latter shines through in this concluding anecdote: it is the story of an elderly man who appeared regularly in the halls of the Geomatikum with a pamphlet in which he claimed to have proven that Pi is a rational number. At one point, Klaus mused (with a subtle ironical smile) that it might be best for him to join this old man to distribute his own work on the metron model.

4.14 Jörg Wolff: The Shortbread Biscuit

There was a conference in Hawaii, which I really wanted to take part in. To get permission, I grabbed a shortbread biscuit, put it on a small plate, and went straight to Klaus' office. Elsa Radmann allowed me to enter and I told Klaus that this would be an attempt of bribery. He looked longingly at the shortbread. I presented my case, he accepted, and ate the biscuit.

[6] Giering, R., & Kaminski, T. (1998). Recipes for adjoint code construction. *ACM Trans Math Softw*, *24*(4), 437–474. https://doi.org/10.1145/293686.293695.

[7] Marotzke, J., Giering, R., Zhang, K. Q., Stammer, D., Hill, C., & Lee, T. (1999). Construction of the adjoint MIT ocean general circulation model and application to Atlantic heat transport sensitivity. *Journal of Geophysical Research*, *104*(29), 529–548. https://doi.org/10.1029/1999jc900236.

Stammer, D., Wunsch, C., Giering, R., Eckert, C., Heimbach, P., Marotzke, J., Adcroft, A., Hill, C. N., & Marshall, J. (2002). Global ocean circulation during 1992–1997, estimated from ocean observations and a general circulation model. *Journal of Geophysical Research*, *107*(C9), 3118–1–27. https://doi.org/10.1029/2001jc000888.

Heimbach, P., Fukumori, I., Hill, C. N., Ponte, R. M., Stammer, D., Wunsch, et al. (2019). Putting It All Together: Adding Value to the Global Ocean and Climate Observing Systems With Complete Self-Consistent Ocean State and Parameter Estimates. *Frontiers in Marine Science*, *6*, 769–10. https://doi.org/10.3389/fmars.2019.0005.

4.15 Ben Santer: A Road Trip with Klaus

One of my favorite personal memories of Klaus was traveling with him to a meeting of the International Detection and Attribution Group (IDAG) in Boulder in the early 1990s. At that time, Klaus was working on the draft of what would later become his seminal 1997 paper on fingerprint detection [129]. When we boarded the international flight to Denver, Klaus informed one of the flight attendants that we were engaged in important scientific research. Were a pair of quiet seats available in business class?

I've never had much luck with polite requests for free upgrades to business class, but Klaus was successful. My "lesson learned" was that it helps to travel with someone who conveys—even to those who do not know him—an impression of quiet authority, of distinction, of being "außergewöhnlich".[8]

And Klaus is "außergewöhnlich". I've never met anyone like him. The essays in this book will surely attest to the extraordinary contributions Klaus has made to many different areas of climate science. Stochastic climate models. PIPs and POPs. Optimal detection of anthropogenic signals. Elucidation of the cold start effect. Development of ocean wave models. Exploration of the economic impacts of climate change. The list of contributions is long and illustrious, each highlighting Klaus's unique ability to see the complex climate system from a novel and interesting perspective.

While such vision and scientific brilliance is "außergewöhnlich", it is the pairing of vision and brilliance with very human qualities—humility, and deep curiosity about the world and people around him—that is truly extraordinary.

Back to our flight to Denver. Klaus worked on the *Climate Dynamics* paper, passed me a draft version, and asked for my comments. I felt that it would be impolite to read a magazine or fall asleep. If your boss is changing the world of anthropogenic signal detection on a flight from Germany to Denver, you don't fall asleep. You pay attention.

During the meeting of the IDAG group at the National Center for Atmospheric Research (NCAR), Klaus provided the scientific direction for the group's efforts to identify a human-caused warming signal. He reminded us of the power of patterns. As he had written back in his famous 1979 paper [54], "*It is necessary to regard the signal and noise fields as multi-dimensional vector quantities and the significance analysis should accordingly be carried out with respect to this multivariate statistical field, rather than in terms of individual grid-point statistics*." Or put simply: Look at patterns, not at individual grid-points.

[8] Langenscheidt's translation of "außergewöhnlich" is "extraordinary, exceptional, outstanding".

Pattern analysis provides you with the power to discriminate between natural internal variability and the forced response to human-caused greenhouse gas increases.

It was a key insight, and it provided a "statistical roadmap for hundreds of climate change detection and attribution studies"—studies which ultimately identified human-caused fingerprints in many different independently monitored climate variables.[9]

After the conclusion of our IDAG meeting in Boulder, Klaus and I had a free afternoon before our return flight to Germany. Why not go for a drive to the Rockies?

What a marvelous experience that was! In Hamburg, given the sheer number of scientists, students, and visitors wanting to see him (and the tight control of his schedule exercised by Frau Radmann), it was difficult to get a few hours of uninterrupted hours of "Hasselmann time." I had that privilege now.

So we drove to Estes Park, the gateway to Rocky Mountain National Park. I recall how good it felt—after hours in airplanes and in a meeting room—to get out and stretch our legs in Estes Park, and to take in the grandeur of the Rockies. And I remember Klaus's humanity. He was genuinely interested in me as a human being, and not just as a scientist. A drive that might have been anxiety-inducing and intimidating for a young post-doc instead became a few truly memorable hours—the opportunity for a fascinating conversation about life and science.

Klaus Hasselmann has accomplished many great things in his scientific career. He published ground-breaking research. He led an institute that became a world-leading research center for climate modeling. He helped the world understand that humans are not merely innocent bystanders in the climate system—human activities are actively changing Earth's climate. But in addition to all of these great achievements, he had a "discernible influence" on the lives of generations of colleagues and students. That contribution will be just as enduring as all of his contributions to climate science.

[9] Santer, B.D., C. Bonfils, Q. Fu, J.C. Fyfe, G.C. Hegerl, C. Mears, J.F. Painter, S. Po-Chedley, F.J. Wentz, M.D. Zelinka, and C.-Z. Zou, 2019: Celebrating the anniversary of three key events in climate. *Nature Climate Change*, **9**, 180–182. https://doi.org/10.1038/s41558-019-0424-x.

4.16 Ulrich Cubasch: How a Postdoc Became an IPCC Convening Lead Author

My first encounter with Klaus Hasselmann was at ECMWF in Reading, where I was working on the development of the next generation of the forecasting model. My colleagues mentioned to me that he (they had given him the nickname "The Kaiser") would be coming to Reading for about a fortnight to do research. I was of course curious to meet the scientist with such a nimbus, and I seized the opportunity to have a brief conversation with him. Later I contacted him about the possibility of doing a Ph.D. in Hamburg. It turned out that, unlike the University of Reading, it was possible to do obtain a Ph.D. from the University of Hamburg without being enrolled. Due to the different curriculum structures at UK and German universities, being enrolled would have meant that I would have had to spend a lot of time attending Ph.D.-courses, which merely repeated what I had learned for my German Diploma. Prof. Günter Fischer agreed to supervise the thesis as an official representative of the University of Hamburg jointly with Klaus Hasselmann, and Hans von Storch did the some of the coaching. ECMWF did not mind this set up, as long as it did not interfere with my normal work. At a later stage, its support became stronger, as the thesis dealt with performing extended range predictions using ensemble techniques. It was anticipated that this methodology had the potential to extend weather forecasting for a longer period.

I was later invited by Klaus Hasselmann to join his group as a postdoc. I found the topic of climate science more interesting than the continued attempt to improve weather forecasts, which was the main focus of ECMWF. Some of its member states insisted that that should be its only goal. I was keen to get my teeth into coupling an atmosphere model, something I was familiar with through my work at ECMWF, with a comprehensive ocean model. At that time, only the University of Oregon had accomplished it and published results, but GFDL and NCAR were already performing test runs.

One day, it must have been in late 1988, Klaus Hasselmann came into my office and asked me if I would volunteer to fly to Princeton in his place for a meeting between groups working on coupled ocean–atmosphere modelling. He told me that they were planning a comparison between various examples of this type of model. My job would be to represent the Institute and its research (to fly the flag). I went there, keen to meet all of my colleagues working on this task. It turned out that it was a high-profile international meeting which had been set up in preparation for the first IPCC-report by working group 1. At that time, I (and maybe also Klaus Hasselmann) had

not really been aware of the importance of this workshop, so I was a bit surprised by the lion's den atmosphere created by some of my high-profile colleagues. As they had been expecting Klaus Hasselmann, not some little known postdoc, it was a bit of a challenge for me to convince the attendant US- and UK-dominated science community that there was also pertinent research been performed in Germany. As the IPCC strives to achieve an internationally balanced membership, they eventually embraced our effort. Our Institute was selected to compile one of the chapters of the IPCC-report, which dealt with the coupled model comparison. Perhaps feeling a bit snubbed by Klaus Hasselmann's absence, Michael Schlesinger suggested that I should be the author of this chapter. He pointed out that Klaus would probably be too busy to deal with the humble task of comparing models and data. They also assigned Robert Cess, a seasoned scientist with a lot of experience in how to integrate the various scientists' attitudes, as a co-author.

I tried to involve Klaus in the IPCC-activities and discussions when I returned to Hamburg, as considerable rivalries had emerged between the institutes which had been asked to contribute to the comparison. From time to time I approached him for comments or suggestions, particularly when there were conflicts. Knowing the characters of many of the persons involved, he advised me "to keep my head down" and to play an integrative role. During this time, he focused on creating results that would improve the IPCC report. With the MPI being part of the authors team, it was assured that his and the MPI's and University of Hamburg's scientific works would be cited and recognised by the international community.

Due to the IPCC's high international profile, more and more institutions and nations became interested. The IPCC grew larger and larger. To fend off the numerous external attacks by special interest groups, it became increasingly formalistic. Nowadays the author of a chapter is selected in an elaborate procedure, where I as a postdoc would not stand a chance. The IPCCs activities (and all of the people who contributed to its success) were honoured with the Nobel Peace Prize in 2007.

Having been drawn into the IPCC in an early phase of my career, it has influenced my research ever since. I had the fortune to be selected as author and coordinator in all of the following reports. These activities brought me into contract with the international science community, the EU funding agencies, and several German government bodies. I had the opportunity to travel around the world, as the IPCC spreads its meetings around the globe to demonstrate its international character.

In summary, I am grateful that Klaus Hasselmann enabled me to obtain a Ph.D. and that his confidence in delegating tasks to his staff provided me with a once-in-a-lifetime opportunity that shaped my entire career.

4.17 Achim Stössel: From Seaman to Professor Thanks to Klaus Hasselmann

Klaus is the most important person to whom I owe my scientific career, which has extended all the way to a tenured professorship. I never imagined any of that when I was still with the merchant navy some 40 years ago, staring brainlessly out to sea from the navigation bridge of a freighter as a nautical officer on watch. I well remember my first encounter with Klaus in his office at the MPI-M where he justifiably worried about my grades (not a C-candidate, but also not a straight A-candidate), and how he was initially reluctant to accept me as a Ph.D. student. I heard (maybe just a rumour) that Susanne had somehow convinced him of the benefits of having a seaman on board in his institute. I also recall Klaus trying to convince me to work on wind-generated waves rather than sea ice, presumably because of the 4 years of seagoing experience I had by then. At some point much later (I believe it was during one of the Salzau meetings), we even argued about the climate relevance of surface gravity waves versus sea ice.

Anyway, after Klaus and Peter Lemke had decided that I would work on sea ice, I remember coming up with the suggestion to first test Bill Hibler's new viscous-plastic rheology sea-ice model in the Baltic Sea, as this was a region with a dense observational network, which meant that we would readily be able to evaluate the realism of the model simulation. Klaus' response was that the size of the Baltic Sea corresponds to just 2 grid cells of the T21 model, so I was to apply the sea-ice model to the Southern Ocean around Antarctica, for which good forcing and verification data was of course much more difficult to obtain. Peter, Breck Owens, and I nevertheless cranked out a convincing paper, and I eventually defended my dissertation on this topic in December 1990.

By then in my mid-30s, I was confronted with what to do next. With our first child underway, I didn't want to jump from one 3-year project to the next. I recall approaching Klaus one day asking about the possibility of continuing to work at MPI-M as a research scientist. That meeting was rather short: he first asked me about my age, then about the number of publications I had. After hearing my response, he said that he would grant me another 6 months. That was a clear message. I nevertheless stayed on for 3.5 years as

a postdoc because of an SFB project for which I obtained funding, but it was clear to me that I would need to look for a more permanent job elsewhere. I therefore submitted some 15 applications for such positions, was interviewed for 3, and was accepted for 1, and that was undoubtedly because of Klaus (recommendation letter) and the fact that I did my doctoral and postdoc research at the MPI-M. Not only that, even when deciding on whether or not to offer me a tenured position, I learned later that my current employers had asked Klaus for a recommendation letter. To sum it all up, I am most grateful and lucky that Klaus accepted me into his Institute back then, and that he continued supporting me all the way to my current position, in spite of my former non-scientific career.

4.18 Robert Sausen: Interactions with Klaus Hasselmann

I first met Klaus Hasselmann when I was a Ph.D. student during a summer school organised by the Studienstiftung des Deutschen Volkes in Alpbach. I was so impressed by his ideas on climate change and the methods he used that I applied for a postdoc position at his Institute and was grateful for the opportunity to start work there in 1982. Once there, I initially found it difficult to understand Klaus's concise way of presenting his ideas as did the other postdocs and Ph.D. students in his group at the MPI for Meteorology. Luckily, we were helped by the "ZKs", the "Zwischenkapazitäten" (the clever minds in between), Jürgen Willebrand and Dirk Olbers. They were already experienced colleagues, both when it came to the science and to understanding Klaus. So, they translated his ideas into a language that a postdoc or a Ph.D. student was able to understand, and, in this way, we learnt a lot.

Following my training phase, Klaus pushed me in the direction of studying averting the initial drift in coupled atmosphere–ocean models. I came up with the idea of "flux correction", whereby a better name would have been "anomaly flux coupling". The method was quite successful, but also controversial. The first time I presented it to an international audience was at the Erice summer school in 1986. The discussion after my short presentation was rather heated, mainly among the lecturers at the summer school, with the Europeans in favour of my ideas and the Americans opposing them. Nevertheless, I, the young scientist, felt fairly safe because I knew that Klaus was protecting me.

I had a similar experience a few years later in 1990 or 1991, when I told a journalist that climate change met with little interest among policy-makers, as the effects of climate change would be felt much later than the legislative period. A high-ranking officer of the German ministry of research complained to Klaus about what I had said. And Klaus simply answered that I told the truth. Klaus taught me to be frank about unpleasant results and news.

I highly appreciated the inspiring and supportive environment that Klaus created at his Institute.

4.19 Dmitry V. Kovalevsky

How did you meet Klaus?

My research collaboration with Klaus began in 2007. Klaus introduced me to socioeconomic modelling related to climate mitigation, and this completely changed my subsequent trajectory in academia. All this began when I was introduced to Klaus at a conference in Berlin in late 2007. Since then, I am indebted to Klaus for all his kind, invaluable, continuous support throughout my career. With the aid of his support with many issues, the socioeconomic research group was established at the Nansen Centre in St. Petersburg (NIERSC)[10] where I was working at that time, and I became the leader of this newly formed group. Klaus provided very active support for the activities of our group and collaborated with us enthusiastically. We developed the models together and published co-authored papers. Thanks to Klaus, we were invited to consortia concerned with a number of major research proposals, and Klaus himself was also a very active contributor to the proposal writing process. As examples of our joint project activities, I would refer to two major EU FP7 projects during the past decade, COMPLEX[11] and EuRuCAS,[12] in the course of which our group at NIERSC collaborated very actively with both Klaus and other project participants on the implementation of the project. Klaus travelled to St. Petersburg several times to present keynote talks at workshops and colloquia organised by our group at NIERSC, and to attend meetings that were important to the group.

During my two research visits to the Max Planck Institute for Meteorology (MPI-M) in 2015, Klaus kindly offered to let me use his desk in the

[10] Nansen International Environmental and Remote Sensing Centre (NIERSC), St. Petersburg, Russia.

[11] EU FP7 COMPLEX, Project No. 308601 "Knowledge Based Climate Mitigation Systems for a Low Carbon Economy" (2012–2016).

[12] EU FP7 EuRuCAS, Project No. 295068 "European-Russian Centre for Cooperation in the Arctic and Sub-Arctic Environmental and Climate Research" (2012–2015).

Emeriti office of MPI-M (the other *Emeriti* desk in the office was for Prof. Lennart Bengtsson). Klaus and Susanne kindly invited me, and following my marriage, my family to stay with them and/or visit them in Munich, Glückstadt, on Sylt, and more recently in Hamburg, and we are always welcomed with the warmest hospitality. Another of our unforgettable experiences was when Klaus and Susanne invited us to attend a choir performance in which they were singing. Klaus is always sharing so many interesting stories with us about his life and career, about his family and relatives, and about his travels all over the world.

What is the legacy of Klaus' scientific work in your field?

With what field should I begin? My research career is connected to several areas in which Klaus was very active, including theoretical physics, oceanography and—as mentioned, thanks to Klaus personally—transdisciplinary modelling for climate mitigation. It is the latter area in which his ideas and contributions have shaped my own thinking and research activities to the largest extent.

Has any personal advice from Klaus helped you in your career?

Klaus gave me a lot of invaluable advice on various topics during our lengthy collaboration, and I could gratefully provide many examples here. For instance, he gave me some comprehensive technical advice relating to IT under very non-trivial circumstances, which continues to help me a lot in my research until the present day. That was in 2008 when we had recently begun our collaboration with Klaus on socioeconomic modelling, and I had to master a specialised software programme that Klaus was systematically using for developing his models (to avoid accusations of hidden advertising, I shall refrain from naming this excellent software package here). To help me learn as quickly as possible, Klaus kindly gave me a personal training course in the most wonderful and hospitable environment one could ever imagine: Klaus and Susanne kindly invited me to stay with them on Sylt. For several days I sat with Klaus over a laptop, whilst he used all his pedagogical talent to teach me step-by-step how to use the various features and options of the programme. In a spirit of full disclosure, I should add that after these intense lessons there were wonderful walks with Klaus and Susanne along the seashore and in other beautiful places in Sylt. Having benefited from this personal IT training from a famous scientist, I am still actively using the knowledge and skills I acquired.

How did Klaus' thinking influence your scientific work?

The impact of our nearly 15-year-long collaboration with Klaus on my research activities and, more broadly, on my way of thinking and problem solving, has been enormous. I am very much obliged to Klaus for so many new and inspiring ideas and for the new methods and tools with which he made me familiar. Had it not been for those years of learning from Klaus as well as communicating and collaborating with him, my own mental model of the world would currently have looked completely different.

4.20 Carola Kauhs: A Non-Scientific View on Professor Klaus Hasselmann from the Institute's Librarian

I have known Mr. Hasselmann for 38 years now, and he has accompanied me throughout my entire active professional life. As a librarian who had just completed her degree, I started working at the Max-Planck-Institute in 1983 as the successor to Mrs. Grimminger, whom Mr. Hasselmann had lured away from his own father in 1975 to build up the joint library at the MPI-M and the university institutes of meteorology and geophysics. Mrs. Grimminger had a very special way of dealing with the Managing Director and was able to convince him with her arguments. Lucky me.

Years later, I learned from my library colleagues that the Managing Directors at the MPG Institutes changed regularly. But at our Institute, we had the same Managing Director for years: Mr. Hasselmann. I couldn't understand why my colleagues were so excited about a special event known as the "Scientific Advisory Board". Either we didn't have anything like that at the Institute or it always completely passed me by.

On behalf of the works council, I sometimes had to make Klaus Hasselmann aware of various things. In those days, it was still possible to hold the works meeting in a medium-sized seminar room in the "MPI-Pavilion". Mr. Hasselmann would sit in the front row, face-to-face with the works council members. When a proposal was made to approve educational leave for scientists at the MPI-M to take additional English classes, a firm, non-evasive look was sufficient to give the Director to understand that the proposal was denied on the grounds that scientists at the MPI know enough English and don't need educational leave for that.

I didn't see Mr. Hasselmann that often during his active time at the Institute. One day, a group of architects were strolling through the library

discussing expansion plans for the computer centre on the same floor using the library space. No information about these plans had reached me in advance, so I was quite annoyed with this procedure and tried to confront the Director, but all I got from his assistant Ms. Radmann was the information: "He won't be back at the Institute for two days". So, I initially vented my anger at the Director of the computer centre. Two days later, however, I had the opportunity to talk to Mr. Hasselmann. Still full of indignation, I entered his office and was welcomed by a smiling gentleman saying: "I must have been lucky that I wasn't in the office two days ago". This charmingly took the wind out of my sails. We were then able to clarify the matter (almost) peacefully. The library was saved, but for very different reasons.

The first scientific lecture I heard from Klaus Hasselmann was slightly disappointing. As I naturally couldn't understand much of the content, I focused on his presentation style and waited for a gripping performance by a professor. It was still in the days of projectors with acetate slides that had to be changed by hand. Professor Hasselmann replaced his slides so quickly that the audience must have felt dizzy. His flow of speech was similarly rapid and unclear. Did he actually speak English or German? Even some of the scientists probably had difficulties in following the lecture.

Following his retirement and with advances in the electronic supply of literature, I would occasionally receive emails with requests for a given article in PDF format. I was glad to be able to send the requested texts quickly. The full texts were often sent as breakfast reading to Sylt, to Glückstadt or other places. Prior to digitisation however, one request reached me scribbled on a beer mat after he had attended a conference. He was obviously in the "service of science" at all times.

At some point during the first years, he promised me that he would never donate his special print collection to the library. It would be useless for others and is organized in a rather personal manner. That calmed me down considerably, as the days of special print collections seemed to be over. But now in 2021, with its help, I was actually able to verify a few analogue sources for the bibliography of this book. They could not be found using current digital research tools. So, in the end, the collection was very helpful after all.

I have never regretted spending all of my working years at the Institute of which Professor Hasselmann is the founding director, and even now I am always pleased when an email from him is waiting in the mailbox in the morning.

4.21 Gerbrand Komen

Memories

I first encountered Klaus in 1978 in Kiel, where he gave a lecture at a GATE symposium. I was late and had missed the introduction. I expected a typical German accent. So when I heard Klaus my first reaction was: this cannot be him. But it was.

A year later Willem de Voogt and I travelled to Hamburg to meet Klaus to discuss our joining the Sea Wave Modeling Project (SWAMP), an intercomparison project that Klaus had started. I vividly remember a subsequent meeting with other SWAMP-participants in the periphery of a wave conference in 1981 in Miami in which Klaus and Susanne both took part. They invited us to their hotel room, where we discussed progress whilst eating dinner from fast food boxes.

After I had presented our wave modelling work at the Miami conference Klaus invited me to spend a summer in Hamburg, which I did in 1983. That summer was quite remarkable. It was great working with Klaus and Susanne (see below), it was equally great to experience their wonderful hospitality. It was not so easy to find suitable accommodation for me and my family (wife + 2 kids). But then Klaus and Susanne let us stay with them in Kayhude, for several weeks. And when my family had returned to Holland they let me join them in their choir, the *Altonaer Singakademie,* for the weekly rehearsals and for a special concert trip. A black suit was obligatory, but I didn't have one with me. Fortunately, Klaus had a spare one, his wedding suit, which fitted me nicely. Highlights were performances in Mölln and Lübeck.

I have many precious memories of our frequent interactions during the 15 years or so following that summer. Too many to list, but a few come to the fore.

In 1985 we were at ECMWF with a team to set up the first version of our wave model. One of the staff members invited Klaus to an evening session of his bell ringing group. Klaus took all of us with him. The world of change ringing opened up for us.

The Wave Modelling (WAM) group held annual meetings, in different places. We worked hard, but often the local organiser would arrange a half-day trip, so we could relax and discuss waves in an informal setting. In 1993 we met at Sylt, the very place at which the JONSWAP-experiment was carried out in 1969. I remember our trip by boat to Hallig Hooge as having been most pleasurable.

Klaus was always very busy. Much of our work was done during travel or during leisure time outside official meetings. We would sit together for

discussions in places like the Wiener Stadtpark, or during concert breaks (I remember a performance of La Traviata, in Estonian, in Tallinn). Once we met in Copenhagen, in the lobby of his hotel, after he had given a lecture at an important climate meeting. He listened patiently to me and took his time. After we finished discussing ocean waves, I suggested we should relax over dinner, but Klaus declined. He was still full of energy, and wanted to work on his Metron Theory, a unified deterministic theory of fields and particles.

In 1993 I visited Luigi Cavaleri in Venice to work with him on the completion of our monograph on ocean waves. We worked through the weekends in an otherwise empty Palazzo Papadopoli, eating lunch from Luigi's desk in his office on the top floor and listening to Italian opera music in the background. There was a lot we needed to discuss with Klaus, but it was not easy to get hold of him, as he always had many commitments. However, we found out that he didn't mind us calling him on Sundays. So we had lengthy phone calls with him on Sunday mornings whilst looking out over the sunlit roofs of Venice.

Impact on my thinking

Before I met Klaus I had studied his work on the origin of slow climate variations. Klaus had used an analogy with Brownian motion to show that white noise can generate red noise in any system with different time scales. This is an important result because it means that there can be slow variations in the climate system without a cause.

When I actually worked with Klaus I was particularly inspired by his way of writing papers, proposals and minutes and the way in which he led meetings. Also influential was his vision on the development of an integrated wind and wave data assimilation system.

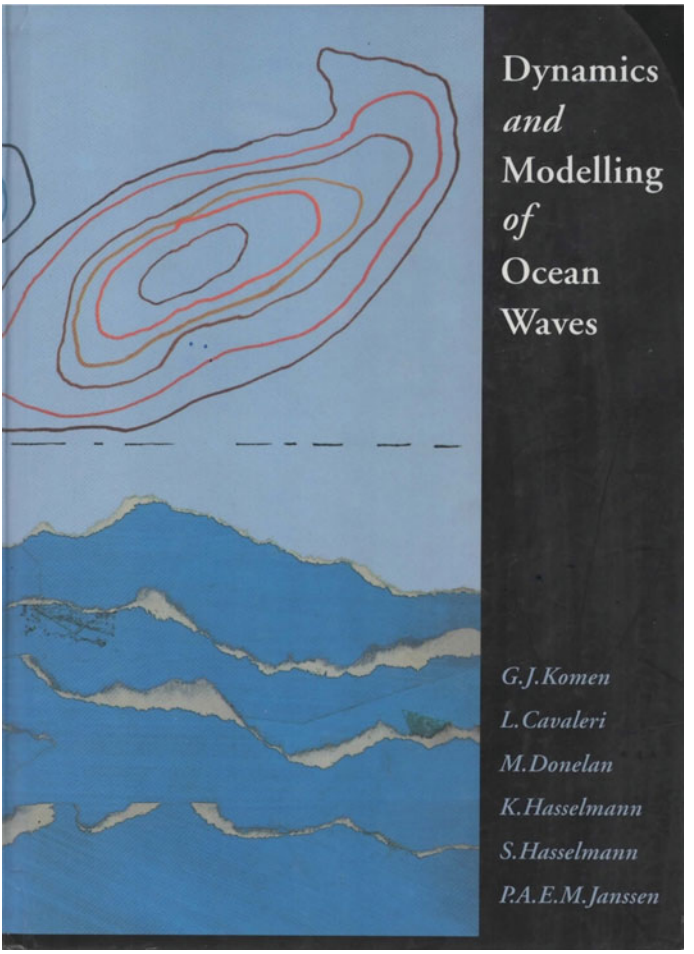
Impact on my career

Jan Sanders of my institute had developed GONO, a numerical model for predicting ocean waves in the North Sea. In 1978 I was charged with the further development. By taking part in SWAMP we were able to connect with the international wave modelling community. This was most stimulating and very fruitful.

My visit in Hamburg in 1983 allowed me to combine the best of my own institute's wave expertise with theoretical and numerical work carried out by Klaus and Susanne. We simulated fetch-limited growth, to see under which conditions a stationary solution can be reached. Comparing the result with

observations allowed us to determine an unknown constant in the dissipation source term. The resulting parametrization is still widely used.

In 1984 Klaus established the international wave modelling group (WAM), and he asked me to chair the group. That kept me busy for the next 10 years or so.



Dynamics and Modelling of Ocean Waves, G.J. Komen, L. Cavaleri, M. Donelan, K. Hasselmann, S. Hasselmann and P.A.E.M. Janssen, Plenum Press, New York & London, 532 pp, 1994

with Wave modelling Group, Sintra, Portugal, 1992

Presentation of the WAM book in De Bilt, 1994. Standing, from left: Klaus Hasselmann, Gerbrand Komen, Susanne Hasselmann, Luigi Cavaleri. Kneeling: Peter Janssen und Mark Donelan.

Klaus' legacy in ocean waves

Klaus' fundamental work on the energy balance equation, non-linear wave-wave interaction and wave dissipation in the 1960s helped provide the foundations for modern wave prediction. He then realised his ambitions by mobilising the international wave modelling community in a long sequence of projects: JONSWAP, MARSEN, SWAMP, NORSWAM, WAM, ECAWOM, resulting in the availability of routine wave observations from space, the development of a third-generation ocean wave model and its implementation in the forecasting system of ECMWF. The model and its descendants now run in many centres worldwide. Our knowledge was consolidated in a multi-authored monograph: *Dynamics and Modelling of Ocean Waves* (1994), which is still used as standard reference text for wind driven ocean (surface) waves.

4.22 Luigi Cavaleri: Writing the WAM Book

I was pleasantly surprised and thrilled when Gerbrand Komen approached me inviting to be a co-author of the planned WAM book [244]. I was not even a co-author of the already well known WAM paper; I was dealing mainly with practical problems and wave measurements in the sea, working, in a way, at the opposite end of Klaus et al., who, with the exception of Mark Donelan, were good (actually extremely good) in terms of their thinking and computer expertise, but who had little experience of a real stormy sea. At the end of the adventure (because an adventure it was), this turned out to be a good combination, joining the ones I considered as descending from the sky, and myself climbing my way up with a lot of effort. Of course, Klaus was the master mind behind it all with Gerbrand acting as the front man, working hard to overcome all the practical difficulties, dealing with the bureaucratic and personal aspects of each co-author.

It is amazing how frustrating it can be spending weeks or months writing and assembling a supposedly eloquent chapter only to see it scratched, cancelled, modified, or scribbled over by someone else. On the wings of my enthusiasm, I was running fast, collecting contributions, and seeing what I considered as "my chapter" growing more and more with what were nice pieces of work. At the end (but wait!, it was not the end) I ended up with more than 100 tightly written pages with a lot of figures summarising all the practical problems and successes associated with the direct application of wave modelling. I packaged it all up and sent it proudly to Gerbrand and

Klaus, and left Venice with my young daughter for our house in the mountains to spend a well-deserved Easter vacation there. It was Easter Sunday, and I had finished cooking our special lunch (I was obviously in very good mood) and ready to enjoy it with my daughter when the phone rang: it was Klaus. I was only able to say a single word: "Luigi?", "yes", and then the storm began. My chapter was a disaster, the worst thing he had ever seen, a completely useless work, everything had to be done again, it had been a stupid mistake to involve me in this task, I had spoiled all the efforts by the other co-authors, the organisation of the chapter was a mess etc. This went on one-way for 17 min. Klaus must have been extremely angry, and I was speechless. Without my having said another word, the phone was slammed down, and I saw the expression of my daughter staring at me with curious eyes.

I sat at the table looking blankly at the food, my mind running wildly from the chapter to Klaus. A few minutes later the phone rang again, and it was Gerbrand to whom Klaus had reported a "perhaps aggressive call" to Luigi. We talked, two-ways this time, for ten minutes, Gerbrand acting as the wise man of the group, soothing me, explaining what Klaus really meant, that it was not a major issue, that with a bit of effort we could achieve a beautiful (even for Klaus) result. At the end I forced myself to have some food, and then my young daughter and I went for a relaxing walk in the snow. Indeed, and in due course (not so long) things did settle, and the rearranged material ended up as this chapter of the WAM book.

As a matter of fact, apart from the pleasure and satisfaction of contributing to such a solid piece of scientific literature, the story had also a pleasant and musical ending. The book was officially presented at KNMI, in De Bilt, in the Netherlands. Authors, friends, colleagues, and others came together there, and each of the authors gave a short presentation of his or her contribution. I decided to do something different. Some weeks before, shuffling CDs in a music shop, I realised a remarkable fact and I bought a few CDs. To the amazement of the audience at the presentation at KNMI I did not talk about the work done or how important are waves in the ocean world. I shaped my presentation by illustrating the parallel between waves and music, and how Susanne, a valid pianist, had been the real inspiration to Klaus for the name WAM: because (and I switched on a tape recorder playing "Eine kleine Nachtmusik") the true meaning of WAM was Wolfgang Amadeus Mozart, and with the music playing I distributed the Mozart CDs to my co-authors.

Turning back to the WAM-book and in particular to this chapter , of course, Klaus's criticism (conveyed in a different form) was correct, and the final product was much improved. It was also a lesson for life. When one has

a specific target in mind, one should always aim for the best result, without unnecessary compromises. There can be fights, discussions, and clashing opinions. However, if these are done honestly and always in the name of the best science, and if people are open-minded, they will ultimately strengthen mutual friendships and respect and the final result will be better.

Many years later, when Klaus and Susanne were in Venice for one of our meetings, we had dinner at a restaurant together with a group of friends and colleagues. My daughter, now grown up, was there as well, sitting at my side, with Klaus and Susanne in front of us on the other side of the table. She remembered that Easter well and when I explained to her who the person in front of her was, she stared at him, and a smiling Klaus said, "that's the best way to become friends".

Of course, he was right again. I still shuffle through the WAM book with pleasure, remembering the effort, but mainly the, often non-linear, interactions that resulted in that product. Klaus is a great friend, and I hope to host Susanne and Klaus in Venice again and to enjoy an opera or a concert together at our beautiful "La Fenice" theatre, born again, as was science, better than before, from the ashes of a momentary decline.

4.23 Kristina Katsaros

I first met Klaus in 1972, when I was sent to his Institute as a postdoc by Joost Businger, whose group I had joined at the University of Washington after completing my Ph.D. Klaus had invited us to a planning meeting for a JONSWAP 2 experiment. I had brought my two and a half year old daughter along as I was planning to continue on to Sweden to see my mother, and Dieter and Hedi Hasselmann arranged for a babysitter. We were going to borrow an elegant 3-D sonic anemometer from Risø, Denmark and measure the momentum flux from a tripod tower in the North Sea, while others were measuring the wavefield in great detail.

The headquarters for the experiment was on the island of Sylt. Two young German Scientists, Jürgen Müller-Glewe and Eggert Clauss, were measuring similar properties of the air from the same tower. They were. We sailed off on the old Gauss research vessel, to install equipment—it was all very exciting for me and the other young scientists such as a graduate student named Thomas Hauf. The first problem we encountered was that the holes in our mounting plate did not match those of the tower's top plate, which had finally arrived, so we had to go back to the island to make adjustments. Finally, a bit late, we installed our equipment, but all the safety measures, such as buoys to

secure distance between tripos and the attending ship had not been arranged as there had been too little time. Just as we were gathering data, a storm came up and caused our attending ship drift, which broke my expensive Danish cable (our technician was on the tower at the time). Others, with their fancy wave devices, had arrays on the bottom of the sea, which were also completely destroyed, which ended the experiment.

Some days later we—i.e., about 20 of us—met in the conference room at University of Hamburg in a rather gloomy mood. Finally, being the Polyanna I am by nature, I raised my voice and said: "Klaus, it may not have been much of an experiment, but it sure was a nice experience". I got a lot of laughs, and jokes are usually not one of my strong points. I think Klaus was glad for the relief—he was already a celebrated theoretician, but the experimental difficulties had been too great this time.

Somehow, I think Klaus had an appreciation for my situation, often being the only woman in a gathering. I had been raised in Sweden where the natural sciences are emphasised in high school, so I hadn't realised that I was something of an oddity elsewhere. Klaus could see that scientific inquiry was important to me, although I think he also knew my limitations. He was definitely supportive over the years—I don't know how often he wrote letters proposing me for a promotion or an award. I was invited to join the Marine Remote Sensing (MARSEN) experiment organised by Klaus the late 1970s and we were quite successful in measuring wind stress and wave field. I co-authored a paper with a graduate student and another one on sea surface temperature (SST) measured by aircraft and ships with several colleagues, notably Armando Fiuza of Portugal and the German aircraft research group. Space-based remote sensing of SST was becoming a matter of routine, but our results of varying SST in the German Bight were new at the time.[13]

Much later, in 1992, I had taken on a new position at IFREMER, France, where we were handling scatterometer and Synthetic Aperture Radar data from the European Earth Research Satellite-1 (ERS-1). Klaus had been involved in the planning and was very anxious to get his hands on some data. So, I was in a good position to get him a tape of Synthetic Aperture Radar data.

About 9 months after my arrival, I wanted to convene a workshop to advance progress on these new data sources. My new colleagues thought it was too soon, but Klaus would be coming to the meeting, and it certainly added some shine to the planning and probably to my prestige and inspired everyone to work extra hard to be ready. Many colleagues from the USA also

[13] Katsaros, K.B., A. Fiuza, F. Sousa, and V. Amann: Sea Surface Temperature Patterns and Air-Sea Fluxes in the German Bight during MARSEN 1979, Phase 1. J. Geophys Res. 88, 9871–9882, 1983.

attended, as they were starved for new data of this sort. It was a great and wonderful help to me in this management job and led to some great collaborations and results. I'm sure my bosses in Paris were impressed. It was a great start to my 5 years at IFREMER.

I have hugely enjoyed this long friendship and support by Klaus and Susanne Hasselmann, who both always made me feel at home, even inviting me to lunch with them in the office (Susanne was good at keeping our Klaus well fed and healthy!). Their kindness has been very valuable, even if it often came to me from a great distance, as my main place of work was at University of Washington. Klaus' support was one of the aspects of my career that I really treasure. I consider him to have been an important mentor because he took me seriously and understood me.

4.24 Peter A.E.M. Janssen: Klaus F. Hasselmann—A Giant in Ocean Science

It is well-known that Klaus has had a considerable influence on several developments in various fields of science. I will focus on the field with which I am most familiar, namely that of ocean gravity waves, and I will show that his work has had far-reaching consequences not only for oceanographic applications but also for other fields in which non-linear phenomena play a role.

The history of ocean waves started in the early part of the nineteenth century with the contributions of Poisson and Cauchy who solved the linear initial value problem. This was followed by Stokes who obtained a series expansion for a single finite amplitude gravity wave where the nonlinear dispersion relation was obtained by means of the first application of the renormalisation method. In fact, Stokes renormalised acceleration of gravity to remove secular behaviour which assured convergence of the solution. At the end of the nineteenth century Korteweg and de Vries (KdV) derived solitary wave solutions of permanent shape for shallow water from the famous KdV equation and later, in the 1960's, it was shown by means of the inverse scattering transformation that these solitary waves were in fact stable entities, which were dubbed solitons. For a while, it was fairly quiet at the water wave front until Sverdrup and Munk, stimulated by the practical need for sea state information for landing operations during the second world war, developed the first ocean wave forecasting system.

Then, in the 1950's, a considerable acceleration of the pace of ocean wave research development occurred through the insightful work of Longuet-Higgins (Gaussian statistics), Pierson (introduction of the wave spectrum), Miles (wind input) and Phillips (resonant four wave interaction). Until then, most researchers had viewed ocean waves as essentially linear, but this view began to be challenged in Klaus Hasselmann's seminal work on the statistical theory of four-wave interactions. A number of important insights were put forward: a key role was played by the action density spectrum, the resonant four-wave interactions gave rise to irreversible changes in the spectrum, picking up energy and momentum provided by the wind from the region around twice the peak frequency and transferring it to higher and lower frequencies in such a way that the resonant transfer resulted in a downshift of the wave spectrum whilst the entropy of the wave system increased at the same time. This important work not only stimulated developments in oceanography but also in other fields of physics such as plasma physics where resonant three- and four-wave interactions have played an important role in trying to understand how to contain a plasma sufficiently long to enable the occurrence of nuclear fusion.

Returning to oceanography, all these new insights caused quite a commotion in a field which was notoriously conservative. These researchers not only required a lot of convincing, but there was clearly a need for collecting observations and studying experimental results in the light of the findings from nonlinear resonant interactions. But such tasks required a considerable amount of effort and expertise so it made sense to set up collaborations. One of the first collaborations resulted in the famous JONSWAP campaign which gave a tremendous boost to ocean wave forecasting.

At the same time, a new development emerged that had the potential to improve our knowledge on ocean waves and air-sea interaction: Satellite Oceanography. Klaus has played an important stimulating role in the development of a number of satellite instruments, such as Altimeters, which could determine wind speed and significant wave height, whilst the wave spectrum could be observed using the SAR. A novelty and major asset of the new generation of (European) Remote Sensing Satellites such as ERS-1, ERS-2,… was that it would be able to collect this data on a global scale providing an important stimulus for global wave modelling. This was one of the main reasons for the formation of the WAM group which would set up the software of a new third generation wave model based on the 'correct' physics and Klaus and Susanne played a key role in the development of the WAM model. I really admired the way that Klaus was able to convince two very diverse communities to collaborate. He would tell the Satellite people with a broad smile

that it was important to use the products of the wave modellers to improve the satellite observations, and would convince the wave modellers to assimilate the satellite products to improve the wave forecasts. At this early stage of development, both satellite products and ocean wave forecasts were of a fairly low quality so the overall impression this made was of a man lifting himself out of the swamp by his own bootstraps. But this type of interaction worked (!) and over the years it resulted in greatly improved wind and wave products from satellites and ocean wave forecasts. Over the past 25 years one day forecasts, error significant wave height, and wind speed has reduced by a factor of two. Satellite products have improved by a similar amount.

Finally, ocean wave forecasting results are quite sensitive to the quality of the surface wind fields, although ocean wave modellers tend not to emphasise this aspect of the wave forecasting problem. Klaus and Gerbrand Komen, therefore, approached the ECMWF, which was at the forefront of weather forecasting, and there has been a very active group of people responsible for the development of the WAM model and the data assimilation software since the mid-eighties. The wave model became part of the ECMWF's operational suite and a considerable amount of attention was paid to improving the quality of the surface winds. Since 1998 there has been a two-way interaction between wind and waves, which benefited wave height forecast results, surface winds, and geopotential height.

At times, Klaus and I also had some heated debates on esoteric fundamental issues in physics such as the corpuscular nature of light and matter (Klaus has an elegant explanation for this) and the role of the 'arrow-of-time' in statistical mechanics and irreversibility. The concept of an arrow-of-time has been made most popular by Prigogine and it indicates that entropy increases when going forward in time. This asymmetry gives rise to particular problems when one considers only resonant wave-wave interactions, but problems may be removed by introducing non-resonant interactions. This realisation suggested a mechanism for the generation of one-dimensional freak waves.

From this sketch of Klaus's work from my perspective it is clear that Klaus has played a pivotal role in the development of a reliable, high-quality ocean wave forecasting system. His efforts on the nonlinear transfer have also triggered numerous developments in other fields of physics.

I remember the many interactions I have had with Klaus with great pleasure. Most of them were rather formal, mainly because he was always working one way or the other. One exception was a discussion I had with him and Susanne at their home about our favourite football teams, in particular about the Dutch football eleven which in the seventies and eighties were famous for

their attractive, elegant play and for their innovative tactics ('total' football) that stimulated football all over the world. If I remember correctly, Klaus favoured Holland to win the 1974 world cup!

4.25 Ola M. Johannessen

I got to know Klaus in 1979 when we both used the JPL SAR flown in NASA CV-990 jet where Klaus used it in the international Maritime Remote Sensing Experiment, MARSEN, in the North Sea in October 1979 and I used it in September in the Norwegian Remote Sensing Experiment, NORSEX 79, in the ice edge region North of Svalbard. After these two major international remote sensing experiments, we began to develop the MIZEX programme as a follow up to the NORSEX 79 project, based on a workshop held at Voss in Norway in October 1980. Klaus was very helpful in getting the German Polar community involved in the MIZEX Programme which was headed by Professor Gotthilf Hempel, Director of the new Alfred Wegener Institute for Polar and Marine Research, which was founded in Bremerhaven in 1980. Professor Hempel agreed that their large new Icebreaker "Polarstern" could take part in the programme. When I held a Chair at the Naval Post Graduate School in Monterey in 1982, I spent a lot of my time drafting the research plan for the Marginal Ice Zone Experiment, which was scheduled the summers of 1983 and 1984, of course with a lot of input from my colleagues based on several earlier workshops. The research plan had to be approved by the MIZEX coordination committee of which Klaus was a member. He was very critical of the draft plan and restructured it, to the great benefit of the programme.

Actually, this MIZEX research plan was later reviewed by a group of eminent US scientists chaired by Professor Richard M. Goody at Harvard University in a meeting in Washington. I was very nervous about presenting it, but fortunately, by chance, Klaus was in Washington that day and took part in the review meeting. He basically told this eminent committee to relax and mentioned the fact that he himself had had some bad experiences with the pilot Wave Experiment, JONSWAP in 1968 (he called it a disaster) before the main experiment had been carried out successfully in 1969. Therefore, he argued, it would be a good idea to do the MIZEX 83 pilot project to sort problems out before the main summer experiment in 1984, which was a huge undertaking which included 7 ships, 8 remote sensing aircraft, 4 helicopters, and over 200 scientists and technicians. Thanks to Klaus' presence and support during this meeting, the review of the MIZEX Programme went well.

After the MIZEX 84 project, I collaborated with Klaus on many occasions. At the Nansen Center we started to become interested in global warming modelling in the 1990s, so I contacted Klaus for help. We were invited to come to his Institute to discuss this and Klaus was very generous as usual. We ended up of going home with a tape containing one of the Max Planck global ocean models for one of our Ph.D. students to implement on our computer. Klaus was also one of the panel members at the student's viva. So, thanks to Klaus, the Nansen Center in Bergen became involved in the field of global modelling.

Both Klaus and I knew Walter Munk very well. Walter had pioneered the field of Acoustic Thermometry of Ocean Climate (ATOC), which was used to measure average temperatures on the basin scale. It has already been shown in 1995 that acoustic transmission across the Arctic Ocean was feasible. This inspired Klaus and me to launch the Acoustic Monitoring of the Ocean Climate in the Arctic modelling project to project what could happen up to 2050 using the Max-Planck ocean model as input for several acoustic models. The result was that acoustic monitoring could be a very useful method to be used in future for monitoring the basin scale temperature in the Arctic Ocean [153]. This is actually now underway in a joint observation programme between the Nansen Center and Scripps Institution of Oceanography.

In 2004, our team, which included Klaus, published the fact in *Tellus* that most of the Ice in the Arctic Ocean would melt during summertime under a doubling of the CO_2, probably before the end of 2100 with much less effect in the winter. This assertion was based on findings made with the Max Planck ECHAM4 global coupled model [158]. This paper led to a lot of subsequent papers about the future of Arctic sea ice.

The European Climate Forum was founded in 2001 with Klaus and Carlo Jaeger as Co-Chairmen. Fortunately, Klaus invited me to be one of the founding members. As a result of the many meetings and workshops, I was able to widen my perspective and knowledge of climate impacts. A group headed by Klaus of which I was a member published the important paper "The Challenge of Long-Term Climate Change" [155] in *Science*, which included projections up to the year 3000. Few papers take in such a long-term perspective.

Klaus and Susanna visited us several times at the Nansen Centers in Bergen and St. Petersburg. In the course of several Nansen Lectures, he introduced us to the important topic of the climate-economy and he launched a programme on this topic with D. Kovalevsky at the Nansen Center in St. Petersburg.

Klaus is my hero in science as well as a very good friend.

4.26 Lennart Bengtsson

I became director of the ECMWF (European Centre for Medium-Range Weather Forecasts) in 1981 after having been involved with the Centre right from its early planning phase. Part of the Centre's remit was to make some of its computer resources available to atmospheric scientists from its member states. Preference was given to projects that were beneficial in terms of the scientific and operational objectives of the ECMWF.

One of my ambitions as director was to try to widen the somewhat limited objectives of the ECMWF to weather predictions in the range 4–10 days. Such ambitions included extending the predictions and to add interesting new products of value for users. Scientifically, I was fascinated by the possibility of using comprehensive models to understand the Earth´s climate system and using all possible ways of doing this as an additional task. This was a time when the Global Weather Experiment had just been completed successfully and a serious research effort was launched to gain a better understanding of the climate system. I was initially not particularly interested in climate change issues as I considered this was a bit premature as the models and data were in my view not yet good enough for use in this context. However, there was now a global observation system in operation, methods for assimilating and analysing global data as well as ever more powerful computers that made it possible to undertake realistic climate simulation studies.

Klaus Hasselmann was the head of a European group that regularly visited the ECMWF to develop a forecasting system for wave prediction. To predict the state of the sea and in particular waves was an important task for the meteorological marine services. They had put simple systems into operation that used empirical relations coupled to surface wind speed. The strategy adopted by Klaus' group was to develop a comprehensive approach including the full spectrum of sea waves including the non-linear interaction between the waves. I found this to be a splendid idea that would fit perfectly into a potentially operational task for the ECMWF. This required considerable political efforts as there were certain member states that strongly believed that wave prediction was the task of the individual meteorological services and not a European agency. However, with the support of Klaus and some other leading European scientists, wave prediction based upon Klaus' ideas has now been an important operational task for the ECMWF for many years and some members of Klaus' group later 'went on to join the ECMWF.

Another area in which I had the great pleasure of collaborating with Klaus was at the European Space Agency, ESA, and in the planning of ENVISAT, a

major initiative concerning a satellite system devoted to both weather prediction and climate research and monitoring. Klaus and I were members of the planning group for ENVISAT so I had the pleasure of seeing Klaus in action. That was all to my liking. ENVISAT was a complex and very ambitious project, and it took many years before it was finally launched in 2002 almost two decades after the early planning phase. ENVISAT provided very important data for weather prediction and climate monitoring which are crucial in monitoring climate change processes.

After a few years as Director at the ECMWF I proposed a long-term strategy to broaden our objective to include extending weather forecasts beyond the medium range as well as a major extension of model-based experimentation and systematic monitoring of the Earth´s climate system. I invited a number of leading scientists including Klaus Hasselmann to provide advice on such a strategy. However, the ECMWF Council was not very pleased, as they had not expected such a wide-ranging initiative. The fact that I had produced a plan in colour was seen as being additionally questionable. I was asked to repeat the exercise with help of the Centre's scientific and technical committees and this time in black and white. I realised that even as a director of an international organisation one's initiatives were still subject to certain limits. Had Klaus been a member of the ECMWF Council I might have succeeded at the first attempt. In the end, and after some years of hard work, I got the strategic plan more or less through. However, I also realised that I would probably fit better in a truly scientific environment. During the whole process I had formed a close friendship with Klaus. A few years later he proposed me as a co-director at the Max Planck Institute in Hamburg, which was later approved by the Max Planck Society. I later accepted the offer, which led to a long and very creative collaboration with Klaus with whom I had discussions almost on a daily basis throughout my time in Hamburg. After many years involvement in operational weather prediction, I was really glad to be back in a true scientific environment once again. I am particularly grateful for the fine scientific collaboration with Klaus over a very long period of time.

4.27 Jürgen Sündermann: Klaus Hasselmann—Colleague and Friend

It was around 1966, when Klaus was thirty-five and I seven years younger, that I first became aware of this rising star in the field of (geo)physics. In a lecture on the large-scale propagation of wind waves in the Pacific given by

Walter Munk in the Auditorium Maximum of the University of Hamburg, he mentioned the significant contribution made by a certain Klaus H. in a recent ocean experiment. That same year I took part in the 2nd Oceanographic World Congress in Moscow and heard an excellent talk given by Klaus as Invited Speaker. A short time later, Klaus, who had relocated to Hamburg by then, was invited to give a general lecture to a public audience at the hotel "Atlantic". This performance wasn't quite as successful, but Klaus was well able to withstand this "baptism of fire" and went on to become the founding director of the new Max Planck Institute for Meteorology (MPI). We became acquainted with each other. As a young assistant in the Institute of Oceanography at the University of Hamburg, I attended his lectures on sea waves and appreciated his inspiring habilitation lecture—a kind of quantum theory in hydrodynamics—among an audience of skeptical classical physicists.

During the following years and decades, he played a significant role in the development and growth of both theoretical and experimental ocean and climate research in Hamburg. This was based on his scientific creativity and on the interdisciplinary collaboration that he inspired and fostered. He was instrumental in overcoming the limitations of the old-fashioned traditional university and in motivating young people from the fields of physics, chemistry, biology, and engineering to work together on new projects. He introduced research structures which integrated the already existing high scientific and logistic potential in Hamburg. He was a key initiator of newly developing model systems for climate simulation and large interdisciplinary field experiments in the North Sea such as JONSWAP (Joint North Sea Wave Project) and FLEX (Fladen Ground Experiment)—to name some highlights.

I was appointed as director of the University of Hamburg's Institute of Oceanography in 1978. Together with Klaus and his MPI colleagues Hans Hinzpeter and Hartmut Graßl—forming what we called the "Gang of Four"—the opportunity arose to greatly strengthen the profile of marine and climate research in Hamburg. Important milestones were the long-term and well-financed Special Research Units (Sonderforschungsbereiche) of the German Research Foundation, the acquisition of the research vessel "Valdivia", the foundation of the German Climate Computing Center (DKRZ), the setting-up of new permanent research units at the university such as "Biogeochemistry" and "Sustainability and Global Change", the foundation of the Climate Service Center Germany, and the establishment of two climate-related Max Planck Research Schools. The next logical step was the formation of a joint research structure: the Center of Marine and Atmospheric Sciences (ZMAW), which included a new common building for the University Institute of Oceanography and the Max Planck Institute for Meteorology. This

success was essentially based on the common conception of research priorities and their practical realisation. We exchanged scientists between the institutes, united our libraries, and designed a joint logo and a common email address. To emphasise and accelerate our efforts to concentrate the working groups from both institutes in a new building, Klaus and I even arranged an appointment with the mayor of Hamburg in the Town Hall. We finally got the present joint residence.

North Sea studies, Sylt 2013

Our scientific work together certainly gained from a warm personal understanding and from social events such football competitions and carnival parties. Last, but not least, our private cycle tours and concert visits together with our wives, should be mentioned. Yes, the professional and private friendship with Klaus has certainly enriched my life.

4.28 Klaus Fraedrich: The 1976 Paper on Stochastic Climate Models

"When the institute (MPI-M) was created, I had two goals. One was understanding the origin of the natural variability of climate. This was not understood at all, but was clearly a key issue if we wished to distinguish between natural climate variability and human made climate change. I had just developed my stochastic model of climate variability, so I could build on that work as a starting point " (see Interview in Section IIa):

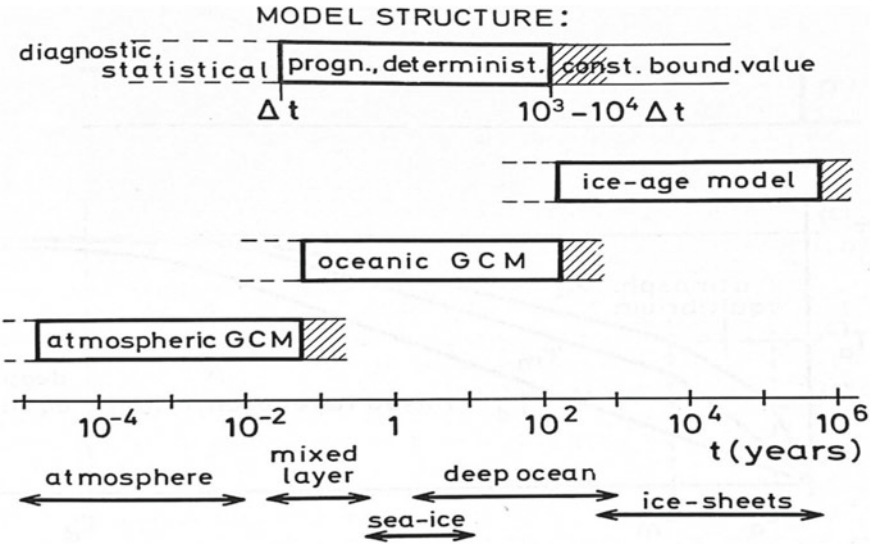
… while Joseph Egger (Munich) employed Hasselmann's Brownian motion analogue to a low-order large scale atmospheric circulation model (1981), which is based on the Jule Charney multiple equilibrium theory of blocking (1979),

… and I (Berlin) incorporated this stochastic noise Ansatz to a low-order climate model (1979) to introduce catastrophes (now: tipping points) and resilience. Both, Jule Charney and I, after participating in the 1975 IIASA-workshop 'Analysis and Computation of Equilibria and Regions of Stability' (H.R. Grümm, Editor), have applied this workshop's new ideas on dynamics in chemistry, climatology, ecology, and economics to their own fields of interest at that time.

This archive photo (below) may also document the fast spread of Klaus Hasselmann's novel approach within the German meteorological community outside of Hamburg. And here it is acknowledged by a private toast on his inspiring idea at the 1978 Berlin international conference on 'Man's Impact on Climate'.

As to the second goal it appears that Klaus has come closer to achieving it: "We needed a good coupled atmosphere–ocean model, but we had no global ocean circulation model of comparable quality to the available global atmospheric circulation models." He introduced this goal in Berlin, setting it out as a conceptual sketch, which has since entered lecture notes introducing the climate prediction and the predictability problem in classes. A seamless prediction which, in those days, has not yet occurred on the horizon, is included here simply by broadening the band width spanning the prognostic-deterministic climate models. Those are just two of the so many highlights of Klaus Hasselmann's achievements, which have stimulated not only us but also a large number of our colleagues to follow various initiatives; and they motivated us to continue on the arduous and cumbersome pursuit of our own goals. These, after all, are open ended.

A toast with Klaus Hasselmann on his landmark 1976-paper



Natural time scales of the climate system and the time-scale band width associated with prognostic-deterministic models.

4.29 Udo Simonis: Klaus, the PIK and Me

The reunification of Germany led to a number of significant scientific innovations accompanied by active collaboration and growing friendship among the scientists involved. In early 1991, the Federal Ministry of Research decided to establish an institute for climate research in Brandenburg under the auspices of the Leibniz Society. The concept for this had been developed by environmentally conscious ministry officials. It was then reviewed by the Science Council in July and—with a significantly reduced scope—recommended for implementation.

The first meeting of a ten-member founding committee (the later Board of Trustees) was held in October 1991, and included Klaus Hasselmann, Director of the Max Planck Institute for Meteorology Hamburg (MPI) and Udo E. Simonis, Director of the International Institute for Environment and Society of the Berlin Social Science Center (WZB). Some issues were quickly agreed upon: Hasselmann was chosen as chairman of the committee, the residence of the new institute was to be the city of Potsdam, and the special location was to be the Telegraphenberg, which is significant in the history of science. A longer, controversial debate began about other questions: What should the special task of the new institute be, what should it be called, and who should be its director?

The MPI was generally considered the incarnation of environmental knowledge and first address for everything related to climate. Klaus, too, was basically of the opinion that he already knew everything about the climate problem, only that more knowledge needed to be generated about the consequences of climate change for the economy, society, and nature. So, there couldn't just be another traditional institute for climate research; it had to carry out climate impact research, and concern itself with climate policy.

At that time, I had no real idea of the dramatic situation regarding the climate, but I did have some experience with the difficulties of formulating and substantiating consistent international environmental policy: I had coined the term "Weltumweltpolitik" (for world environmental policy) at the WZB. Everybody knows that a person less experienced in the field in question can really annoy the expert in the field, but only a few are aware that he can also animate the expert to learn to think differently. I only had to adjust to the relatively precise natural sciences, but Klaus had to get involved with the diverse, occasionally diffuse social sciences. It became a mutual learning process characterised by an increasing respect for one another and a growing genuine friendship.

This learning process had been facilitated when a ten-member "International Scientific Advisory Board (SAB)" was appointed in February 1994, and I became—and remained for eight years—its chairman. The SAB met frequently and usually drafted very detailed minutes, which the PIK Board of Trustees then had to discuss. A recurring dictum appeared in many of these advisory board minutes: the call for a good balance between the natural and social sciences and interactions between the respective practitioners carried out in good faith.

This permanent demand was based on the insight of the American geographer Gilbert F. White, who had formulated it in anticipation of the Anthropocene era as follows: "The future of the globe's interlocking natural and social systems might depend more on human behaviour than on the further investigation of natural processes." Another postulate was also repeatedly called for by the SAB on suitable occasions: "Your work should be theoretically demanding, empirically relevant, and done at the right time".

Whilst the realisation of the second postulate can be considered to have been accomplished well at the PIK, the first one is a task that remains outstanding. However, much work has been and is being done to address this issue. In addition to important natural scientists, significant social scientists were invited to the institute; in addition to the training of young natural scientists, young social scientists were actively promoted; in addition to men, a particularly large number of women were recruited and, what may well be the most important thing, everyone learned to collaborate constructively and to communicate effectively.

In 1992, the year of its founding, the PIK had just 39 employees, 8 of them in the administration; in 2012, twenty years later, the ratio was 340 to 11—a significant indicator of the institute's successful development, but also of efficiency of the institute's administration.

After these 20 years, however, our relationship was by no means at an end. When one's years of membership in the board of trustees and in the advisory board were over, there was first a proper farewell party with the appointment as "Honorary member of the PIK". This immediately gave rise to a new idea: when it was time for Klaus and Udo to leave, we needed more external supporters, because a successful institute not only has internal friends, but also external enviers and opponents.

According to German law, seven members are needed to establish an association; they were quickly at hand and so the "Association of Friends and Supporters of the PIK" was founded in 2002. I was elected chairman and Klaus was elected vice-chairman of the association. In the following years, we regularly held annual meetings, organised numerous award ceremonies for

institute staff and ran events to increase public empathy for the institute. We transferred the chairmanship of the association into other hands in 2016.

For both Klaus and me, the following years were years of reflection and relaxation, but also and especially of joy at the birth and development of a "common child" that had become known worldwide in a relatively short period of time—the "Potsdam Institute for Climate Impact Research (PIK)".

4.30 Hartmut Graßl: Klaus Hasselmann as Creator of Science Infrastructure

Scientists should not only create new knowledge leading to the well-being of humankind, but should also help to improve the conditions to create new knowledge. In order to achieve that we need to convince politicians to invest in science infrastructure. Here I report about only two out of many of Klaus Hasselmann's initiatives to enhance the infrastructure for science, because I could directly observe it. Finally, I mention a science highlight to which the entire Max Planck Institute for Meteorology contributed.

The German Climate Computing Centre

When the Federal Research Ministry's expert panel on "Basic Climatological Research" (Sachverständigenkreis Klimatologische Grundlagenforschung) met in Bonn in 1987, the head of unit for climate, Dr. Irmhild Tannhäuser, approached Klaus Hasselmann prior to the start of the meeting with a surprising message: "Herr Hasselmann, I have found 18 Million Deutschmarks in the Marine Technology budget line earmarked for this year, which cannot be spent this year. Now you could start your long-desired climate computing centre in Hamburg". As chairman of this expert panel I proposed to Klaus to add an item to our agenda called "Discussion about a German Climate Computing Centre". When discussing this agenda item two out of eight members of the panel, from southern Germany, argued for the installation at their research centres. After a long and partly controversial debate I asked for a voting (having realized that a majority for Hamburg is probable), which then went positive for Hamburg. Hence, a small expert panel at the Ministry for Research and Technology has decided to bring this major infrastructure element as close as possible to the MPI for Meteorology institute. Klaus has determined the fate of both institutions at least until the end of 1999, when he had to retire. The expert panel on Basic Climatological Research became—after an initiative of the Bavarian Prime Minister

Franz Josef Strauß in the second chamber of our parliament (Bundesrat) in November 1987—in 1988 the Scientific Advisory Council on Climate of the German Federal Government. We became also members of this council. Klaus' next initiative for new research infrastructure followed soon.

Potsdam Institute for Climate Impact Research (PIK)

This Advisory Council to the Federal German Government recommended, following Klaus' repeated wish in its meetings in 1988 and 1989: Establishment of an Institute for Climate Impact Research in Germany. Klaus' main argument has been: Natural science based climate research has reached a high international level in Germany, but we need an internationally competitive climate impact research institution as well, which also should answer socio-economic research questions related to anthropogenic climate change. The Federal Government accepted the council's recommendation benevolently. When the Berlin wall fell, it became obvious that an institute for climate impact research had to be established in east Germany. In January 1992 the PIK started with its founding director Hans Joachim Schellnhuber.

At the end of my report on joint activities with Klaus Hasselmann I remember a scientific activity of all groups at the MPI for Meteorology, which had as its basis the coupled climate model development in Klaus Hasselmann's group.

Climate change as a consequence of a war

During a meeting in Bonn in early 1991 the federal research minister Riesenhuber approached me and proposed: The Max Planck Institute for Meteorology in Hamburg should assess the climate consequences of the burning oil wells in Kuwait after the attack by Iraq. Our institute, equipped with the only coupled atmosphere/ocean-model in Europe, at that time, would be worldwide the only one to perform these calculations, because it is forbidden to publish such model results for our colleagues in the USA during the Gulf war. Back in Hamburg I learned that Klaus had also been asked and in three weeks the coupled model results were ready to be submitted to Nature [97]. Klaus Hasselmann's coworker Mojib Latif had the task to ask daily all participants in all three departments of the institute about progress and problems. For example, in my group we had a simple but serious calculation error in the amount of solar radiation absorbed by soot (black carbon). The key model result: There is only a small regional cooling around Kuwait and the soot is mostly wet deposited in Asian mountain ranges and East Africa. After all the "doomsday" messages by other groups in the media the

echo to our results in the public was modest. Later we learned that the model results had even overestimated the observed cooling at the surface. However, the exaggerated climate change estimates caused by the fires published in the media by other scientists fell silent.