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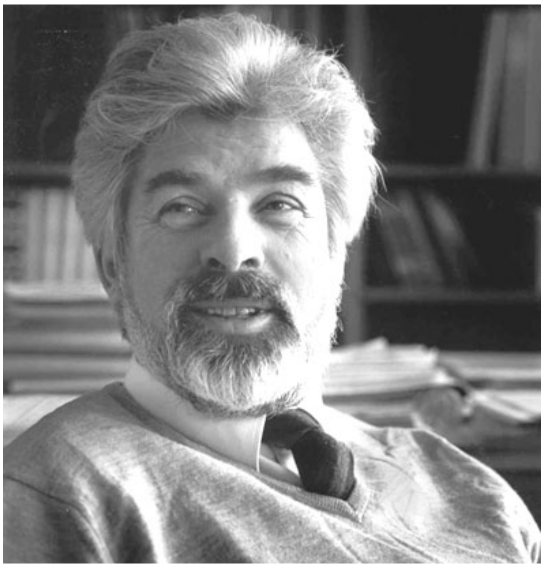
Interview mit Klaus Hasselmann am 15. Februar 2006



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Vorwort

Es gehört zu den Prinzipien der Max-Planck-Gesellschaft, neue Wissenschaftsgebiete, die noch nicht an den Universitäten etabliert sind, aufzugreifen. Die Gründung des Max-Planck-Instituts für Meteorologie ist hierfür ein gutes Beispiel. Zu Beginn der siebziger Jahre war die Klimaforschung ein neues Wissenschaftsgebiet, das in Deutschland weder an der Hochschule noch überhaupt nachhaltig betrieben wurde. Für die Max-Planck-Gesellschaft ergab sich im Jahre 1974 Anlass, sich mit dieser Frage zu beschäftigen. Neben anderen Einrichtungen war auch die Max-Planck-Gesellschaft gefragt worden ob sie bereit sei, das Fraunhofer Institut für Radiometeorologie und Maritime Meteorologie in ihre Trägerschaft zu übernehmen, nachdem der bisherige Leiter, Prof. Karl Brooks gestorben war.

Als damaliger Präsident der Max-Planck-Gesellschaft holte ich mir zunächst Rat bei zwei Wissenschaftlern ein. Der eine war Prof. Hermann Flohn, den ich in Bonn besuchte. Er hatte damals einen Artikel über Klimaschwankungen geschrieben. Der andere war Prof. Bert Bolin in Stockholm, den ich an einem Wochenende besuchte. Ich kannte ihn aus meiner Zeit bei der ESRO. Von diesen beiden Ratgebern wurde mir damals deutlich gemacht, welche Bedeutung die Klimaforschung in Zukunft haben wird. Auch die Gremien in der Max-Planck-Gesellschaft konnte ich überzeugen, während die Politiker damals noch gar nicht besonders interessiert waren.

Die Max-Planck-Gesellschaft ist jedoch nur dann bereit ein Institut zu gründen, wenn für seine Leitung ein international anerkannter Wissenschaftler zur Verfügung steht. Von meinen beiden damaligen Ratgebern Hermann Flohn und Bert Bolin war mir Klaus Hasselmann empfohlen worden. So suchte ich ihn in Hamburg auf, bevor die offizielle Berufungsprozedur in der MPG in Gang gesetzt wurde.

Klaus Hasselmann hätte ich schon Mitte der fünfziger Jahre kennen lernen können, denn in dieser Zeit arbeitete er an seiner Doktorarbeit in Göttingen und rechnete an der ersten elektronischen Rechenmaschine im Max-Planck-Institut für Physik, an dem ich tätig war. Aber der Zufall hat uns zuerst im Jahre 1970 zusammengeführt, als ich einen Vortrag von ihm im Hotel Atlantic in Hamburg hörte. Von diesem berichtet er ja in seinem Interview. Wie er selbst sagt: „Der Vortrag war schlicht eine Katastrophe“. Aber das hat mich nicht davon abgehalten, ihn in Hamburg in seinem professoralen Arbeitszimmer der Universität zu besuchen. Er stellte sich vor, während ich sagte: „Wir kennen uns schon“. Als ich ihm unsere erste Begegnung schilderte, sagte er: „Erinnern Sie mich nicht an die dunkelste Stunde meines Lebens“.

Er wurde als erster Direktor berufen und er hat es in großartiger Weise aufgebaut und geprägt. Als zweiter Direktor stand ihm Hans Hinzpeter zur Seite.

Die klassischen Meteorologen und ebenso die Verantwortlichen des Deutschen Wetterdienstes standen damals der Institutsgründung mit dem Namen „Meteorologie“ sehr skeptisch gegenüber. Denn Klimaforschung hielten sie damals weder für interessant noch für zukunftsträchtig.

Aus kleinen Anfängen im Jahre 1975 ist in Hamburg ein international anerkannter Schwerpunkt für Klimaforschung entstanden, der jetzt auch die volle Zustimmung aller Meteorologen gefunden hat.

Ich bin Herrn Hasselmann sehr dankbar, dass ich am Ende meiner Tätigkeit als Generaldirektor der European Space Agency (ESA) hier an diesem Institut einen Arbeitsplatz bekommen konnte, da ich nicht nach München zurück kehrte, sondern meinen Wohnsitz in Hamburg nahm. Wenn ich mich auch nicht aktiv an der Arbeit des Instituts beteiligt habe, so habe ich doch eine Menge von der Klimaforschung gelernt. Dazu haben vor allem die Gespräche mit Klaus Hasselmann beigetragen.

So fühle ich mich an dem Institut sehr geborgen. Dabei sind Klaus Hasselmann und ich uns auch noch räumlich näher gekommen. Denn als Emeriti haben wir seit einigen Jahren ein gemeinsames Sekretariat. Ich hoffe, die Verbindung zu ihm bleibt auch bestehen, wenn er jetzt seinen Wohnsitz nach München verlegt, einer Stadt, in der ich fast dreißig Jahre gelebt habe.

Reimar Lüst, Hamburg, 6. September 2006

Acknowledgements

Thanks to Ilona Liesner, Beate Gardeike and Andrea Bleyer for the truly difficult task to transform a lively discussion on tape into something readable on paper. Various individuals have helped to improve on details, such as Carl Wunsch or Gerbrand Komen.

Curriculum vitae

25. October 1931: Born in Hamburg

1934: Emigrated to England with family

1936 – 1949. Elementary and Grammar School (High School) in Welwyn Garden City, Herts., England

July 1949. Final High School Exam (Cambridge Higher School Certificate)

Aug. 1949: Return to Hamburg with family

Sept. 1949 – April 1950 : Practical course in Mechanical Engineering, Menck und Hambrock, Hamburg

May 1950 – July 1955 : Study of Physics and Mathematics at the University of Hamburg

Nov. 1952: Pre-Diplom Exam

July 1955: Diplom Exam (Diplom thesis on Turbulence, advisor: Professor K. Wieghardt)

Nov. 1955 – July 1957: Study of Physics and Fluid Dynamics at the University of Göttingen and the Max-Planck-Institute of Fluid Dynamics

July 1957: PhD, University of Göttingen (Professor W. Tollmien)

Aug. 1957: Marriage to Susanne Barthe

Aug. 1957 – Oct. 1961: Research Assistant to Professor K. Wieghardt at the Institute of Naval Architecture at the University of Hamburg

Oct. 1961 – October 1964: Assistant, then Associate Professor at the Institute for Geophysics and Planetary Physics and Scripps Institution of Oceanography, University of California, La Jolla, USA

Feb. 1963: Habilitation in Hamburg

Nov. 1964 – Nov. 1966: Lecturer at the University of Hamburg

Nov. 1966 – Feb. 1969: Professor at the University of Hamburg (leave of absence Sept 1967 – Feb 1968)

Sept 1967 – Feb 1968: Visiting Fellow, University College, Cambridge University

Feb. 1969 – Sept – 1972: Department Director and Professor at the University of Hamburg (leave of absence, July 1970 – July 1972)

July 1970 – July 1972 : Doherty Professor, Woods Hole Oceanographic Institution, Woods Hole, Mass., USA

Sept. 1972 – Jan. 1975: Full Professor for Theoretical Geophysics, Managing Director, Institute of Geophysics at the University of Hamburg

Feb. 1975 – Nov. 1999: Director of the Max-Planck-Institute of Meteorology , Hamburg

Jan. 1988 – Nov. 1999: Scientific Director at the German Climate Computer Centre, Hamburg

Nov. 1999: Emeritus

Awards

Jan. 1963: Carl Christiansen Commemorative Award

April 1964: James B. Macelwane Award of the American Geophysical Union

Nov. 1970: Academic Award for Physics from the Academy of Sciences in Göttingen

Jan. 1971: Sverdrup Medal of the American Meteorological Union

Dec. 1981: Belfotop-Eurosense Award of the Remote Sensing Society

April 1990: Robertson Memorial Lecture Award of the US National Academy of Sciences

Sept. 1990: Förderpreis für die Europäische Wissenschaft of the Körber-Stiftung,Hamburg

June 1993: Nansen Polar Bear Award, Bergen, Norway

December 1994: Oceanography Award sponsored by the Society for Underwater Technology, Portland, UK

March 1996: Oceanology International Lifetime Achievement Award

October 1996: Premio Italgas per la Ricerca e L'Innovazione 1996

May 1997: Symons Memorial Medal of the Royal Meteorological Society

November 1998: Umweltpreis 1998 der Deutschen Bundesstiftung Umwelt

May 1999: Karl-Küpfmüller-Ring der Technischen Universität Darmstadt

July 2000: Dr. honoris causa, University of East Anglia

April 2002: Vilhelm Bjerknes Medal of the European Geophysical Society

Nov. 2005: Goldmedaille der Universität Alcala, Spanien.

Question: How did you become interested in physics?

Hasselmann: One of my early experiences which kindled my interest in physics was buying a crystal detector from a school friend for two shillings and six pence – half a crown – or about the price of a movie ticket. I must have been about 13 years old. I was quite impressed that even without plugging the device into a socket, I could listen to wonderful music through the earphones. I wanted to better understand the puzzling phenomenon that you could get something from nothing. I went to the town library in order to find out in books on physics for beginners how electricity and radios work. That was my introduction to physics. At that time, it was an exciting experience for me, completely independent of the fact that I was taught physics in school. I did not see any connection between our physics lessons in school and my personal learning from the books in the library – I think this experience of personal learning and discovery was very important for me.

Figure 1: A critical inspection of the older sister Almut. Hamburg, shortly before leaving for England in 1934.

We have just heard that the detector had cost half a crown – so you did not attend school in Germany but in England. How did that come about?

Hasselmann: When I was close to three years old my family – my parents and older sister – emigrated to England. My father was a social democrat and did not want to stay in Germany in 1934. Our family moved into a so-called community, consisting mostly of Jewish emigrants from Germany. The English Quakers helped us a lot in those days. Until we returned to Hamburg in 1949, we lived in a very nice small town, Welwyn Garden City, 30 km north of London. I passed my A-levels there (then called Higher School Certificate). I felt very happy in England. So, English is in effect my first language.

Figure 2: In Welwyn Garden City, England, shortly before leaving for Hamburg, 1949.

Hasselmann: I studied in Hamburg. I did a half year practical training in a machine factory first, because I was not sure whether I wanted to study engineering or physics. In addition, I was not yet at home living in Germany – neither were my parents, in fact, because Germany had changed. So I had to find my feet first. When I started studying, the idea of having to work hard for my studies was also a new experience. So I fell back a little during the first year. I had doubts whether I really was talented enough to continue with my studies, so – as a test – I took a study exam (Fleißprüfung), which I passed, and so I continued. I did not regret that period of adaptation, but it was a drastic change between my English school days spent in a healthy, suburban garden town north of London and living in Hamburg, where everything was bombed to ruins. However, I had always wanted to go back to Germany to explore my roots. My parents were always patriotic, in a natural, pre–nazi sense. But I was always very happy in England and did not really experience any difficulties due to my German origin, not even during the war. Still, I wanted to find out where I belonged. In spite of the difficult period of adaptation during the first one or two years, I did not regret returning to Germany.

Did you study only in Hamburg?

Hasselmann: I studied in Hamburg for eleven semesters until I obtained my diploma in physics, in the summer of 1955, with mathematics as a second subject. Then I obtained my PhD at the Max Planck Insitute of Fluid Dynamics and Göttingen University from 1955 until 1957. Afterwards, I returned to Hamburg, where I spent three years as a post-doc working with my former diploma supervisor, Prof. Karl Wieghardt, at the Institute for Naval Architecture, before going to America in 1961.

Would you like to recount the theme of your diploma thesis?

Hasselmann: In my diploma thesis I worked on isotropic turbulence and found an – in my opinion – slightly more elegant derivation for the basic dynamic equations for isotropic turbulence [1]. For my doctoral thesis I changed subject to study the propagation of so-called von Schmidt head waves, elastic waves at the boundaries between two solid objects. In Hamburg I returned again to fluid dynamics research, mostly to experimental work on turbulence in ship wakes, using hot-wire instruments in a wind tunnel and a towing tank. But I also continued working on turbulence theory.

This did not correspond to the mainstream of education in physics. Were not atomic theory and nuclear research considered the normal case in physics already in those days?

Hasselmann: Yes, that was the mainstream, but I wanted to work in an area in which I thought I would be able to contribute something. I always had a practical bent, I wanted to work on problems which I thought I would be able to solve. I did not want to work on abstract, theoretical problems, and I did not have enough self-confidence to think I could make significant contributions to such difficult fields as general relativity or quantum field theory. So I went into fluid dynamics. I was always interested in the way planes and rockets worked. I liked my field of work, and I only gradually drifted into oceanography, meteorology and climate research. Later, I did then become interested in quantum field theory, elementary particle physics and general relativity, through my work on nonlinear interactions in geophysical wave fields, starting from ocean waves. I pursued these investigations for many years in parallel to my regular research, so to speak as a private hobby. However, all this developed in the course of the years. First I had wanted to work on a practical, solvable task as a physicist.

Then there actually was a practical task resolved by you?

Hasselmann: This is an embarrassing question.

The turbulence theory has surely not been resolved.

Hasselmann: Exactly, but then I was young and naive, and I hoped to make some progress in this problem, despite the fact that several generations before had failed. Nevertheless, my struggles with turbulence theory taught me a lot on stochastic processes and interactions in nonlinear systems. This enabled me to solve other problems later on. The first problem I solved theoretically was the question of the nonlinear coupling of ocean wave components. I would not have been able to solve this problem if I had not worked on turbulence before.

Which mark did you get in your doctorate thesis? This question may provide moral support for millions of others..

Hasselmann: Another embarrassing question. I received a 2 (corresponds to B). The reason was presumably that I solved the problem I was posed (propagation of von Schmidt head waves) in a different way than suggested by Prof. Tollmien's assistant. I found out quite early, after a few months, that the way suggested by my supervisor would not work. So I chose another path, which led to the goal, but my supervisor was not enthusiastic. Nevertheless he accepted my thesis and gave me a 2, because I had produced some very nice computational results obtained with Germany's first electronic computer, the G1, which had been developed in Göttingen. It is now in the German Science Museum in Munich. It had a total memory of – believe it or not – 25. It was quite a challenge to use it to solve a system of several equations with many different parameters. I had access to the machine at night, and played table tennis with another student until the alarm bell of the G1 informed me that there was an error, which I would fix by cutting out and replacing part of a holerith paper tape, which was glued together in a closed loop. Different computational loops were realized by different holerith paper tape loops on different readers. One could follow the course of the computation as different readers were switched on and off. I presented my results very nicely in numerous graphs, which apparently impressed my supervisor. So I obtained my PhD in less than two years [3], in spite of the forbidden approach I had used to solve the problem.

Your family did not discuss physics at breakfast. How did you head towards science?

Hasselmann: I was always interested in understanding physical processes. As I already said, one trigger was the crystal detector. But I also constructed electrical motors and such things, and was continually producing short circuits at home. I got good grades in physics in my final school examinations, but without any relation to what I was taught in school. My physics teacher did not inspire me at all; for him I was an unruly trouble maker whom he often kept in after school. „Hasselmann, detention at four!“ is still ringing in my ears.

Later at the university I was strongly motivated by my fellow students, particularly Wolfgang Kundt, Gerd Wibberenz and Ewald Richter. with whom I solved exercises together and had many discussions. That was a very intense period, forming lifelong friendships. Wolfgang Kundt and Gerd Wibberenz became Professors of physics in Bonn and Kiel, and we worked together occasionally also later. Ewald Richter became a professor of philosophy in Hamburg, and we had many interesting discussions with him too. I was also inspired as a student by Pascual Jordan, who taught theoretical physics in Hamburg. I was not in personal contact with him, but I really enjoyed his lectures. After the diploma I mainly instructed myself. I read interesting books and familiarized myself with the literature related to my research – as I suppose all young scientists do. But I never really had a proper mentor, neither at school, nor during my studies. In 1961, when I was already 29, I got to know Walter Munk, who invited me to his institute in La Jolla. I have had a close relationship with him ever since. His open, generous personality as well as his enthusiastic approach to science have always impressed me. Nonetheless, although I wrote one or two joint publications with him, I regard Walter more as a personal than a scientific role model.

Would you say that you had a factual supervisor?

Hasselmann: For my PhD? No, I did not have a real supervisor. Prof. Tollmien, then Director of the Max Planck Institute for Fluid Dynamics, was no longer active. As I explained, his assistant had a different idea on how I had to solve the problem posed for my thesis. I could not really discuss the problem with him. I worked and learnt independently and read the necessary literature. In the following three years in Hamburg I had very good relations with my former diploma supervisor, Prof. Wiegandt, but scientifically, we did not interact very strongly, as he was oriented more towards experimental work. Although I was also involved in experimental turbulence measurements at that time, using hot-wire instruments, I worked more or less on my own – with limited success experimentally, I have to admit. But it was still fun finding out how to build the equipment, learning about feedback systems and the havoc that they can create in trying to construct high level amplifiers to measure weak turbulence signals.

Then you went to America.

Hasselmann: Yes, this was through Prof. Roll, the former president of the German Hydrographical Institute, today called BSH. Parallel to the development of hot-wire measuring instruments, I had become interested in ocean waves. At the Institute for Naval Architecture there was considerable interest in the wave resistance of ships and ship motions in waves, motivated by the director of the institute, Prof. Georg Weinblum, a very kind and supporting person, who was an international expert in the field. The behaviour of vessels in rough seas in particular was a central topic at the institute. In this context, I read some very interesting papers by Owen Phillips and John Miles on the wind generation of ocean waves, which further stimulated my interest in the subject. My own first contribution to the subject was simply the introduction of the spectral energy balance equation for the prediction of ocean wave spectra, which, strangely, nobody had used before. Then it became clear to me that to understand the spectral energy balance of ocean waves, one had to solve the problem of the nonlinear interactions between wave components. I realized that the problem could be solved by the methods I had learnt in struggling with turbulence theory. Although the relevant closure methods were inadequate to solve the strongly nonlinear turbulence problem, they were directly applicable to the problem of weak interactions between ocean wave components. So I was able to derive a closed expression for the nonlinear energy transfer between ocean waves. It was represented by a relatively complicated five-dimensional so-called Boltzmann integral. Basically, I solved this problem to relieve my frustration at not being able to solve the turbulence problem.

I presented my results on the spectral energy balance and the nonlinear energy transfer in a seminar at the Institute for Naval Architecture [4]. Although most of the naval architects were somewhat confused by the mathematics, Prof. Weinblum was enthusiastic and encouraged me to continue with theoretical research. Prof. Wieghardt also concluded that I was probably more effective working theoretically than making painstaking experiments with hot-wire instruments, that had a troubling inclination to oscillate. Prof. Roll, who had been working in air-sea interaction for many years, was also there and was apparently favourably impressed. He proposed that I should attend the coming Ocean Wave Conference in Easton/USA in April 1961, to which he had been invited, but could not go. That is how I came to America, where I again presented my results. At that time – although I had not known this – the problem of the nonlinear interaction between ocean waves was seen as one of the central problems of ocean waves. I immediately received invitations to the Ocean Research Institutions in La Jolla, California, and Woods Hole, Cape Cod, as well as to the University of Illinois. I accepted the position of Assistant Professor in La Jolla offered by

Walter Munk, whom I met for the first time at the Easton Conference. I found the atmosphere at the Institute for Geophysics and Planetary Physics that he had just founded at Scripps Institution of Oceanography very stimulating. So half a year later, at the end of 1961, I went to La Jolla, and enjoyed more than three very fruitful and stimulating years there.

Did you already have the complete resonant interaction theory on surface waves when you were invited to give a talk in the USA? It is known through your publications that the triple interaction of surface waves does not function and that, one must extend interaction theory to higher perturbation order to get reasonable results.

Hasselmann: Actually, independently of my papers [4,6,9,10,11], Owen Phillips had already shown that the necessary conditions for the resonant energy transfer between different wave components could not be satisfied by three wave components, but only by four. However, Phillips had not derived the Boltzmann equation. Before Phillips published his paper, I had already independently derived the complete Boltzmann equation for the lowest-order triple-wave coupling. When I wanted to calculate the integral, however, I found to my dismay that the resonance condition could not be satisfied. That was a shock. I had calculated the complete theory up to the third order, and understood all the details about the energy transfer through resonant interactions in a continuous ocean wave spectrum, only to discover that the third-order resonance conditions could not be satisfied due to the special dispersion relation of ocean waves. That meant that the calculations had to be extended to fifth order.

I went for a three-hour long walk in the town park in Hamburg and debated within myself whether I could muster the energy to carry through two further orders of these quite complicated calculations. I decided to go through with it and spent another two or three months working on the algebra. It proved not as bad as I had first feared, although I had to derive formulas extending over one or two pages. By the time I received the invitation to present my results at the Easton Conference, I had already found a very talented young student of applied mathematics, Herr Krause (students in those days were addressed rather formally in Germany), who programmed the numerical calculation of the Boltzmann integral for me. He used the highest possible resolution available on the computer of the University, which by now was more than the G1, but still quite limited. I was very impressed that within two or three months he came up with the first numerical results. Although we later obtained more accurate results with improved computers, his results were qualitatively correct. However, they did not agree in all aspects with what I had anticipated intuitively, and so when I gave my talk in Easton [6], I pointed out that they were probably incorrect in some details. Later it became clear, however, that his calculations had in fact been qualitatively quite correct. He had even correctly computed the most important process – which I had questioned intuitively – namely the transfer of energy from waves near the peak of the spectrum to still longer waves. Ten years later we were able to show – through the JONSWAP experiment – that this is the dominant process responsible for the continual growth of wind generated waves from shorter to longer and longer waves. I am still grateful for this impressive contribution by Herr Krause. It enabled me to present not only the theory, but also first numerical results in Easton.

Was it customary these days that you did not program yourself? I am slightly astonished that as a relatively young man, as a postdoc, you got someone to program for you. Were there special technical obstacles to be overcome?

Hasselmann: No, you only had to have some experience in programming. Of course, I cooperated with the student. I explained to him which numerical algorithms should be applied, but he implemented that knowledge into the program, carried out the computations, made the usual tests and searched for errors, etc.. He fully understood what he was doing. I simply hired him as a student assistant.

We are talking about 1960/61. Did FORTRAN already exist?

Hasselmann: I can't actually remember. FORTRAN may already have existed, but I cannot recall in which language Krause wrote the program. In know that the first programs I wrote for my Dr. thesis were in machine code, and my later programs were all in FORTRAN, but I am not sure whether Krause was alredy using FORTRAN.

Starting from 1960, can you please tell us when which persons entered your life?

Hasselmann: During the first period in Germany it was Professors Karl Wieghardt, Georg Weinblum and Hans Roll, and Pascual Jordan as a physics teacher and the usual mathemathics professors, but I was not in personal contact with them. In America, as I said, Walter Munk left – and still leaves – a lasting impression on me. I had already known his name from the first classic publication by Sverdrup & Munk (1947) on the prediction of ocean waves, from which I had concluded, however, that his knowledge of physics was rather limited. At first, I underestimated him as a scientist, but when I got to know him personally, I was very impressed not only by his clear scientific thinking but also by his open-minded, positive and supportive generosity. He had a Viennese charm. He was an Austrian, who had emigrated to America already in the twenties, but still spoke with a strong Austrian accent. I gladly accepted his invitation to his new IGPP in La Jolla. I had an office in the beautiful new redwood building of his institute, that his wife Judy had designed, overlooking the Pacific on a cliff. I felt very happy in La Jolla from the beginning, especially with the open American way of welcoming new visitors. Coming from the somewhat, well, perhaps not stuffy, but not particularly creative atmosphere of German science in the fifties and early sixties, to America, where everyone was really enthusiastic, was a great experience for me.

Figure 3: With Susanne and two youngest children, Meike and Knut, in La Jolla, 1963.

Walter Munk was the central figure, but there were also other very stimulating people in La Jolla, such as Michael Longuet-Higgins, a well-known applied mathematician and fluid dynamicist from Cambridge, who had contributed many basic papers on ocean waves, microseisms and other geophysical phenomena. He had a guest professorship in La Jolla while I was there. Other guests were Norman Barber from New Zealand, a pioneer in ocean wave research who had studied the propagation of ocean swell, and David Cartwright, a co-developer of the pitch-and-roll buoy for measuring directional ocean wave spectra, and also a leading expert on tides. At Scripps there were also John Miles, who had developed an important theory on wind-wave generation, and Hugh Bradner, an interesting former high-energy physicist, who measured pressure variations in the deep ocean. I further enjoyed the interaction with George Backus and Freeman Gilbert, two young geophysicists of more or less my age, who had done some very nice work on inverse methods in geophysics and whose basic mathematical knowledge was very impressive. Klaus

Wyrtki[1] who later became one of the leading figures in El Nino research, and Carl Eckart, who had written an impressive book on theoretical oceanography, were also two well known figures in Scripps at that time, although myself had little direct contact with them. Another person who came to Scripps while I was there was David Keeling (he signs his papers Charles Keeling), who was making measurements of CO_2 on Mauna Loa in Hawaii. He had just started the measurements four years earlier. I didn't know at the time that I would later be continually referring to the now famous Keeling curve as the most important observational basis of the climate change debate. Our main contact at that time was through the madrigal choir that a few of us started. It later blossomed into quite a large university choir led by David until he died last year.

So I was immersed in a highly stimulating scientific environment. The discussions continued also in the weekly wine and spaghetti parties in Walter Munk's home – a beautiful spacious redwood bungalow overlooking the Pacific, which his wife Judy had also designed.

There were also many stimulating students. The first student I supervised was Russ Snyder, who worked later also in ocean waves. I kept in contact with him, and several years later we wrote a joint paper, together with my wife and two other colleagues [113]. My wife and I also joined Russ's family on a two-week sail in the Eastern Mediterranean along the beautiful Turkish coast. It was on their way back to America after a three-year sail around the world in a ketch Russ had built himself. My second student was Kern Kenyon, who visited me later in Hamburg and is still at Scripps today. Then there was Brent Gallagher, who also was very talented and did some nice work on nonlinear barotropic waves. He is now somewhere in Hawaii. Finally, there was Tim Barnett, who in his PhD thesis developed the first model for ocean wave prediction based on a realistic representation of the spectral energy balance, including the nonlinear energy transfer. Some years later we worked together in the JONSWAP experiment, and still later, after the Max Planck Institute was created, we cooperated in several papers on climate. Today he is a well-known climate researcher. So, these were my first students. I am glad they all did well.

I know that you were not always seated at your desk, interpreting integrals. You also did experimental research, e. g. on Hawaii.

Hasselmann: This was the first large, ocean-wide wave experiment organised by Walter Munk and coordinated by Frank Snodgrass, a technician and Walter's right hand man in all experimental matters. Similar to Norman Barber, Walter Munk had carried out continuous measurements of the spectral properties of swell arriving at a single coastal station, in his case near La Jolla. He had inferred from the gradual change in the observed swell spectra – the arrival first of very long waves, followed by waves with gradually decreasing wavelengths – that the swell must have originated in storms very far away in the South Pacific and Antarctic. Munk now wanted to find out how the energy of the swell changed as it propagated from its source somewhere south of Australia, in the high-wind region of the "fighting fifties", across the entire Pacific up to Alaska, over a distance of about two thirds of the earth's circumference. Some waves even originated in the Indian Ocean, propagating into the Pacific along a great circle between New Zealand and Australia. So Munk set up a series of wave measuring stations along a great circle extending across the entire Pacific, starting in New Zealand and ending in Alaska. In between there were stations at Samoa, Palmyra, an

[1] Klaus Wyrtki hs been interviewed in English erlier in this series, see von Storch, H., J. Sündermann, and L. Magaard, 2000: Interview mit Klaus Wyrtki, http://w3g.gkss.de/g/reports/ interview_wyrtki.html. (GKSS Report 1999/E/74, 41, pp.)

uninhabited atoll between Samoa and Hawaii, Hawaii, and "Flip". Flip was a special ship anchored between Hawaii and Alaska that could be flipped so that it stood vertically like a float in the water, the bows up high and the stern down below. The idea was that this way the boat stayed almost still in the waves and could be used as a wave measuring station.

Walter Munk, with Judy and his two daughters, stayed in Samoa, a scientist, Gordon Groves, and radio operator were flown to Palmyra, Frank Snodgrass and I myself, with my wife Susanne and three children, were in Hawaii. Frank Snodgrasss took care of the logistic organisation, and I had to tend a wave instrument and check the data from the entire experiment, which was flown to the computer center in La Jolla and then back to Hawaii for a first analysis. The experiment ran for the three summer months of 1963.

We had a wonderful time in Hawaii. One of the first things Frank Snodgrass did was to install a telephone connection from the swell measurement station off Honolulu to our house in Kailua, which was situated on the other (northern) side of the island. My measurement task was to turn on the tape recorder for an hour at 06:00 a.m. and again for an hour at 06:00 p.m, check for a couple of minutes whether the data on the paper tape looked OK and airmail the tapes to Scripps for spectral analysis. And occasionally I would plot up the analyzed spectra from all the stations that were sent back to Hawaii from La Jolla.

Unfortunately, this wonderful time was occasionally interrupted by the electric generators on Palmyra breaking down. They had five generators, of World War II vintage, which one would have thought was sufficiently redundant, but four were usually broken down. I had to drive around Oahu to find replacement parts. Palmyra had served as an airbase during World War II, but was now deserted except for our scientist and the radio operator. Frank Snodgrass felt rather uneasy about leaving two people alone on a deserted island for three months. So he had arranged that if Gordon Groves should inform him via the radio operator that "the second amplifier had failed", this was code for "urgent problem, come immediately". After two weeks we received the message. I went there by plane to find out what was wrong. In the meantime, however, the two had already patched up. Two weeks later the radio went silent and we did not hear anything from the two. Then I received a radio message that Gordon Groves had hurt his hand, which was bleeding strongly. This was followed by another week of total silence. We became quite worried and decided to go there by plane.

The first time I flew there it was in an old B25, a twin-engined bomber from World War II, used by former marine aviators to spray fields. A short time earlier, they had already tried unsuccessfully to fly to Palmyra. They did not have any modern navigational aids. They flew by Dead reckoning, i. e. like a sailor without navigational marks. You fly in a certain direction at a certain speed for a certain time and calculate your position accordingly. In addition, you must know the winds. They arrived at the calculated position, but Palmyra was nowhere to be seen. So they flew on to Tahiti. But there a thunderstorm prevented their landing. So they flew back, again over Palmyra without finding the atoll. With their last drop of fuel they just managed to land in Honolulu. The whole airport had been closed down. No other plane was permitted to land before they had landed. Directly after landing, the two pilots were taken off by the police.

That was the crew I flew to Palmyra with. If my wife had seen those bearded and dirty characters, sparsely clad in shorts, with or without T-shirts, she never would have let me fly. They again had problems finding the atoll. I was seated behind the navigator who was busy with his square search, and I could see pearls of sweat developing on his neck. But suddenly he cried: „There's the island!“

After that first time, Frank Snodgrass decided not to repeat the experience. He was able to obtain a transport aircraft of the US Coastal Survey, a large four-engined machine with a crew of eight, modern navigational aids etc. When we arrived and wanted to rescue our assumedly seriously ill scientist we were met by our two friends, both extremely cheerful, and with Gordon Groves sporting a small band-aid around one finger.

It was a time full of fun and adventure. Walter Munk, however, was a little disappointed by the outcome of the experiment [16] because he had hoped to observe the attenuation of swell by interactions with the local windsea, when the swell crossed the trade wind areas. However, no significant loss of swell wave energy could be found over the entire distance travelled by the waves, from Antarctica to Alaska. This was nevertheless an important result, which was used in the wave prediction models that were developed later. We did infer some energy loss immediately after the wind-generated waves left the area of high winds and started on their long journey as swell, that is, as long waves that are no longer forced by the wind. We were able to explain this by the nonlinear energy transfer. This was perhaps the first observational evidence of the significance of this process for the energy balance of the wave spectrum.

The Pacific swell experiment supplied also the idea for JONSWAP, the Joint North Sea Wave Project, which we carried out in the summer months of 1968 and 1969. JONSWAP was complementary to the Pacific swell experiment. Instead of studying the propagation of swell after the waves had left the wind-generating area, we investigated the growth of wind-generated waves themselves within the wind generating area. To understand the dynamics of waves, this question was clearly fundamental. We used the same strategy as in the Pacific wave experiment, but on a much smaller spatial scale: we observed the change in the wave spectrum under off-shore wind conditions at ten wave stations spaced over a distance of 160 kms off the West coast of Germany, off the island of Sylt near the Danish border, in the North Sea.

Nevertheless, many things were still to happen before the JONSWAP experiment. Your time in the USA ended, and you returned to Germany. Why?

Hasselmann: As I explained, the scientific working conditions in the USA were excellent. However, my wife was less happy, although this improved after we made friends, sang in the San Diego chorale and in the madrigal group that we had founded with Dave Keeling. Susanne had also made friends with a very stimulating piano teacher. But our children were also not as happy as they had been in Germany, especially our oldest, Meike, who had always been a beaming sunshine. At that time California was going through a phase of laissez faire, in which children grew up without any restrictions. They never knew any rules, what was permitted or forbidden, and they always seemed ill-tempered. At least in the kindergardens we knew the children did not seem to be really happy. Meike had become rather unstable. She had a pseudo croup, and we nearly lost her. In the end, we finally decided to return to Germany and bring up the children there.

But the decision was difficult and we did not make it immediately. Before going back I first tried a joint appointment, with six months in Hamburg and then six months again in La Jolla. But then we finally decided to return to Hamburg. It was not an easy decision.

How did you go on? Assistant at the Institute for Shipbuilding. Returning to the much more authoritatively organised German university must have been quite a difference from the more liberal structures in California? And to be taken up only as an assistant.

Hasselmann: No, I really had no problems. I had to give relatively few lectures, and this suited me, because I always felt that I could not explain things better than they were explained already in good text books. I was never a motivated lecturer on basic courses. I liked talking about research in seminars, but I was not motivated to repeat the basics that people could better study in text books that had been prepared with much greater care than I ever devoted to my lectures. I myself also preferred learning from books, at a pace set by myself, rather than being told things by someone else. Presumably, this influenced my attitude. So I was left in relative peace regarding lecture activities. And I tended to choose subjects which attracted only a small number of students, so that contact could be more personal.

Also, although I was in an Institute for Naval Architecture, I was able to follow up on my ocean wave research, in which I was still interested, and prepare the next JONSWAP experiment, which I mentioned earlier. So I was not really hemmed in by Germany's relatively conservative system, because I was in a rather unconventional position.

Concerning this back and forth between Germany and America. The Center for Fluid Mechanics in that time was in England. Had you any time, opportunities or desire to go to England and work there?

Hasselmann: I was in fact invited as a Visiting Fellow for half a year, in 1967, and visited the Department of Applied Mathematics and Theoretical Physics. But I did not have a strong desire to visit Cambridge while I was working in La Jolla because I was more interested at that time in oceanography. There, in Scripps, were the leading scientists in oceanography, in ocean waves, currents and so forth. In England, in Cambridge, the effort was more on pure fluid dynamics and turbulence theory, and my interests had already switched from turbulence theory to wave dynamics in the ocean. I enjoyed my later visit to Cambridge and the relaxed style there, but La Jolla was more stimulating.

So you came back to Hamburg and to the Institut für Schiffbau and then something interesting happened, something what could not happen nowadays, namely people took very swiftly decisions of what to do.

Hasselmann: I was gradually becoming an embarrassment for the Institute for Naval Architecture, because their main interest was in ship resistance, ship stability in waves – and, of course, in the design and construction of ships themselves – but not in the dynamics of ocean waves as such, or in oceanography in general. And I had started a large international experiment to measure the growth of waves under off-shore wind conditions in the North Sea. It evolved into quite an extensive affair, involving several institutions from different countries: Scripps from America, the National Institute of Oceanography from England, the Dutch Weather and Oceanographic Service KNMI, and the German Hydrographic Institute. There were four or five research vessels and other ships, a lot of activity installing wave measurement masts and wind measurement stations etc. All this created a lot of logistic overhead, and so I was tying up the secretaries, technical people, the workshop and so on in the institute for a project that had nothing to do with naval architecture.

So my former diploma thesis advisor, Prof. Wieghardt, in whose department I was working when I came back from America, came in one day and said quietly: Herr Hasselmann, don't you think you should find some other position somewhere, because it is actually not the main task of the Institute of Naval Architecture to measure waves in the North Sea. I wondered what to do, and so I asked Prof. Roll, President of the Deutsches Hydrographisches Institut, whether he could give me a job. He thought about it for a minute and probably decided that it would be a nuisance to have me in his institute as well. So he called the Federal Ministry for Science and Technology and inquired whether they could not provide a position for me in some form or another.

What then happened was that, at very short notice, the Ministry provided the funds to create a Department (Abteilung) of Theoretical Geophysics at the University of Hamburg, of which I was to become the director. An Abteilung had to be part of some institute, so Professor Menzel, the director of the Institute for Geophysics, was asked whether the new Department for Theoretical Geophysics could become part of the Institute of Geophysics. Professor Menzel, a very kind man, agreed. And so I became a member of the Institute of Geophysics. I received some research funds from the Ministry for Science and Technology, as well as a secretary, and a small apartment, of about six rooms, I think, next to the Institute for Geophysics, in the Schlüterstraße. I worked there until the Max Planck Institute for Meteorology was founded in 1975 – apart from a two year stay in America between 1970 and 1972. So the department was created, basically, through an informal discussion between the Ministry for Science and Technology and the director of the Deutsches Hydrographisches Institut, with the good-willing cooperation of everyone involved.

"Short notice" – how short was that notice?

Hasselmann: I cannot remember exactly how short it was, but it was really fast, because I was in the Abteilung when JONSWAP started, already in 1968, and I had just come back from Cambridge in 1967 and was already strongly involved in the planning of JONSWAP when this development began. It must have been less than half a year or so.

This would not be possible nowadays.

Hasselmann: Well, that was in a period of rapid scientific expansion everywhere. The same atmosphere prevailed in America, where a position was offered to me more or less spontaneously and was formalized within a few months. That was a time when one was looking for good young people everywhere, trying to build up a good research environment in response to the challenge of sputnik. Everyone was trying to be in the forefront of science. This was particularly true in Germany, where in the wake of the Wirtschaftswunder one wanted to catch up also in science.

Other people known to work with you entered the stage at that time.

Hasselmann: That's right. When the Department of Theoretical Geophysics was created I took on some PhD physics students who were interested in working in geophysics, in particular in ocean wave theory and in the general theory of nonlinear interactions in geophysical wave fields, such as internal waves. At that time I had a number of good young students, for example, Dirk Olbers, Peter Müller and Jörn Kunstmann.

Kunstmann did not do any oceanography, he was working on plasma physics.

Hasselmann: That's true, I remember. At that time I was interested also in plasma physics. I had written a couple of papers with my former student friend Gerd Wibberenz on the scattering of protons in the solar wind by irregularities of the solar wind magnetic field. As lecturer in physics in Kiel, Wibberenz was working on problems of interplanetary space. I found the problem intriguing because it could be treated by exactly the same formalism that I had applied to determine the nonlinear energy transfer in an ocean wave spectrum. I also found working on this problem was useful because I gained some practice in the notation of relativistic electrodynamics, which was helpful for my recent excursions into particle physics – another of my interests that we can discuss later. Actually, the solar wind community was also not used to the relativistic notation, so that they had some problems reinterpreting our results in their language, but our papers were well received nonetheless [25,26,29].

Anyway, to better understand plasma physics, I decided to hold a seminar course on plasma physics together with Gerd Wibberenz and my other student friend Wolfgang Kundt, who at that time was a physics lecturer at Hamburg University. That's how Jörn Kunstmann came to me. His PhD thesis was on interactions in the solar wind.

You said, you took some students. What you really did was to ensnare a whole seminar group from your friend Wolfgang Kundt. You gave a half of them new topics to work on their diploma, because we did not know what to do at that time.

Hasselmann: Yes, I seem to have hijacked Peter Müller and Dirk Olbers and maybe some others. Arne Richter and Hajo Leschke were also in that group, I think, but they did their diploma and PhDs. with someone else, probably with Wolfgang Kundt. The people that came to me seemed to be quite content just learning methods, physics and mathematics, but had no clear idea of what they should do for their diploma or PhD thesis. So they were quite happy when I suggested some topics to them .

There was an IUGG Conference in Bern in 1966. There you suddenly became the coordinator of the JONSWAP effort.

Hasselmann: I became coordinator to my big surprise, by default, probably because I initiated the idea that we should do a joint experiment. I invited some colleagues I knew – David Cartwright from the National Institute of Oceanography in England, Tim Barnett from Scripps, Karl Richter from the Deutsches Hydrographisches Institut, and some colleagues from the Netherlands, to discuss the idea of a joint experiment on wave growth in the North Sea. We met at the IUGG in Bern. We wanted to measure wave growth under off-shore wind conditions. I remember I had the crazy idea – as a physicist and theoretician – that in case of an east wind, we could measure the waves off the west coast of Germany, and when we had a west wind we could, measure waves off the east coast of England. But then some experimental colleague pointed out that it would be impracticable to install wave measurement stations on both sides of the North Sea, and that ships can not steam fast enough to go from one place to the other when the wind changes. So we decided to have the experiment on the east side of the North Sea, off the island of Sylt.

All this was agreed upon in principle, and then we went off home again. And then we suddenly realized that we have not discussed at all how to organize the experiment, and who should be the coordinator. Everybody assumed that because I had proposed the experiment, I should be the coordinator. I thought this was not a very good idea at all, as I had absolutely no experience in seagoing oceanography, and my past experience with experimental work with hot-wire turbulence measurements had convinced me that I was better employed doing theoretical work. But anyway, I was landed with this task and had to organize it.

The experiment was planned for the three summer months of 1968. A few months before the experiment was due to start, and everybody was geared up to install their equipment, I received a telephone call from the German Ministry of Defence saying that we would have to cancel our experiment. NATO was planning a large sea-to-air missile test in the North Sea at the same time. They would be testing radar methods of tracking missiles, and the ships and wave masts that we were planning to deploy would interfere with their radar signals. I said that it is impossible to cancel our experiment at this late hour, as we had already spent at least two million Deutsch Mark preparing for the experiment. The Ministry of Defense said that this might be true, but that they already spent fifty million on their exercise, so we have to cancel ours. I said, well, we cannot cancel it this way. The only solution I can suggest is that we reduce our experiment this year, without the wave masts and some of the ships, on the condition that you fund us to carry out the full experiment as originally planned next year.

The Ministry of Defence agreed, and so we carried out two experiments, a reduced trial experiment in 1968 and the full experiment in 1969.

In retrospect, we were very fortunate that this happened, because it turned out that, from the point of view of logistics, the first experiment was a complete disaster. I had worked out precisely when every wave-measurement station should start recording, and for how long and how often, based on the wind conditions and the speed of propagation of the waves from one measurement station to the next. So on one particular day a particular station, a wave mast, say, should start recording at 7:30, measuring for half an hour every three hours. Further out a ship, say, should start recording at 11:45, and so on. But the communication system we had installed turned out to be completely inadequate to transmit this information reliably. This was not helped by the Russians jamming our radio stations everytime we went on the air because they thought we were part of the NATO exercise. We did get some nice data in the end, more or less by chance, but much less than we had hoped for. The coordination of the experiment was a continual stream of improvisations.

But we gained a lot of experience, and the next year, when we carried out the full-blown experiment, everything went very smoothly. We had a functioning communication system, a reliable predetermined schedule of measurements, and well organized logistics. All the equipment worked fine, and we obtained a very good dataset. The analysis of the data laid the foundation for the modern wave models that we later developed. So we were very fortunate that the Ministry of Defence interfered with our original plans and gave us a free trial experiment, so that we could carry out a good experiment one year later.

Would you mind assessing the impact of this experiment on your personal career, standing and satisfaction?

Hasselmann: JONSWAP was certainly the most successful experiment I have been involved in. We were extremely lucky, not only because of the free trial experiment, but – still more important – because we were able to explain the principal results of the experiment by the one single process governing the dynamics of wave growth that we were also able to compute theoretically from first principles, without any empirical parameters – namely the nonlinear energy transfer I had derived earlier.

The idea of the experiment was that we would determine the processes governing the dynamics of ocean waves by measuring the change in the wave spectrum as the waves develop under an off-shore wind from small, short waves close to shore, to longer, higher waves further off-shore, out to still larger distances off shore where the waves had reached a fully-developed equilibrium state – assuming such a state exists. The spectral energy balance of the waves is controlled by three main processes: the generation of waves by the wind, the dissipation of wave energy by white capping, and the redistribution of energy across the wave spectrum by the nonlinear energy transfer. Prior to JONSWAP, we had assumed that the nonlinear transfer had only a minor impact on the evolution of the spectrum. This was based on the results I had presented at Easton, which were computed for a fully developed spectrum. But we discovered in JONSWAP that the spectrum of a growing wind sea has a much higher, sharper peak. This greatly enhances the strength of the nonlinear transfer. And it is this feature, the sharply peaked spectral shape, that is the origin of the transfer of energy from the peak to still longer waves – that is, for the continual increase in the wavelengths of a growing windsea. I still remember the excitement when we repeated the nonlinear energy transfer computations for the new JONSWAP spectra and the points came out, one by one, directly on top of the observed spectral growth.

Based on these results the wave community was then able – several years later – to develop the wave model WAM that is used today by more than 200 centres world wide, including operational global weather forecasting centers such as ECMWF, the European Centre for Medium Range Weather Forecasting, that produces daily global forecasts of the two dimensional ocean wave spectrum. The forecasts are supported today by wind and wave data from modern satellites, that the wave community also helped to develop in follow-up experiments of JONSWAP, and for which they developed the necessary retrieval algorithms and assimilation methods. But ultimately, the success of much of this development really hinged on luck: the fact that the one process that we could really compute rigorously, the nonlinear energy transfer, turned out to be the dominant process governing the form and rate of growth of the ocean wave spectrum.

Regarding my own personal career, I was recognized as the lucky person who happened to have developed the relevant theory, initiated the experiment and coordinated the analysis. We carried out the initial analysis first in our various home institutes and completed the analysis in a workshop at the Woods Hole Oceanographic Institution – which I was visiting at that time – in the spring of 1971. The results [35] were presented the same year at the IUGG Conference in Moscow.

For me it was also a great experience that you can carry out an experiment which was a complete fiasco in 1968 and still be respected by your colleagues. In the business world I would have been fired. But the scientific community is extremely tolerant and understanding. I had the same experience later with other experiments, some of which also turned out to be a flop. I was always encouraged by my colleagues, who stood by me and accepted the fact that not everything that you try to do in science works. I personally very much enjoyed the experience of JONSWAP and the follow-up experiments JONSWAP2 – although this was a flop – and MARSEN – this time a full success – in which we tested various remote sensing techniques relevant for the new wave-measuring satellites SEASAT and ERS-1. I also enjoyed the work later in the WAM group, in which we jointly developed the global wave model WAM that I mentioned [90].

All in all, JONSWAP clearly had a positive influence on the way my life developed. Probably, the fact that I was able to combine a field experiment with theory, both of which I had been involved in, also helped when I was later asked to become the director of the Max Planck Institute. It was presumably assumed that this indicated that I had enough flexibility to develop a new research program in climate. But that is only my guess. Anyway, JONSWAP was a lot of fun. It was a period in which we generated many lasting friendships. We had many parties and get-togethers with everybody involved, from the technicians to the radio operators to the ship people to the scientists. There was a great team spirit.

Could you speak about the role of Wolfgang Sell?

Hasselmann: The success of the experiment was due to the team work of many people, but two people in particular deserve mention. One was Addi Hederich, a technician from the Deutsche Hydrographische Institut. He coordinated the entire logistics, the ship schedules, the installation of the wave masts and wave buoys, including the main tower PISA for meteorological and wave measurements, as well as the complex operations for servicing the equipment at sea. He worked tirelessly in 1968-1969 to bring everything together.

The other person was Wolfgang Sell. We had collected an enormous amount of data – for those days – nowadays it would be peanuts. But, for that time, we were immersed in an intimidating array of data from instruments of many different types, with different data formats, obtained at different times and different places. Nobody had really thought seriously about how to bring all these data together into a coherent dataset. Nowadays this is routine. But for us it was quite new. I personally did not think about it at all and simply assumed that we would muddle through somehow. Fortunately, there was Wolfgang Sell in the team who realized that we had a problem. So he immediately sat down and worked out a data analysis scheme of how to store the data, how to process them, bring them together and manipulate them with a single data processing software. Without that input from him we would never have been able to complete the analysis of the JONSWAP data within only two months in Woods Hole – in time to present the results at the IUGG conference later that year in Moscow. Wolfgang Sell and a few other stalwarts, Peter Müller and Dirk Olbers, stayed on after the main workshop and helped clean up the results for the IUGG meeting.

At that time also a number of new persons came on the stage. One was Elsa Radmann.

Hasselmann: That was my secretary, a very reliable person. She came in 1968 when the Department of Theoretical Geophysics was founded and stayed with me until her retirement some thirty years later. She helped first in the organization of JONSWAP. When I went to Woods Hole for two years, in the autumn of 1970, she took care of the institute while I was away, kept up the communication, and so forth. She was an extremely reliable, conscientious person that I owe very much to. If I had to travel somewhere, I never checked where I was staying until I arrived, she had always arranged everything perfectly. She also had various likes und dislikes. If you were unfortunate enough to belong to her few dislikes you had a hard time, but for all others she was very helpful and friendly.

You mentioned the data analysis. I remember that you were doing the energy transfer calculations on many different computers. We were in DESY, in Darmstadt, we were here in Hamburg, on the Hamburg computing center and we were also in Woods Hole. Why did you go to Woods Hole? As far as I can see, Woods Hole is not a classical research centre for surface waves, for ocean waves.

Figure 4: At Woods Hole Oceanographic Institution, before Research Vessel Knorr, 1972.

Hasselmann: That was basically independent of JONSWAP. I received the offer of a professorship in the Woods Hole Oceanographic institution, on a chair that had just been donated by the Doherty foundation, to develop a joint program on oceanography between Woods Hole and MIT. I said that I would be happy to accept the professorship for two years, but could not decide yet whether I would to stay longer or go back to Germany. However, one of the reasons I accepted was that Ferris Webster, who had made the invitation, said that Woods Hole had just obtained a new computer that would be ideal for the JONSWAP analysis. So when I arrived, I talked to Art Maxwell, the director responsible for research at WHOI, and explained that we had this experiment, and that we somehow had to get together to analyze the data. He immediately offered not only the use of the computer, but also all other needed facilities, as well as some funds so that we could carry out the workshop there. That is the reason we had the JONSWAP workshop in Woods Hole.

There must have been a little bridge near by.

Hasselmann: I believe you are referring to my memorable encounter on a bridge with Peter Müller. Peter Müller was one of the members of the JONSWAP working group. We had exactly two months to complete the analysis, because then everybody had to go back home. We had a tremendous amount of work to do, a lot of computations, reorganizing and reanalyzing the data from different aspects, and so forth. I was running back and forth under enormous stress to get all this done, between the computer center and the operations room, where we were all working together. And while I was running back and forth and completely out of breath and stressed, I saw one of the members of the group, namely Peter Müller, leaning over this bridge looking calmly down onto the water. I said: “Hello Peter”. And he answered dreamily, after a long pause: “Yes, life is good … but one needs time for contemplation.”

Peter Müller and Dirk Olbers were responsible for designing the particular parameter representation of the JONSWAP spectrum.

Hasselmann: Yes, that’s right. Peter and Dirk were the creators of the so called JONSWAP spectrum, which has since been widely used. They proposed a very simple three-parameter representation which reproduced the spectral shape very well for the different stages of wind-wave growth.

From your publication list I can see that there were other issues you were interested in, besides the solar wind problem that you mentioned, for example sound waves in the ocean with Hans-Hermann Essen.

Hasselmann: Yes, I wrote a set of papers, mostly with other colleagues or PhD students – although usually the PhD students would carry out the work and publish on their own – looking at different interactions between different types of wave fields in the ocean, the atmosphere and the solid earth. One paper was with Heinz-Hermann Essen [28], on the generation and scattering of sound waves in the ocean by surface waves, one was on surface gravity waves scattering off the ocean bottom, one or two papers were on interactions between internal gravity waves in the ocean and atmosphere, although this subject was mostly well covered by several nice papers by Dirk Olbers and Peter Mueller. One of my early papers was on microseisms [13], the generation of random seismic waves through resonant interactions between surface gravity waves, and between surface gravity waves and the ocean bottom.

In most of these papers we applied the interaction-diagram formalism that Feynman had developed to summarize the interactions between particles. I had slightly modified the Feynman diagram rules in a 1966 paper [18] to adapt the formalism to classical random wave fields.

This brings me to a rather interesting comment on the communication between different scientific disciplines. My standing in the ocean science community was originally founded on my papers on nonlinear interactions between ocean waves. Shortly after coming to America I gave a talk on this work at the Californian Institute of Technology. After the talk my colleague Gerry Whitham came to me and said “That is an interesting talk you gave, but did you ever notice that the plasma physicists appear to be doing similar things to what you are doing?”. I replied, no, this was new to me, could he give me some references? So I looked up the references and discovered that the plasma physicists had indeed been doing exactly the same things that I had been doing, except that they were looking at plasma waves instead of ocean waves. This was a bit easier because they did not have to go to fifth order, the resonances occurring already at third order. But to my surprise they never actually presented the nonlinear computations. They simply took the analysis for granted. Sometimes they quoted a paper by Peierls back in 1929, in which he showed that the diffusion of heat in solids could be explained by the nonlinear interactions between phonons. I looked up the paper and discovered that Peierls had carried out exactly the same analysis as I had, using a different notation, but based on exactly the same approach. At that point I realized that my reputation in oceanography was based on very old results in physics that were simply not known in oceanography. I then started reading other physics papers and discovered that exactly the same formalism was used everywhere in quantum field theory, in describing the interactions between different particles, which are represented in quantum field theory by wave fields. Feynman had developed a well-known set of diagrams and rules summarizing the algebra involved. So I wrote my 1966 paper in which I showed how Feynman diagrams could be applied to geophysical wave fields, with a few simplifications appropriate for classical rather than quantum theoretical fields. We applied this formalism subsequently to the various wave interaction problems we investigated.

It was really an eye-opener to realize how specialized we are in our fields, and that we need to know much more about what was going on in other fields. Through this experience I became interested in particle physics and quantum field theory. So I entered quantum field theory through the back door, through working with real wave fields rather than with particles. From this other vantage point I became convinced – and remain convinced today – that Einstein was right in his criticism of the conceptual foundations of quantum theory, and that there was more to the concept of a particle than can be captured by wave dynamics. So since 1966 I have been exploring other approaches to elementary particle physics, parallel to my official research work. But I did not publish my first results, on the metron theory, until thirty years later [125,126,130,131].

You mentioned already that you carried out the JONSWAP workshop in Woods Hole. And after the workshop we all became engaged in internal waves and a large internal wave experiment, IWEX. WHOI was an institute of oceanography. They did completely different things. What was this about? Did they ask you to do this?

Hasselmann: No, I was already interested on internal waves before I came to Woods Hole. Not experimentally, but with respect to wave dynamics. At Woods Hole they were more interested in ocean currents and water masses in the ocean than in surface waves or internal waves. But they had also developed current meters and thermistor instruments, and had considerable experience in deploying current-meter and thermistor-chain moorings. So I thought that WHOI would find it a challenge to deploy a large triangular array of current meters and thermistors to measure the internal wave spectrum in the main thermocline. This they did, very enthusiastically and professionally. Dirk Olbers and Peter Müller, together with Mel Briscoe, analyzed the data and wrote up the results in some very nice papers.

You finally came back to get a professorship for theoretical geophysics in Hamburg in 1972.

Hasselmann: Yes, Professor Brocks, the director of the Meteorological Institute of the University and the Fraunhofer Institute of Maritime Meteorology and Radio Meteorology, had succeeded, with the support of other colleagues, to create a new chair for me in Theoretical Geophysics, which I accepted.

Also at that time you became a member of the Joint Organizing Committee of the Global Atmospheric Research Program GARP. You were one of the two oceanographers in that committee. In this way you became acquainted with the issues of climate, climate variability, climate change and problems of that sort. How was that?

Hasselmann: I had become a member of the Joint Organization Committee of GARP already in 1971 or 72, before I returned to

Hamburg. They were looking for some young scientist who could contribute to the strengthening of the Global Atmosphere Research Program with respect to climate, the second GARP objective. The first was improving weather prediction. They wanted an oceanographer, because of the importance of the oceans for climate, but also an oceanographer who had some experience in air-sea interaction. There was already one oceanographer with this background on the committee, Bob Stewart, and he probably proposed my name. The work in the JOC of GARP was quite fascinating, as we were laying the foundations of what was later to become the World Climate Research Program.

Figure 5: With Bob Stewart, Brian Tucker and Australian sheep during break of the Joint Organizing Committee meeting of the Global Atmospheric Research Programme in Melbourne, 1974.

Then you participated in a number of historically important meetings, namely the first climate conference in Stockholm 1974, then another one which focused on ocean problems, in Helsinki. You did not present your own work there, but you were part of the overall brainstorming which took place at that time.

Hasselmann: That's right. The Stockholm Conference was on climate in general, with a number of different working groups looking at different aspects of climate. The working groups were introduced by a few general talks, but the purpose of the conference was to work out recommendations on which research should be done in which areas. I was chairing one of the working groups involved in oceans and climate. I had a similar coordinating role in the following Helsinki Conference on Oceans and Climate, which I convened together with Alan Robinson of Harvard University. The two conferences provided the basis for the creation of the World Climate Research Program a year or two later at a conference in Geneva.

There was something else in about 1971/1972, namely the formation of the Sonderforschungsbereich 94 in Hamburg, of which you became the speaker. That was then when you really became responsible for bigger organization of science, for coordinated and interdisciplinary science. How was that?

Hasselmann: The discussions for the Sonderforschungsbereich 94 began before I went to America – around 1968–69. The proposal was written and accepted in about 1971. The first speaker of the SFB 94 was Karl Brocks, who had been the driving person in the formulation of the proposal. I had very good relations with Brocks. His institutes participated in the meteorological measurements and telemetry in JONSWAP. And he gave me much fatherly advice on how to run big projects, of which he had considerable experience. Unfortunately, he died in 1972 just before I returned from Woods Hole, and I was elected as his successor as speaker of the SFB 94.

That was a very interesting time, because the SFB 94 was the biggest Sonderforschungs-bereich at that time – in fact, later, too. It was extremely broad in its ambitions, encompassing oceanography and meteorology, air-sea interaction, ocean chemistry and ocean biology, with many different participating institutions. The challenge was to bring all these research activities together into a joint program. Many of these groups had never cooperated before and had quite different research cultures.

My first task was to start a series of seminars to define the joint projects that we wanted to carry through. We had written down some general objectives in our proposal, but we really had no clear idea of how these objectives were to be achieved. In these seminars we first had to understand how the different groups thought, and had to learn to communicate between these different cultures. Out of these discussions then came some very interesting ideas, for example, the first Fladen Ground experiment FLEX. The experiment took place in 1976 in the so-called Fladen Ground area of the northern North Sea. It was designed to investigate the coupling between the thermocline and mixed layer and the biological productivity and phytoplankton distribution during the main phytoplankton bloom in the spring. It was carried out in corporation with British groups and I believe some Dutch groups. It was quite a successful experiment. I understand the data is still an important reference data set today.

This is just thirty years ago. Could you say something about how difficult you found it – this first time when you truly became interdisciplinary. So far you were just in the realm of physics and as a physicist you should feel confident. But now you suddenly met very different people, very different scientific cultures.

Hasselmann: That was indeed a very interesting period. I remember our first discussions with the biologists. As physicists, we would ask: what happens during a spring-time phytoplankton bloom in the mixed layer? The biologists would answer with a highly detailed description of the various interacting processes that produce the exponential growth and subsequent decay of the bloom. We would reply: that's great, you seem to understand what happens, so let's put that into a model and test the ideas against some measurements. They would reply: but that's impossible, its much too complicated. And we would say: but if its so complicated that you cannot express it in a model, you cannot say you understand it. And so we would talk around each other.

But once the biologists realized that they were not simply slaves making measurements to test the models of high-brow mathematical physicists, and the physicists realized they were not simply slaves producing computer models to test the ideas developed by better educated biologists, a fruitful cooperation developed. In fact, the phytoplankton model that came out of this cooperation with the biologists formed the core of the global carbon cycle model that later became part of the Max Planck climate model.

You mention the modelers. Maybe you can drop some names?

Hasselmann: The two main people involved in the biological modeling were Ernst Maier-Reimer and Günter Radach. Radach developed the details of the phytoplankton model, but Maier-Reimer was the driver. In fact, he was the driver in all areas of modeling. If you tell him any idea about any process, he immediately produces a model. Actually, I have the same mentality: I like to produce models. But I am not as efficient as Maier-Reimer. In one of our first SFB seminars we were listening to what the biologists were telling us about phytoplankton growth in the mixed layer, how the phytoplankton gets mixed down, and how its growth or decay depends on the depths of the mixed layer and the euphotic layer, the layer penetrated by light. I thought that this would be a nice example to demonstrate how such ideas can be expressed in a simple model. So I coded a simple conceptual model on our small computer in the Institute for Geophysics. At the next seminar I was just going to present my simple computations when Ernst Maier-Reimer produced the model he had developed independently. His model was much better than my simple model. It was a detailed one-dimensional mixed layer model including temperature, phytoplankton and the penetration of the light. And he had produced some very nice plots demonstrating how the phytoplankton distribution depended on the various mixed layer parameters. I was quite impressed, and so were the biologists.

The only thing I am surprised about is that Ernst Maier-Reimer came forward with his model.

Hasselmann: You are referring to the many drawers in which Maier-Reimer has stacked away models that he has not yet shown to others, let alone published. Anyway, in this case – and many others – Ernst had a strong positive influence on the cooperative programs we developed in the SFB 94.

So you became engaged in networking, in bringing large groups of different sorts of scientists together to tackle questions of a system – in this case the system of the North Sea. You were also confronted with questions about climate and then, some day, Reimar Lüst[2] came into your office.

Hasselmann: I did not find out the background of why he came into my office until later. Apparently, the Max Planck Society had decided to accept the proposal of the Fraunhofer Society to take over the former Fraunhofer Institute for Maritime Meteorology and Radio Meteorology of Professor Brocks in exchange for an institute of the Max Planck Society. The Fraunhofer Society was dedicated to applied research, but Brocks' Fraunhofer Institute was engaged in basic research on air-sea interaction and radio meteorology. At that time the Max Planck Society had an institute in Würzburg that was engaged very strongly in applied research in solid-state physics. Thus the proposal was that the two societies should simply exchange institutes. It seems that the Max Planck Society had agreed. So the President of the Max Planck Society, Reimar Lüst, came into my office in 1974, apparently looking for a director of this new institute.

The concept was that the institute should not simply continue Brocks' work on air-sea interaction, but should focus primarily on climate research. The principal advisors of the Max Planck Society in this decision appear to have been Hermann Flohn in Bonn and Bert Bolin in Stockholm, the chairman of JOC. The Max Planck Society probably thought that, as a physicist, with experience in various areas of research in the past, I would have enough flexibility to develop an effective program in the new area of climate research. As member of the Joint Organization Committee of GARP, I had been involved in preparing what was later to become the World Climate Research Program, which was probably also one of the reasons they chose me.

The embarrassing thing was that when Lüst came into my office I had only met him once before – he was present at the most disastrous talk I had ever given in my life.

I was supposed to give a formal presentation about oceanography to a lot of high ranking people that were responsible for funding research in Germany. I had intended to work on my talk in the plane on my way over from Woods Hole, but I was tired and I could not concentrate. The next day I was still more tired with jet lag, and felt very uncomfortable when I entered the large lecture room full of people in suits and ties. So I thought that I would break the ice at the beginning by telling a little joke. But the microphone was not working properly, and somebody in the front row said "could you please repeat what you said?" I did not see much point in repeating my feeble joke, and started off on my poorly prepared talk.

So I went off rambling about all sorts of vague things about ocean research in general. I finally tried to escape from this floundering by giving an example of research. I wanted to explain how the random spectrum of ocean waves is

[2] Reimer Lüst has been interviewed in German earlier in this series, see von Storch, H., and K. Hasselmann, 2003: Interview mit Reimar Lüst. http://w3g.gkss.de/pdf/luest.interview.pdf (GKSS Report 2003/16, 39 pp.)

generated by superimposing many different sinsusoidal waves. This part I had prepared back in Woods Hole with a set of transparencies which I superimposed one after another. The result was impressively realistic and quite convincing. This time, however, when I began overlaying the different transparencies, I noticed that the audience was getting uneasy, then it started tittering, and finally it broke down in uncontrolled laughing. So I looked back onto the screen and saw that it had become completely black. The projector was too weak to shine through more than one or two transparencies, and my harmonic superposition, instead of producing a random wave field, had gradually transformed my sinusoidal waves into pitch black darkness. I somehow stumbled through to the end of the talk, but it was the worst talk I have ever given in my life and long haunted my dreams.

Figure 6: With Reimar Lüst, President of the Max Planck Society, at inauguration ceremony of the Max Planck Institut, 1975.

This was in the hotel Atlantic in Hamburg. My colleagues were very mad at me because they thought that this was hardly the way to convince the people that held the purse strings that investment in ocean research was a good idea.

So I was very surprised that, despite having witnessed this disaster, Reimer Lüst was offering this position to me.

S*o you were suddenly confronted with this Max Planck Society. Have you met with people in that group before? There was no Max Planck Institute, there was just the Max-Planck Society President who came in your office offering the position of the director of a new institute. What were the constraints of this offer? Did he provide you up front with a generous budget?*

Hasselmann: When he made this offer, I had of course a discussion with him over the level of support the institute would have. I said that I would need one director for the group from the former Fraunhofer Institute for air-sea interaction.[3] Lüst accepted. I added that I probably would need two more directors, one for climate data, one for the atmospheric part of the climate system. Lüst replied that that would be very difficult, because the Max-Planck Society did not have the budget for this now. But if it turned out to be necessary later on, the Max-Planck Society would consider a third person, at least. This was a gentleman's agreement. We did not have it written down anywhere.

Reimar Lüst then asked whether we needed a computer. I said that I did not need a large computer straightaway, but would want one later. First, we would need to develop our research program. It was clear to me that we had to solve many fundamental issues first. Once they were clarified, we would come back to the issue of a large computer. That we would need a supercomputer sooner or later was clear to me from the beginning. Lüst accepted this too.

[3] This position was later taken over by Hans Hinzpeter, who was also earlier interviewed in this series, see: von Storch, H. and K. Fraedrich 1996: Interview mit Prof. Hans Hinzpeter, Eigenverlag MPI für Meteorologie, Hamburg, 16pp, http://w3g.gkss.de/staff/storch/media/interviews/hinzpeter.pdf

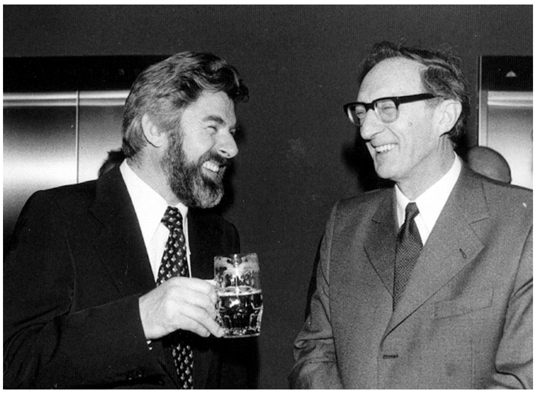

Figure 7: With Karl Wieghardt, diplom thesis advisor and later post-doc employer in Institute for Naval Architecture, at inauguration ceremony, 1975.

So, essentially, I started the institute on the commitment of one additional professor to take over the former group of Professor Brocks and the gentleman's agreement of a possible third director and a supercomputer at a later time. The staff for the climate group consisted of five scientists and some additional technical and administrative staff. The group was not large, but this complied with the general Max Planck Society policy of not assigning more than about five scientists to a director, otherwise the director would turn into a manager rather than remaining a creative scientist.

It took three or four years before I had gradually filled the five scientist positions and the climate research program began to take shape. So this was the starting basis of the institute. Later on, as the institute developed, the other elements of the gentleman's agreement with Reimar Lüst were also eventually realized.

The budget – I forgot what the actual value was – was more or less fixed. It was agreed that it would not be changed significantly from one year to the next. This is also general Max Planck policy. A constant, dependable funding level is clearly a necessary requirement for the development of a long-term research program. If we needed additional funds we could apply for these from third sources, which we did later when it became necessary. The Max Planck Society also had additional funds for special projects, but we normally received supplementary funds later through the climate programs of the Federal Ministry of Science and Technology (BMFT) and the European Commission. I was very grateful that the basic funding through the Max Planck Society was reliable and did not require a fight each year to become renewed.

Concerning models – here was a running atmospheric model in the group of Günther Fischer in Hamburg.

Hasselmann: Yes, the atmospheric model was not a problem. There was a good atmospheric general circulation model available already from Günther Fischer at the Meteorological Institute of the university. And there was a still better operational model developed by the larger group at the European Center for Medium Range Weather Forecasting (ECMWF) in Reading.

Thus, these models were around and here you were with a new institute without a computer. You pushed for analytical approaches and indeed, the first publications and ideas were analytical.

Hasselmann: When the institute 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[4], so I could build on that work as a starting point – we had a ready-made core program. Our first publications were, as you said, in this area. The other goal was developing a good ocean circulation model for climate studies. I knew from the Helsinki meeting that the biggest gap in the development of a climate model was the ocean model. We needed a good coupled

[4] Hasselmann, K., 1976: Stochastic climate models. Part I. Theory. Tellus 28, 473-485.

atmosphere-ocean model, but we had no global ocean circulation model of comparable quality to the available global atmospheric circulation models.

Figure 8: With Peter Fischer Appelt, Präsident of the University of Hamburg, Senator Dieter Biallas of the City of Hamburg and Reimar Lüst during the inauguration ceremony, 1975.

Kirk Bryan had his model at the time?

Hasselmann: Yes, it was a start, but it was not generally regarded as adequate for climate studies. It was a highly diffusive model, with a thermocline that was much too deep.

Later Maier-Reimer's model was based on similar numerics, but maybe the idea was to go different.

Hasselmann: Our goal was to produce a better model. We developed the model concept in a series of mini-seminar meetings in my office. We first explored the idea of building a composite ocean model consisting of different components for different regions, with different resolutions and different physics. The idea was to distinguish between the fast barotropic and slow baroclinic components of the system and treat them separately, and to combine these with models of, say, the Gulf Stream, the equatorial-wave system and the surface layer, all within a complete coupled system. However, we ran into severe problems already through the coupling of the barotropic and baroclinic components via the bottom topography. In the end, Maier-Reimer wisely dumped all these ideas and quietly produced a traditional gridded model, the Large Scale Geostrophic (LSG) Model, but with improved numerics. The LSG model used an implicit scheme that allowed much larger time steps, so it could be integrated over much longer times. The model was also no longer as diffusive as the Bryan model .

At the same time we were developing the global ocean circulation model, we were looking also at the carbon cycle. Maier-Reimer produced a first global carbon cycle model by incorporating the uptake and transport of CO_2 in the LSG ocean circulation model. This he successively extended in the following years by including various biological sources and sinks. The chemistry was also gradually generalized to include further constituents and tracers.

Thus we soon had a full climate model consisting of a coupled ocean-atmosphere general circulation model and the carbon cycle. The improvement of the global climate model, and its application to predictions of both natural and human made climate change, later became the main thrust of the institute's climate program.

HvS: I think it was one of your weaknesses that you have not been very good in telling the full picture. You had that vision, but you did not really share it with your coworkers – maybe you believed everybody would know, because it was so obvious to you. From my time at the Max Planck Institute we had not understood the grand strategy in the beginning.

Hasselmann: That surprises me. I hear this for the first time. So I suppose I was not clear in describing the goals that we were following. But as you say, I thought it was obvious.

DO: The SFB was going on all the time. I remember many, many meetings with the atmospheric modeling group of Günther Fischer, with Erich Roeckner and others. But our message was that we wanted to make progress with analytical means. All the

Postdocs and the PhD students in the first years were working on simpler subsystems like ice propagation, like mixed layer physics etc.

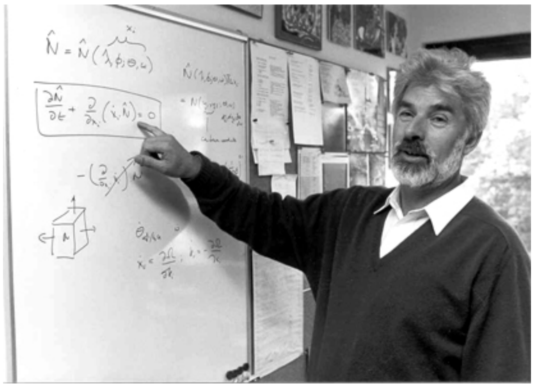

Figure 9: Explaining the stochastic forcing model of climate variability, 1982.

Hasselmann: I think you are confusing the two main branches of research I mentioned. One was looking at natural climate variability. This we could study using simple energy balance models, sea-ice models or mixed-layer models. That was what Klaus Herterich [88], Ernst Walter Trinkl [62], Peter Lemke, Claude Frankignoul [41], Dick Reynolds and others were doing. That was one aspect. I was simply exploring what could be done with the stochastic climate concept that already existed, and a number of publications came out of this approach quite quickly. These efforts were independent of the parallel development of a realistic comprehensive climate model. This took longer, involved more discussions, and the publications came later. The strategy was to first demonstrate the basic principles of how long-time-scale climate variability can be driven by stochastic short-time-scale forcing by the atmosphere, using simple climate models. Once this was achieved, we could apply the concept later to the more sophisticated climate models that Meier-Reimer, Günther Fischer, Erich Roeckner and others were developing. This in fact happened.

After Maier-Reimer had developed the LSG ocean model, he wrote an interesting paper with Uwe Mikolajewicz[5] on the natural long-term variability of the ocean circulation generated by short-term fluctuations in the atmospheric forcing. I had assumed that this strategy was obvious, but perhaps it wasn't.

HvS: I understood that much later, but now I see it and it makes very much sense. The relatively simple concept of a stochastic climate model was very useful for the overall debate because it helped overcoming the traditional concept that if climate is changing then there must be a driver. The role of internal dynamics was simply not seen. On the other hand, the nonlinear issues, chaos and so on, were coming up at that time, to which the stochastic climate model was a useful simple alternative.

If you now speak to students, also here at the Max Planck Institute, hardly anyone would know anything about the stochastic climate models. Even though you have brought it down to a form which is very easy to understand nowadays. In those days it was very complicated. How do you feel or observe that this aspect, at least in the present Max-Planck-Institute, is almost forgotten?

Hasselmann: I think it depends on your background training. If you are used to working with a high resolution general circulation model, looking at all the dynamics and interactions and so forth, you probably never think about Brownian motion or may not even have heard of the Langevin equation. These are simply not part of your basic research experience. If you are accustomed to only one way of thinking, you simply cannot see problems in another way. People are too specialized in the particular techniques they have learned. They are not able to cross their narrow boarders and see things from a different – often simpler and more elegant – perspective. But I don't see this as a basic

[5] Mikolajewicz, U. and E. Maier-Reimer, 1990: Internal secular variability in an OGCM. Clim. Dyn. 4, 145-156.

problem. Sooner or later, ideas that are fruitful will always find acceptance.

In principle these ideas are now well known and this is why we quote it. Also people speak about this concept and your name is associated to it. Hardly anybody has read the 1976 Tellus paper but very many are quoting it[6].

We should hear some more about the stochastic model. You mentioned that you came from turbulence theory, which you were then able to connect to the ocean wave problem. But you had learned all the techniques already. Was this the same situation with the stochastic model?

Hasselmann: Yes, but the stochastic model is on a much simpler level. It is just an application of the concept of Brownian motion as developed by Einstein in one of his famous 1905 papers. Like many of Einstein's concepts, the idea is elegant but basically very simple. The fact that the short-time-scale Brownian forcing is non-differentiable is a slight complication, but otherwise the basic diffusion process is quite elementary. I became acquainted with stochastic processes in various forms through my work both in turbulence theory and with hot-wire turbulence measurements. If you are trying to build a high-level amplifier which is continuously on the verge of oscillating because of feedback, you start reading about systems analysis and very soon come to stochastic processes.

Brownian motion is one of the simplest stochastic processes. The idea that one could explain long-term climate variability very simply by the short-term fluctuations of the atmosphere in analogy with Brownian motion came to me while I was sitting in a plane somewhere, I believe on the way to the Helsinki conference. The idea is really rather obvious, and I thought I would write it up somewhere in a little note.

But it came as a very big surprise in the meteorological and oceanographic quarters.

Hasselmann: And it took a surprisingly long time until it sank in. For many years people did not really look at the paper. The interesting thing is that it was not even the first paper on the subject, as I discovered after I had written the paper, I believe through a reviewer. J.M. Mitchell had expressed the same concept, on the generation of different frequency domains of climate variability by the successive forcing of longer time scales by shorter time scales, already in a very nice paper in 1966. Mitchell's analysis was more qualitative, but he had captured the main idea quite clearly.

How careful have you been reading the literature?

Hasselmann: I tend to read very diagonally. But when I find something interesting then I read it very thoroughly. When I read diagonally I try to grasp the basic idea.

DO: When you were going to Woods Hole, I was sitting in the Schlüterstraße in your room and, there was a huge pile of reprints which had not at all been touched by you. And I, of course, had time enough to look through all these reprints and I was amazed how many things one could pile up without reading. The papers were yellow and dirty from the sun and from the dust. It was clear that you had never read anything from that pile.

Hasselmann: Not all things we plan to do but fail to are so embarrassingly visible.

DO: You said, the first part of the Max Planck story were these more fundamental conceptual aspects of understanding climate dynamics, and the stochastic climate model was an important element to it. The second part was something like the technical challenge, namely to construct a reasonable ocean model which can be integrated over long times. These two efforts took your attention until about the early 80s. The people engaged in these efforts were Peter Lemke, Jürgen Willebrand, Klaus-Peter

[6] At this time, in June 2006, scifinder is listing 513 quotations of this paper.

Herterich, but also Claudia Johnson, Harald Kruse, Volker Jentzsch and Gerd Leipold.

There was a three-level hierarchy. At the top was Klaus, and at the bottom all the PhD students, in the middle level, I think, Kruse had generated this word 'Zwischenkapazitäten' (middle experts). We, Peter Lemke, Jürgen Willebrand and myself were the ZK's. So we were running from one PhD student to another and were engaged in trying to solve their problems with them.

In those times you would still know most developments in some detail that were taken place. So you were intellectually participating, while at later time your control, your participation became more distant.

Hasselmann: I was always looking for experienced people to whom I could transfer some of my responsibilities These either came new to the institute or, more often, evolved from the scientists already there as they gained more experience. Also, we later had a much broader range of activities, so that I could not keep up to date with all activities all the time. In those days of the ZK's – a new term for me, a typical Kruse creation! – we used to have seminars in my office to work out what the next steps should be in a particular program. It was a much more intimate style of research. It was an exciting period, but one which could not be maintained in the same way as the institute became larger.

We had this weekly seminar and Klaus was really very much engaged. We had created these two minutes seminar. Do you know what this means?

Hasselmann: Yes, I used to interrupt every two minutes.

No, you were ***allowed*** *to interrupt the speaker only* ***after*** *two minutes. This was really very lively.*

HvS: I think that we are now in the early 80s and I remember the Lütjenseer Wende-Parteitag. This was the first time I was confronted with Klaus. The Fischer group of the University of Hamburg, of which I was part, was invited to participate in building this climate model. You persuaded Erich Roeckner to do something very wise, namely to replace his own atmospheric model by the European Center's model. Could you elaborate a bit on that as it was a pretty important decision?

Hasselmann: It was clear at that time that we needed a good general atmospheric circulation model as part of the climate model. One needs a critically sized group to do this. The groups that had done this successfully were GFDL, NCAR in the US and – in particular – ECMWF in Europe. ECMWF was producing the world best-global medium range weather forecasts on an operational basis and had at that time the leading general circulation model of the atmosphere. It had a large group of experts working on the model. It was quite obvious that it was rather a waste of time to have excellent people like Günther Fischer and Erich Roeckner trying to compete with this large group, trying to do the same thing.

So the obvious thing was to take the ECMWF experience and to improve upon it using one's own expertise. Everybody agreed, also Günther Fischer and Erich Roeckner, although perhaps with less enthusiasm. Both are extremely competent modelers. After Günther Fischer's retirement, Erich Roeckner moved to the MPI, where he developed the original ECMWF model into the – in our view – world-best climate model, under the later directorship of Lennart Bengtsson. So I think the scientific reputations of both Günther Fischer and Erich Roeckner were enhanced by the decision. And it was, of course, essential for the development of the Hamburg climate model.

Then we are in 1982, you then had the Large Scale Geostrophic ocean model, you were to get the needed atmospheric model, you had a good conceptual framework, but you had no computer. What did you do then?

Hasselmann: In 1979, the World Climate Research Program was created, and one year later, in 1980, the German Climate Research

Program. So there was obviously a need for the German climate research community, and not just the Max Planck institute, to have a good climate model.

But it was also clear that only the Max Planck Institute, together with the Meteorological Institute, would be able to provide the model. However, since there was a general community need for a state-of-the-art climate model, it was also logical that the super-computer needed to run the model should be provided for, and therefore be funded by, the community, in other words, by the Federal Ministry of Science and Technology. This is what ultimately happened, but the route there was not straightforward.

To spin up our modeling activities, we had first applied for a medium sized computer from the Max Planck Society – in accordance with my gentleman's agreement with Reimar Lüst. This we obtained in 1979, I believe a CDC Cyber 173, but only after lengthy battles with lobbyists in the computer committee of the Max Planck Society, who argued that we would be better served by a remote access to the large computer at the Max Planck Institute for Plasma Physics in Garching, near Munich. The next step was to upgrade the Cyber 173 to our first supercomputer, a Cyber 205. This occurred around 1982. The investment was funded already by the BMFT, but the running costs were taken still from the budget of the institute.

Did that also mean that you had a significant increase of personal budget? I guess you had all these operators etc.

Hasselmann: Yes, that was a problem we had to resolve. Our computer staff was not really sufficient to run a supercomputer, and the few additional people we had taken on were already straining the institute's budget. Wolfgang Sell headed the computer staff, Dirk Schriever, who had been responsible for data processing at the former Brocks institute, organized the data archive, and we had a few operators.

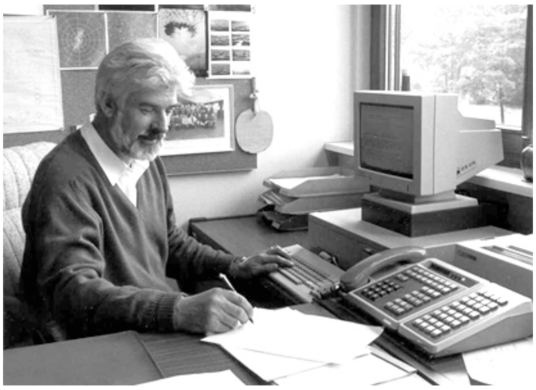

Figure 10: In the new prefab building ("pavillon") behind he Geomatikum, after creation of the DKRZ, 1989.

But we also had a problem with developing the comprehensive climate model. Günther Fischer, who had headed the atmospheric modeling group of the Meteorological Institute, had retired, and it was clear that his successor, whoever it would be, would not be a numerical modeler.

We found a good solution to both problems. I approached Reimar Lüst and reminded him of our second gentleman's agreement. I explained that the time had come when we really needed a third director to take care of the atmospheric modeling activities. His response was positive – in principle. I then approached Frau Tannhäuser, the administrator of the German Climate Research Program, and proposed that our supercomputer should be transferred from the Max Planck Institute to a new-to-be-created German Climate Computing Center (the DKRZ), and that the BMFT should carry also the associated staff costs. She also responded positively – in principle. There followed a period of negotiations between the parties involved regarding the distribution of costs, the distribution of computing time between the Max Planck Institute and other users from the general climate research community, legal formalities, etc.

The net result was that our computing staff was transferred from the Max Planck Institute to the DKRZ, which freed a number of positions that we could now offer to the new third director of the institute. The DKRZ was founded in 1985, with Wolfgang Sell as Technical Director and myself as Scientific Director. The third director of the Max Planck Institute, Lennart Bengtsson, came a few years later, at the end of 1990.

Who, among other appointments, then got Eric Roeckner to move from the Meteorological Institute of the University of Hamburg to the Max Planck Institute?

Hasselmann: This was a very good move. But Lennart also had a lot of experience in atmospheric modeling too, of course, as well as a great deal of organizational experience. He knew the Centre's model very well, and his arrival, together with Roeckner's expertise and hard work, gave us a big push.

He also hired Ulrich Cubasch at that time.

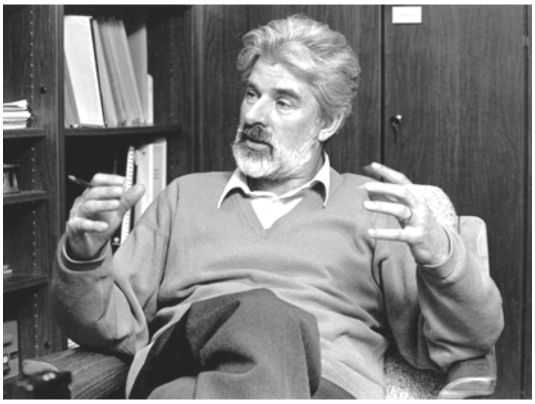

Figure 11: Grasping the complexity of the climate system, 1988.

Hasselmann: That is right. Ulrich Cubasch used to be at the European Center. He was very effective in analyzing the results of our simulation experiments. Lennart Bengtsson also hired Lidia Dümenil, Klaus Arpe, and Bennert Machenhauer, who developed a nested regional atmospheric model. So he built up a very good group. The Hamburg version of the ECMWF atmospheric model, ECHAM was then coupled to our LSG ocean model, including the carbon cycle, to create the ECHAM-LSG coupled climate model. This was done in cooperation with a number of visitors, both to Lennart's group and to my group. Lennart had a continual stream of guests, many of whom had previously visited the European Centre, while we had stimulating visits, for example, from Wally Broecker from the Lamont Observatory and Bob Bacastow from Scripps, who both collaborated with Ernst Maier-Reimer in developing the carbon cycle model.

At the same time people like Dirk Olbers left. There was a change in the general direction. It was more towards the dynamical, quasi-realistic complex models, less dynamical conceptualization, more brute force implementation of experimental tools.

Hasselmann: That's true. We first had to demonstrate some basic concepts regarding natural climate variability using simple models. But once that had been achieved, there was obviously no point in pursuing the analysis further with simple models. We had to first construct more realistic models. So as soon as the LSG ocean circulation model had been created, Maier-Reimer and Mikolajewicz computed its response to stochastic forcing, as I mentioned. The next step would have been to apply these ideas to the full climate system, the coupled ocean-atmosphere general circulation model. But somehow we got side-tracked. I am glad to hear that Jin von Storch has started looking at this problem with one of her PhD students. But there is much that still needs to be done. I think the distinction between the three possible sources of natural climate variability, namely stochastic forcing by short-time-scale atmospheric variability acting on the slow climate system, internal nonlinear interactions on comparable time scales within the slow climate system itself, and external forcing, for example by volcanic activity, or by variations in the sun's radiation or in the earth's orbit, has still not yet been properly clarified.

We were probably distracted from this straightforward goal by the many interesting new problems that came up in connection with the modeling effort. For example, we began looking at the feasibility of the prediction of natural short-term climate variability on time scales up to a year. I worked with Tim Barnet on this, applying purely statistical methods, based on linear multi-time-lag regression models [56,65,67]. Later we applied also a realistic GCM model to El Nino predictions, and a reduced-complexity coupled model of the type was used very effectively by Mojib Latif. Tim Barnett used another, still simpler linear feedback model, also in collaboration with Mojib, which worked quite well too. So we had opened another arena in which we could apply relatively simple dynamical concepts without a full-blown global climate model.

But we also became involved in improving the global climate model itself, by extending the biology and chemistry representation in the ocean sub-system, by improving the sea-ice model, by adding atmospheric chemistry, in collaboration with Paul Crutzen's group at the Max Planck Institute in Mainz, by including surface vegetation, and so forth. This is, of course, an endless task.

Another question I pursued relatively early as a side-line in our modeling activities was the projection of complex models onto simpler models using so-called Principal Interaction Patterns (PIPs) and Principal Oscillation Patterns (POPs) [93, 94]. A basic difficulty of complex models is that, as they become more realistic by incorporating more processes and degrees of freedom, they become just as difficult to understand as the real systems they simulate. I tried to devise methods for constructing simpler models that capture the dominant processes that govern the dynamics of the full complex system in terms of just a few basic interaction patterns – in the general nonlinear case, in terms of PIPs, in the special case of a linear system with stochastic forcing, in terms of POPs.

Finally, we also became more strongly engaged in later years in IPCC activities, in scenario computations of anthropogenic climate change over the next 100 years.

All these tasks were quite fascinating and distracted from our original goal of sorting out the different forms of natural climate variability. But now that the question of anthropogenic climate change has become much more center stage in the public awareness, I believe the distinction between anthropogenic climate change and natural climate variability will rise to high priority in the climate research agenda. We will have to look in earnest again at the structure of natural climate variability. The increased public interest this problem is apparent in the recent discussions over the possible impact of anthropogenic change on the frequency and intensity of extreme events such as hurricanes, flooding and droughts.

Figure 12: Making a point, 1988.

In that sense it had a revival or an important implication in the last years of your directorship. It would not have made sense to think about detection of anthropogenic climate change without a stochastic concept.

Hasselmann: I am not so sure that the stochastic concept as such is important for the detection and attribution problem. The main point is that you are trying to distinguish between the anthoprogenic climate signal – or some other externally forced climate change signal, for example, due to a volcanic eruption

– and the internal natural climate variability. The origin of the natural climate variability, whether through stochastic forcing by the short-term climate variability or through nonlinear interactions within the climate system itself, is irrelevant. The central issue is to distinguish between an externally forced climate change signal and natural climate variability, on the basis of the frequency spectra of the two signals. This is another example of applying a ready-made theory from another field – in this case signal processing in communications – to a climate problem. I pointed this out in a 1979 paper [57], but the paper lay dormant until the detection problem became relevant in the mid 90's, when a spate of papers [115, 124, 132, 134, 135, 141] demonstrated that the anthropogenic climate change signal had now indeed become detectable above the natural climate variability noise.

In the 60s and 70s, people would not necessarily have agreed that there is variability for no specific reasons.

Hasselmann: I think there were already two schools of thought at that time. One school thought that climate variability must indeed be produced by some external forcing mechanism, such as volcanic eruptions or variations in solar radiation. But the second school recognized that you could explain natural climate variability simply by the fact that climate is a nonlinear system containing feedbacks. Such systems, for example, turbulence, are known to exhibit random variations. Both mechanisms can contribute to climate variability. The stochastic forcing model merely points out that there exists a particularly simple realization of the second mechanism, since the climate system contains a ready-made source of natural variability in the form of the turbulent atmosphere. All one has to do is separate the time scales, that is, distinguish between the fast atmosphere and the rest of the climate system, consisting of slow components such as the oceans, cryosphere and carbon cycle. But the idea that internally generated natural variability can be expected in a nonlinear system such as climate was already around at that time.

HvS: My understanding of stochastic variations is that we have very many chaotic components in the system, so that the overall behavior cannot be distinguished from the mathematical construct of noise. Therefore we can describe the nonlinear dynamics very efficiently as noise. In the same way as a random number generator is also a deterministic algorithm on a computer.

Hasselmann: Well, I think, we find this in any nonlinear system.

But it would not necessarily look like noise if you have a few degrees in a system. So for the Lorenz' system you would not conceptualize the behaviour as noise.

Hasselmann: It depends on what you define as noise. If you define noise simply as a statistically stationary stochastic process, then the Lorenz system, in the appropriate parameter range, produces noise – although it is certainly not Gaussian, as assumed in many noise analyses. No, I think the essential point about the stochastic forcing concept is not that one has noise, or that the system has very many degrees of freedom, but that one can understand the origin and structure of the noise in the climate system very simply by separating the time scales. The origin of the noise is the short-time-scale turbulent atmosphere. This then generates variability on much longer time scales in the rest of the climate system. There is no need to understand the detailed dynamics of the atmosphere. It is sufficient to know that the turbulent atmosphere is characterized by a noise spectrum that is concentrated in frequencies corresponding to time scales of hours and days, but – because the system is nonlinear – also extends down to a finite level at very low frequencies. It is this low-frequency range, corresponding to time scales of months, years, decades and even longer – that can be treated as white, i.e. simply as constant – that generates variability in the rest of the climate system, the slow climate system.

In most of our initial applications of the stochastic climate model, we considered some simple component of the climate system– for example, the temperature of the mixed layer, or the sea ice extent– which we could linearize. So there was a popular misconception that the stochastic model could be used only to describe the response of a linear system to white noise forcing. But the concept is valid generally for any climate model, whether linear or nonlinear, as demonstrated by the application of Maier-Reimer and Mikolajewicz to the LSG ocean circulation model. This misunderstanding is perhaps related to the fact that some people may have had difficulties understanding my original stochastic climate

model paper. To treat the general nonlinear case, I used the Fokker-Planck equation, the generalization of the Liouville equation of statistical mechanics to a system including diffusion, as required for Brownian motion. While most people can be assumed to have been familiar with the Liouville equation, the Fokker-Planck equation was perhaps less well-known.

You outlined this whole set up of the Max Planck Institute with the different models and couplings, ideas and so on. At the same time we had a German climate science program. From outside it looked as though MPI ran this program. The MPI made many attempts to draw in people from outside, but other meteorological institutes were only marginally involved with respect to the global modeling efforts. Is that the same as you see it?

Hasselmann: Yes. I think the explanation is in human nature. We certainly tried to draw other groups into the program, but the problem was that to run or contribute to the development of a complex global climate model system, you have to be willing to get your hands dirty, you really have to become involved. You cannot just sit around and have some clever ideas. You cannot work on a complex model some 500 kilometers away. The people we collaborated with came from India, Canada or somewhere else for a year or so. Most Germans – most of them had a family at home – were not willing to come for a longer visit. Another reason that our attempts were not very successful is that most scientists do not get excited at the idea of becoming involved in larger and somewhat anonymous activities.

So it was typical that in the German climate research program we had one global climate modeling group stationed in Hamburg, at the Max Planck Institute and the University Institute of Meteorology, and several smaller groups distributed everywhere else, at the GKSS in Geesthacht, in Jülich, in Karlsruhe, in Bonn and Cologne, all working on regional climate models, because they could do that on their own. I thought it was a waste of time and resources producing five or six different regional models, all of similar quality. We had a regional model in Hamburg, too, nested into the global model. This was a typical case of unnecessary parallelism because people simply had problems in getting involved in a joint program. I tried to overcome this, but I have to admit that I was not successful.

We were more successful with groups that were analyzing the outputs of our models, for example in Cologne, Munich or, later, in Potsdam. But there were rather few groups engaged in such activities. I believe the same problems are encountered everywhere by groups developing large models. One cannot yet effectively decentralize this type of work.

Concerning ocean models you see there was this division between LSG, which was large scale, and the rest of the oceanographers in Kiel and also in Bremerhaven who did eddy resolving models. But my impression was that you did not really value these.

Figure 13: Robertson Memorial Lecture Award, US National Academy of Sciences, 1990 (proposed by Carl Wunsch, second row, first left).

Hasselmann: Well, yes, I was not convinced that the eddy-resolving models were really worth the effort.

They were or were not?

Hasselmann: I thought they were not. They burnt up a lot of computing time. Essentially, they showed that there were eddies, which we knew anyway. I was not convinced that the interaction between the eddies and the mean flow could not be parameterized sufficiently well for climate modeling purposes with a standard eddy transfer approach. Or, at least, the eddy-resolving simulations had not come up with a better parametrization. I am not convinced that we were discovering something basically new. What I have seen in talks to this day are beautiful pictures of the Gulf Stream and all these eddies floating around, but what have we actually learnt? If one can demonstrate that the impact of these eddies is radically different from what we have been putting into our coarser-resolution models, then I will admit that we have to start thinking of something radically different, or maybe even have to give up working with non-eddy-resolving models. But I have not seen this yet. What I have seen are mainly nice movie presentations that are good for public relations.

What do you think about visualization?

Hasselmann: I have mixed views. I think there has been an unnecessary polarization of viewpoints on this topic. The presentation of the results of a complex time-dependent simulation in a visualized form that the non-expert can quickly grasp can be very helpful. For somebody who has never seen satellite or other data on Gulf Stream eddies, the simulation with a good eddy-resolving model of the Gulf Stream can be very illuminating. On the other hand, my experience is that the active scientist doing quantitative data analysis seldom uses visualization. There can be a few cases in which it is useful. I remember one case in which watching a video sequence helped us discover an intermittent instability at a particular gridpoint that we had missed in the snapshot pictures. So I think, even it is not used routinely, it is certainly worthwhile to have a good visualization facility available.

Have you ever been in the caves, this three dimensional visualization?

Hasselmann: I get sick in these things. I find them terrible. I experienced one in the Tyndall Centre in Norwich. Maybe I am too sensitive, but the three-dimensional projection did not seem to work properly, and I got giddy. After a certain time I got really sick. Perhaps I was not sitting in the right location. And maybe the techniques will improve with time. But I was not convinced that the additional information of seeing the data in three dimensions rather than two – in other words. with one eye closed – was terribly important for scientific purposes and justified the technical effort. But again, it may be OK for public relations, once the technique is sufficiently mature.

One climate component which has been tackled by the Max Planck Institute and others as well is the ice sheet. But I've never really seen ice sheets incorporated in climate models at MPI. Is that something which is too complicated?

Hasselmann: I don't think it is terribly complicated. There was probably just not enough push on my part. We had Klaus Herterich's ice sheet model. His model described very nicely how ice sheets grew and melted and when they start to surge.

I was interested in coupling an ice sheet model with an ice-shelf and a sea-ice model. A coupled model of this kind would be very useful to address the question of the stability of the Greenland or Antarctic ice sheet, whether the ice sheet can break down through ice surges. And if this model had been incorporated into our global climate model, we could have carried out simulations to investigate the origin of climate variations on century and millennium time scales, which still pose many open questions. The Milankowitch theory explains only part of the variability. I think that is a very important area of research, and it was probably my fault that I did not apply enough leadership to ensure that such studies, using an ice sheet model coupled with an ocean model and an atmospheric model, were pursued more seriously. It would have required a stronger group than just one person, Klaus Herterich, who later went on to a professorship in Bremen.

Was this overrun by the IPCC scenarios for the next hundred years?

Hasselmann: No, I don't really think so. This was carried out by other people, in particular, Ulrich Cubasch [109]. The IPCC scenarios were, of course, important for IPCC and the general international climate research effort, but they were also important for us. They demonstrated what the models could do. And they were important for the German Climate Research Program, which had to justify its program to policy makers and the public.

We participated also in the international climate model intercomparison project, which involved similar scenario computations. This was an important exercise to identify the strengths and weaknesses of different climate models.

From a scientific point of view, this work was not very exciting, but I don't think it was in the competition with the ice-sheet modeling. I was probably also distracted following up on other problems.

HvS: Perhaps it would be more honest to say we are now in a less focused period of the institute? After 1985, you let the reins loose more and more and at the end you became less and less interested in climate. That is my impression; I would not criticize you for that. Lots of things happened in the institute and this was one just one of these issues. There were many studies which were not related to this big modeling building and the IPCC.

Hasselmann: Yes, maybe that was the case, if you look at the many publications on different topics that were coming out the institute. We had also expanded the research on the carbon cycle and tracers using inverse modeling techniques, led by Martin Heimann, who came to us from Scripps in 1985. With highly competent scientists around like Martin Heimann, who is now director of the Max Planck Institute for Biogeochemical Cycles in Jena, I did indeed let the reigns a little loose and let group leaders take over in many areas – which I don't think was a bad thing.

Global warming was not a dominant issue at the institute in the late eighties. Lots of studies were done which had nothing to do with the overarching goal you just described. People were just entertaining, enjoying themselves.

Hasselmann: I would not put it that drastically. They were exploring many different interesting topics, and quite successfully. But we were also carrying out a good deal of work on global warming too, for example in the scenario computations you referred to. It is true that I myself did become involved in problems other than global warming at that time. However, I was still interested in ice sheets, although, admittedly, not aggressively enough. We had good contacts with Johannes Oerlemans, an international expert in ice sheet dynamics from Utrecht, who visited us several times, and with Bill Hibler from Canada, an expert in sea-ice modeling who stayed with us for a year. As a result, we did incorporate a good sea-ice model into the global climate model, but unfortunately not an ice-sheet model.

Perhaps I should honestly admit that I was also getting a little bored with always having to organize things and was quite happy that the so-called ZK had matured to a level of expertise and international recognition where I could happily let them take the lead in many areas.

I remember in the first period, when we were developing our work on stochastic models and so forth and also on the ocean modeling in the early eighties, Fritz Schott had visited us from Miami and talked to many people at the institute. He came to me afterwards and said that he had never been in an institute where the PhDs and post-docs were so closely guided as in the Max Planck institute.

When did he say that?

Hasselmann: It must have been around the early eighties. I suppose that at that time I was indeed guiding people more strongly than in most institutes in the US, but I think that later on, I tended to let people loose to develop on their own – make their own mistakes rather than mine.

I heard stories that it was really tough for PhD students in the late seventies to work with you.

Hasselmann: We had tough discussions. That is true. But it was never personal. I tried to support the students as well as I could. I can't remember any student actually failing, although one student did decide after a year to become a pastor. He thanked me later for motivating him indirectly to that decision. I'm not sure how. Perhaps I was a little tough.

Figure 14: With Wave modelling Group, Sintra, Portugal, 1992.

On the other hand you were also riding a lot of horses. The climate business was evolving and became useful – if we may call it this way – and this IPCC engagement also and our efforts to come up with prediction schemes for El Nino and things of that sort. This all went very smoothly and nicely and you were guiding all these things. But you did other things as well! We others did not really notice that but you were still engaged in wave aspects, still engaged in remote sensing with respect to wave activity. Can you tell us about that a bit?

Hasselmann: Well, I had decided more or less to stop my ocean wave research around the late

70s. But there were two developments that brought me back into the subject. One was that ESA was preparing to build ERS-1, the European follow-on of SEASAT, the US satellite that had operated for only 100 days in 1978, but had demonstrated the feasibility of measuring ocean waves from space. ESA asked me to serve on the ERS-1 advisory panel. The second development was that my wife Susanne – after a 15 year interruption bringing up children – had just completed her diploma in mathematics. We wanted to do work together. I did not want her to work in the climate area, because there she would have been in direct competition with other members of the institute. So I suggested finding some area where we could work together without overlap with the main work of the institute. Ocean waves was a natural choice.

This was also good timing, because we now understood ocean wave dynamics rather well, through JONSWAP, and we faced the challenge of translating this knowledge into a numerical ocean wave prediction model. Susanne, as mathematician, would be well able to do this. Also, we would need a good global ocean wave prediction model to assimilate the global wave height and two-dimensional wave spectral data that we hoped we would be obtaining continuously in a few years from the altimeter and SAR instruments aboard ERS-1.

So I renewed my activities in ocean wave research. Together with former JONSWAP colleagues we formed the WAM (Wave Model) group, with the goal of developing what was to be called the third generation wave model 3G-WAM. The 3G was dropped later as too cumbersome. We first carried out a comparative study of all existing ocean wave models [76], in which we concluded that the so-called first and second generation wave models were inadequate. First generation models, developed in the sixties, were based on our incorrect understanding of the wave spectral energy balance prior to JONSWAP. Second generation models included the nonlinear transfer in accordance with the JONSWAP picture, but the parametrization was too crude to reproduce the wave spectra for complex wind fields. We needed a third generation model with an improved representation of the nonlinear transfer. So Susanne and I first developed a more realistic approximation of the five-dimensional non-linear transfer integral that could be implemented in a wave model [77,78], and Susanne incorporated this in a first version of the WAM model. The model was then tested and further improved by other members of the WAM group [90]. Heinz Günther from GKSS cleaned up the numerics and documentation and ran the model at the European Centre, while others tested various other aspects of the model. It is now used world-wide in many operational forecasting centers and research institutes.

Figure: 15: Enjoying an icecream in Sintra, 1992.

My work in the ERS-1 advisory committee also took a fair amount of time. I frequently had to travel to ESA headquarters in Paris or to the ESA Technical Centre ESTEC in Noordwijk in Holland. Through ERS-1 I met many interesting people involved in remote sensing, such as Ola Johannessen, director of the Nansen Center in Bergen, Norway. But ERS-1 also involved interesting scientific challenges. One was developing algorithms to retrieve the two-dimensional wave spectrum from the nonlinear ERS-1 SAR image spectra [100]. Another was assimilating the resulting wave spectra in the WAM model [120]. I worked on this together with Susanne. But there were so many other interesting problems, particularly when ERS-1 was launched in 1991 and began producing data, that I also took on some PhD students, contrary to my original intentions. We had a small but very active ocean wave and remote sensing group consisting, in different periods, of Claus Brüning, Susanne Lehner, Patrick Heimbach, Eva Bauer and Georg Barzel. They worked independently of the climate groups, with relatively little interaction apart from seminars and other general institute activities.

What about Werner Alpers?

Figure 16: With authors of the book Dynamics and Modelling of Ocean Waves, 1994 (from left: KH, Peter Janssen, Gerbrand Komen, Susanne Hasselmann, Mark Donelan, Luigi Cavaleri).

Hasselmann: Alpers was not a student of mine. He was a post-doc in the Sonderforschungsbereich. He worked with me on the remote sensing of ocean waves in my first 'ocean wave period', before the Max Planck Institute was created. He then went to the University of Bremen as Professor for Remote Sensing, and later returned to Ham burg, again as Professor for Remote Sensing. I worked together with him again after I revived my ocean wave and remote sensing interests. But I stopped working on ocean waves and remote sensing – this time, for real – after Susanne retired in 1996, and I turned to other interests.

You became interested in what some people say was a very naïve way of describing economics, dabbling in economics. What was that?

Hasselmann: It came through my involvement with the media and public audiences. In the late eighties and nineties, the media, general public and politicians began to become increasingly aware of the climate change problem and wanted to hear more from the climate experts themselves. So I was often invited to interviews on TV or the radio, and to give talks to the general public on climate. At the end of my talks I was always asked the same question: What should we do? And I would say: Well, I do not really know. I'm a climate scientist, not an economist or politician. But they would never let go, and kept persisting until I came up some off-the-cuff answer. So I decided I had better find some better answers and began looking into the problem of the impacts of climate change, and the possible economic and policy responses. I could find little reliable information on climate impacts, and was rather disappointed with the analyses of the economists, who were using – in my view – inappropriate outmoded economic equilibrium models. They were also distorting the critical issue of the proper discounting of future climate change costs. And the political stage, of course, was beset by lobbyists of all hues, which made it difficult to detect a signal in the noise.

So I began developing some simple coupled climate-economic models to determine the optimal CO_2 emission path that minimizes the net economic costs of anthropogenic climate change and climate change mitigation, with emphasis on the intertemporal discounting issue [133,144]. At the same time Hans von Storch wrote some similar papers with Olli Tahvonen, an economist from Finland, whom Hans von Storch had interested in the problem.

I followed up this work with somewhat more realistic but still relatively simple economic models based on non-equilibriium multi-agent dynamics. A few nice PhDs theses came out of this, by Volker Barth, Michael Weber and Georg Hooss [150,155]. As a side product, we created a climate computer game based on our coupled climate-economic model that was implemented in a climate exhibition for a year or so at the German Science Museum in Munich. The game was quite popular.

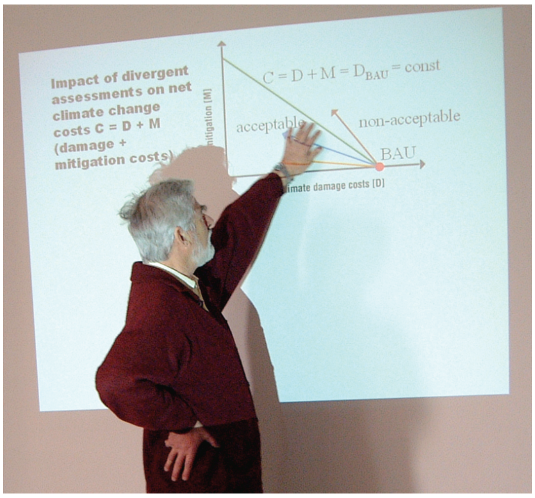

Figure 17: Explaining the multi-agent aspects of a coupled climate-economy model, 2002.

Coupled climate-economic modeling is still a hobby of mine today. I believe there is an urgent need for the economic profession, in cooperation with physicists and social scientists, to develop realistic dynamical non-equilibrium socio-economic models that combine the climate change problem with the general societal issues of globalization, employment, limited resources, etc.

At the time I was becoming interested in these problems, in 1990, I was asked, together with my colleague Hans Hinzpeter, to become a member of an Evaluating Committee of the Academy Institutes of the former GDR. Our task was to recommend what should become of the Academy Institutes in the area of geophysics and the environment, now that the two German states had become unified. We came across a young group doing interesting interdisciplinary work on various climate-change impact problems. We recommended that they should be integrated into a new institute designated to study the societal and economic impacts of climate change and climate change policies. That was the origin of the Potsdam Institute for Climate Impact Research that was created two years later in 1992. PIK developed a good cooperation with the Max Planck Institute, analyzing many of our climate change simulations.

Figure 18: Explaining the detection of an anthropogenic climate signal at 95% statistical confidence level, with the Federal Minister of Research and Technology, Jürgen Rüttgers, 1992.

We tried to establish a similar activity on a smaller scale also in Hamburg. I suggested to the president of the University of Hamburg, Jürgen Lütje, at a cocktail party given by Reimar Lüst in the Bobby Reich Restaurant next to the Alster, that the university should support a group to study the impact of climate change on the economy and society. This was becoming an increasingly important area of research and would be a good bridge between the climate activities at the Max Planck Institute and the strong economics department of the university. Lütje straightaway talked to Michael Otto, the head of a large mail-order firm and a well known sponsor of environmental projects, and convinced him of the idea. Michael Otto offered to endow a professorship for environmental economics for five years and asked for proposals. The first

Figure 19: 60th birthday, Rissen 1991.

time round the university proposal was not accepted, as the university had not committed itself to provide the necessary follow-on funds for the chair after the first five years had elapsed. But in a second round the university made the commitment, and the chair was created. Richard Tol, a very young scientist from the Vrije Universiteit Amsterdam who already had an impressive list of publications, was elected to the professorship.

Unfortunately an intense cooperation did not emerge with Richard?

Hasselmann: It is the old problem of getting two disciplines to work together. Richard Tol turned out to be a rather traditional economist who looked rather sceptically on the attempts of physicists to get involved in economics. For this reason I think not everybody that he could have collaborated with – including myself – was enthusiastic. But Richard is very young and could develop. So perhaps there may be more collaboration in the future – unless Richard decides to accept positions he has been offered elsewhere, as has been rumoured[7].

When you retired in 1999, you did something, which – I thought – was rather unexpected or unpredictable. You had already withdrawn to some extent from the climate field but you engaged in a new issue. The first time you spoke about that publicly was at your 60th birthday, when you gave a talk for something like two hours about your approach to particle theory. You withdrew from the climate field, which is quite something for a person with your authority and recognition in the field. You said I do not mind, I am going on to something else that I am more interested in.

So far you won all battles, you were the young attacker bringing down sclerotic old ideas and replacing them with more modern ideas. This was well done, you were successful in doing so and then you suddenly decided, no, I am doing something else now. I am really attacking

[7] Richard Tol has in the meantime moved from Hamburg to the Economic and Social Research Institute in Dublin.

something totally different and this would be an uphill battle. You would start as newcomer with all the difficulties; you could not really use your recognition in the field. How was that?

Hasselmann: Well, I realized that that would be the situation. I was not surprised. I was a bit surprised at the level of denial – in some cases, even antagonism – of the established particle physicists. Other physicists were more open to my ideas. Of course, they were sceptical, but they were willing to discuss, and in a few cases were even quite positive. But I was aware that for most physicists I would be regarded as slightly crazy, since I was seen as a climatologist who could clearly have no idea of particle physics. I was seen as a dreamer without really knowing what I was talking about. This is perfectly understandable. I have the same reaction to the strange people who sometimes drifted into my office without the slightest knowledge of climate and explained to me why we were or were not experiencing global warming. It did not bother me too much. In my career I have always found that the newer the idea, and the more distant the field it originates in, the more scepticism one encounters. Unfortunately, a sceptical reaction is no guarantee that you have a good idea. It can indeed be a crazy idea. The only way to find out is to press on regardless.

I've been looking at particle physics ever since the mid-sixties when I wrote my Feynman diagram paper on wave-wave interactions in geophysical wave fields. I was convinced that something was basically wrong in quantum field theory. I did not know what it is, but I think many physicists would agree that Einstein had a point in his criticisms of the conceptual foundations of quantum theory. But, of course, everybody says that Einstein worked all his life to find another approach, so why should somebody like Hasselmann be able to solve the problem? Well, I thought it was worth trying. After all, we can't all be paralyzed for ever by Einstein. As you say, I have won most of my battles in the past, and what is the point of having some reputation capital if you cannot spend it on something that's fun?

I published a lengthy four-part paper [125, 126, 130, 131] on the basic ideas of my metron theory in 1996 and 1997, expanding on the first talk I gave on my 60th birthday in October 1992. This was in a journal on the basics of physics, which I discovered later, however, was not taken very seriously by most physicists. I have also published two other papers since then [140], [161] and am right now writing up two further papers on my recent results. Once the theory is published in accepted journals, it will become either accepted or rejected. This is as it should be. I am not really concerned about the outcome, which is beyond by control.

As I mentioned, besides this venture into a new field, I am also still working on coupled climate-economic models. I created the European Climate Forum, chaired by Carlo Jaeger, in which we are trying to bring the stakeholders in the climate change debate – business enterprises, energy companies, manufacturers, insurance companies, NGOs and so forth – together with climate scientists and economists to study the climate change problem, to analyze the various possible mitigation and adaptation policies options.

Figure 20: With Walter Munk, during Hasselmann's 60'th birthday symposium, 1991.

But your heart is with particle theory?

Hasselmann: Yes, my heart is with the particles.

DO: I had the pleasure to attend your 60th birthday meeting and to listen to your metron talk. I thought I understood most of what you said. My impression was that in just a few years and we would see a new Nobel Prize winner. Others thought the same, not only myself. Then I met you here and there, and you always said that you were almost there, you only have to solve these very complicated equations.

My problem with this answer was there was this equation and mathematicians, they know that there are existence theorems, and they do not bother at all how the solution looks. We have the Schrödinger equation and we know for any complex molecule whatever you can in principle say that the wave function must exist. What is the problem with this equation?

Hasselmann: The problem is that the basic metron equations, the Einstein vacuum equations in a higher – eight – dimensional space, are nonlinear equations without an external source term. The hypothesis is that besides the trivial zero solution, the equations have nonlinear eigenvalue solutions of a special soliton type, for which there exists no analogy that I am aware of in other branches of physics. It is not at all clear whether or not the equations have non-trivial solutions. In the Schrödinger equation for the linear eigenfunction of the hydrogen atom, in contrast, the electromagnetic field that traps the eigenmode is given, as the electromagnetic field of the hydrogen nucleus. In the metron model, the trapping field is not given, but is generated by the trapped eigenmodes themselves, by their nonlinear radiation stress. It is not at all obvious whether the two sets of interacting fields, the trapped eigenmodes and the trapping field, a distortion of the higher dimensional metric, are mutually consistent, as I had hypothesized. In my 60th birthday talk and published papers, I demonstrated that solutions of this type do indeed exist for a much simpler scalar analogue of the Einstein equations, but the problem was to show that they exist also for the much more complicated Einstein tensor equations in eight dimensional space.

I believe that I can now indeed show that such solutions exist, by a numerical perturbation expansion, but only if one postulates that space is discretized at the smallest Planck scale. Or, alternatively, if one introduces an additional diffusion term into the Einstein equations that becomes effective only on the Planck scale.

Constructing the nonlinear eigenvalue solutions for the Einstein tensor equations in eight dimensional space was a complex task that took several years. I did this together with Susanne, who wrote the complicated code for the algebraic tensor manipulations. But there is still a long way to go. I have to show that the metron solutions reproduce all the symmetries of the Standard Model of elementary particles, including the 23 or so empirical constants. And I have to show, too, that the metron model is able to explain the enormous amount of empirical data on atomic spectra, scattering cross-sections, superconductivity and so forth that quantum theory has been able to explain in the last eighty years. So the metron model is really more a program than a theory. But if the program is successful, it will automatically unify gravity and microphysics and resolve the many conceptual problems and formal shortcomings, such as divergences, of quantum field theory.

You are referring to numerical solutions. Could it be that there is a convergence problem? So that someone comes along and says this is a numerical solution, I do not believe you.

Hasselmann: That is always a problem with numerical perturbation solutions. But this is not my main concern. I have computed the solutions to nine'th order, and they have every appearance of a well converging series. Once I have written up my results and have them off my chest, I will be happy to discuss existence problems with mathematicians. As an applied mathematician, I tend to be more sanguine about such issues. I have given many talks on the metron model to physicists, and there was never a concern about the formal existence of a numerical series that appeared to be converging. The reactions always concerned the basic ideas, whether they were only odd or outrageous.

I should like to give some more talks to different audiences with a social scientist in attendance. He or she could analyze the different reactions of the audience and correlate them with the various fields of the people that were making comments. The closer the person was to elementary particle physics, the more aggressive were the comments – not the more critical, which I expected and would have understood, but the more aggressive.

I think one of the problems is that as physicists, we have all been brain-washed into believing that quantum theory is an admittedly unusual, but the only possible way of resolving the wave-particle duality paradox of microphysics. Philosophically, one has not been able to refute the fundamental quantum theoretical rejection of the existence of particles or waves as real objective entities in the classical sense. One can object only on aesthetic grounds. Einstein objected strenuously, but did not offer an alternative solution. He is generally seen as having failed. It has even be argued, such as in Bell's famous no-go theorem, that it is in principle impossible to explain quantum phenomena by classical theories. However, it has been shown – although this is widely ignored – that these arguments are all based on the existence of an arrow of time, which is not acceptable for microphysical phenomena. Nevertheless, anybody who tries to propose a classical theory is swimming against a mighty mainstream.

But, finally, must it be that one of the theories is correct and the other one is incorrect? Or could it be that, as in the case of a spectral model or a grid-point model, they are simply different ways of finding the same solution.

Hasselmann: I don't think so. The way I see it is that the problem with quantum field theory is that the theory captures only half the truth, the wave aspect of the wave-particle duality problem. In the metron picture, both particles and fields exist as real objects in the classical sense. Particles are the source of the fields, which therefore do not exist independently, but only together with their particle sources. The different types of fields – electromagnetic, weak and strong – are basically the same as in quantum field theory. And the interactions between the fields are also essentially the same. In addition, the metron model has gravitational fields, since it is a unified theory encompassing all fields. But apart from the additional gravitational field, the field content of the metron model is essentially the same as that of quantum field theory.

The difference is that quantum field theory doesn't have the concept of a particle as a real existing object. It is thus forced to negate also the existence of fields as real objects. Fields are interpreted only as abstract operators acting on a Hilbert space of states. From these states one can infer probabilities for the outcome of experiments – which must be described, nevertheless, in terms of the particles whose existence one has just negated. This is the strange construct that creates not only philosophical unease, but also the technical difficulties of quantum field theory, the divergences and difficulties in unification with gravity. So I don't see the two theories converging to simply two mathematically equivalent pictures of the same physics.

HvS: I would suggest that you read Ludwik Fleck's book "Die Entstehung einer wissenschaftlichen Tatsache", because I think you are just in the centre of the storm which this guy is describing.

Hasselmann: Maybe I should. I had not experienced such strong antagonism before. I had expected scepticism, but not antagonism. I presented a talk at a physical colloquium in Oldenburg, and a couple of people sprung up afterwards and shouted that it was a scandal that somebody should give such a talk in a physical colloquium. It was almost a religious reaction. I felt I was in one of those pre-election political talk shows that sometimes get out of hand.

I had not experienced such violent antagonism before. When I first presented the nonlinear wave interaction theory, people like Bill Pearson or Francis Bretherton emphatically said I was all wrong, but this was in the normal civilized framework of people being sceptical and arguing. And the established SAR experts were critical but not outright hostile when I trespassed in their area to develop a theory for the SAR imaging of ocean waves. Traditional economists also showed only mild irritation, or simply smiled condescendingly, when I came up with alternative economic models. I suppose there was never this feeling that I was attacking anybody's foundations. The Oldenburg hecklers were – I suspect somewhat frustrated – elementary particle physicists.

Figure 21: With Hartmut Graßl, 1996.

HvS: This is just demonstrating for me very clearly that science is a social process. We are a social group, physicists of whatever, and we have certain rituals or ways of defining authorities, who is right or wrong. You were confronted with a different band that has different rules and their authorities try to defend their status. So I find it very brave of you that you changed roads. You had been in one band one of the chiefs. Then you suddenly decided that you would be one of these silly unimportant footsoldiers in another band.

Hasselmann: I find it is a lot of fun. As I say, what is the point of having a reputation if you cannot use it to play.

HvS: This Fleck book analyses what happens when science is in a phase when people just try to repair their knowledge claims. They are inventing new rules and refining old ones and so forth, even though the whole system is already wrong. Then it takes a while until it breaks down.

Hasselmann: I personally am convinced that quantum common field theory as it now exists will break down. That it is has basic problems nobody can seriously argue against.

I presume that you do not say that it is no good. It is good for a certain range of phenomena but then if you try to extend it as an explanatory tool to different phenomena, then it fails, it then needs to be re-written fundamentally.

Hasselmann: There is no doubt that quantum theory and quantum field theory work extremely well for a wide range of phenomena. But I think the problem is different from, say, Newtonian physics needing to be replaced by special relativity, or special relativity by general relativity. I believe that the problem of quantum field theory doesn't lie in the finite range of phenomena it can describe, characterized by some parameter range. It lies rather in the fundamental concepts as such, in the negation of the existence of real objects. Conceptualization in terms of real objects endowed with particular properties is, after all, the foundation not only of classical physics, but of all natural sciences since humankind has started to think scientifically.

But regarding the introduction of new ideas, I take solace in the famous physicist, I forget who it was, who observed that advances in physics are a natural phenomenon that takes care of itself. The old physicists die out and the young ones are not afraid of new ideas. I am encouraged that young physicists are much more open to my ideas.

I don't think that this is a problem of physicists, I think this is a problem of all scientists.

Hasselmann: Yes, of course, this is not limited to physicists or even scientists. People obviously build up their view of the world, everything, the interconnections, the values and so forth. And if that is being attacked they feel threatened.

Another question. What are perspectives on bringing numerical mathematics into the field of climate sciences? Do we need that? Would you expect that we can come up with better algorithms which will help us in a significant way?

Hasselmann: Well, I am not a theoretical numerical mathematician, but an applied numerical mathematician. I simply apply whatever mathematics offers to solve problems. In the particular area in which I work, I find that the numerical techniques that people use have not been developed by mathematicians for their particular application, but are general off-the-shelf methods that have been adapted by meteorologists or physicists for their particular application. When they find them inadequate, they improve them themselves, such as in the question of whether to use Lagrangian or Eulerian propagation schemes in atmospheric models, or whether to use spectral or grid-point representations. The modifications normally evolve from actual practical applications. There have been very few, to my knowledge, really original new ideas that mathematicians have applied to particular problems in our area.

There had been some attempts to use multi-grid or adaptable grids and so forth, but these are again off-the-shelf mathematical methods that the scientists simply apply and adapt as the need arises. Often the theoretically more accurate methods turn out to be computationally less efficient when applied in vector or parallel supercomputers, so that in most of the larger climate models one tends to find rather conventional numerical methods. I know of no real examples where theoretical numerical mathematicians have been called in to upgrade the numerical performance of models. But perhaps I am no longer up to date.

Apart from Klaus Hasselmann, who relied on Herrn Krause in 1961.

Hasselmann: Well, that is in fact just an example that underlines my point. I chose the appropriate numerical algorithms, for example for the treatment of the resonant delta-function factors in the integrand, and the mathematics student implemented them on the computer. It was basically all off-the-shelf.

I have one more question about the relationship with the media or the way scientist should/can/should not/cannot speak to the public through the media. You started as a climate physicist because you were curious to try out certain things, then you found it interesting to construct a wave model and things of that sort. Suddenly you are in the midst of a great public concern and public interest and the public is asking all kinds of questions. Could you tell us about how you experienced that?

Hasselmann: Most scientists are not well prepared to do this job. But it is an obligation for scientists to present their results to the public, as I think we all agree. The only way to present the results effectively to a broader public is through the media. This is particularly true if the results, as in the case of climate change, affect the policies that a country or the society as a whole needs to pursue.

Few scientists have the talent to interact with the media effectively. Fortunately, at the Max Planck Institute we have had two people that could that very well, and also liked doing it. One was Mojib Latif, who was in my group and is now Professor at the Leibnitz Institute of Ocean Sciences in Kiel. He is probably the publicly best-known climate scientist in Germany today. Everybody has seen his clear expositions of the climate problem on TV. The other is Hartmut Graßl, a co-director of the Max Planck Institute who succeeded Hans Hinzpeter as head of the air-sea interaction and atmospheric remote sensing group. Graßl was not only an equally effective communicator with the media, but was also heavily involved in advising policy makers, as chairman or member of various high level Federal advisory committees. For these activities he received the prestigious German Medal of Merit. Through the excellent communication activities of Latif and Graßl, much of the pressure of interacting with the media, public and policy makers was taken off my shoulders, although I also had to carry my share.

Figure 22: With Wolfgang Sell, Lennart Bengtsson and wife Susanne during emeritus dinner, November 1999.

This was sometimes a little frustrating, as the media like to report things that people like to read rather than what they should be reading, namely the facts. These can be rather boring, particularly if they are always the same, as they are for the slowly changing climate. So the media like to present extreme ideas that are not supported by the science community as a whole. The result is that the public tends to be rather confused regarding the climate change problem. But that is something that we have to live with.

Maybe one final question. It is quite personal. You sit on the beach in Sylt and you look out on the ocean, on the waves and on the climate and so on. You see the turbulence. You were in control of wave and climate studies in this early stage of the Max Planck Institute with all these small growing PhD students and then this later stage. What do you think, what period was the most satisfying for you? Were all of the same kind or is there anything which you said I was really satisfied with this.

Hasselmann: I enjoyed all of these phases in different fashions**.** I was always very satisfied when I discovered some new insight, or when something finally worked.

For example, I was exhilarated when I carried out the computation of the nonlinear energy transfer for the JONSWAP spectrum and compared it with the growth data, and they agreed precisely. It took us ten years of work before we achieved this result.

I was absolutely elated when I watched the launch of ERS-1 in Kouru in 1991. It was incredible that after all those many meetings in ESA, discussing an abstract project in endless variations in innumerable committees, the satellite really existed and was roaring up there into space.

And I was enthusiastic when ERS-1 began providing ocean wave images with the SAR, from which we could retrieve two-dimensional wave spectra using the algorithm we had developed. When Patrick Heimbach compared the first three years of retrieved wave spectra in his thesis with the spectra produced with the operational WAM model at ECMWF, he found very good overall agreement [139]. But he also discovered a slight shortcoming of the model, in the propagation of swell, which needed to be brought into closer agreement with the old results of the Pacific swell experiment. All this was very pleasing.

I was also emotionally strongly moved on my 60th birthday surprise colloquium, when suddenly all the people I had worked with in different fields from different countries over many years turned up and gave talks. I had never realized until then how fortunate I had been in experiencing so many rich friendships in my career.

But I also had many satisfactory experiences that did not have this delta-function characteristic. For example, the strengthening and dissemination of the stochastic forcing concept through a number of very nice PhD theses or post-doc papers, or the many influential detection and attribution papers that followed our first paper, in which we had come up with a quantitative estimate of the – very small – probability that the observed recent global warming could be attributed to natural variability. This led very soon to the general acceptance that anthropogenic global warming was real and had been detected.

In your list, you did not include the creation of the DKRZ.

Figure 23: Sailing in the Baltic, 1996.

Hasselmann: I did a lot of things that were simply my obligation as director of the Max Planck Institute, or as the member of some committee, but these were not things in which I was strongly involved emotionally. I pushed, for example, for ERS-1, in various committees – well, I guess I was emotionally involved there and did in fact battle with some lobbyists pushing other priorities. But one of the things that were simply necessary and didn't run into any opposition was the creation of the Climate Computing Center. This was, of course, a key component of the German, and later also the European, climate program, but not something for which I personally deserve particular credit.

You said, there were always two roles you played. One is the wage earner, just doing what you have to do; on the other hand you are the unruly scientist who is just following your curiosity. I guess the answers you gave just to those questions was the unruly part.

Hasselmann: Well, they were both parts. In fact, the successful parts were really the wage-earning parts. I believe most scientists, unless they are obviously geniuses, need to have a professional commitment to work in some field in which they can be reasonably sure to produce results that justify their salary. Climate, ocean waves and satellite remote sensing are three such typical fields. It is clear what needs to be done – within a spectrum of viable options – and if you work on the problems, you can expect to get useful results.

On the other hand, the things that really interested me, like turbulence theory or now quantum phenomena, were problems where it was not at all clear that one would ever be successful. If I were a young physicist today working officially in elementary particle theory, I would have great problems. It is quite clear that there is not an obvious road to a successful solution. But as a young scientist, you need to publish. So you have to jump on some bandwagon which the establishment has created, such as string theory, which joyfully leads everyone to nowhere.

So I think it is important – if you do not regard yourself as a genius – to have a serious obligation to society to do some useful research. This gives you the freedom to engage also in problems that cannot be solved from one day to the next, without the pressure of having to continually publish. But now that I am retired, of course, I am completely free to pursue these hobbies anyway.

Epilogue

While Hans von Storch was preparing the recording device, Klaus Hasselmann and Dirk Olbers were loitering on the 3rd floor gallery discussing the nice architecture (in German). A young man came asking politely (in English): “May I help you?”. Klaus Hasselmann, founder of the institute and director for 25 years, responded (in English): “No, thank you. We are just looking.”

Comment by Walter Munk, 20. January 2007

That is a very interesting interview. I came home last night with some other plans and found myself spending all evening reading the interview.

As Klaus says, we met at the Ocean Wave Conference in Easton in 1961, where Klaus presented his solution to the nonlinear interactions between wave components. As Klaus says (p.9): "Basically, I solved this problem to relieve my frustrations at not being able to solve the turbulence problem." He made the same statement at the start of his talk; there were people in the audience who had tried to solve this problem for years, and they were not pleased with this statement of a twenty-nine year old. Life was simple in the early sixties, and I was able to offer Klaus an Assistant Professorship before the conference had close.

I read K's memories of his early La Jolla days with enormous pleasure. Starting IGPP was certainly a highlight in Judith's and my life. Klaus' tenure, though short, contributed significantly to the subsequent success.

It was fun to read Klaus' account of the "Waves across the Pacific" expedition. Here our memories differ somewhat (but I need to emphasize that I don't have a good memory and that I am impressed with K’s ability to recall names of his former students and colleagues). The secret code should Gordon Groves at Palmyra (an unpopulated equatorial island) have a problem with the radio operator was "the Fourier integrals are not converging" rather than "the second amplifier had failed". The latter statement could well be true, but the former was sufficiently absurd to be a clear call for help. And in fact, he two men had had a serious fight, and we had to fire the radio operator and take him off the island.

At the time the realization that our summer surf originates in the Southern Hemisphere and may be antipodal was a surprise. It is now taken for granted by a large surfing community. I seem to suffer from an anti-podal obsession, and many years later from myself at Heard Island in the Indian Ocean transmitting low frequency sound to receivers half way around the world on both the American west and east coasts (connected by geodesics). That put us into the source region of the southern swell, and all our ten acoustic sources were demolished during a subsequent storm.

Returning to the interview, the casual reader may not appreciated the novelty of the stochastic forcing model of climate variability. My memory of the previous literature is that it consisted of wide variety of deterministic models.

Klaus tells about the hostility of SAR experts to his theory of imaging ocean waves by SAR. Nor did the ocean community welcome the arrival of satellite ocean observations. When

John Apel appeared at Scripps to sell SEASAT the reception was cool indeed; oceanography implied observations from ships, preferably sailing ships.

Klaus' keen sense of humor comes through the interview. He once gave a talk following Willard Pearson. Willard had the habit of starting and ending his talks with profession of great ignorance: "we hardly know anything yet ..." In anticipation, K's first slide showed Willard with his hands in the air saying: "we know nothing yet".

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Memberships in Societies

1. Foreign Member Swedish Academy of Science
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3. Honorary Member of the European Geophysical Union
4. Member of the European Academy of Science and Arts
5. Member of the Deutsche Meteorologische Gesellschaft
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