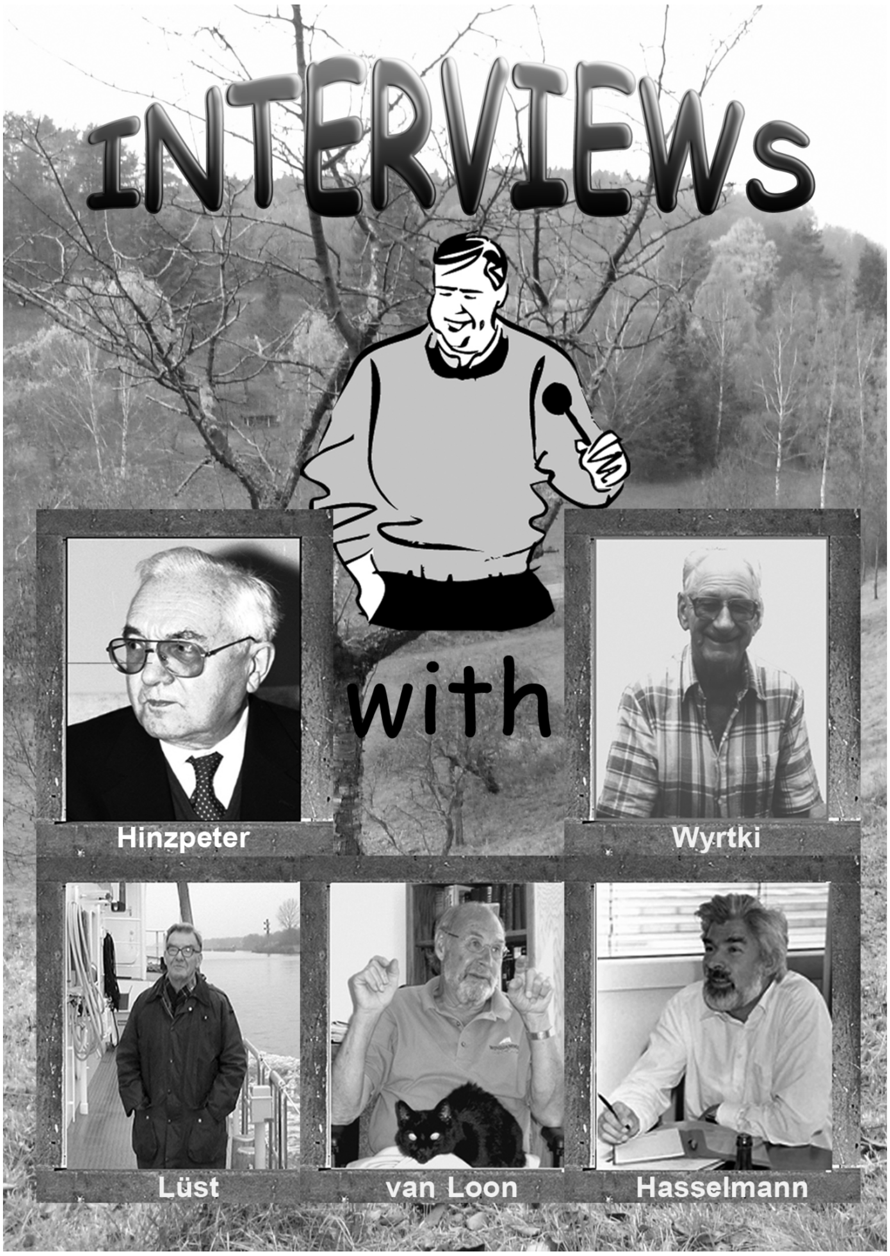
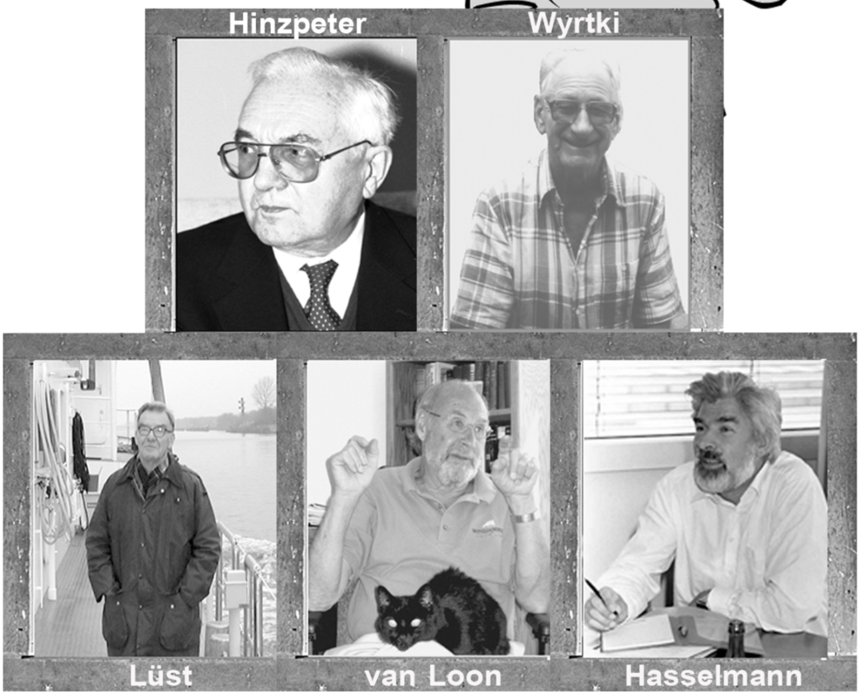

INTERVIEWs
with
Hinzpeter
Wyrtki
Lüst
van Loon
Hasselmann

INTERVIEWs

with



Preface

Science is a social system, which is carried forward by actors – and constrained by cultural conditioning, or, termed differently: by the „Zeitgeist". Thus, science is as much a personal, subjective effort as a quest to generate „objective" knowledge, or as the German terminology is: „Wissenschaft". The process of overcoming the cultural constraints, of the tacit intrusion of other, non-scientific forms of knowledge into the science-generating process, is an issue in the sociology of science; Ludwik Fleck's „Generation of a scientific fact" is a wonderful (albeit difficult to read) analysis of this sometimes difficult and lengthy rectifying process within science.

Here, the other side of the issue is addressed, namely the creative processes of suggesting hypotheses, of developing theories, of opening new avenues of thinking, of falsifying older ideas. We do not do this in a systematic process, which may end up in social science theory, but in a rather pragmatic approach, by presenting examples – case studies of eminent scientists. We interviewed (mostly) climate scientists, who, after 50 and more years of professional experience are looking back on a rich scientific life, to to their errors and achievements and the errors and achievements of their colleagues, to changing conditions in which science is conducted and to new technological opportunities (in particular related to the availability of satellites and remote sensing techniques and computers).

To date, five interviews have been conducted – the choice of the interviewed scientists was entirely subjective – based on personal acquaintance, scientific respect and, of course, availability. They have been published on the internet, but a collection in a single volume seems a good idea to me. More interviews are planned and a sixth is currently being transcribed; others are in preparation. Consequently we hope that in a few years enough material will be available for a second volume.

The purpose of the interview is to speak about the interviewee's perception of what has happened in the past decades. The issue is mostly climate science but also the general conditions for science in the course of time.

The format of the interviews and their editing is as such:

After the initial agreement, I seek one or two co-interviewers, who know the interviewee well in terms of his field and his background (so far only males).

Then, we all sit together with our "victim" and, without being recorded on tape, discuss issues which may be included in the more formal interview. This part of the interview is essential, as it serves as a warm-up for everybody. This more informal discussion often leads to disclosure of surprising details; to the serendipitous discovery of further issues. It is often humorous, pleasant and inspiring – and relaxed, because I do not need to worry that for whatever reasons the tape recorder fails (what it did once …). I noticed that our interviewees enjoyed speaking about old times and about what it may mean for today.

Following the informal acquaintance with the topic, tape recorder turned on. Usually, we speak for several hours; sometimes in several sessions. Purely personal questions, e,.g. related to childhood, marital issues, children and health, were not raised. The issue is the scientific personality.

The difficult part follows – transcribing the interview, which typically consists of several tapes. I have to admit that I seem to add significantly to this difficulty, as my speaking is far from disciplined and clear. (When Klaus Hasselmann is participating the situation becomes even worse.) After a first rough editing by me, a fine editing follows with heavy involvement of the interviewee and the co-interviewers.

The purpose of this editing is not only to delete less interesting parts, to avoid misunderstandings, but also to allow the interviewee to tell his story in his way. We deliberately made no efforts to check if the interviewee made correct statements or if others would contest statements made and descriptions provided. The interviews contain only subjective knowledge. My experience is, however, that none of the scientists significantly changed their taped statements; hardly any statements have been contested. It seems that the accounts given are mostly consistent with reality.

To date five interviews have been taped and published.

1) In February 1996, Hans Hinzpeter, who was professor for meteorology at University of Hamburg and director of the Max-Planck-Institute of Meteorology, was the first interviewee. My co-interviewer was Klaus Fraedrich; the transcription was done by Ursula Fiebig. The interview was first published as an internal report of the Max-Planck-Institute of Meteorology.

Unfortunately, Hans Hinzpeter died in December 2000.

2) Klaus Wyrtki became my second "victim" on 25 February 1999, whom I interviewed together with Jürgen Sündermann and Lorenz Magaard in his apartment in Honolulu. The oceanographer Klaus Wyrtki was a pioneer in studying El Nino dynamics and sea level variations. He withdrew from active science in 1994.

This interview, as the following three, was transcribed by Ilona Liesner and first published as report of the GKSS Research Center.

3) Reimar Lüst is not a climate scientist, but was as former president of the Max Planck Society and instrumental in founding the Max Planck Institute of Meteorology. As a former neighbor down the hallway, Reimar Lüst saved me once when an overzealous management tried to discipline me.

My co-interviewer was Klaus Hasselmann; the interview was conducted on 2 December 2000 on-board RV "Ludwig Prandtl" on the Kiel Canal – recognizing the early challenging maritime episode in Reimar Lüst's life.

4) The forth interview was with Harry van Loon, who was difficult to convince that he would be a reasonable candidate for our series. In his characteristic modesty, this Danish-US gentleman pointed to many other scientists who would deserve attention much more than himself. But finally, we were successful, and George Kiladis, Rol Madden and I finally met Harry van Loon in George Kiladis' home in Boulder on 4 September 2004. The interview led to an interesting account of the history of the National Center for Atmospheric Research (NCAR) in Boulder, on research on ENSO, the NAO and the solar influence of climate.

5) The last of the series of five interviews was conducted with Klaus Hasselmann, the interviewee, on 15 February 2006 in the then new

building of the Center for Marine and Atmospheric Sciences (ZMAW) in Hamburg. Co-interviewer was Dirk Olbers. Klaus Hasselmann became famous for his contributions to ocean wave dynamics, remote sensing, climate dynamics and climate modeling.

The interviews are freely available on the internet; some are stored at the Niels-Bohr Library and Archives of the Center for History of Physics. Here I have brought them together, fully illustrated, with the help of Beate Gardeike.

Another detailed interview was done with Walter Munk – von Storch, H., and K. Hasselmann, 2010: *Seventy Years of Exploration in Oceanography. A prolonged weekend discussion with Walter Munk*. Springer Publisher, 137pp, DOI 10.1007/978-3-642-12087-9 – by now, the pdf of this book is also freely available on the internet, for instance here:

http://www.academia.edu/1541025/Seventy_Years_of_Exploration_in_Oceanography._A_prolonged_weekend_discussion_with_Walter_Munk.

To some extent, the interviews with Klaus Hasselmann in this collection and the one with Walter Munk match, as they describe similar episodes. Also the interviews of Reimar Lüst, Klaus Hasselmann and Hans Hinzpeter illuminate partly the same events.

Hamburg, 6 January 2013

Prof. Dr. Hans von Storch

Contents

Interview with Hans Hinzpeter

prepared by Hans von Storch and Klaus Fraedrich
in spring 1995

Question: Mr. Hinzpeter, on 31st January 1996 you will turn 75. You have been dedicating yourself to meteorology already as a young man and have thus witnessed crucial developments in this science. What were your first impressions?

Hinzpeter: I joined the air force during the war when I had just entered the job market and was a young meteorologist. Precise formulation was necessary to give useful advice. A typical forecast of a front was: Approaching cirrus will reach the airfield at 13 o'clock, at 16 o'clock the lower level of the clouds will have sunk to 3000 metres and at 17 o'clock it will start raining. Such a prognosis was based on insufficient data and partly also on questionable methods. Nevertheless, I found that challenging, and the need to formulate accurately as very beneficial in forcing one to think precisely.

Hans von Storch, Hans Hinzpeter and Klaus Fraedrich

I came across also other predictions for larger areas and a longer periods. Inevitably, these were less accurate and would have mislead me into vague thinking.

Having beceme acquainted also with other branches of meteorology, however, I was disappointed by the prevailing, mostly non-reproducible forecasts, especially since even eminent meteorologists considered weather forecasting to exhibit more the character of an art than an exact science. Therefore, I wanted to study physics after the war, and had saved the necessary money. After the war, however, the occupying powers cleared all bank accounts, and so I could not carry out my intention.

I had to earn money. First I worked as an assistant teacher at a grammar school. After several unsuccessful applications I finally managed to find employment in the radiation research division of the Meteorological Observatory Potsdam.

After all, that was very different from the meteorology you knew and from what you were presumably taught during your studies?

Hinzpeter: That is certainly true. The Potsdam Observatory and especially Mr. Feußner, the head of radiation research, had a good international reputation, and during these years I learnt a lot about the experimental technique of radiation measurement, but also about radiative transport – Chandrasekhar's book "Radiative Transfer" was published in 1950. In general, Potsdam had a very favorable scientific climate at that time, if one disregards the political constraints. The Geomagnetic Institute, the Geodetic Institute and the Astrophysical Institute were on the same grounds, so that there were quite a number of inspiring colloquia, not only on meteorology. There were many young scientists at the institutes. After the years of war, everyone was striving to do scientific work as independently as possible. It was a very pleasant time.

Hinzpeter 1967

Nevertheless, you went to Dresden. How did this come about?

Hinzpeter: I cannot really tell. When you enjoy science, you work, you are interested in all kinds of new questions, and you do not think about what today one would call a career. Then, I was asked whether I wanted to take over the management of the observatory in Wahnsdorf near Dresden. I went there because I was tempted by the greater independence. A disadvantage was the larger distance to West Berlin, to which we still could go unhindered in those days. But at the same time there was a larger distance from the office in Potsdam, which was more politically biased.

I had to adapt to new tasks at the institute in Wahndorf, since the work there included not only radiation observations but also air-chemical analyses of the natural and artificial radioactivity of the air. It was the time of the H-bomb tests, and we were concerned with the retention time of natural radioactivity in the atmosphere and the spectrum of the hot particles originating from H-bomb tests. In addition, the observatory operated an air chemical measurement network which monitored, among other things, the variation of the near-surface ozone concentration.. In those days, I thought this was rather uninteresting and should better be the task of a hygienic institute. Today these observations belong to the longest series of ozone observations and have a very topical scientific value.

Although a reasonable political climate prevailed at the observatory itself, the general situation at that time had deteriorated. It was the period of the "peasant clearance", when even the last small farmers were forced into the agricultural cooperatives. The directors of institutes were also supposed to visit peasants and subject them to moral pressure. Even though I could avoid that, the general situation had become quite unpleasant.

> *You then transferred to the Institute of Marine Sciences in Kiel which is, after all, a completely different field of activity. What was your reason?*

Hinzpeter: Through my work on radiation, I had been able to become acquainted with Fritz Möller, a professor in Munich, originally from Thuringen in East Germany, who was very open minded towards young scientists in the GDR. He nominated me for election into the IAMAP International Radiation Commission, which gave me the opportunity to participate in conferences in western countries. In the beginning of August 1961 I went to a meeting of the Ozone Commission in Arosa and afterwards to a meeting of the Radiation Commission in Vienna. During this time the GDR erected the Wall. Not without some scruples, I decided to stay in the west, which was possible only because my family was in Freiburg in the west at that time.

Through my radiation research, I already had developed some relation to marine research. At the beginning of the fifties, Mr. Georgi from Hamburg paid us a visit in Potsdam, in order to compare his radiation instruments with the standards in Potsdam. Georgi had overwintered in „Eismitte“ during Wegener’s Greenland expedition, and thanks to this experience he had been able to establish good relations with the French Greenland expedition. He asked me whether I would join a French expedition to Greenland. I accepted enthusiastically, the French approved, but the Danish Ministry of Greenland refused a visa. At that time the International Geophysical Year was being prepared, and in a shipyard in Rostock the research vessel “Lomonossow” was being built for the Soviet Union. Probably provoked by the failure to participate in the Greenland expedition, the administration of the (East-German) Meteorological Office achieved a participation of GDR scientists in maritime expeditions of the “Lomonossow”. That way, in 1958 I became a participant in a 4-months expedition in the North Atlantic. Apart from measurements of the radiation and energy budgets I mainly dealt with diurnal temperature variations and their explanation by the divergence of short-wave and long-wave radiation fluxes. Such an expedition is also marked by months of monotony, because the sea looks the same everywhere, and the measurements gain their value by constant repetition. Thus, I also had gained some - admittedly small - understanding of maritime meteorology.

I encountered favorable conditions in Kiel. Succeeding Mr. Wüst[1], Mr. Dietrich had become director of the Marine Research Institute, and on that occasion a division for maritime meteorology had been established, headed by the young Defant, the son of Albert Defant. I acted as assistant in this division, which meant adapting again to new conditions, , but at least I could work again. However, the change from being the director of an institute to an assistantship was not always easy for me.

You made a new start in Kiel, while in Hamburg there was the Meteorological Institute which had been dealing successfully with issues of maritime meteorology for many years. Was this not a difficult situation?

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[1] See also the interview with Klaus Wyrtki in this volume. Professor Wyrtki earned his doctorate under the supervision of Professor Wüst in Kiel.

1983, Antarctic

Demonstration of the start of a radiosonde on board „Polarstern“

Polish station „Arctowski“

Russian station „Drunaya“

Hinzpeter: Yes and no. We began new in Kiel and did not have any equipment at first. However, everyone at the Hamburg institute provided excellent support. Nevertheless, I was looking for a field of activity which was not already covered in Hamburg. Based on my experience gained on the expeditions, I decided to study the viscous boundary layers at the air-sea- interface, and its impact on temperature variation at the water surface, and to determine the divergence of long-wave radiation near the water surface. Afterwards I obtained my Habilitation[2] with the results of this work. During that time I participated in two expeditions, the Indian Ocean Expedition and the Trade Wind expedition in the Atlantic. After that I spent almost a year at the Meteorological Institute of UCLA. I gave lectures there following a prescribed concept, but, in doing so, I learnt a lot.

Yet, you were soon to change institution and field of activity again. Why was that?

1965, Kiel

On board Kiel´s research vessel with students from Kiel

Hinzpeter: I was very happy in Kiel, but the situation – I had become senior assistant in the meantime – could not satisfy me in the long run, after my independent position in Dresden. When I was offered to head a small, but independent institute in Freiburg, I accepted the call. As I had come into contact with turbulence during my work in Kiel, in Freiburg I wanted to examine turbulent transports above forests and the interaction between the turbulence field and the forest. For this purpose, we set up a research station above a young spruce forest in the Rhein meadows. As I spent only two

[2] A traditional degree in German academic life. Usually received a few years after the doctorate-degree, it formally qualifies for professor positions. There have been attempts to abandon the Habilitation, but it seem deeply rooted in the academic system, and young scientists still apply for the degree in the hope of improving their chances when searching professor positions.

years there, and the Trade Wind experiment I mentioned took place during that time, I failed to really complete the planned study. The only achievement was the development of an instrument for measuring the turbulent heat flux. In my measurements of the profiles of wind, temperature and heat flux, the impact of the very small heat capacity of the fir needles surprised me. When the sky was almost clear and the sun was high, the heat flux was, of course directed upwards. However, when a small cumulus veiled the sun, the direction of the heat flux abruptly reversed, only to immediately change its direction again when the sky cleared.

When I accepted an offer by the Meteorological Institute at Mainz University, I knew that they had already a strong group dealing with atmospheric radiation, and that this subject was also covered at other German universities. On the other hand, the question of turbulence and its impact on the processes in the boundary layer had not been treated in detail in the Federal Republic. Therefore, I built up a small group in Mainz dealing especially with processes in boundary layers. I myself worked on the damping of turbulence by long-wave radiation.

1969, Freiburg

Attempt to gain experimental experience in a glider

Because of my participation in several maritime expeditions, Mr. Brocks [Professor of Meteorology in Hamburg] asked me to join GATE (the GARP [Global Atmospheric Reseach Program] Tropical

Atlantic Experiment) and help in its preparation. After the death of Mr. Brocks I became responsible for the international cooperation of the three vessels participating in the experiment. Thus I became involved in management. Among my tasks was to set up a German group in the international steering centre in Dakar and later to organize the analysis of this very large experiment.

An attempt to call me to Munich University failed, because at that time I had spent only two years in Mainz, and the Ministry in Mainz did not agree to the change. But after having stayed in Mainz for five years, I could then accept an offer by the University of Hamburg.

1979, Peking

With the delegation of the Max-Planck Society in Peking

Hamburg had a very strong group working on maritime meteorology, which was internationally well-known for its very accurate experimental studies. It was also supported by a large Collaborative Research Centre[3], so that the financial conditions were attractive. At first, this group had also been supported by a Fraunhofer Institute, which was later closed and partially taken over by the new Max Planck Institute for Meteorology in Hamburg.[4] This offered great future prospects; so I went to Hamburg and stayed there until I re-

[3] "Sonderforschungsbereich"

[4] See interview in this volume with Klaus Hasselmann, who speaks about this process in more detail.

ceived emeritus status. Following Mr. Hasselmann, I became speaker of the Collaborative Research Centre.

In spite of the favourable conditions in Hamburg, it was not easy for me to leave the very inspiring scientific environment in Mainz.

Did you get engaged into scientific management even more strongly then?

Hinzpeter: This is certainly true. During that time I was managing director of the university institute and director of the Max Planck Institute in the division Atmospheric Physics, as well as speaker of the Collaborative Research Centre. After Mr. Möller had received emeritus status, I had also taken on the chair of the German GARP Committee.

The chair of the Meteorological Society presents Prof. Möller with the „Alfred Wegener Medal“

But another function was added. I had noticed, already back in Kiel, a significant difference in the style of communication among meteorologists and oceanographers in Germany. The personal exchange of ideas between meteorologists was limited to the conversation between tenured professors at the meetings of the scientific advisory board of the weather service twice a year. The Priority Programme[5] of the oceanographic community, in contrast, enabled the integration of all scientists in the scientific exchange of ideas. Because of the

[5] „Schwerpunktprogramm“

increased involvement of meteorologists in international programmes, it became possible to establish a Senate Commission on Atmospheric Sciences with the German Research Foundation DGF[6], which had quite a positive influence on the development of meteorology in Germany. As initiator I had to take the chair of this commission, too.

Today, however, through the greatly increased funding of climate research through the Federal Ministry for Research and Technology[7] BMFT, the significance of the DFG for climate research has been strongly reduced.

You still remained active after receiving emeritus status. How did that come about?

Hinzpeter: After I became emeritus I wanted to concentrate on the issue of cloud-radiation interaction. We had dealt theoretically, and also as in the framework of larger experiments, with the development of clouds in the boundary layer. The problem is not satisfactorily solved, “however” for application in climate models.

1984, KONTROL

Husum airport

But then, totally unexpected by me, there suddenly came the opportunity for the reunification of the GDR and the Federal Republic. I had grown up in a unified Germany, and for me the division of Germany had been a disaster. The reunification was very fortunate

[6] Deutsche Forschungsgemeinschaft
[7] Bundesministerium für Forschung und Technology

for me. When at the age of 70 I was asked to participate in evaluating and re-establishing scientific institutes in the former GDR, I gladly accepted the challenge, probably also because, due to my past, I thought I could understand the situation of the people there better than someone who had grown up in the west.

I cooperated in the evaluation of the observatories of the Meteorological Service and of university and academy institutes, which pursued environmental research in the broader sense. The result were proposals to found the Institute of Atmospheric Physics, the Baltic Sea Research Institute, and the Institute of Tropospheric Research. I am still a member of advisory boards and boards of trustees of these institutes. It is still interesting and I hope I am still able to help. However, as I am not chairman of these boards, the work load is relatively light.

Unfortunately, I was unable to secure my former observatory near Dresden, because its profile was inconsistent with the functions of a weather service according to the weather service law of the Federal Republic.

During the establishment of the Institute of Atmospheric Physics in Kühlungsborn, I worked as founding director until April 1993. The main work, however, was done by Professor Schmitz.

What was the procedure with such an evaluation and re-establishment?

Hinzpeter: Among other things, the Scientific Council[8] had formed a group to visit and evaluate institutes working in the field of environmental research. The members and especially the chairman of the group were very objective and anxious to understand the difficult situation of those to be evaluated. I perfectly understood that GDR citizens, who had arranged their lives in the GDR, after having heard from the Federal Republic for many years that the GDR was not to be destabilized, We therefore limited ourselves to evaluating the scientific potential and did not ask for party affiliation and the like.

Nevertheless, our task was difficult. We had only one day to visit and evaluate an institute with 100 employees. Part of the decision

[8] „Wissenschaftsrat“

was to determine how many scientists and technicians should belong to an institution which should be re-founded. This number was always smaller than that in the former GDR institute. Sometimes we felt this task was unacceptable, and many asked themselves whether they should cooperate. We finally told ourselves that if we withdrew, others would take our place who would not make it better. I think, however, that we solved the problem reasonably well, in spite of the need to finish the process by June 1991.

The number of re-foundations proposed by the different evaluation groups was undoubtedly larger than the financial bounds set by the politics. The resolution of this problem was left to the Scientific Council, which included the chairmen of the evaluation groups. .

After the evaluations were concluded, I heard nothing further about the matter until in August 1991, when I was invited to participate in the Founding Committee for the Baltic Sea Research Institute. In October 1991, the state ministries[9] in charge and the BMFT asked me to chair the Founding Committees for the Institute of Tropospheric Research in Leipzig and the Institute of Atmospheric Physics in Kühlungsborn.

In the short time from mid-October until shortly before Christmas, we had to establish the institute in Kühlungsborn, to advertise the positions, to evaluate and decide upon the applications, and to fix the budget, including the salaries. This was quite a strange period. Other hard decisions also had to be made. According to the Scientific Council's recommendations, 10% of the scientific positions should be reserved for applicants from the west, and 20% of all positions should be filled only temporarily, in order to leave some freedom inl appointmenst for the directors who were to be elected in the future. Further important decisions had to be made with the goal of achieving a reasonable age structure. The institute was finally founded, , but other interests also entered.

Were these also of a party-political kind?

Hinzpeter: In a certain sense, yes. The State of Mecklenburg was quite formal in this respect. It wanted to ensure that scientists who

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[9] „Landesministerien“

had been dismissed from the university for party-political reasons would not find a position in the newly founded non-university research centres. Therefore the performance of every single applicant during the last 30 years had to be assessed. Rules were established for which sanctions were to be imposed. The rules were very formal. For instance, a former party secretary had either to be barred from managerial functions for several years or to be removed from the institute. In the case of a smaller institute with, e.g., only 10 party members, of course, everyone had once been party secretary. As founding director you automatically belonged to the honour commission[10] which had to assess the behaviour of the institute's members. That was an unpleasant time. Other states acted in a less formal way.

13./14. October 1994

Laying the foundation stone for the building of the institute in Kühlungsborn

A different style of working prevailed in the GDR. In what way did that affect the re-foundations of the institutes?

Hinzpeter: In the scientific field, I hardly noticed any difference in the way of working. Of course, there were institutes of different quality.

But let me come back to the institute in Kühlungsborn. During the crucial years 1990 and 1991 a wide range of good manuscripts was published by the scientists of the institute, to a large part in American journals, although this time was certainly very irksome for the

[10] Ehrenkommission

members of the institute, who had to adapt to the new extended bureaucracy.

You were also concerned with the observatories of the GDR's Meteorological Service. Can you comment on that?

Hinzpeter: Before World War I and partly still after World War II, the observatories Lindenberg and Potsdam had a very high international reputation. I was against closing both or even one of them, and I was committed to preserve them and to equip with modern instrumentation. They were finally maintained, thanks to the head of the environmental evaluation group.

As I had been working in Potsdam until 1958, I met many familiar colleagues again. This happened totally without prejudice, perhaps because I had grown up in the original unified Germany and thus had neither a West German nor an East German identity.

You took part in initializing fundamental climate research. How do you judge the significance of the models for climate science?

Hinzpeter: The climate models have developed from the weather prediction models via general circulation models. As I said, I was disappointed by the rules and methods of forecasting weather in the forties, and that is why I wanted to study physics. I have since seen an extraordinary improvement in the development of numerical weather forecasting Even if numerical predictions were not better than the classical ones, they were superior, because of their reproducibility – or at least their improved reproducibility. I was quite impressed by Smagorinski's work, which was published in 1964. It mainly provided a synthetic climatology. Although, of course, if the water surface temperature is given, the result must be more or less correct. It was clear that it would need to be coupled to an ocean model. It took, however, nearly another 30 years to overcome the inherent difficulties of doing so.

Another event impressed me very much. The first satellite film was shown at the IUGG Conference in Helsinki. It was a camera film shot from a non-stabilised satellite. The pictures taken from the irregularly swaying satellite were confusing. But suddenly the spiral cloud picture of a large low pressure area could be discerned.

Though it was possible to construct such a picture from ground observations, this image was quite convincing. The following developments produced an important tool for medium-term weather forecast and for the assessment of the radiation budget.

When later five geostationary and two polar circulating satellites were planned, I became concerned that one central office would be created to collect the data necessary for a global weather forecast, and thus only one centre for the forecast for all regions of the world would emerge. This was a very theoretical idea. I similarly thought it would be sufficient to collect all necessary meteorologists in one place, and to provide forecasts for all places in the world from there. This was at least premature, because even if numerical forecasts are reproducible, the predictions themselves have not yet reached that stage. The conclusions a meteorologist draws from the fields of wind, temperature and pressure will be different for the islands in the North Sea and for the Alpine foothills. Experience gathered on site still plays an important role. Further progress in Model Output Statistics, however, will gradually reduce the importance of experience.

> *You also addressed the issue of satellite development, a field that should be of special interest to you who comes from radiation research. Can you tell us anything more about that?*

Hinzpeter: I think remote sensing possibilities are not nearly exhausted. So far, for reasons of energy supply, almost exclusively passive remote sensing methods are used on satellites. Profiles of temperature and water vapor are deduced from spectral measurements in the CO_2 band and the water vapour bands. For this purpose, integrals must be inverted, in which the relevant matrices are very badly conditioned This is still very unsatisfactory today, so that methods of active remote sensing will no doubt be more used in the future. For example, the profile of backscatter yields estimates of aerosol layers, which are almost always correlated with changes in the temperature gradient. That alone could improve the present methods. Adding line width and Doppler shift, the profiles of temperature and wind would become even better, independent of cloud occurrence. This requires a larger energy source on the satellite, but with the active radar used on the ERS 1, this possibility has been demonstrated.

Perhaps I should insert a word on the importance of geostationary satellites. The images obtained in the visual range replace many-hours of lecturing on climatology and general circulation. A series of such images clearly illustrates the variability of the circulation.

I am more cautious about the possibility of deriving fluxes at the ocean-atmosphere interface from satellite data. Many parameterizations enter here. Relevant is not whether one can obtain high skill in determining the fluxes averaged over all oceans. This is not difficult, because most of the area is represented by subtropical seas, where there is hardly any weather but only climate. In general, it should always be checked to what extent the fluxes gained by satellite are better than the fluxes calculated on the basis of other data - clouds, geostrophic wind, classical determination of water temperature.

Can we get back to the models again?

Hinzpeter: Climate models, as well as their builders, face more difficult challenges than weather forecast models. The latter can be examined and, if necessary, improved every day in different weather situations on the basis of daily comparisons of prediction and reality. The climate models are much more complex; verifiable predictions are impossible. One can verify only the simulation of the global temperature change observed during the last 130 years. For many issues, however, the change in precipitation rates is more important than the temperature. This applies also to my somewahat cautious assessment of predictions based on changes in carbon dioxide concentration.

In my view the problem of cloud formation and dispersal has not yet been solved satisfactorily in climate models. Nevertheless, the further development of climate models is necessary, because they are the only possibility to improve our knowledge. With the help of such models we have greatly improved our understanding of climate, especially regarding the role of the ocean. However, we also learnt that there may be quasiperiodic variations with periods of several hundred years in the ocean. Their amplitude and phases are unknown for the present and are an uncertainty factor in climate forecasts.

What do you think of the significance of atmospheric trace gases in general?

Hinzpeter: I have thoroughly changed my views on that during the past 20 years.[11] We now know of a whole series of trace gases which are of similar importance as CO_2 regarding the reinforcement of the greenhouse effect. They must be considered in the models. Another trace substance are aerosol particles which might partly compensate the greenhouse effect. The increase of condensation nuclei can lead to a change in the cloud drop spectrum, to a larger number of drops, and thus to an increased reflectivity in some types of clouds. At least over the oceans, an increased number of aerosol particles alone - without clouds – would cause a similar effect. It is difficult to extract the impact of, e.g., the altered concentration of a trace substance from the results obtained with a very complex climate model. In view of this great complexity the development of simple, transparent models should not be neglected. In the former GDR, for example, simpler models were generated which could more easily be used to gain basic insights.

Climate impact research is presently much called for. How do you see this?

Hinzpeter: This research should have been intensified already 15 years ago. Unfortunately, there was nobody who focused on it the way Mr. Hasselmann focused on climate research. It was often said that it was necessary to wait until the models could produce reliable results on the impact of an increase in CO_2 on climate change in, e.g., Bavaria. I never shared this opinion. I I always considered it advisable to specify plausible scenarios for precipitation and temperature and to establish the resultant impact on plant growth, harvests, etc. However, this is not yet a description of atmosphere/biosphere interaction. Presently, studies of the cycles of matter related to the biosphere are developing, so that climate impact research can be expected to expand in this direction.

Since when have you been dealing with climate issues? Were you not a meteorologist earlier and have become a climate researcher only recently?

[11] The interview was taped in 1995.

Hinzpeter: If you define meteorologist as weather forecaster, I can understand your question. However, for me the term meteorologist includes climate researcher. At the beginning, I said that I had been disappointed by the qualitative methods of synoptic meteorology. If I had gone to the weather service, I would have tried to work in the climate division, because they work with quantitative methods there and thus obtain reproducible statements. Especially with modern statistical methods interesting problems can be studied. Of course, this is not directly related to climate models. But climate research is more than climate modeling.

On board "Meteor"

1965 –search for buoys: Grassl is searching

1969, APEX

I do not see myself as a climate researcher. My colleagues and I have examined the development of boundary layer clouds with the aid of experiments as well as with models. The results, of course, were also of significance for climate models. I took part in developing the logistics of fundamental climate research. Based on my experience in the field of marine research, where the research vessel "Meteor" was administered by a governmental institution and was at the scientists' disposal during 50% of its operating time, I was in favour of providing and operating the necessary large-capacity computer by a governmental institution, so that the scientists would not be burdened with the associated logistic questions. However, a different and by today's standards better way was chosen.

In addition, I have always been of the opinion that the task of fundamental climate research is to examine climate variations and to separate between system immanent components and components caused by modified boundary conditions. Among the basic insights, which were not linked to climate models, was that of Mr. Hasselmann, showing that a white noise atmosphere induces red noise in the thermally inert ocean. Mr. Mikolajewicz then showed numerically that in an Atlantic model forced by white noise quasi-periodic variations of the circulation of several hundred years emerge. We do not know, however, in which phase the ocean is presently in.

What do you think will be the most interesting developments in the next 10 years?

Hinzpeter: I can only make assumptions here. The climate models' development was made possible by computers with increasing capacities. In future, parallel computers will become more important, and that will allow further models' advancements. The presently still unsatisfactory performance with respect to small regions will be improved, and the correct integration of further trace gases as well as the gas exchange between biosphere and atmosphere will be taken into account. Keeping in mind, how significant the development of computer technology -, which could not be foreseen 20 years ago - has been, for climate modeling, I am cautious with prognoses.

Are there other works which made a special impression on you?

Hinzpeter: There was certainly the work of Ed. Lorenz. Initially I saw its relevance mostly for the limits of weather forecasting. The additional fundamental meaning I perceived only later.

We want to ask a question concerning scientific policy. How do you judge such institutions as, e.g., the IPCC?

Hinzpeter: I have to restrain myself, because I cannot really judge this. It is an institution for advising the UNO, i.e., acting in the political arena. In the Federal Republic, climate research is supported by the BMFT. I always see in such a situation the danger that a certain policy is supported, and results are interpreted, according to the governing ideology. But I want to be careful and not judge anything.

I once was requested to comment on the issue of a possible intensification of storms and their implications for the insurance industry. We climatologists find ourselves in a different role than 20 years ago. The public is interested in the results of our work, and this changes our self-image. Did you have similar experiences?

Hinzpeter: Climatologists had to submit statements and reports in the past, too. Today, many of them are asked for statements for which they have to rely on climate predictions. In addition, radio and TV journalists address this question to a much larger extent than before. In these cases it is essential to unemotionally describe the obtained results as well as their limitations and uncertainties.

Did you act in public yourself? Did you give interviews concerning these problems?

Hinzpeter: I always watched climate research with great interest, but that does not qualify me as a climate researcher. As I said, we dealt with boundary layer clouds, and also with remote sensing. Therefore, I always referred people who asked me for an interview to other, more competent persons.

From my point of view, advice to politicians must be limited to the presentation of our knowledge, the obtained results, and the uncertainty those are afflicted with. The politician, not the scientist, must make decisions on the basis of this information. As information is never complete, and the estimations for the future are afflicted with uncertainty, these decisions may, of course, be wrong. Otherwise they would not be decisions. In my opinion, the development of the automobile is a good example. If 100 years ago the proposal had been made to develop a means of transportation, designed to increase individual mobility and thus individual freedom, this proposal would have been welcomed enthusiastically. If, however, the proposal would have included an increase of the number of deaths on the road, we may imagine what would have happened to this proposal in an emotionally stirred time like today. Nowadays, we lose the inhabitants of a medium-sized town to traffic each year, and we live with that.

In addition, the politician must also balance causes and prioritize. Do I spend several billions on a flight to the moon, or do I use this money for social purposes?

Is it good for science that there are so many questions related to applications, and that science partly turns into project science, so that the sponsoring institutions only support basic research as far as it is necessary?

Hinzpeter: This question cannot simply be answered yes or no. When a discipline is sponsored with large sums, and there is a large public interest in its object of research, there is a certain pressure for justification. This pressure strongly depends on public interest.

In addition, the term basic research is a little vague. It is often used to describe pure research, which is an important feature of our western culture and the results of which are, in our experience, often used for applied research many years later. Of course, the BMFT must apply other criteria in research funding than, e.g., the DFG or the Max Planck Society. I somewhat regret that the DFG has lost some of its importance for our field.

How would you compare German research funding to that in other countries?

Hinzpeter: Some other countries invest more into research, although less so in our field. A serious disadvantage for our country is the animosity against science which prevails in public discussions and, in a large part, in public opinion. It is concerning that too few people clearly oppose this animosity with strong statements.

What will become of the Centre for Marine and Atmospheric Sciences ZMAW[12]?

Hinzpeter: I do not know. I initiated it some time ago, and Mr. Sündermann has promoted it energetically. Its realization was then delayed for a long time, and now funds are scarce everywhere. One

[12] Zentrum für Marine und Atmosphärische Wissenschaften in Hamburg. The ZMAW represents an institutionalised cooperation between the geoscience institutes of the University of Hamburg and the Max Planck Institute of Meteorology. The ZMAW was created in ??? to combine and strengthen Hamburg's existing expertise in marine, climate and earth system research.

problem was perhaps that I wanted it to be a centre at the university, taking the Kiel Institute of Marine Sciences as an example. This met with opposition.

What is your opinion on the democratization of the universities? As a managing director of the university's Meteorological Institute you were re-elected every two years. Were there also voices of dissent?

Hinzpeter: Oh yes, every time! Once, there were four votes for me, including my own, and four votes against me. The election was repeated twice, and the final result was one abstention and three votes against me. I had not consulted all panels regarding the acquisition of the mainframe computer.

I have never fully understood the reasons for the democratization. There may have been disciplines where a tenured professor, hovering high above all others, issued instructions without allowing discussions. I do not think that this was the case in our discipline or, e. g., in physics. Whatever the structure, clear decision-making processes and responsibilities are necessary. When a panel votes, perhaps even by secret ballot, nobody can be held accountable.

Can research be effective in an institute customized to a single person?

Hinzpeter: The Max Planck Society works on this principle, and largely with success. When the director makes a decision, it is based on the best possible information which he has obtained in discussions within and outside the institute. Of course, this includes conversations with younger scientists from which scientific initiative and innovations are expected

Elections at universities have become pure issues of power, which are also politically influenced. This is not good for scientific quality. The opinion expressed by an eminent politician that an increase in the number of students is associated with a lowering of their qualifications, and that therefore the university should reduce its standards, is very dangerous for the university.

As you can see, I do not understand what democratization is, but perhaps you can teach me.

Where do you seek advice, and where do you find competent colleagues for a discussion, when a decision is necessary?

Hinzpeter: Democratization or not, this is always the essential problem. Only experience and insight into human nature help.

There is the famous fundamental theorem by Hinzpeter, saying that the number of good students is constant. Would you still say so today?

Hinzpeter: Yes, because I think that the corresponding distribution function of the characteristics of a sufficiently large group changes very slowly.

Is Humboldt's ideal of the unity of research and teaching still valid in a time when very large institutes are created?

Hinzpeter: I still deem it correct, even if it is hard to achieve today. When a professor must give lectures for eight hours a week, and he continuously reworks his lectures, so that they correspond to the actual state of science, this requires much time. When science develops as quickly as it does today, this means that he cannot dedicate himself sufficiently to research. The professors' strong involvement in teaching will hardly change, so that I advocate all the more that the institutes associated with the universities, in particular the Max-Planck Institute or Blue List Institutes[13], also participate in teaching. They can give a few, challenging lectures, and thereby attract and win the best students. But we must clearly distinguish here between students before and after their pre-diploma. Before the pre-diploma, the main emphasis must be on mathematics and physics, because without good basic knowledge in these fields studying meteorology makes little sense.

You were always talking about meteorology and did not mention oceanography. Is that just a habit?

Hinzpeter: I was talking about the study of meteorology. The same is valid for oceanography. Anyhow, both curricula are nearly identical before the pre-diploma. Afterwards there are major differences. In oceanography it is more important to take the boundaries into ac-

[13] „Blaue Liste Institute“ of what is nowadays called Leibniz-Gemeinschaft

count and the linearized equations – wave solutions – play a larger role. In meteorology the phase transformations of water vapor and radiation are more important, but ocean and atmosphere share the same basic physics.

Do you think that exercises are necessary and useful? It seems artificial when the student knows that there is a solution to the task, while in a diploma project he does not know whether there is a right way and a solution. There are many students who do well in the exercises but fail in their diploma thesis.

Hinzpeter: I think that exercises and tests are necessary. The students see whether they master the tools of the trade, and they learn to analyze a scientific problem. I do not know the present conditions, but when I came to Hamburg, grades for exercises and tests had been abolished in the frame of democratization and reforms, and hardly anybody was allowed to fail. I consider it discouraging for good and ambitious students, when a very good achievement is formally not rated any differently than a just adequate performance.

After the pre-diploma, students can be led to science, e. g., as student assistants. During this phase the exercises should be challenging enough that the students will approach their diploma problem thesis with self-confidence. Again it is important to call on scientists from institutes outside the universities for teaching. Especially good younger ones should be encouraged to obtain their Habilitation.

Did you enjoy teaching, and were your lectures good?

Hinzpeter: The latter question must be answered by others. I did not enjoy lectures very much, but I liked the cooperation with diploma students and doctorate students.

You wrote a book on radiation.

Hinzpeter: Together with Mr. Foitzik, I wrote it in 1953. It was published in 1958 and was already then outdated. German publications on radiation issues were unsatisfactory at that time, because there were no clear definitions of the different radiation quantities. Then I read Chandrasekhar's book, which was published in 1950, and partly translated it for me, and I felt relieved by the clear definitions. I also learnt that many questions regarding "remote sensing" had alreadybeen treated by astrophysicists. The astrophysicists could work with a satellite moving around the sun and had developed the instruments to explore the sun. The meteorologists face the difficulty that, contrary to astrophysics, they deal with complicated band structures. In addition, on their small artificial satellite they cannot operate with as elaborate instruments as an astrophysical observatory.

Can your scientific life be divided into phases; some specific for you and others typical for any scientist striving upwards in the hierarchy?

Hinzpeter: I think I already mentioned this at the beginning. There are a number of specific phases. To begin with, the collapse of unified Germany. Then a normal phase began. I worked at a scientific institute, and without any initiative on my part I then became the director of another institute. In those times I was requested to give lectures at the University of Leipzig. If you like to express it that way, I moved up in the hierarchy.

The next phase is specific for me. The GDR built the Wall[14] while I was abroad. I did not go back, but started from the beginning again as an assistant at the University of Kiel. It was certainly not normal to begin as an assistant at more than 40 years of age. My further path was professional advancement in the academic hierarchy. I received my Habilitation in Kiel, became the director of a small university institute, then went to Mainz as a tenure professor, and after five years as a tenure professor to the University of Hamburg, and a little later I was appointed director at the Max Planck Institute. Especially considering my advanced age, this was partly due to the lucky circumstance that there were chairs to be filled.

You were pushed more and more firmly into scientific organization and thus removed from detailed research issues. Is that normal?

Hinzpeter: This is surely not normal. During the preparation of the GATE Experiment I was a kind of assistant to Mr. Brocks. After his death I had to take over the organization of the German contribution, which was the largest, with three vessels. When Mr. Möller retired

[14] Berlin Wall, erected in 1961.

after a stroke, I took the chair of the German GARP Committee. As it seemed to me that meteorology was insufficiently represented in the DFG, the Senate Commission on Atmospheric Sciences was established at the DFG. I then went into many other commission and councils.

I hope I have acted not only in my interests, but also in the interests of meteorology. Funding by the DFG was of great importance for our science, especially for establishing Priority Programs which brought the young scientists from different meteorological institutes together. In many respects, this was more important for meteorology than a large Collaborative Research Centre. In the sum, the development of meteorology was good, especially comparing it to the time 35 ago when only tenure professors met twice a year.

I see a certain analogy to thematic priorities in the research projects of the European Union, where scientists of different countries are brought together.

When someone has finished a PhD and has graduated, it is an enormous satisfaction to observe that the graduate now shows much more self-confidence, and has gained personal stature.

Hinzpeter: This is true. Mr. Geiger once jokingly remarked, when a non-PhD is asked something he does not know, he answers: "I do not know"; the PhD answers: "That is not of interest to me.". Such justifiable pride of the PhDs can also be observed in students who have successfully worked out not too simple exercises or tests.

Were there any events which made a particular impression on you?

Hinzpeter: After the war, we were isolated from international science. When I was about 27, I considered it a great scientific experience when Rossby accepted Ertel's invitation to visit Berlin as the first prominent scientist. He gave a convincing, wonderfully precise talk on the derivation of Rossby waves. I already commented on Smagorinski's, Lorenz' and Hasselmann's works. Smagorinski's work satisfied me because it was published at a time when also graduated meteorologists still believed that meteorology was too complicated to be solved with differential equations.

1959,Berliner Akademie

Hinzpeter: "To my right, there is Ertel, Leibniz is above.
I am a mediocrity and do not really belong here."

Which role did climate variations play for your understanding in earlier years?

Hinzpeter: Already as a student, I was unsatisfied with the proposition that climate should be defined as a mean state over a period of 30 years. The efforts to explain all observed variations as periodical were also disturbing – because none of these were statistically verified. I saw the main task of modern climate research in establishing the DFG's Priority Program to distinguish between internal fluctuations and climate variations caused by altered boundary conditions and to verify these findings with the help of climate models. This included, e.g., modeling the Little Ice Age. Now, quasiperiodic variations emerging in climate models have become more likely.

Were there any teachers who served as a model for you?

Hinzpeter: Three teachers left a lasting impression on me: Ertel, Albert Defant and Julius Bartels.

Ertel gave didactically excellent lectures. Actually, they were didactically too good. He let the subject matter seem very easy, but the students did not always understand everything so that they ran the risk of not reworking, and thus the things they heard did not stick in their minds.

Defants' lectures were an excellent combination of the theory of oceanic processes with observations, and thus always enthralling.

Bartels impressed me as a personality who confidently knew his stuff. But, he was less interested in good didactics.

One final question: If once more you had the choice, would you become a meteorologist again?

Hans Hinzpeter

Hinzpeter: When I started my studies, nobody could suspect what a fascinating development meteorology would undergo. Today I am glad I could witness it. Anyway, I do not regret having chosen this discipline. A second time, however, I would chose the path via the study of physics.

Interview with Klaus Wyrtki

prepared by Hans von Storch, Jürgen Sündermann and Lorenz Magaard
on 25 February 1999

Dr. Wyrtki, you started your studies in Marburg just after the war and then you continued in Kiel. Could you explain and tell us a little bit about your university studies?

Wyrtki: It was after the war in 1945 and I traveled up and down through Western Germany to find admission at a university. I finally succeeded in Marburg. When I was asked what to study I chose physics and mathematics because ship building what I intended to study was no longer being taught in Germany. After a while I got interested in applications and I read books about meteorology and in doing so I found out that oceanography existed. I read Defant's "Dynamische Ozeanographie" and other books. Eventually I went to my geography professor – I think his name was Schmitthenner - and asked him where oceanography was taught. He said that there was a famous institute in Berlin, but that it was bombed out and that most of the people had probably moved to Kiel. In the summer of 1947 I went up to Kiel to visit the Institut für Meereskunde[1]. When I climbed up to the tower of the villa, Hohenbergstraße 2, where the Institut für Meereskunde as well as the Geological Institute were located, I found Georg Wüst and I told him my story. When I had finished he said, "well that"s nice. Now I have a student". That's how it started with me. He arranged for an exchange of student places which was possible at that time. In the summer of 1948 I went up to Kiel.


Georg Wüst

There comes to mind the story about my dissertation. After a year or so I asked Wüst, I would like to make a Ph.D. and he said, "fine, let us do. There is someone in the German Hydrographic Institute who has an instrument that measures turbidity in the ocean and you just take the instrument and go out to sea and measure more often than

[1] Institute of Oceanography.

anybody has measured with it. And you will find something new.[2] Dr. Krey has worked with the instrument, go and see him." I had to calibrate the instrument. When talking with Krey about it, he gave me two big volumes of colloid chemistry which I had never heard anything about. I put them in the lowest drawer on my desk and never opened them until I had my Ph.D.[3]. I didn't intend to do anything about chemistry, but he thought that the substances that were in the ocean and would be measured by the light were mainly of chemical nature.

Kiel, 1951

Anyway let us go on. You asked what I learned from Wüst. It's basically the general overview, to look at large connections, not at the details, but to integrate things, to see the big picture.

You asked for the little story about an attachment to a bicycle. We students were somewhat annoyed that we had to carry boxes of water samples and instruments from the institute to the research ship and back. We wanted some easier way of transportation. Wüst approved of that and told us to buy a little cart to hang behind a bicycle. The university administration did not approve that. It was not a scientific instrument. We came to use the name "transporteur" which is actually a measuring device used by surveyors to plot angles on

[2] See also page 46.

[3] Wyrtki, K., 1950: Über die Verteilung der Trübung in den Wassermassen der Beltsee und ihren Zusammenhang mit den hydrographischen Faktoren., Ph.D. dissertation, Univ. Kiel, FRG, 49 pp.

charts. We submitted that to the administration; it was approved as ‘transporteur’ and the bicycle dealer actually sold us one of the two wheel carts to hang behind a bicycle. That is the way, how we mislead the administration.

Thank you very much for this advice. We keep that in mind.

Wyrtki: You keep that in mind. That is good.

You finished your studies at the university with receiving your Ph.D. Does it mean that you never had a classical examination at the university?

almost 50 years later

Wyrtki: Not really, except for a few little examinations. As a student in the natural sciences I had to take one course in Germanistics. It was a seminar on an obscure German poet, who had written a lot of novels and we were supposed to read all these novels. When examination came I had read none, not a single one. About twelve students were sitting around a big table with the professor and he started to ask the first one about one novel, the second one about the second novel. I saw that it wouldn’t go very smoothly, and I was sitting in the middle. When he was at the fifth, I interrupted him. I thought, attack is the best defense, and discussed with him something about the ethics of the knights, die Ethik der Ritter, because one of the novels was about the knights. We discussed that for a while, then he took the next student, then he skipped me and he went on and when we finally got our slips, it said ‘good’, that was fine, that was my examination.

This was a little footnote of my student days. There was of course a final examination for my Ph.D.

Your university studies were significantly different from today. Today everything is regulated, more or less. Do you find that your way of taking the university was somewhat better?

It was a wonderful freedom that we had. You could study, you could not study. You could do what you wanted. You had to have responsibility. That wasn't taken away from you. If you failed, you failed. You were out. Today we are giving remedial courses. Students shouldn't get remedial courses, they should be thrown out. That's my opinion. That's not the university opinion.

Klaus in 1953

After I had my Ph.D. I had a very short stint in Hamburg. At that time Dietrich had a position with the British Navy to oversee German oceanography and to collect material from the war and to hand it over to the British. Dietrich got a university appointment at that time. There were six months of salary left in that position which was under the control of a British admiral Carruthers. I moved to Hamburg for six months and my room was one floor above Bönecke, the director, because I was the representative of His Majesty. From time to time Bönecke gave me a call, "Wyrtki, kommen Sie runter[4], you have to sign a document on behalf of His Majesty". He was smiling about these things. That is the way, things go.

You were asking about salaries. When I was research assistant, I had 300 marks. That was barely sufficient to get along as a student, and suddenly with my appointment in Hamburg, I got 800 marks and I felt like a king. I suddenly had everything I wanted.

What did you do with all the money?

[4] Here, Wyrtki changed spontaneously into German: "come down"

Wyrtki: Amazing. At that time you still had to buy clothing, you could go out a little bit. You could live.

> *We should compare that with how much you had to pay for a car, for a Volkswagen, for instance.*

Wyrtki: A car at that time, about 1500 marks, Volkswagen beetle. It's amazing, but that's it.

Windverhältnisse	Wind Conditions
über den Meeren	over theSeas
um die britischen Inseln	around Britain
im Zeitraum 1900–1949	during the Period 1900–1949
von	by
G. Dietrich	G. Dietrich
(Deutsches Hydrographisches Institut, Hamburg)	(German Hydrographic Institute, Hamburg)
K. Wyrtki	K. Wyrtki
(Hamburg)	(Hamburg)
und	and
J.N. Carruthers, A.L. Lawford	J.N. Carruthers, A.L. Lawford
und	and
H.C. Parmeter	H.C. Parmeter
(Hydrographic Department, Admiralty, London)	(Hydrographic Department, Admiralty, London)
Deutsches Hydrographisches Institut	German Hydrographic Institute

Hamburg 1952

Document prepared on behalf of His Majesty

After the six months in Hamburg I returned to Kiel and I got a Forschungsauftrag von der Notgemeinschaft Deutscher Wissenschaften[5]. That was for the studies of the water exchange between the Baltic and the North Sea which I did then for three years. We made a lot of measurements in the Fehmarn Belt and elsewhere, with paddle wheel current meters to study water movements. I analyzed data. Interpretation of data was always what interested me.

When the three years of the research grant were finished I was looking for a job. Neither Wüst nor Bönecke had one for me. A friend of mine, Willi Brogmus, got a letter from Indonesia asking whether he wanted to come to Indonesia as a scientist.

May I ask something between before you go to Indonesia? I noticed that you had this project from German Science Foundation. Who were the reviewers in those days? There were only very few oceanographers in Germany.

Wyrtki: I would say that was in France and elsewhere, maybe not in England.

Could you say a few names? What persons worked in oceanography just after the war at that time?

After the war there was Hansen, at the DHI[6], Joseph in physical oceanography, there was of course Dietrich. There was Neumann and Roll at the Institute of Geophysics at Hamburg. There were some more people. Tomczak, the father. Weidemann was assistant to Wüst.

They mainly worked in the German Hydrographic Institute?

Wyrtki: Yes.

We stopped at Willi Brogmus. He declared he would rather go to the North Pole than into the tropics. So he gave me that letter. I wrote to Indonesia, a few months later I was on the way to Indonesia. This went all pretty easy. When I arrived in Indonesia, they were phasing out the Dutch at that time and they were looking for other people. Since Germany had no colonial attachments we were somewhat welcome in these countries. In Indonesia I found myself not only the

[5] a research grant from the German Science Foundation

[6] Deutsches Hydrographisches Institut = German Hydrographic Institute in Hamburg.

only scientist in the institute, because all the Dutch had left, but I was also the director of it. I had a research vessel of about 200 tons, a nice yacht type vessel, the ”Samudera”. I made many voyages with it, with very little instrumentation. We did a few surveys with Nansen bottles down to a few hundred meters but could not reach the deep sea basins in Indonesia because of a lack of a long wire, and that restricted us to the surface layers.

On board “Samudera”, 1955

I discovered there was a lot of actual information about these waters that had never been summarized. I started to work on a book, the physical oceanography of the Southeast Asian waters; it became known as the NAGA-Report[7] later on when it was published at Scripps. I wrote that book on many long voyages through the Indonesian waters. That proved actually quite a hit, it was even translated into Chinese. Because the information about these waters had never been summarized the book remained a valuable reference for decades because the Indonesians were very hesitant in the decades that followed to let foreigners doing research in their waters. We come back to that when we talk about international cooperation.[8]

Did you find at that time the Indonesian through-flow?

[7] Wyrtki, K., 1961: Physical oceanography of the southeast Asian waters. Univ. Calif., NAGA Rept., No. 2, 195 pp.

[8] See page 61

Wyrtki: Yes, when analyzing the data from both the Dana and the Snellius expeditions. The Snellius expedition was not completely published by that time. I could analyze existing sea level data, I could make dynamic calculation, both in the Pacific and in the Indian Ocean. I could identify the fact that there was a pressure difference between the two. I analyzed surface circulation which indicated that there was a monsoon dependent through-flow. That was the start of that type of research.[9]

After your time in Indonesia you went to Australia.

Wyrtki: From Indonesia I was sent to Tokyo, in 1955 for a UNESCO conference. There were all the famous oceanographers, including Roger Revelle, Deacon from England, Hidaka, Bönecke and so on. That time I met Roger Revelle and that turned out to be a very profitable meeting in the long run. We talked quite a while and I met Roger Revelle again at the Pacific Science Congress in Bangkok in 1957 when I was on the way back to Germany from Indonesia.

I actually gave up my position in Indonesia, and didn't extend my three years contract because there started a civil war in Sumatra at that time and conditions were restless. I had several months of vacation coming up anyway and a free trip back to Germany. I went via Bangkok, where I met Roger Revelle again, I met Townsend Cromwell, the discoverer of the equatorial undercurrent, and other people.

When I came back to Germany in 1958, Bönecke had lined up a job for me. That was in Monaco. Bönecke at that time was promoting the general bathymetric charts of the oceans. The International Hydrographic Bureau in Monaco was supposed to do them. I went down to Monaco for about 6 months. This was basically a post office. It was scientifically not challenging in any way and for that reason I didn't stay there. I could have stayed, but it was a dead end career. Recognizing that early enough I looked into other positions available.

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[9] Wyrtki, K., 1958: The water exchange between the Pacific and the Indian Oceans in relation to upwelling processes. *Proc. Ninth Pac. Sci. Cong.*, **16**, 61-65.

There was one position in Australia offered in 'Nature'. I applied for it and actually got the position. After the Monaco stay was over, I went in November 1958 to Australia. There in Australia I had a wonderful time with the CSIRO Division of Fisheries and Oceanography. It was similar to what in Germany are the Max-Planck-institutions. That means, research institutions granted by the government. I had very fine colleagues. We had Neil Brown who with Bruce Hamon constructed the first CTD and we tried it out at sea. We had David Rochford. There was the International Indian Ocean Expedition going on in which I did not participate because my work was on the oceanography in the Tasman and Coral Sea. My interest developed at that time into Antarctic circulation. That was really following in the footsteps of Wüst, deep ocean circulation and the Antarctic water ring that connects the deep circulation of all the oceans.

Did you know that at that time already?

Wyrtki: This was known by Sverdrup and by Deacon. Science is always a progress. You want to know something better. In fact many good ideas you get just from reading older papers. What kind of speculations good scientists make about the things that are unknown. That are not readily accessible to them. The data are limiting. If you look up their ideas and follow them through with new data you are probably onto something. That is when I wrote the papers on thermohaline circulation and on the oxygen minima in the oceans.[10] The oxygen minimum paper has been widely used by geochemists to explain the distribution of properties.

That was the time when it became clear to me that vertical movements are the main links in ocean circulation - like the Antarctic upwelling, like the vertical movements in the deep ocean basins that must bring slowly up water to the surface and are counteracted by vertical diffusion. All these problems were at that time addressed.

[10] Wyrtki, K., 1961: The thermohaline circulation in relation to general circulation in the oceans. *Deep-Sea Res.*, **8** (1), 39-64.
Wyrtki, K., 1962: The oxygen minima in relation to ocean circulation. *Deep-Sea Res.*, **9**, 11-23.

At the same time it became quite clear that surface circulation in contrast to deep circulation was very variable, as we could see from surveys that we made in the East Australia Current.

While I was in Australia a colleague of mine, a zoologist, spent a sabbatical at Scripps. When he came back he said, ”Klaus, the people at Scripps want your curriculum vitae”. I sent them my curriculum vitae. Of course in the curriculum vitae you had to give references. One of the references was Georg Wüst, who at that time was at Columbia University. After about two weeks I got a job offer from Columbia University. That went that fast.

I tried to find out what the future would offer. At Scripps I would belong to a tuna research program that stretched all the way from California to Peru, throughout the eastern tropical Pacific investigating the environment of the tuna population. At Columbia I would be assigned to a new research ship, the Eltanin, and I would go into the Antarctic Ocean. Arnold Gordon eventually got the job, because I said, ”no, no. No Antarctic Ocean, no seasickness, no roaring forties, I stay in the tropics”. After Indonesia I was spoiled, I didn’t want to go back to the cold climate, so Scripps institution won.

Likely Wüst was disappointed.

Wyrtki: Wüst was disappointed, of course, but he got Arnold Gordon. That was fine.

On the way from Australia to California I stopped in Hawaii for a Pacific Science Congress. That was the Pacific Science Congress during which the corner stone for the Hawaii Institute of Geophysics was being laid but at that time I was not aware that I would finish up there.

So, I came to Scripps and the work there was most interesting. It was not data taking, other people were doing that. It was studying the upper ocean variability. At that time it had become clear that fisheries and long-term weather prediction are dependent on oceanographic knowledge on a real-time basis. One needed to know what happened in the ocean from month to month and from year to year in order to explain, how the environment reacts.

Did you learn also something from biology at that time? Or from biologists?

Wyrtki: I didn't have to know much, I had enough fishery biologists around me and we had very close interaction with the people who were doing the tuna research in biology, the tuna marketing and catching, the fishery people actually running the fishing fleets. We gave them BTs - that was the study on the Costa Rica Dome[11], on upwelling, where cold water comes up to within 10 meters of the surface and where the tuna boats can put the big nets around a whole school of tunas and fishes, and get tens of tons of tuna out. The Peruvian fishery was growing at that time, at a tremendous rate.

It was a very exciting and productive era, I met Jakob Bjerknes at that time, he came often down from Los Angeles. My neighbors were Jonny Knauss, Joe Reid, Warren Wooster, all these people, we were all together there; Benny Schäfer was the director of fisheries research.

Was that the time when you started using a computer?

Wyrtki: Yes, that was the time when we first wanted to get maps of surface temperature on a monthly basis and if you do that, you need data in a short time. Ship observations were collected. They came in by radio through the meteorological network and you had to collect and to process them. We had the task with thousands of observations that we wanted to map and so one day I said we have to use computers and we looked for someone who could do computer programming. We found a graduate chemistry student. He came up to me and I explained to him what we needed, he said that he could do that, but I would have to write him some instructions. In a couple of days I wrote down the instructions, and when he came back the next time, I handed him the sheet and he looked at the sheet, then he looked at me and he said, "oh, you have written a computer program". This was a list of instructions on how to go in sequence through the mass of data. I had no idea about computer programming at that time.

[11] Wyrtki, K., 1964: Upwelling in the Costa Rica Dome. *Fish. Bull.*, **63** (2), 355-372.

Did you yourself any programming?

Wyrtki: No, never.

At times I had up to four, five computer programmers working for me. I knew what goes in and what comes out, but that was it. Like with an appendix. I don't start to study medicine when I want my appendix out. I go to a doctor.

Did you begin to use a personal computer for writing and e-mail?

Wyrtki: Yes, in the NORPAX project we were among the first to use email, because we were on the Office of Naval Research circuit. For the Test Shuttle we used it as early as 1975. That was "telemail". My secretary used it every morning.

But you did not use it yourself, you did not type yourself?

Wyrtki: No.

Another thing. My first computer programmer was hired for the Indian Ocean Atlas, it was done largely by computer. Then she had a baby and she retired for a year and then she wanted her job back and I took her back with great welcome. Then she got her second baby and she wanted to work at home and we bought her a little computer, with which she could use her home telephone and connect to the university computer. So, she could work at home while waiting for the baby. These were the first explorations in computer. It was an exciting time.

Now Scripps. Why I got out of Scripps? The answer to that is very simple. In Scripps at that time - it has changed by now - there were two sorts of people, researchers and professors. When you were researcher, you never could become a professor.

You did not know this before?

Wyrtki: I had no idea of the structure of an American institution. But this was general - that was the case in Woods Hole, that was the case atColumbia, Lamont, New York University, Miami. This was the situation in most of the institutions. Since my goal was really to become a professor, to teach, to do research, I was very happy, when one morning someone knocked at my door in La Jolla and introduced himself as being the acting chairman of the new oceanography department in Hawaii. This fellow, who became later president of Texas University, was the first department chairman; his toy were analogue computers. He knocked at my door and made me an offer and I said, ”yes, I come”. And so I moved to Hawaii in the summer of 1964.

first computer–made atlas of Indian Ocean available

Jan. 16, 1972 Honolulu Star Bulletin

Wyrtki, atlas and computerized plotter.

By DOROTHY H. MILES
Special to The Advertiser

The first cornputer-made atlas and the first atlas resulting from the 1961-65 International Indian Ocean Expedition (IIOE) is off the press ant in the hands of its editor, Dr, Klaus Wyrtki, University of Hawail professor of oceanography.

Wyrtki has been involved with the production of the oceanographic atlas since 1966 wheen he was appointed its editor by the National Science Foundation.

He said that IIOE data, used in the atlas was obtained by scientists on 70 research vessel of 18 nations.

THE DATA from12,000 research stations -points at which research vessels made measurements - was stored on some 200,000 computercards.

,,Computer techniques were used throughout, Maps were plotted by computer then drawn by hand, Tables were reproduced directly from magnetic tapes through a television screen then to the printer's plate," he explained.

The Computer technique avoids all clerical errors and speeds production. Without the use of computers, he said the same amount of work might have taken an estimated 200 man-years

„Wyrtki noted tlat„ no other ocean has an atlas as comprehensive or which includes such a variety of subjects, not only in terms of properties mapped, but in ways presented - horizontal, vertical, or in layers to show the three-dimensional structure of the ocean, that is,salinity, oxygen, and temperature.

„One can look up any place on the ocean and find the kind of conditions which exist there at any depth," hesaid.

THE IIOE was an intensive study of the 28 mi
square-mile
by

In the first few years in Hawaii, George Woollard was the director of the Institute of Geophysics, and money was flowing easily - we had Office of Naval Research contracts to do current measurements around the islands, to study island circulation and heat advection in the North Pacific - but I started with a project that I always wanted to do, namely, investigating the circulation of the Indian Ocean. I wrote a proposal to the National Science Foundation to make the Indian Ocean Atlas on the physical oceanography. That was basically my main activity from the time of my arrival here to 1970. It was essentially in the tradition of Wüst, studying the deep circulation.

There were two motivations. The deep circulation was per se of interest, but the deep circulation was basically considered stationary: once you know it you know it for the century, at least. But at Scripps I had learned how fast the upper ocean moves and that it is necessary to study the changes that are going on within weeks and months. For that reason I concentrated the work on the Indian Ocean Atlas on the study of the annual variation, which is of course natural for the Indian Ocean because of the monsoons. But if you do these things you are getting new results.

By the way, that was something I learned from Wüst: “if you take a new instrument or measure something more frequently, you will find something new.” This is a basic principle and this is how my Ph.D. thesis came into being.[12]

There was no idea what you will find?

Wyrtki: There was no idea what one might find. You take a new instrument, measure more frequently than anybody before you and you are going to find something. This was the philosophy. For instance, if everybody looks at the mean stationary state, then you look at the variability and you will get something new. In this way I found most interesting things.

You have here in your list[13] the question ‘Wie entsteht wissenschaftlicher Fortschritt?’[14] and you list four items ‘Förderung’, ‘Gelegenheit’, ‘Personen’, ‘Zufall’[15]. In my opinion all items are important. But a basic prerequisite for scientific Fortschritt ist, daß man sich wundert.[16] Man wundert sich über etwas, was nicht leicht

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[12] see page 33.
[13] In the tentatively list of questions prepared for the interview.
[14] How is scientific progress generated?
[15] Funding, opportunity, people, coincidence.
[16] Here, Dr. Wyrtki spontaneously changed into German: ”a prerequisite for scientific progress is that one is wondering. One is wondering about something not easily explainable. I was amazed over two things that both finally led to El Niño. The first were the seiches in the Baltic. At a certain day[17] the Hinderburgufer[18] in Kiel as flooded. On the next morning Wüst called me into his office and said, "Herr Wyrtki have you seen the flooding of the Hindenburgufer?" I said, ”yes, yes”. "We must know, how this happened. Collect all data, and analyze them.”

erklärbar ist. Ich habe mich über zwei Dinge gewundert, die schließlich beide zum El Niño geführt haben.

Kieler Nachrichten Jan.1954 Von GERD SCHARNHORST

Wie kam es zu der großen Flut ?

Wenn der Wind sich dreht, birgt auch die Ostsee überraschende Gefahren

Eine Sturmflutvon ungewöhnlicher Stärke hat am Wochenanfang die Deutsche Ostseeküste heimgesucht. In Lübeck, Kiel und Flensburg stand das Wasser in den Straßen. Allein in dem bekannten Badeort Timmendorf wurden Wochenendhäuser und Wohnwagen von den Fluten weggerissen.

Auf der ostzonalen Insel Rügen mußten verschiedene Ortsteile geräumt werden, die Insel Hiddensee wurde teilweise überspült. An der mecklenburgischen Küste sind mehrere Deiche gebrochen, und auch Rostock, Stralsund und Warnemünde hatten unter dem Hochwasser zu leiden.

Die entstandenen Schäden sind noch nicht abzuschätzen. Sturmflut -das ist ein Wort, vor dem nicht nur die Menschen an der Küste erzittern. Auch tief im Binnenland weiß, man von dessen gefahr-bringender Bedeutung.Und wenn man geneigt war, katastrophale Auswirkungen einer Sturmflut in längst vergessene Zeiten zu verbannen, so wurde man durch die Katastrophe in Holland vor einem Jahr eines besseren belehrt. Daß nun auch die Ostsee die so oft als harmlose Schwester des "Blanken Hans" angesehen wird, die Küste angefallen hat, zeigt uns wieder einmal im Kampf mit den Elementen und wir auch im Atomzeitalter oft genug Unterlegenen.

Wie konnte es zu dieser für die Ostküste verheerendsten Sturmflut seit Jahren kommen? Bereits am vergangenen Sonntag zog ein Tiefdruckgebiet mit großer Geschwindigkeit von Island auf die norwegische Küste zu und verursachte dort orkanartige Stürme. Am Sonntag erreichte es die Ostsee und schickte das Wasser bei mittleren Windstärken in östlicher und nordöstlicher Richtung ab. Über der mittleren Ostsee, im Gebiet nördlich der Danziger Bucht, drehte der Wind unvermittelt auf Nordost und fegte in einem relativ schmalen Streifen mit Stärken zwischen 8 und 10 gegen die Küste. Mit großer Gewalt langte die See in die Buchten von Lübeck, Kiel, Eckernförde, Flensburg und in die Schleimündung, die genau in der Nordostrichtung liegen.

Der Wasserpegel stieg bereits in der Nacht zum Sonntag ständig und erreichte in den Nachmitttagsstunden Er fiel sofort wieder ab, als das Sturmtief das Binnenland abgezogen war. hatte das Deutsche Hydographische Institut rechtzeitig das vermutlich einsetzende Hochwasser mit 1.90Meter angegeben.

Dazu erklärte uns Dr. Klaus Wyrtki von Institut für Meereskunde, Kiel: " In einem abgeschlossenen Seegebiet, wie es die Ostsee darstellt, wird durch die Einwirkung des Windes das Wasser an der Luvküste angestaut, während der Wasserspiegel an der Leeküste fällt. Diese Erscheinung nennt man den Windstau, der jedoch nicht ausreicht, die extrem hoben Wasserstände an der Ostseeküste zu erklären. Es kommt noch ein weiterer Faktor hinzu: Die Wassermassen der Ostsee führen als Ganzes Schwingungen aus, die durch den Wind hervorgerufen werden.

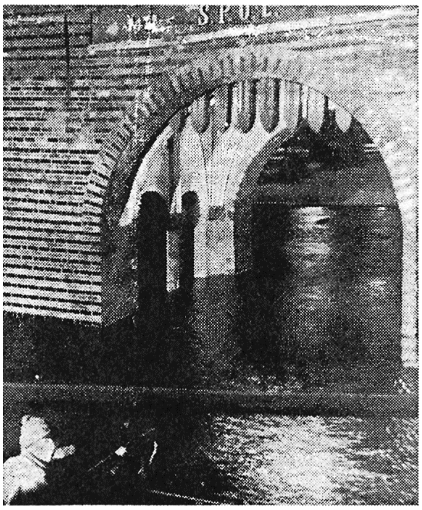
Einen ähnlichen Vorgang kann man In einer Badewanne leicht nachahmen. Auch hier befindet sich das Wasser ohne äußere Einwirkung zunächst in Ruhe. Macht man jedoch mit der Hand periodische Bewegungen, beginnt das Wasser mitzuschwingen. An den Enden der Badewanne sind die Wasserstandsschwankungen am größten, während sich der Wasserspiegel in der Mitte kaum verändert.Entsprechend ist es im großen in der Ostsee. Hier werden die größten Hochwasser in der Kieler und Lübecker Bucht sowie in Leningrad und am Nordende des Bottnischen Meerbusens beobachtet. Bei Reval,Stockholm und Pillau hingegen kommt es niemals zu so extrem hohen Wasserständen. Vorbedingung für eine solche starke Schwingung, die in der vergangenen Woche das Hochwasser ausgelöst hat, ist, daß die Geschwindigkeit des Tiefdruckgebietes gerade so groß ist, daß der Anstoß der Wassermassen im Takte mit der Eigenperiode der Ostsee erfolgt. Im anderen Falle tritt das Maximum des ,Windstaues und das Maximum der Schwingung nicht zu gleicher Zeit ein, und die Höhe der Flut bleibt unbedeutend. Bei der Sturmflut am vergangenen Montag verursachte ein Zusammentreffen meteorologischer und ozeanographlscher Faktoren gleichsam ein ,Uberschwappen' der Wassermassen an der deutschen Ostseeküste."

Obwohl die Schäden sehr erheblich sind, muß man sagen : es hätte noch schlimmer kommen können.Ein mehrstündiges Anhalten des Sturmes, zwei bis drei Dezimeter mehr Hochwasser, und die Verwüstungen wären unendlich viel größer gewesen. Vor allem deshalb, weil das Hinterland von Travemünde bis Haffkrug nur durch die natürliche Dünenwelle gesichert ist und die Düne bereits an mehreren Stellen durchbrochen war. Sowohl an der Küste als auch auf der Insel Fehmarn haben die Deiche jedoch gehalten. Aber - es gibt nur dort Deiche wo sich der finanzielle Aufwand lohnt. Wird der Wert des Hinterlandes von den Kosten des Deichbaues (die übrigens die Anlieger zu tragen haben) ühertroffen wird eben keiner gebaut. Eventuelle Verwüstungen der Küste in gewissen Zeitabständen werden dabei in Kauf genommen.

Hätte die Hochwasserkatastrophe ein ähnliches Ausmaß erreichen können wie in Holland ? Die Frage a priori mit einer klaren ,,Nein' zu beantworten, ist vielleicht etwas gewagt, aber nach menschlichem Ermessen ist das unwahrscheinlich. Das liegt an grundsätzlichen Unterschieden. Wie die Sturmflut an der Ostsee nur in Verbindung mit den Eigenschwingungen des Meeres gefährlich wird tritt die Sturmflut an der Nordsee nur in Zusammenhang mit den Gezeiten ein. Bei Ebbe würde sie für die Küste ungefährlich sein, da das Hochwasser vom Watt bereits abgefangen wird. Die eigentliche Gefahr für die Nordseeküste tritt erst ein, wenn die Deiche durchbrochen sind und die nach folgende Tide neue Wassermassen in die Einbruchstellen drängt. Da zudem - wie im holländischen Küstengebiet - der Boden sehr tief liegt, sind die Ausmaße der Katastrophen größer als an der Ostsee, wo die Küste auf weiten Strecken in Meeresnähe stark ansteigt und selbst von einer großen Flutwelle nicht zu erreichen ist.

Eine Sturmflut in der Ostsee ist nicht so selten, wie man allgemein an nimmt In den Ostseebuchten rechnet man im Maximum mit einem Absinken und Ansteigen des Wasserspiegels um je zwei Meter. Die relativ große Differenz ist durch die bereits erwähnte Pendelbewegung zu erklären. Eine Flut mit nahe zwei Meter Hochwasser wiederholt sich alle paar Jahre, erreicht jedoch selten die Zwei-Meter-Marke, wie die vorletzte Sturmflut im Dezember 1949 beweist. Damals wurden bei 8 bis 10 Windstärke im Kieler Hafen 1,60 Meter und in Flensburg sowie an der Trave 1,70 Meter über Normal gemessen.

Eines hat sich bei der Sturmflut vom 4. Januar 1954 wieder mit aller Deutlichkeit gezeigt: auch an der Ostseeküste reichen die Befestigungen nicht aus. Die Polizei erkannte, daß sie bei einerausgedehnten Sturmflut trotz aller menschen möglicher Anstrengungen nicht in der Lage gewesen wäre, die Küste zu halten. An der Ostküste von Schleswig-Holstein gibt es keinen organisierten Deichschutz.Nur an wenigen Stellen gibt es hochwasserfeste Deiche. Die schwächsten Punkte der Küste sind der Raum von Schönberg an der Kieler Bucht, die Insel Fehmarn und die Hohwachter Bucht Schleswig-Holsteins. Ministerpräsident Lübke hat die Gefahren erkannt, sagte, nur durch einen schnellen Rückgang des Wassers sei eine Katastrophe grösseren Ausmaßes verhindert worden. Er hat sich besondere Beschlüsse vorbehalten, um künftigen Sturmfluten wirksam entgegentreten zu können.



TRAVEWASSER AN DEN FUNDAMENTEN DES HOLSTENTORES

Newspaper Report about Baltic Sea flood “What was behind the big flood? When the wind turns, then also the Baltic may be surprisingly dangerous.”

Wyrtki: Das erste waren die Seiches in the Baltic. Eines schönen Tages – und da kommen wir wieder auf Wüst zurück - war in Kiel

Hochwasser.[17] Das Hindenburgufer[18] war überflutet und am nächsten Morgen rief mich Wüst in sein Office und sagte:"Herr Wyrtki haben Sie sich das Hochwasser am Hindenburgufer angesehen?" Ich sagte: "Ja, ja". "Ja, aber wir müssen doch wissen, warum das zustande kommt. Suchen Sie sich mal all die Daten zusammen und dann werden Sie das analysieren." That were wind induced seiches of the Baltic. There were southwest winds ahead of a cold front.

Twelve hours later there were northeast winds, very strong behind the cold front, and the Baltic was excited; seiches were induced, and the Baltic schwabberte, mit der bekannten 24h-Periode.[19] Seit dem Tage, wo ich diese seiches in der Ostsee beobachtet und gesehen habe, habe ich mich gewundert, ob der große weite, offene Ozean nicht mehr schwabbert. Das war eine Fragestellung.[20]The other thing was related to Peru. I made a current chart for the eastern tropical Pacific and I was amazed that certain currents start nearly out of nothing and end somewhere in a very diffuse way: the huge South Equatorial Current that transports fifty Sverdrups, starts from this little Peru Current that transports 10 Sv - where is all the water coming from? And the South Equatorial Current ends near New Guinea in the Coral Sea and you cannot see how it ends, it disappears. Where does all the water go? This was the next question.

When making the Indian Ocean Atlas we drew maps for every month of the topography of the 20^{o} isotherms, i.e., of the thermocline, in the Indian Ocean. It was obvious that in certain parts of the ocean the thermocline was seasonally going down and in other parts it was seasonally going up. So the idea came, if the thermocline goes down by 20 or 30 m, how much water does it really transport out of an area? I made the rough calculation and it showed that a substantial amount – 10 to 20 Sverdrups – leaves Somalia and goes over to Sumatra. And so I was looking at current charts and there was the

[17] 3 December 1952; see Wyrtki, K., 1953: Die Dynamik der Wasserbewegungen im Fehmarnbelt I. *Kiel. Meeresforsch.*, **9** (2), 155-170.

[18] A promenade in Kiel at the banks of the Kiel Bight.

[19] the Baltic wobbled with the known 24 hour period.

[20] Since that day, when I had observed the seiches in the Baltic, I was wondering whether something like that happens in the big ocean, and why the wide open ocean is not wobbling more. That was the question.

equatorial jet in the Indian Ocean, going from one area where the thermocline lifts up to the other side of the ocean where the thermocline goes down. That was really the next step on the road to El Niño.

Did you make your own measurements in the Indian Ocean?

Wyrtki: No, that was the International Indian Ocean Expedition in which I did not participate, because at that time I was working in the Tasman and Coral Sea. But, David Rochford, my colleague in Australia, was one of the main participants in the International Indian Ocean Expedition.

That were the years from 66-70, when I was working on the Indian Ocean Atlas[21]. In 1971 I spent half a year at Kiel with Dietrich on a sabbatical and when I came back, climate research started. This was the International Decade of Ocean Exploration and the National Science Foundation started to fund big projects. There was GEOSECS, MODE, the Southern Ocean, NORPAX. In the beginning I participated in the NORPAX project. After having seen in the Indian Ocean, how important annual variability is, and having known from my tuna research years that year-to-year changes are quite important, I looked at the data from Hawaii and I found out that we really didn't know how the big trade wind field varies from year to year. When I asked the meteorologists, they could not tell me. That is when we started to get the ship observations, the wind observations, and crunched 25 years of ship observations – there were 3 million observations at that time for the equatorial Pacific Ocean. We learned that the trade wind fields undergo massive changes from year to year. Analyzing these changes I found out that the biggest changes are not off Peru or somewhere near the Galapagos, but they are in the Central Pacific, real massive changes of the Southeast trade winds.

At the same time we were looking at the ideas of Bjerknes, who was working on the tropical ocean and tropical ocean- atmosphere interaction. There was Namias at Scripps who was working on the North

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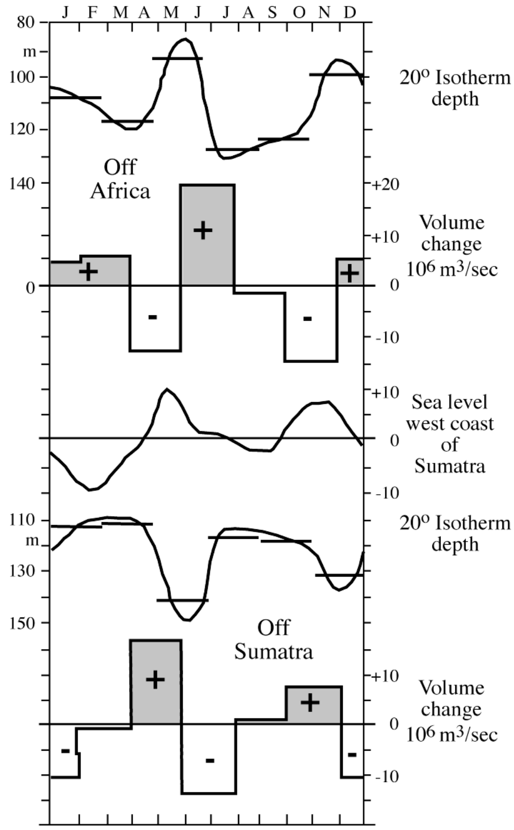
[21] Wyrtki, K., 1971: *Oceanographic Atlas of the International Indian Ocean Expedition*. National Science Foundation Publication, OCE/NSF 86-00-001Washington, DC, 531 pp.

Pacific - US mainland interactions. In my personal case, came the insight that the fluctuations of the wind stress on the equator are producing El Niño. Of course we had to prove it, which brought the sea level data in, because the claim was that the thermocline in the western Pacific goes up and the thermocline in the eastern Pacific goes down. We could prove by means of sea level data that these two things really happen, because there is a direct relationship between sea level changes and thermocline changes. Putting these things together gave the El Niño theory and also the knowledge that was developed at that time about equatorial Kelvin waves. But it was basically an observational fact-finding, an analysis of observations and putting the pieces together.



Seasonal water mass exchange
in the Indian Ocean.
1973 Science 181.

The fact that sea level is a very convenient variable to monitor the ocean gave the impetus for establishing the sea level net work in the Pacific. With this you could study dynamics – that was before TOPEX.

Has your work become more systematic over the years? You have told us that you have dealt with various interesting pieces in the first part and after you have started in Hawaii that you really zoomed in on one thing and became more and more systematic. Is that a fair description?

Wyrtki: Yes and no, there is certainly a truth in that, but I don't think, that it is intentional, it is simply based on the fact that your experience grows. You are exposed to more information; you learn about more processes and therefore you start to integrate your knowledge. Integrating knowledge is a very important thing.

So it is more or less normal, just a fact of getting older and more experienced.

Wyrtki: It is a natural process.

Have you always been in a beginning of a new period, at a new investigation, of new phenomena in your different stations - first in Indonesia, later in Australia, then in Scripps, and finally in Hawaii?

Again, yes and no. You know you jump at opportunities. Recognizing the opportunities is important and may be part of learning. These were all natural developments - it had to come to that, once you study the variability you necessarily get into climate and into climate change. If you think on the large scale then that is a natural way to go. Most people actually differentiate. If you give a child a toy, the first action is to take it apart and scientists do the same. They see a problem and immediately they take the problem apart, into pieces. Very few scientists integrate, that means, put things together.

Would that mean that you must be concerned in several topics? You got some idea on El Niño by studying the seiches in the Baltic. It is a completely different phenomenon. The integration in this case was that you had the association that they might be relevant. This would mean, it will help if you are curious about many things in the ocean and study many different things for this integration.

Wyrtki: Definitely.

There are other activities of which you are probably even more proud of than about your scientific papers.

Wyrtki: The start of ocean monitoring - now everybody is monitoring the ocean, the big TOGA TAO array, that constantly gives you interesting data, there are satellites - you don't believe, what fights we had to get funding for ocean monitoring. While we argued ”we need to observe the same thing year after year, because only if we do that we see changes. We need to know the ocean month after month, if we want to have weather prediction. We cannot go out once every five years and make an experiment. You need to monitor." there was a constant fight about ocean monitoring. I am very proud about the fact, that I was involved in that and was very vigorously participating in this fight.

Another thing is the freedom of data exchange. I don't know how often I preached when I was chairman of NORPAX "in meteorology data are instantly available. Whenever a radio sonde is launched, the next minute the data go on the radio and into the World Weather Watch." Oceanographers keep their little black boxes and the data they have in them for years in their laboratories and don't want to relinquish them. Data have to be available, in particular if you want to make forecasts. Another project I am very proud of is the establishment of GLOSS, the global sea level observing system. This worldwide network of sea level stations is giving us reliable information about the relative changes of land and sea and will provide a reference system for the calibration of altimeters.

Your life up to the Hawaii position was very much changing. You always changed. Why did you remain after that so long in Hawaii?

Wyrtki: I had three years Indonesia, three years Australia, three years Scripps. People were watching, if I have three years Hawaii, too. Hawaii is too nice to leave it. It is the best place in the world to live, I enjoyed the years thoroughly - certainly I have no desire to change anymore.

Maybe now it is time to come to the end of your career - Abschied von der Wissenschaft[22]*. Sometimes ago you retired and we hear that you have really withdrawn from science.*

Wyrtki: That is a part of my way of doing it. I am a person who can change rapidly. There is a time for everything. There is a time to be young; there is a time to work and to travel and there is a time to retire when you have deserved it. There are lots of young people who

[22] Departing from science.

are looking to do the next great thing. Why should we not quit one day and enjoy the life.

Nowadays you are no longer working in science?

Wyrtki: I am not working on scientific problems. That is true. I am still interested in what is going on in oceanography and climate research.

Your last paper is written?

Wyrtki: Is written in 1993, quite a while ago.[23]

Let's talk about changing themes, the effect of new methods and opportunities, experiments, models, remote sensing.

Wyrtki: Some of it we have already touched. The big subjects, that I just mentioned like ocean monitoring, free data exchange, and so on - these are problems, that science faces and that have to be solved beside the scientific problems. When it came to ocean monitoring, there are always new things – for instance, during my lifetime the satellites came up. I was one of the members of the initial TOPEX committee that Carl Wunsch started up. We where discussing and were very, very excited about the possibility of monitoring global sea level variability in areas without islands or fixed observation points. That is of course a step into the future of oceanography. The continuous observation of our environment is an enormous step forward.

Could you try to describe, what the big topic in the forties was, in the fifties and so forth? We just go through these six decades and you try to outline what to you was of most interest or significance.

Wyrtki: This is a good way to start. Before the World War deep ocean circulation was the interesting stuff, Defant and Wüst and Sverdrup. In the 40s, I can not really tell you. In the 50s it were surely the ocean eddies.

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[23] Wyrtki, K., 1993: Global sea level rise. Proc. Circum-Pacific Int. Symp. Earth Environment, National Fisheries Univ. Pusan, Pusan. D. Kim and Y. Kim, Eds., 215-226.

Science is per se a matter of fashion. When I was a student, every physicist had to study atomic physics, and if you were studying acoustic or anything else, you were second-rate.

So, in this sense I am asking for the fashions, wie lang waren die Röcke, die wissenschaftlichen[24], in the 70s?

Wyrtki: The eddies ... of course, biochemical cycles.

Already in the fifties or sixties?

Wyrtki: GEOSECS – seventies. The eddies were the first big problem after the war. I don't think the eddies started in the 50s, definitely in the 60s.

Once I got a student who wanted to make a Ph.D.. Peter Duncan came from South Africa and he brought along the results of one cruise that they made to the Southwest of Africa and I did nothing but apply another principle of Wüst. If you have observations, which haven't been used yet, you write a paper about it. I made him immediately write a paper about an eddy in the subtropical convergence south of South Africa. He wrote that paper in ten days and it was accepted by JGR. The background was a frivolous statement I had made in a class: any graduate student can write a paper that will be accepted by JGR and I gave them the recipe: New observations that haven't been published, a straight forward analysis, no controversial statements, 4 pages, 3 illustrations.

Four pages text?

Wyrtki: Yes, at most. Today I would say two pages.

Why was the interest in eddies so big? One thing of course, it was possible to observe eddies; on the other side, could you already estimate what the role, what the importance of eddies in the general climate dynamics is?

Wyrtki: That was more ocean dynamics than climate dynamics. People thought that a better knowledge of ocean eddies would explain the energy dissipation in the western boundary currents and in

[24] How long were the skirts in science?

ocean circulation in general, because all ocean circulation theories were dependent on dissipation.

So, that was the time of the fifties and sixties.

Wyrtki: These were the 50s, 60s, early 70s.

Then came the International Decade of Ocean Exploration. All these big projects were started in the seventies: the biochemical cycles, Antarctica, the Drake Passage, there was NORPAX, which was the project I joined in.

Is it fair to say that before the war people were interested more in the deep ocean circulation and the overall picture and after the war more in processes and in case studies on eddies. In the seventies it was the phase of integration, so that the people were more interested in longer observations, in variability. Is that right?

Wyrtki: You can say so. Actually NORPAX was the first big project that studied ocean atmosphere interaction. The database became sufficient to look at a larger picture - that means how an ocean affects the weather over a continent.

Was Namias very important in this respect?

Wyrtki: Of course, I had a very close relationship with Namias. When you ask for people: there were of course Bjerknes and Namias. We were together very often in meetings and had many long discussions.

Was he approximately your age?

Wyrtki: Namias was 14 years older, he died in 1997, and Bjerknes was much older - he could have been my father. This was a time of enormous cross fertilization.

What's about the nineties?

Wyrtki: The nineties are clearly climate, the chemical and biological cycles in the climate system. These are the next big topics, not the physical cycles of climate.

Could you say something on the role of experiments? Like in GARP when people came together to make a big effort to ob-

serve the atmosphere or the ocean or the boundary layers, intensively for a limited time, and then go back into the laboratories?

Wyrtki: Experiments are absolutely necessary. Experiments are the basis of physics. We do process experiments, which are real physics in the ocean, where you try to learn...

Could you give an example?

Wyrtki: Such as, how does the Ekman layer work? These are physics experiments. But then you have to make other experiments and these are very often not recognized as experiments: is Global Change an experiment? Now, you see, one important thing about geophysics is, and I tried to explain that to my students, physics is based on experiments where you can control one factor at a time. But in geophysics all factors are changing simultaneously. Nature is making experiments for us and as geophysicists we are very often simply put in the role of the observers. We can't control the experiment. If you make an experiment on an hurricane, you don't control the experiment.

How did you feel the assistance of numerical models, which was increasing with time? Numerical experiments...

Wyrtki: Numerical experiments, you mean models. Models are an essential part of physics and sciences today. There is no question about that. You need models for everything. You only have to use them in the right way. There are many different kinds of models. Models are to simulate certain physical processes. But a model is an approximation that can be used to study physical processes. Then there are models that predict weather. They have limitations. What are the limitations? In prediction it is chaos and turbulence. Then you make models of the tides. They are probably very good, because they have solid physics behind it and the process is truly repeatable, because it is forced. Then you can make models that are plainly speculative, that means, where we are trying ideas. The question is what you make with models. There is nothing wrong with models, but how you interpret the model, that's the important item.

Could you just give an example of a speculative model?

Wyrtki: I would say, modeling hundred years of climate change is speculative.

You are not talking about models like Stommel's?

Wyrtki: No, Stommel's model is a conceptional model of a process in which he explores the effect of β. He explains.

During your scientific career the role of models must have changed. I guess, when you were in Kiel there were no models.

Wyrtki: There were of course Stommel's models. Conceptual models have always been part of physics. And experimental models also.

Then came computer models and took more and more part of the science. How did you experience that?

Wyrtki: With a certain amount of skepticism, but the same skepticism I would have to an experiment. That means, I don't challenge the model, but the conclusions that people draw from the model.

Go back to basic physics. An astronomer makes an observation, first he speculates what happens. That is the first step, may be right, may be wrong. Then he makes a theory. Mathematically a model is equivalent to a theory. Then he asks what goes into the theory? What are the basic assumptions of the model?

I have a question to you. The big ocean circulation models that we are having today and that show many details of ocean circulation, do they include tides?

We[25] have in the meantime a circulation model, which includes tides.

Wyrtki: For the world ocean? Do the tides interact with the circulation? They must interact. They cause mixing and will dissipate energy.

Yes, for the world ocean. The tides interact in this model. Normally the circulation models do not include tides.

Wyrtki: When somebody comes with a result on ocean circulation you ask what does it resolve? What's the effect of tides? You can

[25] Statement by Jürgen Sündermann

put a mixing parameter in your models which specifies a number. But how good is that? These are the challenges to the models.

During the interview in Klaus'apartment. From left:
Klaus Wyrtki, Lorenz Magaard, Hans von Storch and Jürgen Sündermann.

But you said you would challenge the modeler, not the models.

The conclusions from the models.

Are there some systematic problems with models?

Wyrtki: No, I don't think so. Models are part of physics, but you have to be skeptical about the results. Models are as much part of physics as experiments are. They are only a different way of conducting experiments. Don't misunderstand that.

Another thing, which came up in your career was remote sensing. Suddenly there were satellites and you could observe the whole world from space. What did they change?

Wyrtki: Well, again a personal approach. If I want my appendix out, I hire a doctor, if I want to compute I hire a computer programmer and if I want to do engineering I hire a competent engineer. I don't do these things myself. That is simply my approach to the satellites. There were other people there who did it much better than I would have done it. I am very pragmatic.

Did the advent of satellites change your science?

Wyrtki: Oh yes, it has changed. It began with surface temperatures. That was the first parameter for which we got global coverage. Then came the clouds, cloud motion vectors, that gave us the winds. This was an enormous advance.

What about sea level elevations?

Wyrtki: Eventually TOPEX and the altimeters. I did not participate in the use of altimeters anymore. We had younger people who were doing an excellent job at that. It is not necessary that you do everything.

In 1948 the theories about the westward intensification of the big gyres were published.

Wyrtki: I was a student at that time and I remember that Wüst showed me the paper by Stommel and it was a big surprise and everybody thought that it is a wonderful thing that happened. So, these insights are being recognized when they happen.

The physics behind the β-effect and the driving by the wind is relatively simple - Why has it not been detected earlier?

Wyrtki: Because nobody had the idea. That is the reason.

What are the causes of scientific progress?

All the four points you put here[26]. Gelegenheit ist Zufall[27], it is certainly not planned. The progress in science I don't think is planned. It happens when certain problems are ripe for a solution. Most people will say that progress in the sciences happens through logical thinking. This is certainly an important ingredient, but I strongly believe that most progress is due to imagination and intuition, much like art is being created. Logical thinking and experimentation are of course very important in confirming and solidifying the ideas born by intuition and imagination.

What is the role of nations?

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[26] On the tentative list of questions, the items funding, opportunity, people and coincidence were listed.
[27] Opportunity is chance.

Wyrtki: Well, we can keep that short. First of all the role of the various nations in ocean research is basically dependent on their wealth. The wealthy nations can put a lot of effort into research and they will succeed because research after all is expensive. I don't really know what to answer to that. Different nations are definitely interested in different things. Japan for instance, is a lot interested in resources in fishery and so on. Other nations are interested in other aspects such as oil or geology.

...such as ....military?

Wyrtki: Military is of course an option. Russia and the US have been tremendously interested in military aspects of oceanography.

Nations can also act in the opposite way. This is what I want to point out with regard to Indonesia. You know, when Arnold Gordon planned this big through flow experiment, Fritz Schott wanted to do the moorings, Arnold Gordon the hydrography and I came in a little bit with sea level, but the Indonesians didn't want international participation. I remember one international meeting on which an Indonesian admiral said flatly "we don't want any damned foreign ship in our waters." So Indonesia has excluded to a large extent progress because they did not allow other nations to come in and work with them. And this has hampered progress in the knowledge about their waters and especially about the throughflow from the Pacific to the Indian Ocean.

What about international organizations?

Wyrtki: International organizations are necessary, in order to get ships into foreign waters, to make data exchange and similar things, to enable international cooperation, because you can't install observing stations somewhere unless you have permission of that country. You can't do research within the 200 hundred mile zone unless you cooperate with that country. All these international organizations are necessary. Some do very good jobs, some not. But there is a need for it.

What about physical oceanography as part of a more general environmental science?

Physical oceanography is in some way basic to all the other branches of oceanography, because all the others are simply embedded in the physical environment. In order for biologists, chemists in particular, to explain their results they have to go back to ocean circulation and to physical processes. For that reason it will always be the main part of oceanography. Maybe not the most important one, but the main-indispensable part. You cannot explain plankton distributions and productivity without knowing about circulation, mixing and other processes.

Has oceanography become also a sub-discipline of climate research, or global change research?

Wyrtki: Oceanography exists quite independently of climate research. It is certainly not a sub-discipline, but a very important component of it, because of ocean- atmosphere interaction. The ocean definitely plays more than the role of a copper plate.

A wet copper plate.

Wyrtki: Yes, something like that. The ocean is awfully active. The ocean is handling the storage of heat. When it comes to climate prediction or long-term weather prediction, then the ocean plays a major role in providing the heat storage and in advecting heat. Advection is a much neglected phenomenon in most studies or explanations of the ocean-atmosphere system.

What was the background of you mentioning the copper plate? Were there people who said, the ocean is just a copper plate providing heat for the atmosphere?

Wyrtki: This claim has been made by some meteorologists. It has seriously been claimed that the ocean doesn't count, but we are beyond that now.

Could you say names of proponents?

Wyrtki: I would say, GFDL.

Should the physical oceanographers give more interest to the other disciplines, to biology, to chemistry, in order to give more exact explanations into these sciences?

Oh yes, it doesn't hurt, there will always be physicists who are just physicists, but for an oceanographer general knowledge of the surrounding fields of interest is very important, if he wants to make his knowledge applicable. If he wants to talk with a planktologist about vertical mixing or such things then it is very important that he has understanding of the mutual subject. So I would say it is a general principle: additional knowledge doesn't hurt.

What does it mean for the education of the students? Should we still have this classical education that they study physics, mathematics or so on? Or should we have some general education in marine sciences?

Wyrtki: It should not be mandatory but it should be very much encouraged. To make things mandatory is not a good idea. That means you would prevent a computer programmer to become an oceanographer by forcing him to do some biology in which he is not interested at all.

In your career there was always some link to applications. When you did the tuna research, when you were in Indonesia, there was always an element of usefulness. Is that so?

Wyrtki: No, not useful, but realistic. I'm a realist and I want to work on things that represent the real world that give an understanding of what there is. I am not a friend of speculations and fancy theories, I like to analyze facts and put them together and explain them.

Did you have to write in your proposals "this is important for fisheries or for ..."?

Wyrtki: You usually say that and it is generally recognized that this is lip-service.

On your list of items, you ask about the role of science organizations, big science, universities, centralization. Big projects are necessary, for the very simple, pragmatic reason, that an individual can't do them. An individual cannot launch a satellite and use all the data that come back. For big experiments you need cooperation of many people. This is a practical question. But big science does not mean, that one should take the funding away from all the individual scientists. Individuals have their own ideas and often very good ones. There are enough scientists that don't like to be involved in community projects. So one has to keep a balance between them. The same basically applies to universities versus government organizations. The universities are providing diversity and individualists. They allow the individual scientists to do work outside the mass, and they give him the freedom to do what he likes to do. In contrast, government science is mostly directed science, that means, the people involved in it are being told what they have to do.

But there are also research institutes like Max-Planck-Institutes.

Wyrtki: They are taking a middle position between the two. Depending on the country, some of these research institutes are tending more to be like university institutes, others more like government institutes. So, there is a real spectrum between a concerted government effort by the Navy and a small university with individuals. The whole spectrum exists, and any part of the spectrum is useful.

When you came to Hawaii in 1964, the Department of Oceanography had just been established. You were among the first professors of that department. The department grew relatively quickly over, say, 25 years and then this new school[28] *was formed. So the number of colleagues grew tremendously. How has this growth influenced your work as a professor, as a teacher?*

Wyrtki: I personally prefer to be in a small university, in a small institute that is relatively independent. I do see the need for bigger organizations, but there is as much good science coming out of small institutions and individual efforts as out of big institutions. The growth did not at all effect myself - I was in a position to remain sufficiently independent from the big institution to do what I wanted to do. This may not be the case for all scientists in that institute.

How efficient is the steering through soft money projects? When the government is saying they want to support certain type of research and they offer soft money.

[28] School of Ocean and Earth Science and Technology (SOEST)

Wyrtki: They have said that many times to me and I had to say, ”no, thank you”. One day the Office of Naval Research representative told me ”climate is out. Forget climate funding, anything climate related.” I said ”fine. What can I do, I go to the next agency”.

Is it not a very efficient type of control, which is exerted by the government?

Wyrtki: No, the agencies have their own priorities and there is a good reason for that. The Navy has certain priorities, they can't just support the Honolulu symphony.

You had sufficient sponsoring organizations to get money for any idea you would like to realize?

Wyrtki: Yes, you are right. We have been in the US in the fortunate situation that we had over decades surplus funding - my opinion. We have enough funding to keep all the good scientists busy. There will always be people who say ”I should get funded.” No doubt about that. There are always people who say funding is not enough.

Big projects. There are certain things for which the big projects are necessary. The weather service can't live without big projects, nor can the fishery service. But this is applied science, this is in some way even technology, but when it goes beyond that and it comes on the National Science Foundation level, then, the peer review system works well and there should be no centralization. I am not much in favor of these centralized projects. I've been for many years chairman of NORPAX. It was really not that centralized, but nonetheless funding was in some way restricted to the program.

A new term I like to bring in is ”political science”. When politicians use science it gets hairy. There is a story being told in recent months that a government scientist and a government official were talking with each other and the government scientist said, ”oh, my data show this” and the government official said, ”why don't you change the data”. That is “political science”. And that's what scientists should avoid.

Is this a real problem in the United States now, or world wide?

Wyrtki: It is a real problem for all countries, if politicians want to tell their population something that is contrary to scientific evidence.

In industry, this situation has existed for a long time, but it becomes dangerous to scientific freedom if such situations would happen and science would be exploited for political purposes.

What is influence of media and the impact of media attention that certain people receive?

Wyrtki: Media attention is good for science but media attention very often confuses the issues, because they might very well get practical and political aspects into it.

Another problem is ”truth in science”. In this case you have to differentiate between science and scientists. Science per se eventually converges on the truth. We learn things and they become knowledge. Scientists are not necessarily very objective when it comes to make propaganda for a cause, like the blown-up predictions that are now being made of weather and climate, of El Niño in particular. We are hearing predictions, that are being blown-up by the press and of scientists making statements, which they cannot defend in the long run. This is dangerous for science.

Why do they make these statements?

Wyrtki: Because they are human. They want to show off. If you stand before a TV camera, you give a big talk, you say El Niño is coming....

What do you think about present day forecast of El Niño and La Niña? How good are they, for how long are they good?

Wyrtki: Scientists like to make forecasts. Forecasts are made about the weather and we know reasonably well, what the limitations are. Forecasts of climate are a lot more uncertain and in particular El Niño forecasts. There are several models on El Niño. If seven forecasters are making an El Niño forecast, then four may be correct, three may be not correct. The four who are correct claim in front of the TV camera that it was a success, the three who were incorrect are being quiet until the next time. Most forecasters - I could show you examples - are saying after the fact that they did made a valid forecast.

Then they say they have made a forecast nine months in advance. The question is what did they forecast? Did they forecast the beginning of El Niño or the peak of El Niño? You will find out that they forecasted the peak of El Niño, which was, say, in August. The El Niño started in March and they made the forecast in December. December to August are nine months, so they claim they made a nine months forecast, when actually they made only a three months forecast.

When you make a forecast, you have to be awfully specific what you are forecasting, and not just make a press release that something will happen. Therefore, I am quite skeptical about these forecasts. I had a nice email exchange with my friend Glantz in Boulder - he is an expert on social-economic impacts of El Niño and he would like to use forecasts to tell the farmers what they have to do, to seed rice or cotton, for example. He asked whether the last El Niño has been forecast and he came to the conclusion ”not really”. When El Niño started, when the first indications came up, people started to claim that they had forecast it.

There should be a better control about what El Niño forecasts are made. And scientists should be a lot more honest.

Is it time for one big international center, such as the European Center of Medium Range Weather Forecast, for El Niño forecasting?

Wyrtki: Yes, it may be necessary and economical to have a center that collects all the data because the data collecting effort would be common to all. Making a forecast is the use of the data. That comes one step afterwards, and can be made on the basis of the same data by many different people.

The success of the European Center of Medium Range Weather Forecast is based on their data collection and data analysis processes.

Wyrtki: And then you give the data to the forecaster in Moscow, Frankfurt or elsewhere. And the forecaster makes his particular forecast for a region that he knows better than the others. In the end one best model may develop. We are at the beginning of the era of models. There are great things to come.

The role of your colleagues, of the working team, of schools. Did you experience during your scientific career that there are existing schools, groups which have certain minds, certain theories, is this important in oceanography?

Wyrtki: The exchange of ideas, opinions, plans and so on is most important for a scientist. Otherwise you become very soon sterile. It happens on large scales, through conferences in an objective way, through personal friendships most consistently, and most scientists participate in this interaction.

Die lieben Kollegen[29] come of course in all sizes and shapes. There are the nice ones, the ones that are generous, that are stimulating and that are open-minded – Hank Stommel was a prime example of that. And then the average that doesn't care and is uninterested or irrelevant to you. Then of course the bad guys, the people that are arrogant, trouble makers and are vicious. You have them all, scientists are just like any other people.

You essentially select a group with which you feel comfortable and want to do things. That group changes with time, with the interests that you have. Some people stay a whole life in the same group because they never get away from a particular subject. You change the groups when you change topics; you talk to other people when you deal with deep circulation than when you do El Niño or climate.

Are there different ways of thinking? Is there an American way of thinking in oceanography, or a western European or a Russian way of thinking?

Wyrtki: There will always be schools, that means interest groupings around a problem like NORPAX or like GEOSECS. GEOSECS was one of the closest groups that I have ever seen in scientific cooperation. There are more loose groups, but it is hard to say - I haven't been too much involved in group efforts.

Have you experience that certain groups were blocking progress?

[29] The dear colleagues

Wyrtki: Oh yes. As already said, science is very often a matter of fashion. When everybody was in ocean eddies, we had to fight long battles to get ocean monitoring going. In later years the people who wanted to make so-called process-oriented experiments were fighting bitter battles at the National Science Foundation with other groups who wanted to make ocean surveys like GEOSECS or like the WOCE sections.

In the sixties there have been long standing battles between the US East Coast and West Coast, Woods Hole versus Scripps. That went on. It was a competition of opinions, very often. The Woods Hole people were interested in controlled experiments like MODE and POLYMODE and the kind, and the Scripps people were largely interested in the larger ocean surveys that had relation to fisheries, climate, and to large scale features. These are opinions that go back and forth. There is fashion in science and group building, no doubt.

What are your forecasts of the future of science.

Taping the interview: Hans von Storch and Klaus Wyrtki

Wyrtki: My general forecast of what will happen in the future is that first of all we will get truly global coverage of observations, from satellite and eventually from other systems like the TOGA TAO and similar systems, because the satellites don't penetrate inside the ocean.

So far the Southern Hemisphere is grossly neglected. The Southern Hemisphere will be in the end more decisive for the interpretation of climate change than the Northern Hemisphere, because it connects the three oceans, and it is the most powerful ocean - atmosphere engine that we have and it has not been sufficiently studied because of the lack of data. People study these things first when they have good data.

No wonder, if certain people in the sixties did not want to go to Antarctica because they became sea sick and found it too cold.[30]

Wyrtki: My general forecast of what will happen in the future is that first of all we will get truly global coverage of observations, from satellite and eventually from other systems like the TOGA TAO and similar systems, because the satellites don't penetrate inside the ocean.

You are so right about that. But there are other people who love it.

Could you make another kind of forecast, not about science, but about the nature itself. Within the next fifty years, will there be global warming? How will the average temperature at the sea surface change within the next fifty years?

Wyrtki: My general forecast of what will happen in the future is that first of all we will get truly global coverage of observations, from satellite and eventually from other systems like the TOGA TAO and similar systems, because the satellites don't penetrate inside the ocean.

There are many people working on that problem. I have only an opinion. We will see a continuation of global warming, whereby I am not quite positive whether it is primarily natural, or primarily man-induced. Probably both components are important. When you ask me how big that change will be over fifty years, I would say, not more than it has been in the last fifty years. With regard to sea level my successor[31] in the sea level project has made a very interesting plot. It starts with the first prediction of three meters over one cen-

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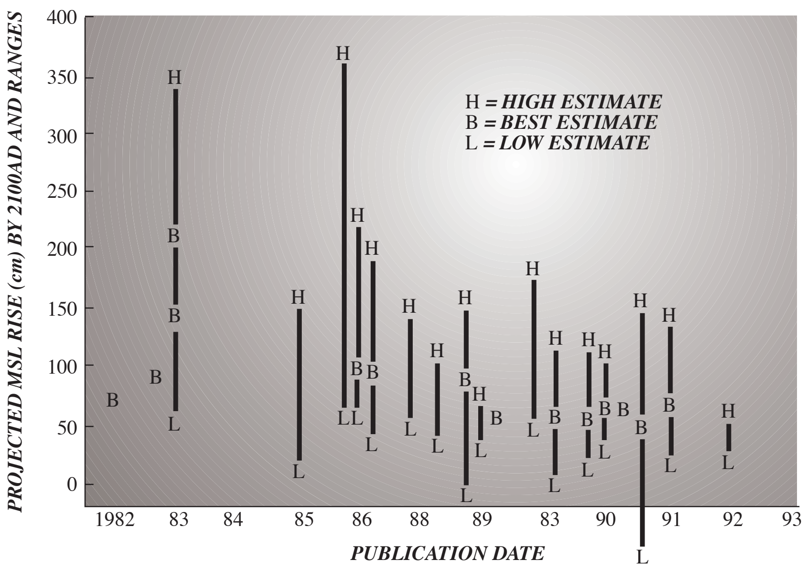
[30] see page 41.
[31] Gary Mitchum.

tury, or something like that by the club of Rome. Then came a few years later one to one-and-half meter and then 0.2 to 1 m and later forty cm. He put a regression line through that cloud of dots which has an exponential decay to the average value of the last hundred years. So this is where the forecasts go. They converge towards the extrapolation of the last hundred years. That is approximately correct for the next fifty years, 10 cm in fifty years, which is a little more than in the last century, which was 15 cm.

Climate change will always be of interest, ocean-atmosphere interaction in connection with climate change. It will lose in importance. What will gain in importance, will be chemical pollution, biological change - which is of course embedded into climate change - water resources. Years ago in Cabo San Lucas in Mexico, I had to spend three dollars for a liter of drinking water. I said to my students, "before you die you will see that water is more expensive than gas".



Gary Mitchum's summary of predictions of sea level changes since the early 80s.

But water resources have a lot to do with climate change.

Wyrtki: My general forecast of what will happen in the future is that first of all we will get truly global coverage of observations, from satellite and eventually from other systems like the TOGA

TAO and similar systems, because the satellites don't penetrate inside the ocean.

They do, they are a fundamental part of climate change. But no doubt, the warmer, the more rain you get. It may not fall at the right places. But basically it will still fall in the same places as now. There may be shifts, but unless we get a total change of atmosphere circulation the monsoons will always happen.

Do you think, that independently of climate change we are running out of water?

Wyrtki: Yes, I think so. It will be a scarce commodity.

Have you anything to do with paleoclimatology?

Wyrtki: No, I shied away from it intentionally because to me it was too speculative.

Do you think it will play an important role in the future?

Wyrtki: Any part of science that can be thoroughly documented is important.

Will it become fashion?

Wyrtki: It has been a fashion. If it will remain a fashion, this is another thing. I don't have an opinion on that. It lends itself to lots of speculations and hypotheses, because it is so difficult to prove anything.

There are many established facts about paleoclimate. No doubt that we know a lot about the Ice Ages. That is beyond speculation, but if you start to link Ice Ages and ocean circulation you get into speculation.

Do you believe in these results indicating sudden climate changes?

Wyrtki: It depends what you call sudden.

Within decades of years.

Wyrtki: Decades it seems to be a little fast. Hundred years I would say is perfectly possible. But this is again just an opinion. In order to get climate changes you have to start substantial melting processes or accumulation processes and they do not happen in decades.

When we have what you call truly global monitoring systems, will we get long range forecasts with models based on the good knowledge of the dynamical state of the ocean?

Wyrtki: What I said before - models are in their early stages of development. That means we will get many more surprises out of models, we will get much, much better models in the future. I am talking about climate models, not necessarily applied models like ship routing or so. Better and more comprehensive observations will feed better information to models. I don't know to what extent the physics of the models need to be improved, but I think they will be. Science doesn't give up on these things, there is always something that can be done better. Our understanding of the processes, for instance the basic processes of ocean atmosphere interaction, that govern nature will increase, and therefore the models will improve.

But there are limits to predictability. Many scientists and certainly many outsiders do not want to accept this. People always want to have certainty about a prediction. They think, if somebody gives them a prediction it should be certain. But this is by no means so. A correlation of seventy per cent means that two times you are right and one time you are wrong, roughly speaking. So if you make forecasts that go beyond the dynamical range of the model where turbulence or chaos takes over your forecast becomes essentially statistical. You can run 25 models hundred times each and you have two thousand five hundred predictions and you average that and you think you have made a solid forecast. No, because only one will be realized by nature. Nature will not realize the average. There is a limit to forecasting.

Another technique of forecast is basically the extrapolation; actually, it is more than an extrapolation, for instance, when you predict climate, you are projecting into the future. This is better than an extrapolation. You are projecting what developments or what changes can go on and you may give a certain envelope to this projection. The envelope will become wider and wider with time. These things are all recognized by reasonable scientists. I don't tell anything new.

Do you expect new developments or breakthroughs by new instruments?

Wyrtki: I have too little knowledge about instruments. The satellites are new instruments, if you want to say so. We will see more.

The basic principle of Dr. Wyrtki is, if you look closer at something with a new instrument you find something.

Wyrtki: That's what Wüst said and I demonstrated it.

Will there still be interest in science in fifty years? Will people listen to scientists?

Wyrtki: There will always be curiosity, science is driven by curiosity. There are always people who are curious about things and they want to know it better.

We haven't finished the prediction. You ought to look at developments that in the future may take place. One point that is totally unknown to me is warfare, fortunately. I do not have the slightest idea what the role of oceanography will be. It has had a considerable role in the last thirty years. More money has definitely gone into anti-submarine warfare than into academic research. The other open problem is of course the population explosion and what to do about it. These problems will occupy us in the next fifty years.

You wrote about that. I remember you had an article when you discussed the prospects of climate change.

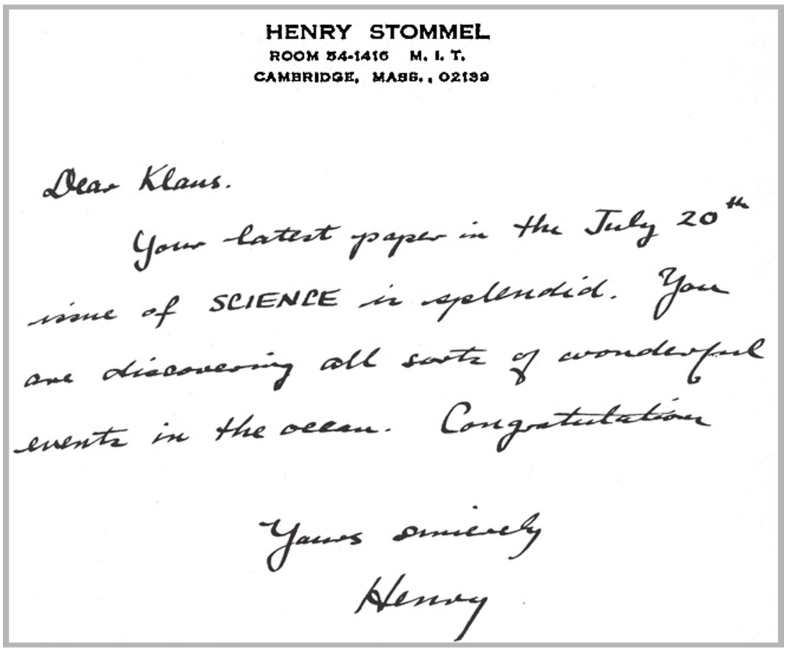
Wyrtki: I said that sea level rise will be a picnic compared with the population explosion.[32]

You have already spoken a bit about what you consider your most important achievements. You said freedom of data exchange, the monitoring idea and other things. Is there anything else you would say which has been a major achievement of yourselves?

The other items are plainly scientific ones. There is of course El Niño; its explanation as the ocean response to the atmosphere and

[32] Wyrtki, K., 1989: Sea level rise—the facts and the future. *Pac. Sci.*, **44** (1), 1-16.

later on the explanation of the El Niño cycle as an accumulation of warm water that eventually changes atmospheric circulation and triggers the next event, it constitutes a kind of heat relaxation of the ocean-atmosphere system of the Pacific.[33]



HENRY STOMMEL
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Dear Klaus.

Your latest paper in the July 20th issue of SCIENCE is splendid. You are discovering all sorts of wonderful events in the ocean. Congratulations

Yours sincerely
Henry

Letter from Hank Stommel

Which are your favorite own publications?

Wyrtki: These are the thermohaline circulation from 1961[34], and the deep sea basins, the oxygen minima from 1962[35]. Then I would mention the Peru Current, which linked the horizontal and vertical movement in a very large area of the ocean.[36] Then you have the In-

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[33] Wyrtki, K., 1985: Water displacements in the Pacific and the genesis of El Niño cycles, *J. Geophys. Res.-Oceans*, **90**, 7129-7132.

[34] Wyrtki, K., 1961: The thermohaline circulation in relation to general circulation in the oceans. *Deep-Sea Res.*, **8** (1), 39-64.

[35] Wyrtki, K., 1962: The oxygen minima in relation to ocean circulation. *Deep-Sea Res.*, **9**, 11-23.

[36] Wyrtki, K., 1963. The horizontal and vertical field of motion in the Peru Current. *Bull. Scripps Inst. Oceanogr.* Univ. Calif., **8** (4), 313-346.

dian Ocean Atlas and the analysis of the Indian Ocean circulation and with that came the Indian Ocean jet.[37]

What about your Baltic studies?

Wyrtki: The Baltic study was an important piece of work for me, it was an effort to understand the water budget of a small sea that has sufficient information, and to understand both the annual cycle of exchange and the fact that this annual cycle was basically wind driven.[38]

Is the Baltic a model of the global ocean?

Wyrtki: In some ways, yes. It has a wind driven exchange, the Baltic is either pushing water out or holding water in, depending on the weather. The study about the water balance of the Baltic basically sumrized the whole story. The Fehmarn belt papers were about the dynamics of the exchange. [39]

Then afterwards the El Niño papers, and finally sea level and of course all the things that had to do with the dynamics of the Pacific upper ocean.

Sometimes people say scientists are creative when they are twenty five/thirty years. Then, after that the creativity is declining. Is that so in your view?

Wyrtki: That is putting it too early. Our typical Ph.D. age is 30 now. I was 25. But even at that time it was an exception, it was more like 27 or so. Unless you make an exceptional discovery as a graduate student, you start to be a scientist by 30. You need a build up time of maybe ten years. Between 40 and 50 you should have your peak productivity in new things. Between 50 and 60 should be a period where you consolidate knowledge and integrate.

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[37] Wyrtki, K., 1973: An equatorial jet in the Indian Ocean. *Science*, **181**, 262-264.

[38] Wyrtki, K., 1954: Schwankungen im Wasserhaushalt der Ostsee. *Dtsch. Hydrogr. Z.*, **7** (3/4), 91-129.

[39] Wyrtki, K., 1953: Die Dynamik der Wasserbewegungen im Fehmarnbelt I. *Kiel. Meeresforsch.*, **9** (2), 155-170.

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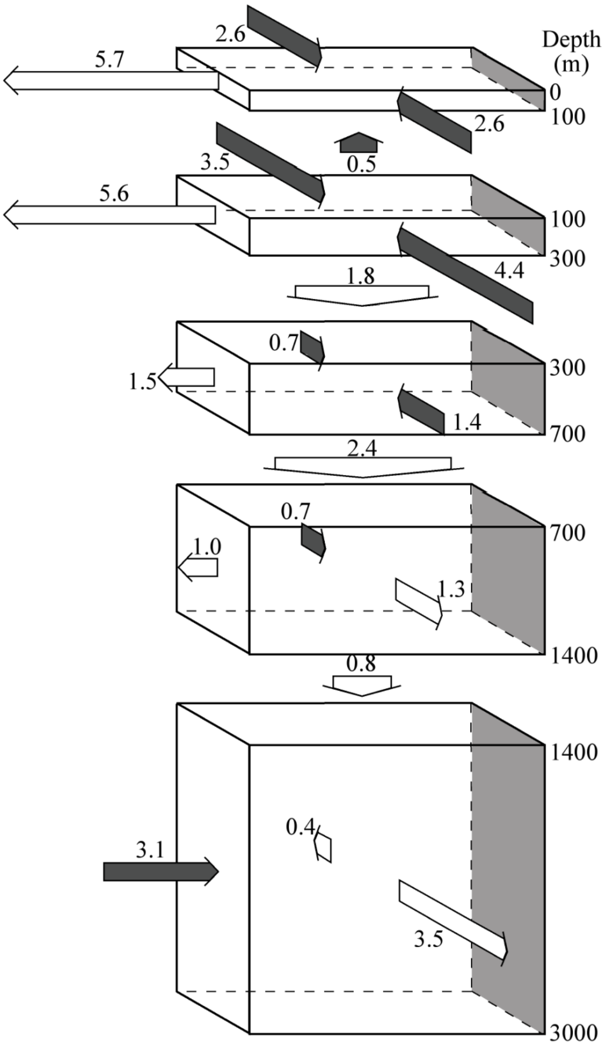


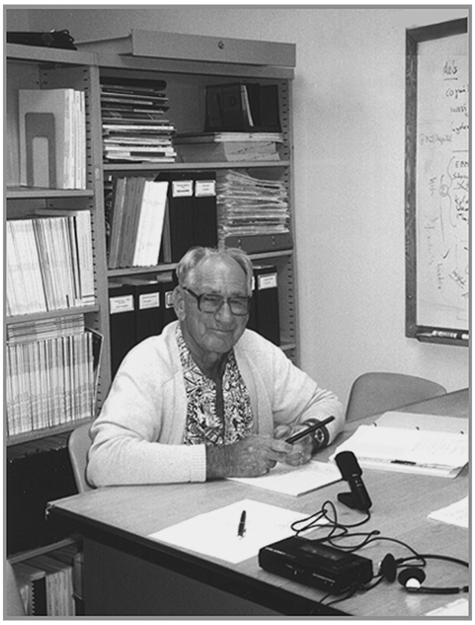
Block-diagram of the the water balance in different layers off the coast of Peru in million m^3/sec, giving horizontal transports in the five layers and vertical flow between these layers. From Wyrtki, 1963, Bull. Scripps Inst. Oceanogr.

Have you thought of writing a book?

Wyrtki: Yes, I have. What came nearest to a book was the NAGA Report[40] which you may call a monograph, also the Indian Ocean At-

[40] Wyrtki, K., 1961: Physical oceanography of the southeast Asian waters. Univ. Calif., NAGA Rept., No. 2, 195 pp.

las[41] is a big piece of work. I intended to write a book with the title "The Water Masses and Circulation of the Indian Ocean" and I gave it up since it takes about five to six years to write and by that time much of the information is superseded by new knowledge. Knowledge is accumulating these days at a rate that you can say after a decade things are old. That's too short a lifetime for a book.



Klaus in February 1999

You always had interest not only in science but you traveled a lot and you enjoyed also the nice environment here in Hawaii. To what extent was this part of your life also important for the science? This mixing of more private life and scientific life.

Wyrtki: It was a very lucky and favorable choice. First of all it was a true choice to come to Hawaii. After I had been here in 1961 for the first time I decided essentially that I would like to live here. Then it was the opportunity that a new institute was being built up in the middle of the Pacific.

We have to come to a conclusion ... the tape is ending.

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[41] Wyrtki, K., 1971: *Oceanographic Atlas of the International Indian Ocean Expedition*. National Science Foundation Publication, OCE/NSF 86-00-001 Washington, DC, 531 pp.

Wyrtki: I have no regrets about the things I have done. I have enjoyed the scientific career that I have made. I would do the same thing, it may not turn out the same way because we are subject to chance, you know, but basically I would do the same.

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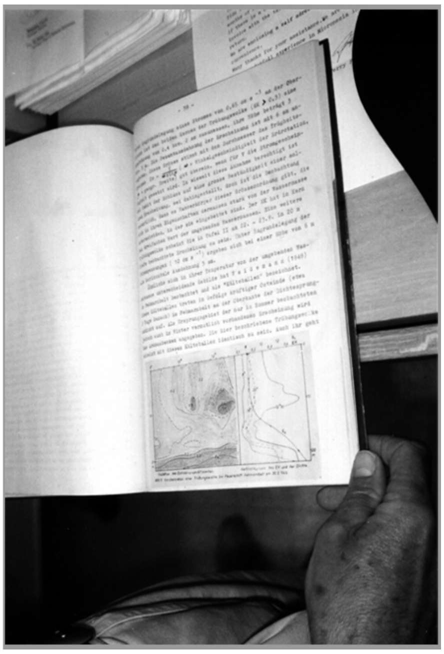

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SHORT BIOGRAPHY

Prof. Dr. Klaus Wyrtki
Born: February 7, 1925 in Tarnowitz, Germany
Married. Children: daughter born 1954; son born 1962
Naturalized U.S. citizen, January 5, 1977

Education

University of Marburg, Germany, 1945 – 48
Mathematics, physics, geography
University of Kiel, Germany, 1948 – 50
Oceanography, physics, mathematics
May 20, 1950 – promotion to Doctor of Natural Sciences
with magna cum laude

Experience

1950 –51	*German Hydrographic Institute, Hamburg*
1951 – 54	*German Research Council, post-doctoral Research Fellowship at the University of Kiel*
1954 – 57	*Head of the Institute of Marine Research, Djakarta, Indonesia*
1958 – 61	*Commonwealth Scientific and Industrial Research Organization, Division of Fisheries and Oceanography, Sydney, Australia; Senior Research Officer; later, Principal Research Officer*
1961 – 64	*University of California, Scripps Institution of Oceanography; Associate Research Oceanographer; Research Oceanographer*
1964 – present	*University of Hawaii, Professor of Oceanography*

Professional Activities

Editor of Atlas on Physical Oceanography of the International Indian Ocean Expedition
Member, Editorial Board, Journal of Physical Oceanography 1971 – 79

Chairman, North Pacific Experiment (NORPAX) 1974 – 80
Member, SCOR Working Group on the Prediction of El Niño
Member, Science Working Group on the Topography Experiment (TOPEX)
Chairman, IAPSO Committee on Climate Changes and the Ocean
Member, NOAA Panel on Climate and Global Change

Invited speaker at numerous international and national symposia and conferences

Participant in numerous international conferences and member of scientific panels of international organizations such as:

Intergovernmental Ocean Commission (IOC)
World Meteorological Organization (WMO)
International Oceanography Data Exchange (IODE)
UNESCO Special Committee on Ocean Research (SCOR)
International Association of the Physical Science of the Ocean (IAPSO)

Awards

Excellence in Research Award, University of Hawaii 1980
Rosenstiel Award in Oceanographic Sciences, University of Miami 1981
Fellow, American Geophysical Union 1982
Maurice Ewing Medal, American Geophysical Union 1989
Sverdrup Gold Medal, American Meteorological Society 1991
Achievement Rewards for College Scientists, ARCS Foundation, Inc. 1991
Albert-Defant-Medaille, Deutsche Meteorologische Gesellschaft 1992

Interview with Reimar Lüst

prepared by Hans von Storch and Klaus Hasselmann
on 2 December 2002

Interview: Mr. Lüst, we are here on board „Ludwig Prandtl“ today, and if we are correctly informed, you knew Ludwig Prandtl. Shall we start with that story?

Prof. Lüst, Prof. von Storch and Prof. Hasselmann
on board „Ludwig Prandtl“, December 2002

Lüst: I met him when I started my doctoral studies in Göttingen. For my theme I had to apply the hydrodynamical equations in order to examine a rotating gas mass. For this, turbulence theory with its so-called mixing path-length of the turbulence elements was very important. It had been introduced by Prandtl. Therefore, I was advised to talk with Prandtl, whose office was in the adjacent building. The Max Planck Institute for physics was in Böttinger Str. 16, and next to it was the Max Planck Institute for Fluid Dynamics with Tolmien as director. Prandtl still visited the institute regularly. I still see him entering the building in his stooped manner, I also think he showed me some experiment at that time. At least that is how I remember him with his short beard. I do not know how old he was in 1950, but he was surely far over 70. I do not know whether you remember him, Mr. Hasselmann.

I never got to know him, but I wrote my diploma thesis wish a student of his, Karl Wieghardt, in Hamburg, and later I graduated with Tolmien

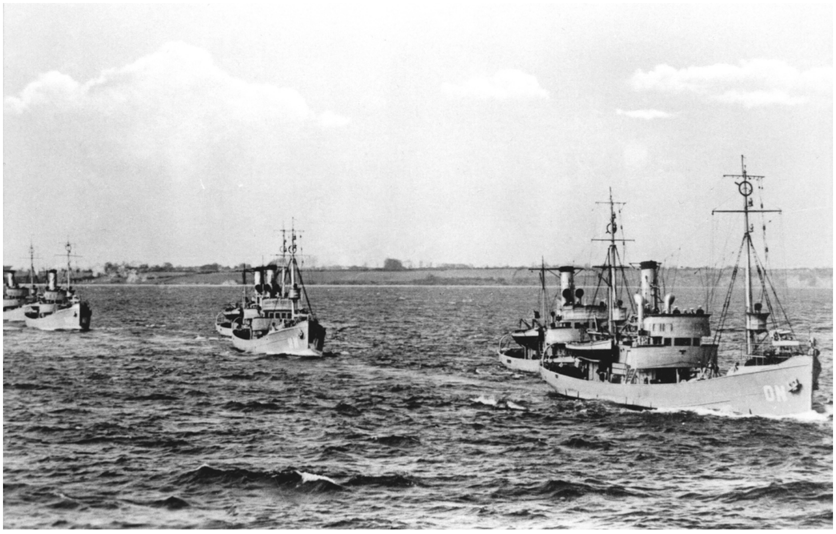
Lüst: Tolmien, an aerodynamicist and fluid researcher, was co-advisor to my doctoral thesis, and he had to evaluate it later. I also chose fluid research as subsidiary subject for my doctoral examination. This was, because with Tolmien it was always clear what he would ask. He used to write the main key statements on the blackboard during his lectures. Those who remembered these key statements were sure to pass the examination with Tolmien with 'magna cum laude'. Choosing fluid research as subsidiary subject was a sure method if a student was able to learn something by heart.

Spring 1941

I studied in Frankfurt and as a postgraduate I worked at the University of Göttingen. In Göttingen one was reqired to attend lectures before graduating. However, I did not have much money at that time, and so I chose two lectures which were free of charge but accepted. One lecture was called 'lntroduction to Göttingen's library'. It was given by the director of the library. The second one, 'Everyday psychology' was given by the psychologist Alex. My book of studies was thus adorned with these two attestations. I attended the other lectures illegally, including those of Tolmien.

We discussed in advance that your biographical data contain a great deal of material. Would you like to narrate something without us asking concrete questions?

Lüst: This vessel's captain declaring himself a coastal skipper reminded me of my grandfather who was a coastal skipper, too. He was from Esens, and my father was born there in Eastern Frisia. Although I never met my grandfather – he had died before I was born – he must have been the reason for my absolutely wanting to join the navy. Therefore, I enjoy our today's cruise with this coaster.



Outpost boats departing from Gotenhafen, 1941

My original plan for the future was to study naval architecture, and that was why I volunteered for the navy in 1940. At that time it was common practice to be released from school with only a maturity notation in one's school report which means I never actually took a final school leaving examination, because in January 1941 1 was drafted into the navy. The detachment of recruits was billeted in Brake/Unterweser. After leaving the recruits, I completed a repair course in Kiel, because I had chosen an engineer's career. Then I started to go to sea on an outpost vessel, a rebuilt trawler, constructed in 1914. It was a hard time.

It appears that, among other things, you learnt how to stoke a ship's engine.

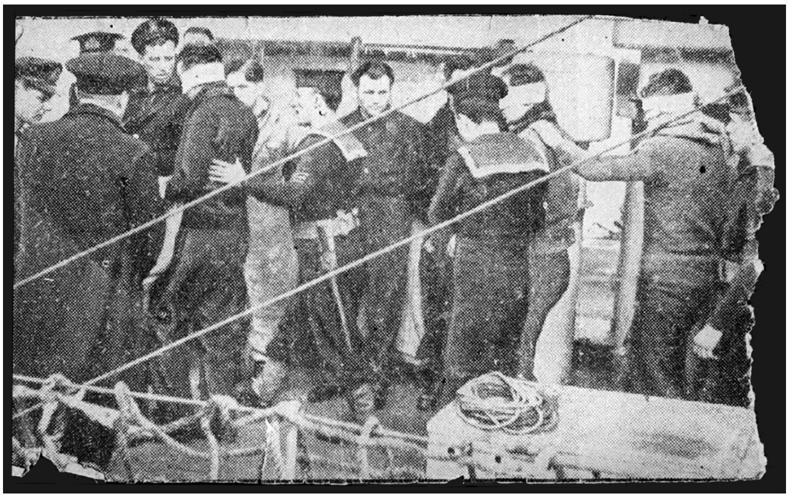
Lüst: Yes, in those days fuel was in a solid state, that is to say, coal. I had to stand below deck in the boiler room and trim coal, and I also had to fetch coal from the bunker. Another of my duties was to crack coal. Lots of really large blocks had to be crushed. The most exhausting job was to clean the flue boiler. When the sea was rough. I was often seasick. It was perhaps the physically hardest time I ever endured.

But shortly afterwards you said goodbye to coastal shipping and ventured out on the ocean.

Lüst: Yes, it was like that in the navy. You went to sea and were then again ordered to the marine school in Kiel. During the next period I went to sea again on the outpost boat. One couldn't volunteer, you just suddenly received a command. My next assignment was on board a submarine. The submarine was located in Memel with a flotilla for training commanders. There I had to learn how to live on board a submarine.

You were then sent to war quite soon, and it did not go well.

Lüst: No. The training on board the submarine took about half a year, first in Memel, later in Pillau. Then I was assigned to a new submarine which had just passed its commissioning tests. I entered the boat as engineer-officer in Szczecin. We went from Szczecin to Kiel where we picked up torpedoes and all the other equipment needed for an encounter with the enemy. In April 1943, we were seen off with the usual fanfares and a band in Kiel, although the navy's leadership must have known that we hardly had a chance to survive. We reached the Atlantic via Norway, between the Faroe Islands and Greenland. Our first action was to intercept and attack a large convoy. In this operation we were bombed by an airplane which damaged us to such an extent that we were no longer fully submersible. After another convoy battle we had to retreat. Two days before reaching our home port Lorient another plane got us. This was the time when the British were making full use of radar for the first time. We had a device with which we were supposed to be able to detect radar rays. We called it the German Cross in Wood. It was placed on the conning tower when moving above water. However, we were always worried that something might not be quite correct. Later, in captivity, I learnt that it was quite good for detecting radar rays, but it radiated at least as strongly itself, which meant that the planes no longer needed to emit radar rays to find us, but could already track us directly this way. That plane called up two destroyers that chased us for twenty hours, after which we finally sank to about 320 m. Looking at my commander's face, I knew this was the end. There was hardly a chance to survive.



As prisoners of war we were led from board the British aircraft carrier in Schottland

Did the submarine deliberately descend? Or was it damaged?

Lüst: We were damaged, slowly filling up with water which we could barely control. The commander then decided to try to resurface, which meant blowing compressed air into the diving cells. It was not clear whether there was still enough compressed air available, but suddenly the air parcel must have expanded. We shot upwards and then saw the destroyers at the horizon. The commander tried to send radio messages, but the destroyers began firing at us with artillery. The commander then ordered the boat to be scuttled. Two hours later one of the destroyers had finally fished up most of the crew. Thank God, I was among them!

Long after the war, an American colleague, the head of the American space administration (NASA), sent me the report of the British commander who sank us. With pictures! Along with excerpts from the war log of the German submarine force including our last radio messages.

Did this bring about any new insights into the operation?

Lüst: No. But it was quite interesting to learn about these details. In those days one was still trying to capture a submarine, but in our case without success.

But you once mentioned that they succeeded in capturing a submarine which you ran into again later in your career. And the story is that you contributed to displaying it more realistically for the public.

Lüst: During the war, the Americans deployed a special aircraft carrier unit to capture a submarine.

They succeeded in 1944. This submarine was taken to Chicago after the war. After I had just arrived in Chicago, I saw a large sign next to the motorway, which runs parallel to the lake: 'Attention. Drive carefully. Submarine crossing.' Because they were lifting it out of the water there. The Museum for Science and Industry is very near to the University of Chicago. Occasionally, repairs for the museum were carried out at the Physics Institute. One day the submarine's manometers arrived. An employee in the workshop knew that I was working at the institute as a Fulbright fellow and informed the director of the museum. So, they asked me to help prepare the submarine for the museum. Finally a long article was published in the Chicago Sunday Times with a picture of myself sitting in the submarine. When you go there now, you may perhaps still hear my voice on tape calling out commands in German.

A happy ending of this nonetheless terrible chapter.

Lüst: Basically, however, being captured in May 1943 was the best that could happen to me. I was finally taken to America via Gibraltar and England and landed in Texas in a large German prisoner of war camp, consisting of 1000 officers from the Africa corps, captured in Africa, and 3000 soldiers of lower rank. The camp had its own university, consisting of, among others, a physicist, two mathematicians, who were assistants at a German university, and quite a number of other scientists. They gave lectures, properly organised into semesters. At the end of a semester you had to pass exams in all the courses you had attended. I never solved as many exercise problems as during that time in captivity. I started with engineering, because I wanted to study naval architecture, but after to semesters I discovered that mathematics and physics came more naturally to me than machine designs and technical drawing. So, I switched to mathematics and physics already during captivity.

It is difficult to imagine what captivity was like I hear you were paid 20 dollars. What did you do with that money? Was there a shop?

Lüst: There was a canteen selling toothpaste and other goods.

But not the Courant and Hilbert?

Lüst: It was possible to order books. During the war, the Americans photocopied and reprinted a large number of German textbooks, especially the yellow Springer series, including ‘The mathematical methods of physics’ by Courant/Hilbert. Later ‘The mathematical tools of the physicist’ by Madelung and several other books were of particular importance to me. I finally had a considerable library, all of which I took home. Of course, I bought American textbooks, too.

What was the price for a Courant Hubert then?

Lüst: I think two or three dollars. It cannot have been much more.

Those must have been really open minded Americans who provided such possibilities in the prisoner of war camps during the war, don’t you think?

Lüst: Of course, the Americans were interested in keeping us busy. As officers we did not have to work according to the Geneva Convention. During the day, I was fully occupied by attending lectures. However, I also did a lot of sports. In the evenings, we played skat or bridge.

Did your period of captivity end after 1946?

Lüst: A year after the war had ended, we were finally taken back, to France first. We feared we might be handed over to the French, as was often done. Had this been the case, I surely would have had to spend another two years working in a French mine. Fortunately, I was set free exactly on my birthday, the 25th of March, 1946.

You then noticed that the immatriculation deadline in Göttingen had already been passed a week ago, and nobody had pity with you, at least not in Göttingen.

Lüst: Two days after my release I took a train, which was not easy back then, because between Kassel and Göttingen there was the borderline between the American and the British zones. In Eichenberg everyone had to step off the train and undergo a check. When I arrived in Göttingen, I went to the dean's office, and there they told me they were very sorry, but the immatriculation deadline was over. I even consulted the dean, Arnold Eugen, a famous, but quite choleric phsico-chemist who told me that too many people came, and they could not consider everyone. Even my argument that I had been a prisoner of war for three years and had only returned the day before did not interest him.

Was he the same person you mentioned in one of your seminars?

Lüst: Yes, the person who attacked me later. I drove on to Marburg the next day where I had a similar experience. I had a nice conversation with the physicist there, privy councillor Grüneisen, but I did not melt his heart, either. So, I came to Frankfurt. Before, I had not even known that Frankfurt had a university. I talked with the dean there, Erwin Madelung. When I recounted my experience, especially when I extracted from my bag the book I brought with me from captivity, namely his book, he was so pleased that he immediately organised everything. I could begin to study. He asked me whether I would present the book to him. He would hand me a new copy. I still have it at home with an inscription inside. I was lucky to begin my studies in Frankfurt, because technically I would have had to go to school for I did not have an 'Abitur' certificate, However, there was a regulation saying that three semesters of successful studies would he validated as 'Abitur'. Madelung recommended that I should study for one semester, and if I then passed an examination, two semesters of my captivity would be validated as well, which would provide me with my 'Abitur' certificate. And that is what happened. After five semesters I obtained my diploma.

We could perhaps say that your whole life career is characterised by this account. First your persistency, that you did not let yourself be discouraged in Göttingen, but found another way, and that you then hit the right note with the person in charge. I think this is a very nice story which really summarises much of your success in life.

Lüst: Retrospectively I have to say that it was a stroke of luck that I did not start my studies in Göttingen, because already in those days Göttingen was relatively full. In Frankfurt I could start with theoretical physics immediately which, most probably. I would not have been allowed to in Göttingen. We only were four or six students in Madelung's lectures on theoretical physics, which ensured that for every exercise every student was asked to come to the blackboard. This small circle around Madelung was a stroke of luck for my studies. I later went to Göttingen after my diploma. Madelung was quite understanding. He realized that I would not want to write also my doctoral thesis with him.

Those days must have been quite different from the present if you simply went to see people and knocked on their doors. They were there, and you said, Tell me, may I... ?' That way you also came to your doctoral thesis with von Weizsäcker.

Lüst: Yes, without prior appointment by phone. This was not common practice then. What I might have done was write a letter, but that would have taken much too long. So, in March 1949. I went from Kassel to Göttingen. I rang the bell at the Max Planck Institute in Böttinger Str. and asked the doorman if I might speak with Mr. von Weizsäcker. He explained he had to call first. He called, and I was immediately allowed to see Weizsäcker. Weizsäcker listened to my story. He said he was in a hurry because the institute's colloquium was to begin in a moment, but I could accompany him to the small room at the other side of the corridor where perhaps twenty persons could be seated. There I was in the last row, the door opened, someone entered and asked what was on the agenda. That was Heisenherg. That was how I got to know Heisenberg. The lecturer

turer was Arnulf Schlüter, presenting his first work on plasma physics which later became important for my whole scientific work, Von Weizsäcker accepted me as a doctoral student. At first he wanted to give me a theme regarding the general theory of relativity, but the experts said this was too difficult. I was therefore provided with another problem which I found more interesting, namely the question "What had slowed down the sun's rotation? How had the angular momentum been transported?" For the sun rotates relatively slowly in our planetary system, while most of the total angular momentum of the solar system resides in Jupiter. So my task was to calculate, using hydrodynamical equations, whether such an angular momentum transfer was actually feasible in a gas disk.

Federal President Scheel and Werner Heisenberg
at the Alexander von Humboldt Foundation, 1974

In those days Weizsäcker had just completed his theory with Heisenherg on turbulence. Were you, applying this turbulence theory?

Lüst: Heisenberg's doctoral thesis was already on turbulence theory, a work that gets cited again only now. In detention after the war Weizsäcker and Heisenberg wrote a paper on turbulence theory and the Kolmogorov spectrum, as it is now called. Already during the war, Weizsäcker had dealt with the origin of the planetary system and in 1948 had published a manuscript in which he first formulated the hydrodynamical equations to be used, without yet calculating anything himself. I was the first to make practical use of these hydrodynamical equations.

Abholung nach bestandener Doktorprüfung in Göttingen, May 1951

Perhaps one more word concerning the doctoral thesis. In those days it had been common practise to publish a doctoral thesis in German in the German 'Zeitschrift für Naturforschung'. It was thereby effectively buried. Twenty years later two Englishmen dealt with the issue of accretion disks that play a role in connection with neutron stars, and they practically solved the same equations and drew quite similar conclusions. Biermann thought that something would need to be done. He then made sure that in the seventies my doctoral thesis was translated into English by the Bavarian Academy, and since then it has been cited. Now, it is customary that anyone writing on accretion disks cites my work, too. It had been practically for twenty years.

When did you finish you doctoral thesis?

Lüst: In 1951. I finished it after two years, in May 1951.

As is right and proper. Under 30.

Lüst: Yes, that is true. I was 28 years old, indeed I had started relatively late.

No one wants to imply that you were a perpetual student. How did it go on after your doctoral thesis?

Lüst: First, when Weizsäcker had noticed that I had relatively little money - my father had died towards the end of the war – I was awarded a scholarship from his own funds. Weizsäcker gave many talks at that time, and the fees went into a fund from which I received a grant. I was allowed to take, I think, 50 Deutschmarks every month. When Weizsäcker spent half a year in America, his brother was responsible for his family affairs. I was thus entitled to pick up my scholarship money from our former Federal President for half a year. That is how I got to know him in Bunsenstraße 16, where the families of Laue, Biermann, and von Weizsäcker all lived together. After my graduation I stayed with the institute and received a scholarship. The salary of about 150 Deutschmarks was princely for those times. In 1955, the physicist John Simpson from Chicago asked me whether I would like to spend a year at the Enrico Fermi Institute. He had little money himself; but I could apply for a scholarship. It worked. In autumn 1955 I went to Chicago as a Fulbright fellow.

May we broach the subject again: what did you live on during the time of your studies and post graduation?

Lüst: During my studies in Frankfurt until the currency reform I got along along. The service pay had been continuously disbursed during my captivity, so I had a Reichsmark account to live on. In Frankfurt, and partly also in Göttingen, I gave private lessons. Once I even was on the dole as an unemployed person. This is not quite legal for a student, but the employment agency paid me unemployment benefits for about half a year, until I was awarded the scholarship by Weizsäcker.

But in principle your postgraduate time was unsalaried?

Lüst: It would never have occurred to anybody to pay a postgraduate student. I got my first real position as an assistant when I returned from America. After half a year in Chicago I went to Princeton, because I also wanted to learn from Martin Schwarzschild. I then returned to Chicago, where I was already paid with a grant. At the end of 1956 I went back to Göttingen.

The art of programming differed slightly from today's procedur, if I understood it correctly. There was a different way of creating the loops.

Lüst: In Chicago I could use the reconstruction of Neumann's electronic calculators. The machine was situated in the National Laboratory in Argonne. It was the first large electronic calculator, and for memory it had a cathode ray tube with a memory of 1,000. In those days you still had to programme every single step, add number ‘a’ to number ‘b’, save number ‘c’. fetch the number from memory. Programming ‘extract a root’ already required a small loop, and you had to take care that it did not become too narrow, so that the memory point in the middle was not burnt. This really was programming step by step. After having written the programme down, you had to go to the punched card machine in order to save everything on punched tape or punched cards, with which ou then fed the machine. This meant you also had to deal with all the algorithms, how to solve differential equtions. In Göttingen, Schlüter and I had already started to calculate the path of charged particles in the Earths magnetic field. This had already been done in the thirties by the Norwegian mathematician Stormer who took an interest in aurora borealis and used a hand calculator. With the electronic machines it went much faster. I could use the original machine of von Neumann at the Institute for Advanced Studies where I dealt with problems of solar physics, with flairs, that is, I tried to calculate how waves propagate in a magnetic field. However, as the memory was very small, I always had to fight against boundary effects, which entered from the sides, and from disturbances, and I continually had to dampen them.

Mr. Hasselmann probably knew much better how to solve numerical systems. The Courant-Friedrichs instability had to be considered, so that the time steps did not become too large. I already had to grapple with that back in Princeton.

Did this bring you to the Courant Institute as a mathematics professor in 1959?

Lüst: Courant and Friedrich had written a book on shock waves. I did not realise until later why Courant and Friedrich dealt with shockwaves. It was connected with the development of the atomic bomb during the war. In 1953, I had written a paper on hydromagnetic shock waxes which was published in 'Zeitschrift für Naturforschung'. It was the first paper ever on this problem. Courant and Friedrich had seen it and therefore invited me and asked whether I would like to work at the Courant Institute for a year. First I lived in New York without my family, later they followed me. I could live in an apartment of the Institute for Advanced Studies in Princeton, and I thus spent one day a week in Princeton. The other days I went from Princeton to New York early in the mornings. That was during a phase when I changed from astrophysics to nuclear fusion. In 1956, the institute in Göttingen decided to also engage in nuclear fusion, both theoretically and experimentally. I spent three years working with them.

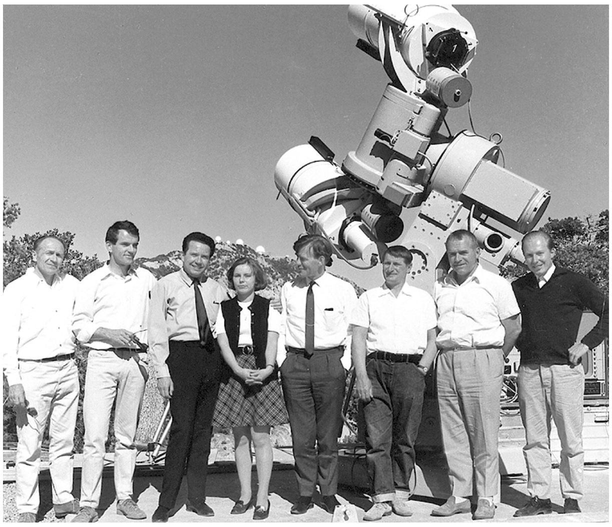
In Göttingen?

Lüst: In Göttingen, but also at the Courant Institute. There, I worked on stability problems. When I returned from New York, I decided that I would prefer to return to astrophysics. I then started again in astrophysics, while Schlüter concentrated completely on fusion.

You talk about your teachers Heisenberg, Weizsäcker and Biermann. Were there similar persons in America who impressed and influenced you as much? Did you get to know Courant back then?

Lüst: I got to know Courant when he visited Göttingen. He was a regular visitor. Indeed, he was one of the first emigrants who was open minded and returned to Germany. Courant had a daughter ho was married to a mathematician, Moser, in America. In this way I was also in contact with his family in New York. But if I have to say who influenced me in America, it was John Simpson, an experimental physicist into whose group I had been admitted. And also, particularly, Martin Schwarzschild, the son of the famous Karl Schwarzschild who had died of gas poisoning quite young during the war, I think in 1916. Martin Schwarzschild had emigrated to America in 1934.

He was a great astrophysicist. I spent half a year working with him. He was an especially open, forthcoming person. The most remarkable aspect was that Schwarzschild as well as his codirector Spitzer, who played a major role in fusion, were jews. Nevertheless, the accepted me, a German.



Schmidt-Spiegel on Kitt–Peak, Arizona

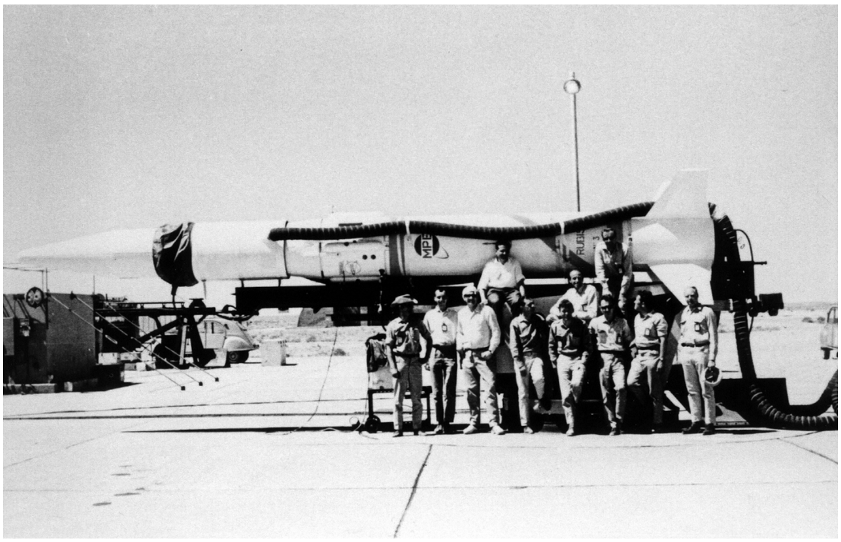
Those were the two persons I learnt a lot of new things from, who influenced me in their way of doing physics. In Chicago, I had adopted the habit there of always leaving the door to my office open, and I introduced that in Garching later: to always keep the doors open.

Your time in Garching was soon to come. You habilitated in theoretical physics in Munich How did it then go on with the MPI for Physics and Astrophysics?

Lüst: The Max Planck Institute for Physics had been moved en bloc from Göttingen to Munich in 1958. Heisenherg wanted to return to Munich. The Free State of Bavaria had placed a new building at the disposal of the Max Planck Society, free of charge. So, I also moved from Göttingen to Munich. The new name after the move was Institute for Physics and Astrophysics. When I returned from New York in 1959, I had fully reverted to astrophysics. In 1958, the first Sputnik had been started. The work on the paths of charged particles in the Earth's magnetic field which I had written with Schlüter, became relevant for the first satellite observations. Von Allen had been the first to measure that there were particles caught in the Earth's magnetic field, the to famous radiation belts. I asked my first diploma student to do detailed calculations on the radiation belts, In this way I became more and more engaged into problems arising from satellite measurements.

Max-Planck-Institut in Garching

In 1961, the question suddenly arose whether Germany should participate in space research. There was a meeting with Siegfried Balke, who at that time was the responsible minister, with Heisenberg, Butenandt, the President of the Max Planck Society, and also Biermann. They decided that the Max Planck Society should participate. Biermann had the idea, which he discussed in detail with me, whether it would be possible to produce an artificial comet's tail. In 1951, he had developed the theory that cometary tails, if they are electrically charged, that is, as. Plasma plumes, cannot not be blown outwards, as observed, by the radiation pressure of the sun. He postulated that the sun must be emit a corpuscular radiation. Corpuscular radiation was already known in connection with magnetic storms. I discussed with Biermann whether this could be the basis for the institute's first experiment. We wrote a paper, together with my first wife Rhea Kulka and Hans-Ulrich Schmidt. Carbon monoxide, CO, as seen in the comet tails, was discarded because the required quantity would have been much too large. So we decided upon barium. I started to form a new group in a prefab hut in Garching. We wanted to use a research rocket to transport barium up into space, where it would be evaporated. That way, I was transformated from a theoretician into an experimental physicist.



Rubi's Rocket in the Sahara in 1966

That was in 1961?

Lüst: It started in1961.

In 1961 you were also a guest professor at MIT?

Lüst: Yes, before we started all this, I had already given a positive answer to the Massachusetts Institute for Technology (MIT). I had an offer for a full professorship, interestingly enough for mathematics. And I had told them I first wanted to spend half a year there in order to see whether working at MIT would suit me. That was why I came as a guest professor. I gave lectures on plasma physics there.

You were there as a guest professor for mathematics and gave lectures on plasma physics?

Lüst: Yes, plasma physics. Hans Wolfgang Liepmann, the aerodynamist whom I befriended more and more, – he also had had to emigrate – then called me and said that MIT was boring and much too large. If I was to go anywhere, it could only be Caltec. During the next half year I should give lectures at Caltec. Then he would take care that I become a full professor at Caltec. During the entire project build-up phase in Garching I returned to Germany many times to keep the project going. But I came very close to staying at Caltec in Pasadena at the end of that half year for ever, because it was the best thing I could ever imagine.

In Munich, you were the director of the new institute at the same time?

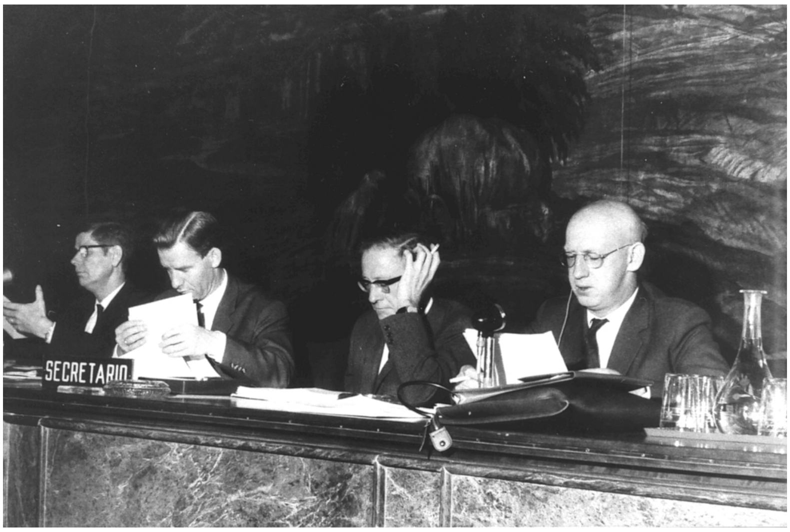
Lüst: Not at that time. Then negotiations were only just beginning. Biermann and Heisenberg said I could not possibly desert them. My place was in Munich. They considered the possibility of establishing a so-called Lex Lüst of the Max Planck Society which would allow me to spend half a year at Caltec and half a year at the Max Planck Institute. However, I found that too difficult. Also because of my family, I decided in the end in favour of the Max Planck Society.

I had seen that the children of many colleagues who had emigrated to America wanted to be Americans when they grew up. Their parents, however, still spoke English with an accent, which meant that already at an early stage an alienation occurred between parents and children, and we did not want to risk that. That was a consideration that was not unimportant. But Caltec has always remained my ideal of how a university should be.

The offer 'full professorship' required no more than three hours of lecturing a week. I was also offered a position as further co-director at the Mount Palomar Observatory. The position was to be a double-appointment between astrophysics and aeronautics.

Another development also intervened, the establishment of the European Space Research Organisation ESRO. Coincidence again played a role. When the Max Planck Society decided to become involved in space there were efforts to establish a European organisation for space research, similar to CERN. The Royal Society had issued invitations for a meeting in London, to which every European country should send a representative.

I was sent there with Mr. Kerscher of the German Research Foundation (DFG) in October 1961. A few months later, the governments made an agreement on the preparation of the foundation of the European Space Research Organisation (ESRO). Following the suggestion of the Dutch astronomer Henk van de Hulst I was commissioned to plan and coordinate a scientific programme as a Coordinating Secretary. Later I was elected as the first scientific director of ESRO.



Foundation ESRO in 1962

At the meeting in London in 1961 I met, Jacques Blamont, a French physicist who experimented with research rockets by evaporating sodium in the atmosphere in order to measure atmospheric winds. When he heard that I was planning barium cloud experiments, he said I should bring my experiment along for one of his rockets, he would manage to include it somehow. This led to my first experiments in high altitude research rockets.

Teil II

These were your last years as a free scientist, if we may say so. Then you got more and more involved in management

Lüst: Yes, but nevertheless these were really still the free years. From 1962/1963 onwards, we normally carried out two or three launch campaigns a year with high altitude rockets. We went to the Sahara, to Kiruna and Fort Churchill. I often spent three or four weeks with these teams. That was important for me to have the opportunity to follow one's own ideas away from everywhere. The Max Planck Institute for Extra-Terrestial Physics was to be established out of my research group; it became an autonomous institute in 1963. Then, in 1965, there was a certain discontinuous change when I was unexpectedly elected into the German Scientific Council (Wissenschaftsrat). Until that date, I hardly had anything to do with scientific policy in Germany, only when establishing ESRO. From 1965 onwards, I also became involved in university issues.

In the Sahara

Then in 1969, you even became the chairman?

Lüst: My predecessor was Leussink. He became a minister and - I was unexpectedly asked whether I would take the chair, which I accepted for three years.

Are there still traces of this three-year period in the German research scene?

Lüst: Oh yes, the University Framework Plan, presented in 1970, was an important recommendation. It suggested a study time of 6 semesters, and it seems that it may be finally realised in the near future.

That was already suggested in those days?

Lüst: The department system for re-structuring the universities was also recommended. On principle, the Framework Plan already contained a lot, but that drifted away because hardly any state went along with it. It was transformed into a much larger monster, the education master plan which, however, was quickly stifled in the Conference of Culture Ministers. It ended up in the hands of bureaucracy, and nothing happened. I had argued for the introduction of a bonus system, i. e., the universities participating in the reform should receive financial incentives. Even the thought of introducing such a bonus system was absolutely inconceivable for the administration. I had suggested that a university in Hamburg, a university in Munich and two technical universities should be given the chance to experiment.

Centaur-rocket

Was it the concept of the federal government that the federal government should provide more support for the states?

Lüst: Yes, the federal government had implemented the Special Collaborative research Units (Sonderforschungsbereiche – SFBs) which received an additional several hundred million Deutschmarks, I think. This was the first time the federal government had ever provided a large amount of funds for the universities through the German Research Council (DFG). Even the DFG had rejected the SFBs at first. I still remember the heated discussions in the DFG.

SFBs at first. I still remember the heated discussions in the DFG. They said no, it would ruin their whole system. They were in favour of single projects. SFBs would not fit into the system.

Nevertheless, you were successful, and Hamburg, in particular, profited a great deal from the SFBs.

Lüst: Leussink as federal minister forced it through with the power of a bull.

This must have been about 1972.

Handing over of office in Bremen in 1972

Lüst: One more thing regarding the end of that period: We needed rockets for our experiments. The first rockets rose to about 200-300 km, or even 400 km altitude. In our experiments we found that even at that altitude we obtained scientifically useful results, because the barium clouds interacted with the Earth's magnetic field. This enabled us to measure the drift of the Earth's magnetic field lines, meaning that we were able to measure the Earth's electric field for the first time. At first, this method was mostly used to assess the electric field in these altitudes, particularly in the area of aurora borealis. But we were able to carry out our first experiment in the magnetosphere -, although not yet in the solar wind – with a large American rocket in 1970 or 1971. Haerendel finally succeeded in producing an artificial comet tail in 1984. My time at the institute, however, ended in 1972.

With your being elected President of the Max Planck Society?

Lüst: Yes.

The term 'one-third' parity had become fashionable at that time You were surely confronted with it during your time as a president?

Lüst: My election came quite unexpected for me. The annual General Assembly of the Max Planck Society took place in Berlin in 1971, and it created quite a stir. The assistants in the Max Planck Society had formed their own organisation, the Assistant Conference, and they called loudly for the one-third parity. The meeting of the Scientific Council was highly emotional.

Strauss on the occasion of the presentation of the Bavarian Order of Merit in 1981

Why one-third parity? There are no students at the Max Planck Institutes.

Lüst: The parity was to be divided between professors, assistants and employees. After the very emotional and chaotic meeting of the Scientific Council, Heisenberg took me aside in Berlin and suggested we take a walk together. He explained that I was both young enough and old enough, and I had to be ready to run for president in November. He had heard that I had an offer from the industry, namely as a board member at Siemens. He said I could not do that to him; I had to stay with the Max Planck Society. In fact. I then rejected the offer from Siemens, without knowing whether I would be elected. My rival candidate, Wolfgang Gentner from Heidelberg, withdrew his candidature at the last moment. So, I was elected by the senate in November.

Two days later, during a newspaper interview with the 'Süddeutsche Zeitung' I suggested that every institute should send a representative with unlimited voting rights into the sections. This caused severe consternation in parts of the Max Planck Society. They said I had given away everything.

The majority of our section, the Physical-Chemical-Technical section, was in favour of this proposal. The Biological-Medical section was radically opposed, virtually unanimously, while opinions in the Humanistic section were split. There was a dramatic meeting of the Scientific Council in April, where my proposal was accepted by a narrow majority. Before the handing over of office, the proposal was put to the vote of the General Assembly of the Max Planck Society, because a two-third majority was needed for a change in the Constitution of the Society. The Biological-Medical section was still predominantly opposed. The former State Prime Minister Stoltenberg got op during the General Assembly and explained that if the universities had understood the need for change in time, surely many things would have been avoided at the universities. He could only advise the Max Planck Society to accept the model. His intervention helped that the change in the constitution was accepted by a narrow two-thirds majority.

How many percent of the votes came from the employees of a section?

Lüst: Of course, I also had long discussions with the employees. During my visits to the institutes I explained to them: 'Forget the word 'co-determination'. It is your cooperation that counts. The critical thing is that you can actually raise your voice and be taken seri-

seriously, not that you count your votes. The only thing that matters in the section is whether you have good arguments. If you are convincing, you can really achieve something.' A particularly important issue was whether the employees should also have a say in appointments. That was a special sticking point for the biologists and physicians. I suggested the compromise that the employees would not be given unlimited voting rights at first - just to get the changes accepted in the first place. After the employees had participated for three years in the section meetings - perhaps it may even have taken six years – most of my colleagues perceived that they did not bring about the ruin of the Max Planck Society.

I think, the impact of these rules is demonstrated by the simple fact that – although I have been experiencing the involvement of the section's employees for more than 20 years – I never knew whether the employees had a voting right or not. Decisions in the section are always made by concensus on the basis of detailed discussions – also including the employees. Voting rights are indeed unimportant.

Lüst: There was another point that I found important. The employees, but also the directors, have no voting right regarding the election of a new director to their own institute. But I also thought it was important that, if possible, there was at least one employee present as observer in every appointment committee, so that they could watch the procedure. This helped a lot in establishing peace. Two years later, the issue no longer played role anyway.

During your total of twelve years in office, another important task for you was the question new institutes, old institutes…

Lüst: That was the situation I found when I became president. In the era Butenandt in the previous twelve years, the Max Planck Society had been able to expand rapidly. Many new institutes had been founded. When I took office, the budget stagnated, worsened further by the two oil shocks, so that there was no real growth increment during my whole term in office. Not even the rate of inflation, which then amounted to about 10%, could be fully compensated. We have completely forgotten that Willi Brandt was practically removed from office due to the strike at the ÖTV (a large public service union) triggered by the high rates of inflation. In my view, the most important task for the Max Planck Society was the appointment of new directors and the founding of new institutes. Butt his implied that we needed to radically close old institutes. I still remember the first institute I had to close down. It was situated in Bad Kreuznach. There were three members of parliament with a wide influence: Mr. Pieroth of the CDU, later Senator in Berlin, Mr. Friedrich of the FDP, minister for economic affairs, as well as Mr. Ahlers of the SPD, a government spokesman. Each of them wrote me a letter telling me how impossible it was to close down the best of all institutes in their election district. So I learnt that perhaps it was better to proceed with tactics. But still, twenty institutes and departments, genuine independent departments, were closed in those twelve years. In this manner we were able to free 680 positions of new appointments. In the same period twenty new institutes were founded, among others in meteorology, polymer research, etc. The founding of the Institute for Polymer Research took place against the vote of the Physical-Technical-Chemical section, because they were afraid of losing funds for their section.

So one function of closing down old institutes is to set funds free for new directions. Did it also fulfill psychological functions?

Lüst: Yes, of course, it demonstrated that the Max Planck Society was the only institution that within its own authority was able to close down institutes. All other institutions, even our state, are no longer able to withdraw from anything, as shown by today's discussions. The Max Planck Society has a structure which makes that possible – even against the will of a section that is, against the faculty. It was even possible to close down the well-known institute in Starnberg. I was attacked a lot because of Starnberg, by 'Spiegel', for example, personally and below the belt. The Senator in Hamburg, Meyer-Abich, even wanted to bring me before the Federal Constitutional Court for disregarding the freedom of science, established in article 5 of the Federal Constitution.

Was he not in Starnberg himself in those days?

Lüst: He was a scholar of Weizsäcker, the director. Of course, the Max-Planck-Society also had a social responsibility. In the majority of cases it took five or six years to close down an institute. Social plans had to be set up. It is considerably easier to found an institute than to close it.

Perhaps you should recount a bit more the story of Starnberg, because in a way it has still left you with a stigma. Therefore, it would be good if you could clarify your position once again.

Lüst: In 1969, the institute in Starnberg was founded for a scientist, namely for von Weizsäcker. Weizsäcker had been with the Institute for Physics for a long time. In 1958, when the institute moved to Munich, he went to Hamburg to accept a chair for philosophy. However, he wanted to be able to investigate the long-term perspectives of our scientific-technical world. That was the reason for the foundation of the institute. He then called Habermas to the institute as a co-director. Their work was definitely of considerable scientific interest, particularly with regard to the prevention of the consequences of the war, but also with regard to economic aspects. At the beginning of the seventies, however, the issue of Weizsäcker's succession was raised, and finally the deliberations concentrated on Dahrendorf as his successor. Two days before the senate's decision, Dahrendorf backed off, because he wanted to stay in England. Since no one saw any other way of continuing work in this difficult field, Weizsäcker's department had to be closed. It was quite difficult for me to discuss the closing of the institute with Weizsäcker. He was of two minds. On the one hand he understood the Max Planck principle. On the other hand he found it hard to accept that just this institute which was so enormously important to him, should no longer exist. He thought, the employees could carry on by themselves. The plan was for Habermas to manage the entire institute during an interim period, but in that case Habermas would have had to sign the notices of termination of those who had to be dismissed, and that was something Habermas did not want to do. He felt that he, Habermas, could not appear in court to advocate their dismissal.

So I had to explain to Habermas that if he was not willing to accept this responsibility as institute director, he would have to resign from his function as a director of the institute. Apart from this, it was already forseen that Habermas would move to Munich with his department. Habermas wanted to be near the university. It was a mystery to me why the LMU was not willing to give Habermas an honorary professorship, but it just shows how deep the resentments actually went. The trenches were drawn. Habermas decided to return to Frankfurt, and that was the end of the institute in Starnberg.

However, we had already elected Mr. Weinert to be a co-director of the institute, which would have brought three directors to the future institute: Dahrendorf, Weinert and Habermas. I had a long conversation with Weinert who had already given up his professorship in Heidelberg. In a quick decision I founded an Institute for Psychological Research for him in Munich. In the autumn I was summoned, as the president to the Humanitie's Section meeting – I still see Mr. Zacher and others who accused me of breaking the rules. How could the president found a new institute without asking the section? I said they would have to take a look at the constitution. The president did have the right to make decisions in absolutely urgent cases, but he had to inform the organs afterwards. I said, here I was, sitting there and informing them. You as lawyers will surely not say that I broke the rules. Mr. Zacher dislikes remembering this situation, too, his saying with a sonorous voice: Mr. President and so on .... That was the origin of the Institute for Psychological Research in Munich. I accepted responsibility for it.

> *It must have been a very difficult situation. Weizsäcker was a strong personality. But you cannot lead an institute of this type without a strong director. There are many examples, e. g. in peace research. Without a strong personality, you achieve nothing.*

Lüst: Speaking of peace research: When I chaired the Scientific Council, a programme for peace research was to be installed under Heinemann. The Scientific Council was asked to give an opinion and summoned a working group of peace researchers. I took the chair. After an hour I had enough and said: "I come from a family of theologians. I know that even theologians are not peaceful at times, but now I must attest that theologians and peace researchers are the least peaceful people I know." The closing down in Starnberg pursued me for some time, also because so many myths were spread and wrong things reported. I did not want to publicly expose Weizsäcker and say: “No. It was not like that”. Only on the occasion of his 90th birthday I mentioned Dahrendorf’s name for the first time. Weizsäcker did not seem to know that he had been considered as his successor.

He must have repressed that there was a real effort to continue with the institute. Dahrendorf had quite different ideas on what he wanted to do.

Dahrendorf then got a leading position at a London university?

Lüst: He was the Head of the London School of Economics, later the Master of an Oxford college.

It was also important that Scientific Advisory Committees were introduced at the beginning of my period of office. For the first time there was a really serious quality control for our institutes, and not all directors were fond of that. They thought that we ourselves knew well enough how good we were.

I remember well the time when our institute had just been founded. Schmidt attended the annual General Assembly of the Max Planck Society. He said that the Society would need to get used to no longer being able to expand. This heralded the budget situation that you described. He spoke without a manuscript for one and a half hours quite impressive.

Lüst: He had just become chancellor when the General Assembly took place in 1975. He invited me a few days before his speech in order to find out what he was supposed to say. The main problem in those days were the fixed-term contracts. He said in the armed forces it was taken for granted that the physical working capacity decreased with age, while it was not accepted that the intellectual performance of scientists could also decrease.

The customary procedure of the assembly was: the mayor spoke first, then the state Prime Minister, and then the chancellor. The president spoke only at the end. Schmidt said: “This is impossible. First I want to hear what you say so that I can reply.“ I answered: “I want to hear what you say, so that I can reply“. He absolutely did not want this. I explained that the order was prescribed by the tradition of the Society. When Mr. Schmidt drove up at the CCH, I received him wearing my chain of office. He said: “Mr. Lüst, you are dressed up like a prize bull. Nobody wears these things any more.„ (That was in 1975.) After the assembly he took me to dinner at the Vier Jahreszeiten in his car. I said: “Mr. Chancellor, look, there are the names of all my predecessors on this chain of office. Shall I really be the first not to wear it any more?„ He saidoh well, as a chancellor would also have liked to wear a chain of office with Adenauer’s name on it. I should carry on wearing my chain of office, this was all part of Hamburg. Another story, perhaps: We had a new chief of protocol from the Foreign Office who always took care of events. Shortly before the Hamburg assembly, he became afraid that there may not be enough people to fill the large hall in the CCH. He reported to me that he had already taken care of that problem by sending about 300 cards to ASTA (the student association). I said: “Then we can forget about the event. When they hear that the chancellor is coming, all is over. We have to find another hall out-of-town.“ I called Fischer-Appelt (the president of the University) who could hardly calm himself down over the naivety of sending 300 cards to ASTA. How could we get them back? He promised to call me back half an hour later. Then he called again. saying: “Mr. Lüst. I went over to the office of ASTA, where the 300 cards were lying about, and I just took them with me.„

In 1984, you no longer wore that chain of office. Did you get a new one?

Lüst: No. There was nothing like that. I had already arranged everything for spending a year at Caltec, because I wanted to go back to research. At that time, Caltec had a special guest professorship, called the Fairchild professorship. The nice thing was that it included a house and a car. All that had already been organised. The house and the car were there, and I intended to go to Pasadena on the 1st of September – my period of office ended in July. However, in the meantime I was asked to become the Director General of ESA. I was already elected, but I still wanted to spend half a year in Pasadena. But Minister Riesenhuber said I had to go to Paris immediately, because Conferences of Ministers were due. I thus had to go to

Paris already in September and did not get the half-year recuperation break I had been looking forward to.

How did you survive the fact that you had to work in France without being able to speak French? After all, you spent six years there.

Lüst: The bonus was that I got to know Paris. You can live in Paris without speaking French very well – my wife learnt French well. My language as a scientist at the ESA was neither French nor English, but BE, Broken English. I managed quite well that way, but I still regret that I did not learn French properly.

Which were the highlights you remember from your time at ESA?

Lüst: In contrast to my period at the Max Planck Society, I arrived at ESA during a phase of expansion. That was lucky. Suddenly budgets increased by 5 % annually, also at ESA. Everyone was in an euphoric mood. Finally everything was to get better. We started a cooperation on space stations with the Americans. That was difficult. I could never get excited about manned space flight myself, but it was a political necessity. The 'Ariane 5' was to be constructed, then the aerospace transporter 'Hermes' for human beings. The scientific programme could finally be extended again to the so-called Horizon-2000 programme. These were all new tasks. It as useful to me – and that as noticed at ESA, too – that during my time in the Scientific Council and in the Max Planck Society I had learnt to deal with politicians. In Germany, those were the Prime Ministers and Ministers of Research. It therefore went without saving that my first act was to make official visits to all ministers, I really tried to establish relations with the politicians. The first Conference of Ministers already took place in January 1985. My predecessor had had a hard furrow to plough. He had worked during a time of standstill. When I came, a strike at ESTEC (ESA's technical centre) was in full swing. From my time at the Max Planck Society I was familiar with such things and knew how to deal with employees. Everything I had learnt at the Max Planck Society was very useful for me now, also the fact that I was a scientist. My predecessors had mostly been administrators. As a scientist I could speak my mind more freely. Once during a sluggish council meeting I burst out in exasperation and said: ‘This is worse than having to dance with an octopus.’ Word got around of that incident and also of the fact that I succeeded in pushing a lot of things through – after detailed counselling and preparation. I cannot remember any council meeting – the lengths of which were often frustrating – which ended without my eventually getting agreement on the important issues. This was the case for example for the large Conference of Ministers in Den Haag in which the English Minister was absolutely opposed to all plans. A discussion forum usually started in alphabetic order, in other words, with Austria or Belgium. As I knew, however, that there was not a thing the English minister would agree to, I asked the chairman not to start from the right this time, but with the Englishman, who was the last in the alphabet as United Kingdom. So, the English minister began, and everyone afterwards was against him. Had I started to the right, he would have had the last word. That is one of the strategic tricks you learn in the international arena. There were also highlights. The ‘Ariane’ functioned well, particularly with the launch of Giotto to Halley’s Comet. Nevertheless, Giotto gave me one of my darkest hours. It was early in the morning, and I was still at home when I got a call from Darmstadt informing me that they had lost contact with Giotto. I thought: ‘How can we face the press now?’ We spent the whole day desperately trying to make contact. As the Americans had much larger antennae in Goldstone, I finally called the Head of NASA, who told me that they were just watching their space probe passing Saturn, neither of their two antennae was available. By the evening, however, I had persuaded him to place one antenna at our disposal for half an hour to begin with. In the meantime it had already been announced that ESA had lost contact with Giotto. It was in the media. At midnight I got a call at home telling me that the contact was re-established. From then on everything went as anticipated.

Another exciting story happened during a brief vacation on Sylt. I received a call informing me that Mitterand had decided to fly to Kourou the day after tomorrow in order to watch the ‘Ariane’ launch. I thus had to hurry to return from SyIt 10 Paris in time. I was then allowed to accompany the president in the Concorde. It is great to see how a French president flies, A Concorde has two sections. In the front, there was only the president with a bed, a desk and all the paraphernalia. In the rear, there were six ministers in normal seats, with me among them. It was hot; we had to stew in the Concorde for half an hour before the president drove up and entered majestically. The Concorde rolled to take-off, then just before take-off, put on the brakes, the take-off was cancelled. We rolled back. Half an hour later we rolled to the take-off again, and the same thing happened, another break. The president got off. We had to remain seated. Half an hour later someone came, took the name tags from our seats and told us to change for another Concorde. We changed. The new Concorde was not furnished in a president-like style, but all backrests in the front cabin were turned down. Only one backrest was up for Mitterand. We sat in the rear. We were already delayed by two hours. The Concorde had to make an intermediate landing in Nigeria, where the president awaited us. We finally reached Kourou with a delay of two hours. I immediately drove on with the minister while Mitterand greeted the dignitaries. He arrived ten minutes before the start and did not want to be seated on the tribune, but in the control centre. To his one side sat minister Curien, to his other side, myself. The countdown began, everything went according to schedule, the 'Ariane' lifted off right on time. The planned and real trajectories on which the 'Ariane' were flying, were projected onto a screen, first stage – perfect agreement, second stage – also, third stage – the first point was slightly lower. I put on my headphones and heard: embarrassing, embarrassed stuttering, no ignition. The TV people realised it and aimed their camera fully on Mitterand.

After the second point it was obvious that the launch had failed. I looked at the minister, a good friend. I bent over and said: 'Mon président, c'est fini!'

Mitterand stood up, it was towards midnight, we were on the third floor, no elevator, the president passed us and ran down the stairs. Curien and I had difficulty following him. Then he climbed into his car and disappeared into the night. Curien and I stood there alone. What should we do? I said: "It's no good, we have to go up to face the press." Mitterand returned ten minutes later. There is usually a big party after a successful launch, and the champagne flows freely. After an unsuccessful launch there is, of course, no champagne. Mitterand delivered a speech, and afterwards he went to the helicopter together with several others. There were two helicopters which were to bring him back to the airport, and one of them was out of order. I finally said with my limited language proficiency: 'Mon président, this is all my fault.' He looked at me, why, and I replied: "Yes. Mr. Président, today is Friday the 13th. I as a marine officer should have known. On Friday the 13th you do not put out to sea." He was not sure whether he should smile. But two years later, on the occasion of a 30th anniversary, he remembered my remark. This had happened on his way to Tahiti, where the French secret service had sunk a Greenpeace vessel. He had wanted to brighten the atmosphere and hoped to have the press on his side again after a successful launch of the 'Ariane'. And then there was the failed launch. It was terrible.

Heisenberg and H. Pfeiffer
Alexander von Humboldt Foundation in 1963

You must admit that the French were better organised. They actually had two Concordes and two helicopters while you only had the one 'Ariane'.

Lüst: It would have been impossible to make another 'Ariane' ready for launch that fast. You are right, the French were well organised. A successful launch – the French 'Ariane'. An unsuccessful launch – the European 'Ariane' failed.

This phase of your life ended in 1990, and you entered slightly calmer channels. You resided at the end of the corridor in the MPI in Hamburger and, what was perhaps more important, you became president of the Alexander-von-Humboldt Society.

Lüst: No, the chance to sit at the end of the corridor was really important for me. I still know how I came to you, Mr. Hasselmann, and asked whether I had a chance to stay at your institute, and you agreed spontaneously.

Yes, of course, I was very glad that you, who had strongly supported our institute over so many years, and had attended all our Kuratorium and Scientific Advisory meetings, wanted to join us. I found that very nice.

Lüst: On the one hand I was glad not to have returned to my old institute. In general, this might sometimes cause problems. In my particular case it would really not have been good, because I had established the institute, and perhaps it would have disturbed me that some things were no longer the same. The old employees would surely still have come to me, which might have annoyed the younger colleagues who had succeeded me. The other reason was that I had never really been able to participate in scientific work in Hamburg, but suddenly I was able to follow up a new field of knowledge. These were the two reasons. I also liked the fact that I did not have to go to Bonn, but could manage the Humboldt Foundation from a distance, from Hamburg. The Humboldt Foundation was a new challenge, simply because I had another chance to see more of the world.

You are still active today, namely in the supervisory boards of private universities

Lüst: Even at a state university, namely in Würzburg. The Bavarian universities gave themselves something similar to supervisory boards, but above all I got involved in Bremen. One day the city's mayor, Scherf, called me and asked whether I would be ready to help found an entirely new university. I told him on the phone that my commitment was subject to four conditions which I would formulate in writing immediately. If they confirmed those also in writting, I would be willing to help. The first condition was for the university to have an American structure with a hoard electing the president. The president would have nobody to answer to but the hoard. Second: tuition fees. Third: entrance examination. Everyone who passes the exam must be able to study, i.e. scholarships. Fourth: Whenever possible, not against the existing university, but in consent. Mr. Scherf agreed to these conditins, and I accepted the chair of the planning group. The result was the now actually functioning university, in my opinion the only campus university in Germany. It is remarkable that that was possible in Bremen.

Why do you say 'remarkable'?

Lüst: The previous history of the state university's beginnings was not exactly prestigious. It was ideologically oriented. But Mr. Timm, who was to become the president later on, has really succeeded in turning it around. The University of Bremen is now a quite respectable, successful university. For that reason I did not want to work against it, but in a mutually supportive mode.

One theme that you have often referred to is the role of science in the scientific society, the issue of a term like 'scientific excellence with social relevance'. How do you see the role of science in our society?

Lüst: I have a problem with the catchword social relevance. It was introduced by the social-liberal coalition, especially by the SPD, in 1970. I greatly annoyed Leussink back then, when I had to give a talk on the occasion of the 10th anniversary of the Institute for Plasmaphysics, and I said: 'This institute emerged from another institute, the Institute for Astrophysics which did not and does not have any social relevance. Nevertheless, I think that astrophysics is of considerable importance for our society'. That is why I have difficulties with the term social relevance. I think, if we take the term seriously, we have to accept that there is a relevance for our society which does not have anything to do with direct use. This is equally valid for the particle physics of the theoretical physicists or for the astronomers who discover a black hole. This is particularly evident in astrophysics. The fact that Hasinger and his colleagues have discovered two black holes is presented as a remarkable scientific discovery on the front page of the 'Herald Tribune'. I think for the science that you, Mr. Hasselmann, and I and many others conduct the scientific quality is much more decisive than the relevance. Of course, the question then arises how scientific quality can be measured. Any scientist can distinguish quality from humbug quite fast. This is a first criterion. The fact that from certain scientific results something important for our society may emerge is another bonus, as is the case here in climate research which is certainly of social relevance. Or take carbon research, where the Fischer-Trobsch process of fuel liquefaction was invented. Ziegler discovered polyethylene, but through pure basic research, without having that particular application in mind. Or genetic research–which is, of course, most controversial now. I accept the fact that every scientist has an obligation towards society, but in my opinion that is on a different level than focussing on social relevance from the start. I had some heated discussions, sometimes even disputes, on that issue with Helmut Schmidt. I actually rate Helmut Schmidt highly that he was open to these discussions. He also introduced the term 'Bringschuld' (obligation for public dissemination of information). Is that your point? Or would you like to argue with me?

v.S.: No, I, for one, do not want to argue with you, or perhaps I do after all, but not now. I would rather ask a question of current relevance: Are the efforts of the Helmholtz Society to implement a programme-oriented management, which relates to that catchword, in fact counterproductive?

Lüst: The Helmholtz Society has a different task than that of the Max Planck Society. You may argue a lot over the Helmholtz Society. For me there are really only two organisations with clear-cut functions. One is the Max Planck Society, commissioned to conduct excellent research and finding the best people, giving them the opportunity to work freely. Fortunately, the state does not influence these decisions. The other is the Fraunhofer Society, with the well-defined objective to conduct industry-related research and raise the necessary funds. I do not want to say that the function of Helmholtz is somehow suspect to my mind. The state has a legitimate right to set priorities. Major research institutions were founded for that reason, such as Karlsruhe or the DLR. But how can we draw clear lines? The state's authority is personified by the administrative directors and undersecretaries who have the money and thus the power. That is a difficult thing, I think. The decision what is accepted as socially relevant thus depends on which government happens to be in power.

To what extent there should be a division of labour between the different institutions? You say that the Max Planck Society is responsible for excellent basic research. Is it reasonable for those who come from another organisation to state: 'Max Planck, you must actually be up to this standard, you have to conduct really excellent research.'

Lüst: Oh yes, of course. If not, the guillotine will fall.

For example, I closed the institute for farm labour and agricultural technology. Even the wine was bad there, it was high time to close down the institute.

Well, that is really a forceful argument. You did not mention the blue list institutes.

Lüst: Those are even more problematic. They gave themselves the nice name Leibniz Society. According to the original definition, the blue list consisted of institutions of major national interest. The motivation for their foundation (they were still called blue list in those days) was to finance economic research institutes, to finance the German Museum, or the Museum König. They were to receive federal grants, but as soon as the dam was broken, every federal state claimed to have at least one institute of equal importance. Now the institutes are distributed over the whole landscape. There are no doubt some very good ones, and they have all been evaluated. Now they argue that all flaws have been eliminated. My major problem with the blue list is in this case it is still more true that me principal administrator in some ministry X holds his protective hand over an institute and claims that it is excellent.

Is it possible to close blue list institutes down?

Lüst: I think it happened in three cases in recent years, although.

I cannot say whether it really happened. Anyhow, the Scientific Council recommended it. I did not pursue the matter. The Scientific Council also recommended to close the economic research institute in Hamburg, but this was not done.

v. S.: Yes, once they even wanted to close GKSS, but it did not happen. I therefore ask myself whether it is actually possible to close institutions of that dimension which are not financed by a central institution like the Max Planck Society, but which represent mixed interests like the Helmholtz Association or the blue list.

Lüst: I really think that the main reason why the Max Planck Society can close institutes is that our senate is truly independent, with representatives of the public authorities, from science, and others. The president is elected by the senate and is answerable only to the senate, not to any ministries, let alone the Ministry of Research. I think it is very important that the president of the Max Planck Society does not act under such constraints.

v.S.: Do you think that the Helmholtz Association with their new president will he able to create something similar.

Lüst: No.

v.S.: Fine. At least that is a clear statement.

H.: But why should this be so? Is the Helmholtz Society not producing a formally similar structure to that of the Max Planck Society, with its president, senate, etc.? The senate is regarded as impartial, not representing the interests of the institutions of its members, and it is independent.

v.S.: Yes, they were obviously oriented by the success of the Max Planck Society. ...in order to protect themselves against the bureaucracy through the senate. Whether they succeed is a different issue.

Lüst: No, I simply got the impression that it will not succeed in this form, because the funding administrators exert much more direct pressure and constraint.

Can we talk about populism? To what extent does research comply with the task of providing society with the necessary information and advice, particularly in the field of environmental research? Does environmental research really supply society with the answers needed to judge things more rationally and alleviate fears?

Lüst: The problem is that on the one hand the public and political expectations are very high, in my view, much too high. This leads individual scientists to believe that they must live up to these expectations. Mr. Latif, a scholar of Hasselmann, always did that very well. He has the gift of being able to explain science. The most difficult thing is to convey the meaning of the term probability.

TV discussion with UlrichWickert and Joschka Fischer in 1988

There exist no absolutely certain scientific statements. The public as well as the politicians demand and expect conclusive answers from the scientists. I think environmental researchers have learnt a lot in the meantime. Of course, it is enormously difficult to cope with media like BILD. Another problem is to find politicians who are really willing and able to listen, and who take the time to listen. Our former environment minister Töpfer, who is now responsible for ecological policy at the UNO, always impressed me in this respect. He always took the time to listen carefully, and was truly able to translate problems into the political arena.

The instinct and intelligence of politicians like Mr. Töpfer must indeed be rated very highly. In spite of the frequently distorted presentation of the climate problem by the media and the skewed disinformations of interest groups, they generally know very well what it is all about, even if they have not read *the IPCC reports (Intergovernmental Panel on Climate Change) of the UN. So cannot one be optimistic that in spite of the general frenzy and misunderstandings, the basic messages are getting through – although it is then still difficult to implement the necessary policies.*

Lüst: Well, in environmental and climate research this is slightly easier than in genetic research, because there ideological issues play a stronger role. The current debate on genetic research clearly reveals the genetic researchers' difficulties.

General assembly MPG in Hamburg in 1975

In this context one could ask further: to what extent are the efforts to inform the public by science and by NGOs complementary, and to what extent and contradictory? To what extent are these two ways of communication and information competing with each other? And, if so, who will win this competition? But we should perhaps come to an end. How is it that you know Helmut Schmidt so well?

Lüst: I ask myself the same question today. I got to know him while he was still minister of defence. It started with Mrs. Schmidt, who was already very much interested in the behaviourist Konrad Lorenz months before I became president of the Max Planck Society. I visited Konrad Lorenz and his institute in Seewiesen with her before my assumption of office. Immediately after I had taken office, I went to see the large 100 in radiotelescope of the Max Planck Institute for Radioastronomy in Effelsberg near Bonn with Helmut Schmidt during a public holiday. Helmut Schmidt was still economics and finance minister. He spent the whole day inspecting the telescope and, above all, informing himself of the results of radioastronomy.

Finally he asked me about the telescope's price, and I told him it had cost the tax payer nothing, because it had been financed by the Volkswagen Foundation with 24,5 million DM. Helmut Schmidt spontaneously exclaimed: 'What, not more than a patrol boat!'

Saudi-Arabian King in 1981

On the occasion of the General Assembly of the Max Planck Society in Hamburg in 1975 – that I referred to earlier – Mrs. Schmidt told me that she herself would like to participate in research, particularly in the work of the institute in Seewiesen and its expeditions. She was enthusiastic when I said that this could surely be arranged. So she went on expeditions to Africa, Indonesia and South America with the institute for several consecutive years. She paid for all of these trips herself.

Later my conversations with Helmut Schmidt developed further.

I should tell a little story. In 1975 or 19761 wrote a letter to Helmut Schmidt explaining the financial situation of the Max Planck Society and asking him for support. A few days later I accepted an invitation of the Empress of Persia in Teheran. She wanted to found something similar to the Max Planck Society. Helmut Schmidt returned from China that evening and had a stopover in Teheran. Dinner was served in the embassy in the evening. When he greeted me he said: 'Mr. Lüst, this is the right place for you. They have a Wailing Wall. You may lament there...'

Trip to China in 1974, 1974 – Chinese wall

That was an auspicious beginning.

Lüst: After dinner we had a vigorous discussion. Loki Schmidt was kind enough to separate us. He was in the habit of always taking six personalities on his journeys abroad. He did not take a whole bus load as was common practice with Kohl and is still customary today. In those days he took along two economists, two unionists, and two scientists. That way I accompanied him three times.

Once he took me to America on the occasion of the 200th anniversary celebration. As soon as were on the plane – together with Mr. Schleyer and Mr. Körber, Mr. Loderer of the metal union and Mr. Vetter of the DGB – he wanted to learn about the experiences of America and the future plans there. Every evening at a late hour we met in the hotel in which Helmut Schmidt spent the night, and everybody had to report on his daily experiences. On the flight back, in the night, I had a long conversation with Helmut Schmidt. He finally said: ‘Well, Mr. Lüst, once I no longer hold this office, I would like to have more relations with the Max Planck Society. On the evening of 1st October 1982, when he had been voted out of office, I called his Bungalow. Actually, I wanted to comfort Mrs. Schmidt a little, but he was on the phone, and I said: ‘Mr. Bundeskanzler...’, but he interrupted: ‘Mr. Lüst, from now on I am Mr. Schmidt.’ We had a long conversation, and he finally said: ‘Mr. Lüst, do you remember our talk on the plane?’ I replied: ‘Is there any chance that you would come into our senate?’


Flight to Washington in 1976

„Yes, if you think so. So I arranged it; I was able to find a free senate seat, and he really became involved. When I came to Hamburg, he asked whether I wanted to join his Friday Society.

The Friday Society?

Lüst: It is a society that meets every second Friday a month. Everybody must give a talk. Mr. Rühe, the former minister of defence, two former mayors. Siegfried Lenz, a sculptor, and an architect are among the participants. The Friday talks have meanwhile been published in a book. These are my points of contact with Helmut Schmidt.

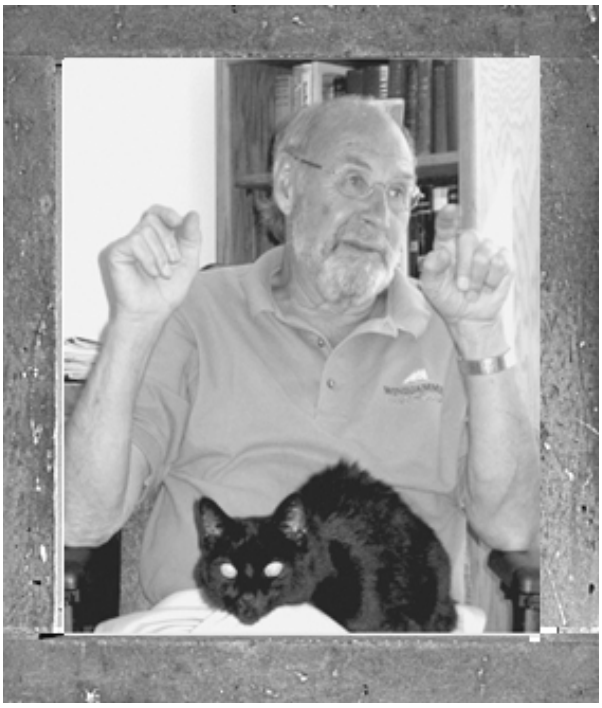
This was a very interesting and gratifying story. After all, we must realize that you are looking back on fifty years, in the course of which science, scientific culture, society, and people have, of course, undergone great changes.

Lüst: Just like the Max Planck Society.

BIOGRAPHIE

Professor Dr. Reimar Lüst

25.03.1923	*born in Wuppertal-Barmen Studies of physics (University of Frankfurt am Main)*
1951	*PhD in theoretical physics under C.F. v. Weizsäcker (University of Göttingen) as a scientific employee of the Max-Planck-Institute for Physics in Göttingen, headed by W. Heisenberg*
1955–1956	*Fulbright fellow at the Enrico-Fermi-Institute of the University of Chicago and at Princeton University*
1959	*Habilitation for the subject physics (University München) scientific member of the Max-Planck-Institute for Physics and Astrophysics, Munich*
1959–1962	*Guest professor for mathematics at the University of New York, for mathematics at the Massachusetts Institute for Technology, Cambridge, and for aeronautics and astrophysics at the California Institute for Technologye, Pasadena*
1960	*member of the Max-Planck-Institute for Physics and Astrophysics (today: Max-Planck-Institute for Physics) in Munich*
1962–1964	*Scientific Director of the European Space Research Organization (ESRO)*
1963–1972	*Director of the Max-Planck-Institute for Extraterrestrial Physics, Garching*
1965–	*Honorary professor at the Technical University Munich*
1969–1972	*President of the Scientific Council*
1972–1984	*President of the Max Planck Society*
1984–1990	*General Director of the European Space Agency(ESA).*
1989–1999	*President of the Alexander von Humboldt Foundation, Bonn*
1992	*Professor at the University of Hamburg*
seit 1999	*Chairman of the Board of Directors of the IUB (International University Bremen)*
Nov. 2001	Honorary Citizen of Bremen



Interview with Harry van Loon

prepared by Hans von Storch, George Kiladis and Roland Madden
on 4 September 2004

Harry, what would you say nowadays about the quality of the weather maps you prepared in the 50's and 60's on the Southern Hemisphere? How much were those dependent on data, and to what extent was that fantasy?

van Loon: Well, we – my good friend Jan Taljaard and I – wrote a paper that came out in the *Bull. Amer. Meteor. Soc.* in 1964. We assessed the reliability of the maps. Actually, we assessed the reliability of the historical maps of the IGY (International Geophysical Year), but it goes for the earlier maps as well. If you look it up you can see how much confidence we had in the various areas, because it varies from area to area.

I remember you saying once that there was a problem with somebody who would always draw anticyclones…

van Loon: Before Taljaard and I got on to the historical map series, there were two colleagues analyzing the Southern Hemisphere maps in Pretoria. We took over in 1954. All I can say is they had a wonderful imagination.

In the South Pacific Ocean south of roughly 30¼ there were no stations. Our colleagues drew daily maps with very few observations. North of 30S there was string of stations toward Tahiti and south of there: nothing. They would show you the trades north of 30S and south of there draw a big anticyclone reaching almost to the Antarctic coast – so the mean maps of the Pacific in those years are useless.

What happened when you and Taljaard finally got access to satellite images? You had one paper.

van Loon: That was with Aylmer Thompson. As an experiment, we wanted to see if the analyses with satellite data would be improved, first analyzing a map without the satellite data and then adding in the TIROS satellite data. It was just for fun.

What do you think what was the major result?

van Loon: We could place systems like fronts and vortices accurately on the map but we couldn't get the intensity.

Did you analyze the central pressure of a low by drawing contours until you ran out of room?

van Loon: We would have a ship, say, at 45 south. It had a strong westerly wind and we would then use the pressure gradient inferred from the geostrophic wind relation-ship. But of course we had many surprises when ships came in later where we had had no data. There could be a pressure difference of 40–60 millibars between our analysis and what the ship showed later.

Taljaard and I wrote a paper about that.

You finished university when you were 21 years old.

van Loon: I didn't finish. In order to be able to attend the university in Denmark in those days you had to have matriculated, to have graduated from a high school that gave access to the university. Then as part of being allowed to study you had to have a small philosophy degree, including logic, and that's all fallen away today. Then I took some courses in chemistry, physics and mathematics to make up for the fact that I had not learned much of that in high school. Then I got to MIT.

I graduated from High School in 1943. Also I didn't start studying meteorology right away, I studied prehistoric European archeology. During the period when the ice withdrew after about 12000 years ago from Northern Europe, there were enormous climate changes, as you know. They were analyzed through various means: pollen analyses, lake varves, etc. I got interested in these climate changes, particular since I took a student job in the Weather Bureau in Denmark, just to keep a roof over my head and clothes on my body. It was a flex job. We worked Saturday, Sunday, at night, and holidays. And I had to go to lectures in the daytime. It was hard to stay awake. I worked every day then as a technician in the Weather Bureau. Learned a bit of meteorology. I had a wonderful chief in the forecasting section, Leo Lysgaard, who was very interested in climate change, he taught me a lot. So I got interested in climate change, at a ripe young age.

van Loon: I started in the Weather Bureau in October 1944 more than a year after I had graduated from high school and the job was for 36 hours a week. Then I had to disappear for a while and came back during the summer of 1945, after the war. I began archeological studies; on the side, as it were, I read about climate change, about the climatic optimum, the Iron Age cool period, the "Viking" climatic optimum, etc.. Because of my interest in that, I started taking courses in mathematics and physics. My boss Leo Lysgaard had written extensively on climate fluctuations. He got me a research assistantship with Hurd

C. Willett at MIT, because at that time you could not study meteorology in Denmark. They did not teach meteorology until a Norwegian, Fjørtoft, came as professor, that must have been in the fifties. There was no professor, no department of meteorology. You had climatology within geography. That was all.

So, did you decide to forget about archeology at that point?

van Loon: Yes, I got more and more involved in climate. Also I took a course at the Technical University, it was called Modern Meteorology, but actually it was quite old fashioned, even for then. It was given by the then director of the Weather Bureau who had fought against introduction of air mass and frontal analyses in the 30s. I had a little background from my daily routine, from working with maps, studying climate, and from just being in the Weather Bureau.

Which were then the favored theories and authors in those days about climate change?

van Loon: Nobody really had a clear idea of the causes of climate change. There was talk of solar influences, of volcanoes, the Milankovitch theory and so on. Still today we don't know everything about climate variability and the causes of changes. Lysgaard wrote a good book on recent climate fluctuations which was meant to be his PhD thesis but it was turned down, unfortunately. He really knew about all theories, but he didn't point to a specific one. This was a period when continental drift was still rejected by some.

Say, before we go too much further. Would you say a few words about the time you had to disappear.

van Loon: No, not really.

It was the underground?

van Loon: It was. But it wasn't dramatic. Neither dramatic nor romantic.

You said you came back in July. That was well after May 1945.

van Loon: That's right. Because I was part of a wartime organization called "Free Denmark". They used us after the war to guard German fugitives that came to Denmark from the Eastern areas, from the Memel area, from East Prussia, and from what is now western Poland .

Close to 400,000 came into Denmark, more than 10 % of the whole population. They were housed in schools and other public places. And we had to guard them. That's why it took me a little while to get back to the Weather Bureau.

Harry's Officers' Cadet School unit in Denmark
(ca. 1946. Harry is second from the left, middle row.).

I was drafted in '46. There was a lack of officers in the Army, because none had been educated since '43 and many Danish officers had joined the SS Divisions and had been killed or kicked out of the

Army. They took any suitable human they could – put them in officers' cadet school, and made them second lieutenants.

How long were you an officer?

van Loon: At first you were an officer's cadet, a sub-species of lieutenant, then finally, you were a lieutenant for the last nine months or so. Then I got out. Thanks to that uniform I met my wife. That uniform was certainly a great draw.

You had this interest in climate, and climate change. Nevertheless you went to the Army.

van Loon: I HAD to go to the Army. You were conscripted. I had to go to the Officers' ment. What they did in those days was to cadet school. Although I did my best to take in mathematicians and physicists, and avoid it.

After this time was over, after two or three years, you went back to climate?

van Loon: I went back to the Weather Bureau to work as an assistant there. Then I got more and more involved in courses, and daily weather.

So, then you went to MIT?

van Loon: In 1951. I talked to my benefactor Leo Lysgaard in DMI about climate change and I told him I would like to learn more about it. I couldn't learn more in Denmark, there was no faculty, no departtrain them as meteorologists, in the Weather Bureau. So, Lysgaard said, I have a good friend in MIT, named Hurd C. Willett who is interested in climate change, and we correspond frequently. I could try to get you a job there so you can study there. He did it for me and another fellow called Hans Buch.

We went over there. First we started working for Willett, both of us, then Prof. Victor Starr took Buch away to work for him.

Tell us about the flight over when you went to Boston. It was very different?

van Loon: My wife, Kirsten, and I took the train down to Schiphol airport in Holland. We almost did not make the train, because the driver chose to go through the vegetable market. In those days when farmers came in the early morning to sell their produce, it was crowded with wagons, and we were stuck in there. We got to the train one minute before it left. So we got on board the plane and flew first to Lerwick in Scotland, then to Keflavik in Iceland. Then we flew to Goose Bay in Labrador, and finally to New York.

We stayed a week in New York, fascinated by the city. Then we took a Greyhound bus out to Boston.

We rented, with Hans Buch, a couple of rooms and a kitchen on Massachusetts Avenue, in a house owned by an old Italian woman.

That would be not far from MIT then? Is it on Boston side?

van Loon: On the Boston side. We walked every day across the bridge to MIT. After a year in this apartment, Buch moved into a dormitory and Kirsten and I rented an apartment on Beacon Hill.

You mentioned Starr and Willett. Were there any other professors?

van Loon: Yes, Austin gave a synoptic course, and he used Petterssen's books. Tom Malone, I think it was Malone, did a climatology course using Bernhard Haurwitz's book "Climatology". Then in synoptic lab, there were two teaching assistants, one was Dick Reed and the other was Fred Sanders. Every day, four days a week, all afternoon, for four hours, we had a synoptic lab, for two semesters. That was actually wonderful. I loved that, because both Dick Reed and Sanders are fabulous synopticians. We had forecasting competitions. Starr was a fantastic teacher of dynamic meteorology, and Willett in what you might call descriptive meteorology.

Had Jule Charney arrived?

van Loon: No, this is before Charney and Norm Phillips. Murray Mitchell, a well-known climatologist, came the year I left, in 1954 to work for Willett.

Was Namias there?

van Loon: No, Namias had left. Then, of course we had a connection with the Air Force. Starr had a large general circulation project with the Air Force Cambridge Research Laboratory.

There was a strong interaction between them and the department at MIT. A lot of people were associated, like Ed Lorenz and Bob White, for example. Starr's students participated, and Phil Thompson came as a PhD student too.

It was wonderful to study there, everybody was enthusiastic. There was a lot of GI Bill people there. You probably don't know any of them. Stu Muench and Pete Leavit were in my class along with Dan Lufkin, and several others. Stu did some very good early work on wave propagation in the stratosphere.

MIT dance, Kirsten and Harry (center) relaxing with friends
Dan and Pat Lufkin during their MIT days, Boston ca. 1952.

What about Larry Gates?

van Loon: In my last semester Larry Gates was doing his PhD on a one-dimensional numerical model. Our class was used to compute for him.

Did you compute on adding machines then?

van Loon: Yes, those things you turn a handle on.

How about Joe Smagorinski?

van Loon: Yes, he was with Starr at that time.

They started running models at that time.

van Loon: This was 1951 to 1954. All the activity was at Princeton then. Phil went right from MIT to Princeton. Ed Lorenz was also a PhD student at MIT at that time.

At that time you hadn't seen any computers?

van Loon: MIT certainly didn't have computers in the Meteorology Department. This was taking place at Princeton, under von Neumann. Experimental model building and so on.

But had you heard about computers in those days? What did you and your colleagues think about this?

van Loon: We had not heard much as students. This was in 1951–54. It had not "seeped out" as it were.

You have been a computer, you have been one of Larry Gates' early computers.

van Loon: I have been a cog in a computer.

It was on a very small scale like Richardson's idea about using a whole theater.

When I look back at the teachers I had in my life, Starr ranks very high among them. He was an absolutely wonderful teacher. He and my Latin teacher in High School probably were the best teachers of my life.

This wonderful time came to an end. You got a Masters degree then?

van Loon: No, I didn't. You know, what a tuition fee was in those days? $600 per semester. I got $167 per month working for Willett. It could just barely take me through life as it were, because the IRS took 35 %, since we were nonresident aliens. Willett had started a Southern Hemisphere Project. He wanted circulation statistics from the Southern Hemisphere and none existed. I worked for him and a guy called Mort Rubin.

Rubin was at MIT?

van Loon: He was going for his Master's degree. He was overseer of this small Southern Hemisphere project. This project had gotten some funding for, a couple of women assitants, Buch, Mort Rubin, and me.

One day we were visited by the then assistant director from the South African Weather Bureau, who told us that he was starting a similar one in South Africa.

What is the name of this guy?

van Loon: M.P.R. van Rooy. Dyed in the wool Afrikaner. A real gentleman. So he said to me, what are you going to do when you finish here? I said I am going back to Denmark. I hope to get a job in the Air Force Weather Service. He said, why don't you come and work on our project? You know, this was before South Africa was recognized as a skunk among nations. Nobody really paid attention in the fifties to what they did there. The Afrikaner nationalists had come to power in '48. He said we will send you a contract. You can sign it in the embassy in Washington on your way back, or you can put it in a pocket and think it over. I stuck it in my pocket. We went home. It took us about a month to get home. We were on an old Italian liner from 1926 called Volcania, – it had three large smokestacks.We lived down in the hold for about a month. Our son Mikael was half a year old. A cabin in the hold cost $460 for all three of us. We stopped in Azores on the way, in Portugal, in Casablanca, in Gibraltar, in Barcelona, in Palermo, in Genoa, in Cannes, then we got off in Napoli. We stayed in Napoli a couple of days, then we went to Rome for ten days. We took the train to Firenze, stayed in Firenze, then took the train to Milano. Finally we got on the train back to Denmark.

The following anecdote has nothing to do with that what we are doing here. It was an 18 hour train ride from Milano to Copenhagen. We got into a little compartment with Mikael, and Kirsten and I were lying down on the seats. I thought we could lie and sleep all the way. Then we heard two voices, one voice saying, "Da liegen nur zwei ganz ausgestreckt." Two Germans came in and chased us up to sit all the way through Germany. They wanted a seat. Couldn't really blame them.

It sounds like your first paper "Aspects on circulation of the Southern Hemisphere" was written at MIT.

van Loon: I wrote two papers at MIT. One was never published. One was based on the daily analyses. We had always been taught that the southern circulation is very zonal, with nothing like the anticyclonic polar outbreaks you get in the Northern Hemisphere. During the analyses I noticed there were lots of polar outbreaks in the Southern Hemisphere. The zonality of the mean comes of the constant movement of pressure systems eastward, so the mean maps look just like bulls eye's. I saw this big high form in the Scotia Sea, move up across Tristan da Cunha, and finally south of Cape Town, bringing very cold air all the way. I told Mort Rubin, "There are polar outbreaks. This one is associated with a high of at least 1025 mbar." He agreed, and I suggested that we write a note on it. And that was the first paper.

What happened to Rubin, do you know?

van Loon: He got into the Weather Bureau, and became an administrator. In the IGY he was sent as the American representative to the Russian station Mirnyi. He was there for a year. When he came back he spent a year at the Scott Polar Institute and he wrote some papers there, then he became an administrator again.

What kind of data were you using for these analyses?

van Loon: We had all land and island stations, and the ships that sent in weather reports, but of course nobody sent in weather reports south of 40S. So there was a fairly big open space from the Falklands eastward. There was no Gough station. We had an occasional ship going across from South America to South Africa and back. We had all the ships in the trades. However, that was out of the westerlies. There were two one-year stations in Antarctica.

Willett got it all sent from WMO and other places. And the same thing happened then in South Africa, we had a very fine port officer in Cape Town. He got us data from all the whaling ships in summer.

Did you do your own quality control of these data?

van Loon: When you have so few data, you are very careful about it. You study every aspect of the observation, pressure, pressure tendency, clouds, cloud heights, cloud types, temperature, dew point, wind, weather, every aspect is closely scrutinized. So you get the most out of every observation. Of course, if you have only one ship in thousands of miles without any other observations, you can't say for sure whether its pressure is correct. There is nothing to compare with. You must take it at face value.

Where have we been? You were on the way on the train with the two German tourists.

van Loon: I came back to the Weather Bureau in Denmark. Analyzing upper-air maps, 500 millibars, 700 millibars. Actually I invented a thermal wind machine to derive quickly a thermal wind. I got a little award for it. I think I still have the machine somewhere. I also learnt Fj¿rtoft's graphical method which was, I thought, very elegant. Where you take the vortices out and you have the basic flow field in which you advect the vortices. I liked that. It is probably almost as good as any 24 hour forecast you can get.

For two reasons we left. Not, because Denmark is not a fine country, which it is. But – while I was away, even when I was in the army, they had pushed others ahead of me. You know how it is in the civil service. If you are there long enough, you are promoted. That annoyed me. Also the general attitude of the Weather Bureau vexed me; I asked the then head of forecasting if I could take part in the forecasting. He said, no, you haven't got an education that would justify it.

I asked Kirsten if she would like to go to South Africa. She was all for it, she loves to travel. So, I went to the South African Embassy in Copenhagen and said I have this contract; I'd like to sign it, if you could arrange our transportation to South Africa. Yes, I signed the contract, got to Southampton, onto the mail boat and sailed to South Africa. I got there on one of the coldest days they had ever had. -7 °C in Pretoria, I think.

You were taking classes and working to-ward your degree. But you did not get a degree.

van Loon: I wasn’t really serious about a degree, I just wanted to learn something.

When I got to South Africa, I thought, I had to have something. So, I got a geography degree in South Africa.

I got into the Southern Hemisphere project. Part of my work was to forecast for shipping. It was for several shipping routes, first for the coast of southern Africa, from Angola all the way round to Mozambique. Then for the southeast trades for ships coming down from or going to the Northern Hemisphere, and also for the shipping routes to South America to somewhere west of Tristan da Cunha And we forecast for the shipping routes in the Indian Ocean, one going to Indonesia, one going to Australia; and then in summer, on top of it, the whaling ship forecast, all the way from the Scotia Sea to the center of the Indian Ocean, south of about 45S.

These were 24 hour forecasts. We issued them twice a day, in the morning and in the late afternoon, and they were broadcast from Cape Town. It was hard work, because you started early in the morning, went home in the heat for a siesta, and ended about 6:00 or 7:00 in the evening and it was seven days a week, because there were only three of us who did it.

You have published several papers in Notos.

van Loon: *Notos* was discontinued, after about 18 volumes.

You wrote a paper in 1956, which got a lot of notice, with many citations – Blocking Action in the Southern Hemisphere

van Loon: It was part of my realization that there was blocking in the Southern Hemisphere. Nobody thought so, but there was blocking. So I defined the areas and the duration. I used Dan Rex’s *Tellus* papers as an example.

They were also a few years earlier so you weren’t really isolated….

van Loon: In those days there were few journals and few papers, and you could manage to read everything. Nowadays I’ve given up.

Which journals were these?

van Loon: *Tellus* was one. The American Meteorological Society Journal of Meteorology, what is now divided into many journals. It came out only four times a year.

That's where we published that first Southern Hemisphere paper. The Quarterly Journal of the Royal Meteorological Society, and the AMS Bulletin of course. It was different in those days from what is now, less social. Monthly Weather Review and Met Magazine were also around.

Meteorologische Zeitschrift had disappeared in a way, it came back later. But Archiv, we read. It was an Austrian journal, published in Vienna.

Which year are we now?

van Loon: We are in 1956.

The paper on the blocking action in the Southern Hemisphere gave at least 44 citations.

van Loon: That was the only one written until Kevin Trenberth wrote on the same theme and until Harald Lejenäs wrote a good paper too.

You did these papers at the same time you were serving as a forecaster?

van Loon: Yes, and also at the same time as I analyzed historical Southern Hemisphere maps. We had to do everything, we had to go out to the telex room, tear off paper, take it in, plot the data, analyze it and issue the forecast. It was tough.

This was still pre-computer time.

van Loon: Certainly in South Africa. It was going full strength in Princeton and other places.

I am interested in this paper: 700 mb mean maps from the Southern Hemisphere -- and it was in Miscelania Geophysica …

van Loon: It was the tenth anniversary of the Angola Weather Service and we were invited to write a paper for it. I wrote that paper, because we were then preparing for the IGY. South Africa's duty was to analyze the Southern Hemisphere historical maps of the IGY, from 20 south to the South Pole. The Germans did it from 20 south to 20 north, but we overlapped by 5 degrees so we analyzed to 15 S. There were very few upper-air observations in those days. So I devised a method to construct 500 mb maps and the beginning of that method is in that paper: how from surface observations together with a few upper observations you could build thickness maps. These you could then add to the 1000 mb height which you got from the sea level pressure. Later I wrote a paper on the whole method. The method, which was taken over by the Australians, consisted of anomalies before and after cold front, strong cold front, behind a low, in front of a low, in the middle of a low, in a high and so on.

About this time you actually went to Antarctica.

Harry briefing colleagues at little America, Antartica (1958).

van Loon: What then happened was that I had met Harry Wexler at MIT. He came to MIT to see our Southern Hemisphere project. He was very dynamic, a very friendly, very nice guy. He said, in the IGY we have to have an International Weather Center in the Antarctic. That was established in 1956, before the IGY, and called Little

America III, near the old Little America Byrd stations from the late thirties and just after the Second World War. It was a truly international center, there were Australians, Russians, Argentinians, French, and American meteorologists. They invited the South African Weather Bureau to send someone too, but nobody wanted to go. They were used to the sunshine and warmth. In any case, I was keen to go. Since I could both forecast and analyze historical maps, there was no problem.

I went. I came down there at the end of October and left at the end of March. That's when the summer activity takes place. We had to forecast for the flights down from New Zealand, and for the ships, for the snow trains, and all that.

I had a great time there. We worked around the clock, as you can imagine. Ate four square meals a day, saw movies, and skied for hours and hours.

You stayed in South Africa until 1963.

van Loon: There were several German scientists there, German meteorologists. The Afrikaners kept us all down. The system of Afrikaner nationalism totally dominated the country. The civil service was at least 95 % Afrikaans. And they were all promoted all the time. The Peter Principle was truly at work. There were two Englishmen, me, and about half a dozen Germans in the Weather Bureau. We were all kept at a low level. Most left after a while. I think, only one drunken German stayed, one died while there.

My contract expired in 1960 and I was going to leave. At that time we were really getting into analyzing the IGY. We got all the data after '58. IGY ran to the end of '58, and started in July '57. The data were streaming in and we were right in the middle of that. They asked me to stay. I said, OK, I'll take a contract for three more years.

The IGY was planned for a year, but lasted 18 months.

van Loon: Yes, and then there was an extension into '59, but not full-scale. The IGY was planned because it was at a solar maximum. It was actually the first very large solar maximum since at least 1750.

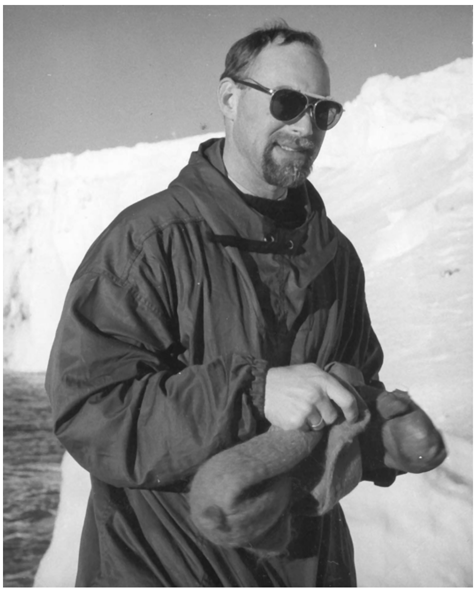
This whole business was without computers for you and there were no satellites.

van Loon: Absolutely. It was all hand-work. It took us a day to analyze a surface map, a day to make a thickness map and a day to add the two to get a final map. So, it took each of us three days to make a day. It was hard work. Two women plotted the data for us. We had to be careful, since they would plot anything as it was in the message.

This culminated in the 1964 paper, published in the Bulletin of the American Meteorological Society?

van Loon: Taljaard and I wrote this paper on the reliability of the IGY analyses in the Southern Hemisphere.

After this publiccation the type of publication changed and that is because you moved NCAR.



Harry in Antarctica 1958

van Loon: Yes, but first I applied to Australia. I knew Bill Gibbs who was director of the Australian Bureau of Meteorology. He had gotten his master's, while I was at MIT. He was very interested in our analyses there. We often talked about it. I met him again on my way to and from Little America in 1957/58. So I wrote to Bill, and he said, “Yes, send an application, we'd love to have you”. I did so, I waited a little and got no response. I knew Phil Thompson was at a place in Boulder, Colorado. Aksel Wiin-Nielsen, whom I knew vaguely from Denmark, was also there. Thus, I wrote to Phil and got an immediate answer. Yes, come. I waited a little while, months actually, for an answer from Australia. My case was in the Public Service Commission, that’s where all such things went, and finally I gave up. I accepted Phil’s invitation. We went to Boulder in 1963.

Nowadays, when somebody gets a job, it seems you need to have a contract, they know this is going to be paid for and that is going to paid for ….

van Loon: Phil hated administration. Just come over here and bring your family and your furniture if you have any. That’s it. I went into his little office in Cockerell Hall, where he and Axel were sitting in a dense fog of cigarette smoke, on the first of July 1963. I asked Phil, "What do you want me to do?" He said, "Is there anything you'd like to do?" I said yes. "Just go ahead and do it", he said. I was put in the synoptic group, which did not yet exist because the chief, Chester Newton, didn't come until a month later.

Was Paul Julian there when you came?

van Loon: Julian was in HAO and came to NCAR to the synoptic group, along with me, Henry van de Boogaard, Jim Fankhauser, and Chester. It was only a question of a month. Warren Washington and Akira Kasahara came at about the same time.

Did you know that Chester would be the boss of that group?

van Loon: I had no idea who Chester Newton was. When he came and introduced himself to me he said, "Oh, you are the guy that writes two-page papers."

I think Roy Jenne came in 1966/67. He knew to handle computers, he had been in the Air Force. He had done a lot of data handling.

What did it mean for you? Did you consider this as a new opportunity?

van Loon: Absolutely. We could do a hundred times more. When you calculated derived data from maps by the old method, it took several months. Now it could be done within a few seconds when the program was written and when the grid points were read. It was a totally new world.

Did you do any programming yourself?

van Loon: Well, I tried, but I never really did. Not like Madden, for example. The programs were ready for everything I wanted to do: geostrophic wind analysis, standard deviations, harmonic analysis, and so on.

Somebody had to write the programs. Was each scientist assigned a programmer in those days?

van Loon: Not really. I cannot say for sure, because I worked closely with Roy Jenne for many years. He and Will Spangler, and sometimes Dennis Joseph, would do the programming. I don't know what other scientists had in terms of programmers. I closely associated with these guys through research, computation, and analysis.

We see now from your list of publications that something else happened, namely satellites. In 1966.

van Loon: That was a brief, peripheral thing.

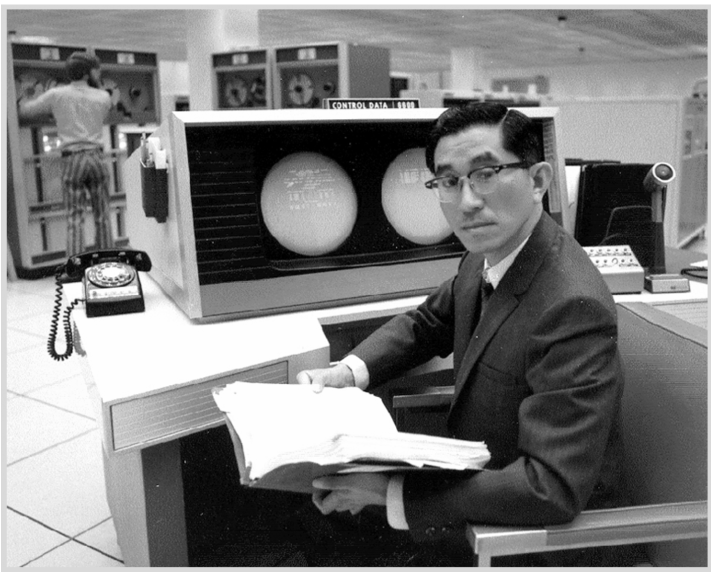
You saw for the first time images of cloud cover, it must have been very impressive, wasn't it?

van Loon: Yes. But I remember a paper by Harry Wexler, in the Bulletin when he had taken a weather map and he had drawn a cloud cover as he thought it should look for that weather map and then he compared with the satellite's clouds for the same day, and it looked almost the same. The cloud distribution around a low, along a front in a high and in the trades and so on.

Weren't weather maps produced with the help of satellite images?

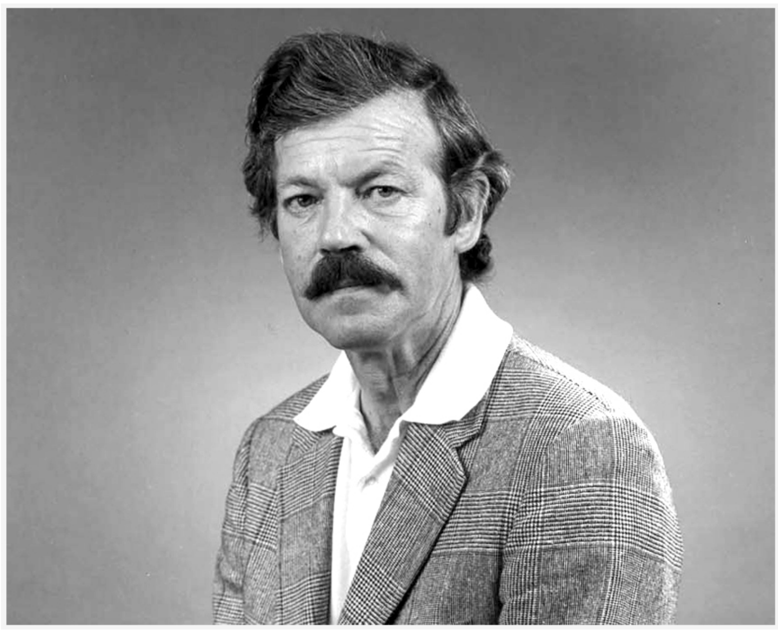
van Loon: I am not knocking the satellites. On the contrary, I was admiring Harry Wexler's skill. Satellite images are good to have, absolutely. In the Southern Hemisphere nowadays they are indispensable. For hurricane forecasting they are indispensable.

Will Kellogg, director of NCAR's laboratory for atmospheric research
about the time Harry joined the synoptic meteorology group.
The synoptic meteorology group was a part of LAS. (ca. 1963).

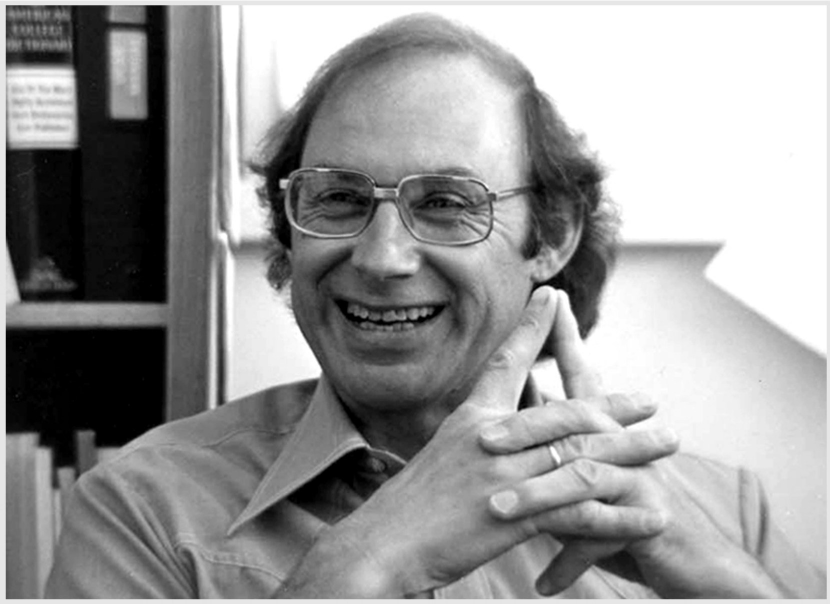


Akira Kasahara (ca. 1967) at the console of the NCAR cdc 6600 computer.
The 6600 had a clock speed of 10 MHz and a memory of 64 K Bytes
and was the fastest and biggest computer devoted to meteorological
studies at the time.

Chester Newton (ca. 1967), head of the synoptic meteorology section at NCAR. Harry was an original and long-time member of the group.



Phil Thompson, associate director of NCAR about the time Harry arrived in boulder (ca.1963).



Harry in his NCAR office.

Warren Washington checking output of the NCAR general circulation model (ca. 1967).

You never really much worked with satellites.

van Loon: No, I wrote a paper, actually two. I did it with Aylmer Thompson from Texas A&M. We talked about him earlier. Just to see how much I could improve the analysis of given days. The position of things were nailed down by the satellites.

Thereafter you had left it to the synopticians and the operational meteorologists

van Loon: I never used satellites for analysis again. Nowadays, a lot of data you get are from satellites, vertical soundings, winds, etc.. That's different.

Then in the late sixties or so you did a comprehensive summary of climate in the Southern Hemisphere.

van Loon: Taljaard came, and we began a climatological analysis. We started with the IGY maps. Then we collected data, made the atlases and derived data. All of it was aimed at making a monograph, Roy came in the middle of it. We had the grid points read by Crutcher's people in Asheville. We asked Roy to check for time and hydrostatic consistency in the data. To quote George Platzman's words, "If it couldn't be accurate it could at least be precise." Roy did this marvelously. He did a very good job.

We analyzed the height and pressure maps, the temperature maps, and the dewpoint temperature maps, and then all the derived maps were computed by Roy. They were sent to Ashville and gridded in Ashville. The numbers were punched on cards.

The maps were all hand drawn. I still have the sheets. We plotted everything. We drew every line on those maps. This was before machine plotting.

The computer was just used to provide consistency and do the derivations.

van Loon: In the atlas all grid points are printed on the right hand side of a map. And zonal averages. These were very useful maps.

The maps were printed so beautifully, in color. They were drafted. We did the analyses, then they were drafted in black ink in Asheville, then they were printed.

This explains that then Hal Crutcher appears in your publication list. This was the cooperation with Asheville.

van Loon: Yes. We made enough volumes so each of us could have our name first on one of them.

In these days, there was also that semiannual wave paper.

van Loon: Werner Schwerdtfeger, a German meteorologist, left for Argentina after the war. After Argentina he came to Wisconsin together with other Germans like Lettau and Wahl. He was a very fine synoptician, he had been forecasting in Germany during the war. I think he went out in submarines too and took observations in the Atlantic. In any case, he had got to Argentina, and being interested in climate as well, he made some very good climate analyses. But he didn't discover the semiannual oscillation. It was in the first analyses by Meinardus analyzing the maps of the polar year 1902–04. Meinardus and Mecking published atlases and papers. From the derived data like movements of cyclones Meinardus saw the semiannual component in the winds and cyclone movement. That was the first step. Then Reuter in Germany wrote a thesis in the thirties, where he analyzed all pressure data available in the Southern Hemisphere and he found a large semiannual component in the few stations he had. Then Wahl made an even a better analysis 1943 in his Diplomarbeit or thesis.

Schwerdtfeger took off from there. He had already shown the rough outline of the semiannual oscillation. He made several very good papers on the semiannual oscillation. He did some very detailed climatic analyses of ship data in the Drake Passage where it is very strong. He wrote a whole book and a smaller book on the Antarctic Peninsula where it is all very well described, plus some papers in German and some in English. I got to it, when we did the IGY analyses, I made some time sections and I noticed the thing. I hadn't read any of the old papers. Later I found Schwerdtfeger's work, and there I found Meinardus and all the other guys. They are all referenced in the 1967 paper.

The 1967 paper won the NCAR publication prize. It was the first paper to receive such an award.

van Loon: That's right. $600. I got a new engine for my jeep.

And it had important implications for the development of meteorology in the United States because of certain people who did not get it and moved…

van Loon: Nomina odiosa sunt, as we said in Rome. Names are odious.

Did you know that your paper might win that award and so you just….

van Loon: I didn't have the slightest idea. Chester had nominated it. I know now why he did it. I was making a heat balance study. I wanted to explain the semiannual oscillation physically, and then how it worked dawned upon me. I was enthusiastic, and Chester and I were having coffee, and there was a blackboard, and I explained it to him. I could see, if you know Donald Duck's cousin, the bulb light up on top of Chester. From that point on he thought he would nominate it, apparently. But it had a very funny fate. Is the name Clarence Palmer known to you?

That tropical meteorologist was a very fine meteorologist, but as alcoholic as you could be. He was out at UCLA. I submitted the paper in late 1966. I heard nothing from them and I was a greenhorn in those days, I should written in and said what the hell is going on. After eight months it was too much for me. I wrote to AMS "Why haven't I gotten the reviews of my paper?" They called Clarence Palmer. He must have been sober at the time. He wrote a wonderful letter saying that this is the best paper he had ever read. Publish it immediately without changes. So it came out without any changes.

That was Clarence Palmer. He died not long after this. He was the one who wrote the Southern Hemisphere chapter in the Handbook of Meteorology in 1944, a very good chapter, considering how little was known at that time. He also wrote a very nice paper on solar influence on low latitude pressures. He had to test the results against a random series. For the random series he took the thick telephone directory in Los Angeles and took the last number of all the telephone numbers.

Your publication list in the sixties reveals something else, namely the first paper with Karin Labitzke in 1965.

van Loon: Karin Labitzke had been brought over here by Walter Roberts, who was very keen on solar influence on weather and climate. He went to Richard Scherhag who was professor in Berlin at Freie Universität, asking if he had any good doctoral students interested in the stratosphere who would be willing to come to NCAR for 18 or 12 months. He needed help in analyzing solar influences. Karin had just gotten her PhD and she accepted. When she came she must have been about 28 years old.

Nobody could speak German. She spoke very little, broken English. I was the only one who could speak German. Her husband was with her, too. We invited them home, played poker on Saturday nights, etc. I got interested in her stuff on midwinter warmings. She had done some work on that while in Scherhag's outfit. I said to her there is not much we can do about the Southern Hemisphere and its stratosphere, but let us look at what little there is. So we did and wrote that paper.

This is, important and interesting, because you really continued to work with her for many, many years. You produced more than 30 many papers with her. You worked with her for almost 40 years.

van Loon: Yes, we still work together a little. We are working on a paper right now. That will be the last one then. Well, I will be 80 next year (2005) for heaven's sake.

Let us get to NCAR. These were the golden times. At NCAR you were really free to do whatever you wanted to do.

van Loon: Yes, but in the 70's it started changing. It began in 1972 with that ….

Rol Madden: ...Joint Evaluation Committee Report. NCAR got re-organized because of the Joint Evaluation Report and Harry and I were put into a climate section. After the first meeting of the climate section everything seemed OK to me but Harry said "We have to get out of this group".

Then we moved to a different group of which Akira Kasahara was in charge.

It turned out Harry's insight was true because we flourished in this new group much better than we would have in the other.

van Loon: Let us go back to an old French proverb which I cannot say in French: The more it changes, the more it remains the same. For Madden and me it has been like that. Not for us alone, but for other people, too. There was an urge to change things every now and then. The people reorganized, reorganized and continued as before.

Does it mean that in 1972 the Joint Evaluation business had no implication, no impact?

van Loon: Oh yes, it did. First of all we got a terrible hierarchical system with scientists 1,2,3, senior; and up or out promotion system. And secondly, two categories of scientists, where we usually had only one before. Associate scientists and ordinary scientists. The whole thing became structured.

You were not affected by this change, because you were a senior scientist.

van Loon: I became a senior scientist in 1976. Madden became one too. You cannot fire a senior scientist, if he doesn't fit in, as long as he publishes and doesn't commit illegal acts. Unless you abolish the whole group he is in.

We are now at the end of 1972. The next paper we should address is the Jenne, Labitzke piece, on zonal harmonic standing waves, which got 136 quotes. How did you think of displaying the waves in this way?

van Loon: People still hadn't realized that the Southern Hemisphere was not just a zonal circulation. Through synoptic work one notices features such as blocking, the meridional movement of lows and highs and so on. There must be something that steers these things,if that's the right way of putting it. I decided to look, I already new the difference between the South Atlantic and the Pacific Ocean in terms of mean seasurface temperature and pressure. So I thought let me see if there are any quasistationary waves. Wave one was obvious, but then wave number 3 popped up which I hadn't expected to that extent.

Well, I think the 1973 paper on zonal harmonic standing waves in the Northern Hemisphere which appeared in the Journal of Geophysical Research had a big impact both on observationalists and theoreticians.

van Loon: Do you really think so? I did some work with Jill Williams in 1976/1977 which showed the role of advection in climate variability and which actually was straightforward. Francis Bretherton once came in, looked over my shoulder and I showed him how the changes in wave number 3 affected the temperature trends in the Northern Hemisphere. He said, "I never thought of that but it is very simple minded." I took this as a compliment.

Harry, we forgot to speak about the book in 1972.

van Loon: I rate that as one of my few real accomplishments even if it is not the first book on the Southern Hemisphere. In 1938 Meinardus wrote one on the Antarctic, which took in a lot of the Southern Hemisphere too. It was very good for that time. You have always to judge things in their own period.

How does an AMS Meteorological Monograph come about? Do the authors approach AMS ...

van Loon: Jan and I had done all the work with the data, and also written it up in papers. It was nice to get as complete a picture as one could in those days. So, we decided to ask the AMS to print the Monograph. I looked around for authors for those chapters that I did not write myself. Jan was an obvious one. It's a very fine synoptic chapter that he wrote. Never been superceded. Obviously Takashi Sasamori would be good, he was a very knowledgeable person on radiation. He wrote a very good chapter with Julius London and Doug Hoyt. Chester was the obvious candidate for the general circulation. I had first asked Paul Julian to write about the stratosphere. He said one couldn't, there was not enough data. So I said to Karin Labitzke, let’s take a look at it and see if we can write a chapter together. It is a very modest chapter. We didn’t have many data but there is still a lot of information in it.

So I collected these guys and I swung the whip over them so that it would not take too many years. Then we got it out. I am happy about it. It was a good monograph for the time.

There is a lot more in the new one, which came out 25 years later, obviously since there are a lot more new data since then.

You are not a member of the AMS. Could you just say that why.

van Loon: After we had done all these analyses, published all the atlases, papers and the book, Chester nominated me for fellow of the AMS. He said, now it is time, its going to be easy. Three years in a row I was voted down. So I wrote to Spengler and said I want to leave the AMS.

In your publications list a few more new names come up. One is a young fellow with the name Madden, then Jill Williams and Jeff Rogers. Do you mind saying something about these papers?

van Loon: Jill – I wasn't really her advisor, she got a PhD with Roger Barry. But I was sort of semi-advisor together with Warren Washington. She is very clever, good at programming too. I asked her to help me with some analyses, so after that we wrote a series of papers together.

Jeff Rogers was my first graduate student in the Geography Department at the University of Colorado. Roger Barry was Professor in geography. A climatologist, he has written several synoptic climatological books. He asked me to join the faculty as adjoint professor, and take some of their graduate students. I had a good topic, the North Atlantic Oscillation, so he referred two students to me: Jeffrey Rogers for his PhD, and Jerry Meehl for his Master's degree. We started on the NAO, although lots had already been written on that long before me.

These papers with Jill looked mostly at the Atlantic and temperature over Europe. So that was sort of the beginning on NAO work.

van Loon: That's how I got into it. It was on the Northern Hemisphere as a whole, because we looked also at the importance of changes in wavenumber three. Out of that came the association between long waves and climate variability.

Would you say that the North Atlantic Oscillation was the first oscillation you had really worked intensely with?

van Loon: I knew about the Southern Oscillation. We talked about it in South Africa and Willett was very keen on it. Mort Rubin wrote a paper on it when we were together at MIT. But I had been turned off the SO by Robert Montgomery, who in 1938 went through Walker's correlations and showed that many had fallen by the roadside and even changed sign. So I was not too enamored by the Southern Oscillation at that time.

You thought maybe it was a statistical artifact?

van Loon: No, I didn't. We knew the effect of the 1957 classical warm event. We had a gigantic and classical Warm Event. I read a paper written by an American and a Japanese. Do you know that paper of 1958 about the warming and the abnormal equatorial rainfall and SST? A very nice paper. So it was in the back on my mind. Also, I'd gotten a letter from Jacob Bjerknes. He had gotten interested in the Southern Oscillation. He asked if I could get him some pressures and winds from the South Pacific Ocean. I gave what I had from the IGY. All geostrophic winds of course. It was all lying there, waiting.

This work with Jeff Rogers about the North Atlantic Oscillation is the most quoted paper of yours, more than 350. How did it come about that you really concentrated on that as opposed to the Southern Oscillation which was much closer to you in a sense.

van Loon: Don't forget I was born at one end of the NAO and lived for 26 years at its receiving end. Some of the correlations with Copenhagen are the highest. In any case, also historically it interested me. I dug out all the old stuff.

I knew about the Southern Oscillation and its possible use in interseasonal forecasting, and I thought the North Atlantic Oscillation might have some potential as well. But as far as I can see, it has no persistence in the same sense as the Southern Oscillation. You can't really use it in long range forecasting. There are also no clear precursors. Therefore, I gave up on that. Also, my interest in the NAO goes way back to my time with Lysgaard, who had done lot of work on the condition in the North Atlantic although he had never published much on it. He examined the North Atlantic Ocean, to see whether there would be characteristic sequences of events in fall, winter and spring. He found nothing of value.

Would you say that you in a sense had rediscovered the NAO? At that time only very few people spoke about the NAO.

van Loon: No, Loewe had dealt with it in the 1940's–50's. Much later, I encouraged Jim Hurrell to work on it. He sent a good paper to “Science”. Science had sent reviews among others to me but to also some Dutch meteorologists. They wrote back that we know all about the NAO. There is nothing new you can tell us about that.

This is in the nineties.

van Loon: Yes, but this just shows you, even then, people thought they knew all about it. the Dutch said yes when the low is deep the we get strong westerly winds, cloudiness, and rain. We know all about it, but they didn’t know all about it.

Where does the name NAO come from?

van Loon: From Sir Gilbert Walker. It’s a pair – the North Atlantic and Southern Oscillations. The Pacific Oscillation is basically part of the Southern Oscillation.

What do you say now that your adopted daughter has received a new name, namely that it was rediscovered as Arctic Oscillation.

van Loon: There was a paper by Clara Deser who found a 0.96 correlation, which is very high in meteorology between the Arctic Oscillation and the NAO, and you can take it from there.

How would you then understand that both – the Antarctic and the Arctic oscillation have received very much attention?

van Loon: If I really get into this I will have to insult people that I like. But let me just say – just as Clinton’s people said, "it is the economy, stupid" – it is the waves, stupid. Both, in the Northern and in the Southern Hemisphere. The North Atlantic Oscillation is part of a long wave pattern and you cannot disregard that.

So few people take the trouble to go back and see what has been done before on a topic that they deal with. They would have found, for example, the older Defant in 1925, wrote an excellent paper on the NAO, but even now, they don't refer to it.

Members of NCAR's synoptic meteorology group,
Paul Julian, Roland Madden, Dennis Shea, Chester Newton (left to right)
listening attentively to Harry (ca. 1980).

There is a story with a missing minus sign?

van Loon: John Walsh and I were commissioned to write a report on "Climate change in the Arctic". I had seen the these sort of abrupt changes – step functions in 1920 and in 1976. I had got the idea that you get this very sudden change of climate within two/three years and you are in another mode. So I was working with Greenland stations. There was one in the far northeast, but there were no other stations near it. I noticed that that station suddenly had a jump in its mean temperature. I wrote it down and sent it to John. He wrote back and said, Harry, look at that decade in the World Weather Records, they forgot to put the minus sign in front of the temperatures. That's terrible. Such is life.

This paper about the SO in 1981 is also one of the papers which had been quoted very often, namely 180 times. The Southern Oscillation. Part I.

van Loon: Madden's contribution to it is very important, because he showed through cospectrum analysis from one period to another that the correlations change, the frequencies change in importance. He was the one that suggested dividing it into four periods and see where you always have the same correlation, and where it fluctuated from period to period. Most of what is good in that paper is Madden's contribution.

Who was responsible for the first line of that paper: "the Southern Oscillation needs little introduction"?

van Loon: I was. To me it needed little introduction, but to a lot of people apparently it did.

The real revival came with Rasmussen and Carpenter's paper in 1982. This is one of the best ever written on the SO. That one really got people thinking. It was well written and methodologically sound. I would say that really rekindled the interest.

It is remarkable that you actually have this series on the Southern Oscillation. Part I to part IX, which began from 1981.

Are you aware of anybody else who made a series of ten papers? Over 25 years.

van Loon: Sure. They may not have numbered it. I numbered them.

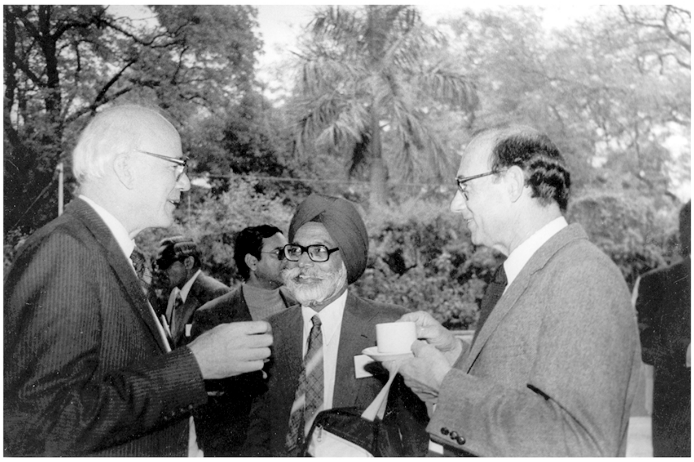
Part II also was received very well with more than 70 references. That was with Jeff Rogers.

van Loon: He did a good job. And he continued with the topic from then on. Much of what he has done comes out the work that we did together. But, of course, he has done it independently. Some very good papers.

Is it about that time or was it even earlier that you started to interact with Christos Zerefos?

van Loon: Zerefos had been at NCAR as a visitor. We had been talking together about various things. In Greece they were interested in the stratosphere. So he just said why don't you come to Greece for a while, which of course I jumped at. We had a nice half year in Athens in 1981, and came back to Thessaloniki in 1993. This latter stay was not quite that successful because he didn't have time when I was in. I had only one graduate student. We wrote one small paper published in Argentina and that is totally forgotten.

Christos Repapis was head of the group in Athens. Actually, there was an old Greek in his 90's they treated as head of the department. That is out of the Greek attitude toward old men. Repapis was the real head, and he had several graduate students. We had a lot of interaction.



Southern Oscillation conference in Dehli,
Roy Jenne, Harry (ca. 1985)

During these years you also regularly went to Berlin.

van Loon: Every year, sometimes several times a year. Roy Jenne and I got Karin to collect the stratospheric data, and with her we published a stratospheric climatology in the Meteorologische Abhandlungen. That was later improved by Steve Pawson and his collaborators. A few years ago they collected all the new data. It is an excellent work.

You wrote a book chapter with Taljaard in 1984.

van Loon: Volume 15 of the World Survey of Climatology. I also edited that volume. That is another thing which I like. It is 760 pages or so, and really gives a thorough review of the climate of the oceans.

It was sold out quickly, went like hot cakes. The only book I ever earned money on. Although in terms of hourly pay it was very little.

When I was working in Berlin in 1974, I got a call from Helmut Landsberg, who was the chief-editor of the World Survey of Climatology series. He said to me, this is terrible, volume XV, which is on the climate on the oceans, is in bad shape. He said, "First I gave it to DeRuyter, a Dutch oceanographer, and he died. Then I gave it to a Danish oceanographer, and he gave up. Would you mind taking it over?" I thought a little about it and said "OK, I'll do it." He sent back all that had been written up to that time. I was not satisfied with a lot of it. The one on South Pacific Ocean had been written by a well known climatologist. A junior in high school could have written it. It was terrible. I got John Zillman and Neil Streten to write it. They did a wonderful job. Brian Tucker had written the chapter on the North Atlantic Ocean. He wrote back and said throw it away, it is too old now. So, I called Roger Barry and said, listen, Brian Tucker has written a very nice chapter on the North Atlantic but he doesn't want it published. Would you mind helping him revising it. Barry did that. It is Tucker and Barry now. It is a good chapter. The worst problem I had was with Colin Ramage. He was supposed to write about the climate of the Indian Ocean, but he had written only about the tropics, and two or three pages about the parts south of the Equator. I said to him, Colin you got to extend it to the Antarctic. No, he didn't want to. Ok I said, we are making two chapters. North of the axis of the southern subtropical high by Ramage, then Jan Taljaard and I will work from 35 South to the Antarctic. So we did. A nice German, Höflich – the name itself says it all, had written a long chapter on the South Atlantic Ocean, 100 and some pages, a whole book, in German. I said to him, we'll translate it. Since I could not find anybody to do it, I did it myself. It is a very thorough chapter. That book cost me so many thousands hours of work, you can't imagine.

The Japanese chapter I almost gave up on. I had to rewrite the Japanese English into English. The Japanese is still shining through. On top of it, the chapter on Iceland which is in there should have been in the *Survey of Climatology* that Svenn Orvig and Vowinckel wrote. It got submitted too late. They had just published that Volume, so I agreed to put it in my book.

It is interesting when you say how many hours it took you, because it came out in 1984 and I just counted 7 publications in 1984.

van Loon: That was routine stuff. I worked at home on that thing every day for eight or nine years. I got the nicest letter from Landsberg when it came out.

Now we are in the mid of the 1980s, right? There were the papers with Kingtse Mo.

van Loon: Yes, we wrote two papers, one on trends and the other on interannual variability in the SH. Also a couple of WMO reports. I really enjoyed working with Kingtse, she is very skillful with data analysis and computing, and has good ideas.

You started to do things with people like von Storch and Kiladis. This paper was exceptional since it was the first time that you were engaged in modeling of climate.

van Loon: Yes. I liked that. I am sure, it doesn't show anything startling. We wanted to find features that might be important in the development of a warm event. It was nice to see that a model could reproduce the observations.

The next big thing is in 1987 with Labitzke.

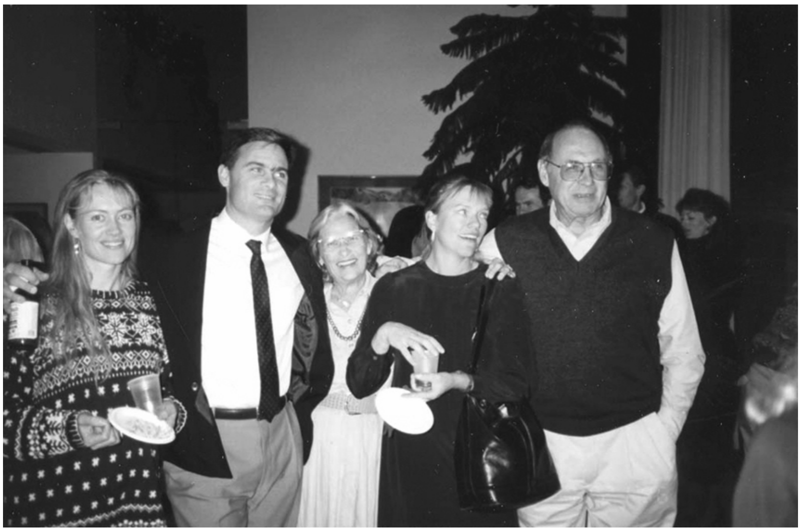
van Loon: She had been fiddling with some data and she saw a solar influence in the winter stratosphere in the Northern Hemisphere if she divided the data into the phases of the QBO, east and west phase. I said, why don't you write a note for GRL. She sent it in. The editor sent it back: A solar influence doesn't exist. I went to Ray Roble, who was a friend of this editor and said to him that this is very interesting stuff. It has high correlations and there might be physical links to equatorial stratospheric dynamics. So Karin got it printed. After that we just continued working with probable solar effects in the stratosphere.

We got a wonderful review from Jim Holton who said these correlations are too high to ignore. Now it is referred to quite often.

Also that had meant that you came in contact with a very different community. You can see that on the type of journals you were sending this. I guess there were also very different people.

van Loon: Karin and I wrote a book together that came out in 1999. That was a book on the stratosphere for general audiences. There were also several review articles on solar relationships with Karin.

In a sense Karin started other people working on the sun, she reintroduced the interest in solar climate relationships. It was that little 1987 note. It is rolling along so much now that some people have forgotten how it started.



Myanna Larsen, Harry and Kirsten's son Mikael van Loon, Kirsten van Loon, Harry and Kirsten's daughter-in law, Dana, and Harry (left to right) at Harry's retirement symposium, NCAR 1996.

Back to NCAR, you said before you stepped down from the position as scientist IV in 1991.

van Loon: I was 65 years old. It was about time.

But you continued. You have been on this part-time position from 1991 at NCAR until 2000.

van Loon: They gave me an office and $12,000 a year. But personnel had decided that people like me, Holland and other retirees shouldn't get $12,000 a year, but should get $22,000. So they gave us $22,000 a year. Then one year, after ten years or so, a year, when Karin and I had published a book, and I had three good papers out and 119 first author references, and the Division Director needed $42,000 dollars to remodel his office, so he took my $22,000 away

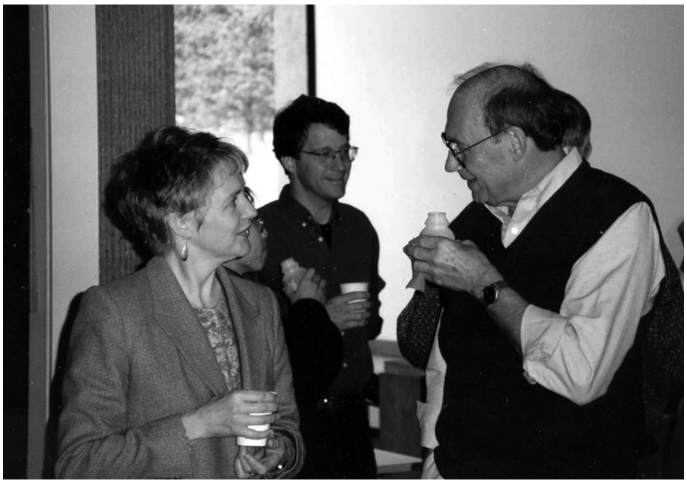
Jin Song von Storch (back to camera), Harry, Will Kellogg, Roger Barry (back to camera), Karen Labitzke, Byron Boville, Roy Jenne, Bob Chervin (left to right) at the van Loon symposium NCAR 1996.

Then you moved to CoRA, Colorado Research Associates.

van Loon: I really didn't know anything about Colorado Research Associates at first. Ralph Milliff, who is a fine scientist, very thorough, insightful, and reliable in what he does, was not treated very well by the same NCAR Division Director, because he didn't want to take part in the team modeling effort, so he moved to CoRA. The problem with CoRA is you have to bring your own money. It has no funding. So everybody there writes proposals like mad and I wasted almost two years, writing two proposals. But the spirit and working conditions at CoRA are phenomenal.

Finally Jerry Meehl, wanted to work with me. He persuaded Warren Washington to employ me as an independent consultant for $1,000 a month. Jerry and I have worked together since. For which I owe Warren gratitude. Not that I need the money, but it is the principle: if you are productive and still useful you should also get some remuneration, not necessarily $100,000 a year, but $12,000 seems fair.

It seems that you have not been engaged in what people call anthropogenic climate change research.



AMS Award presentation, Jill Williams and Harry, behind Tim Hoar at the van Loon symposium NCAR 1996.

van Loon: No, I haven't….

Did you have in your career ever communicated with the public, with the media or with policy makers?

van Loon: Not much. I remember an occasion when I was called by Reader's Digest some years ago. They were going to have an issue on sun and climate. He asked me what do you think of anthropogenic global warming. I said, you know, if you had called me twenty years ago, you would've asked me what do you think of global cooling. He said yes, in those days I wrote a book called "The Cooling". So I said, "now you can write one called 'The Warming' and you will be just as right. Climate changes on all time scales. Because the change happens on our watch doesn't necessarily mean we are responsible.

What do you think what is the role of people who are called very often by the media and who are actually influencing the public opinion. You have not participated in this debate with the public. On theother hand the public deserves some, needs to have a few people like you.

van Loon: The public deserves reliable, proper information on politics, science, health, etc., but it is not so that everybody, who has a strong faith in an issue is necessarily the one to give the public information. He or she may be very biased because of their conviction or faith in this issue. But how do you sort them out? They have to be able to say "I don't know."

How would you do that? How would we do that? You have the opinion some people do better than others. How do you judge that?

van Loon: I refer to Plato who said, don't ever give power to those who wish power. Give it to those who don't want power. Because they will do their duty and then they will relinquish the power. Also, don't let those who are keen to be in the limelight be the ones who tell the public what *they* believe is going on.

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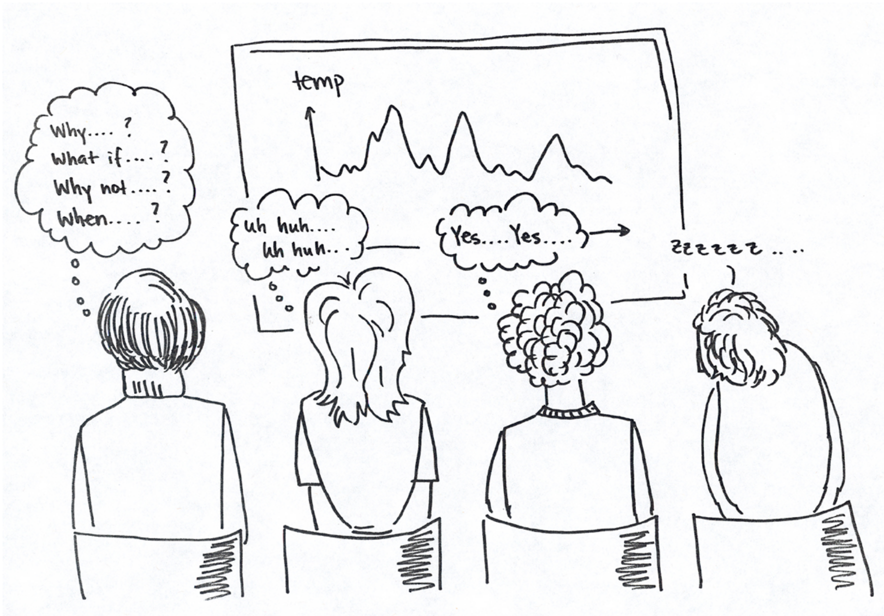

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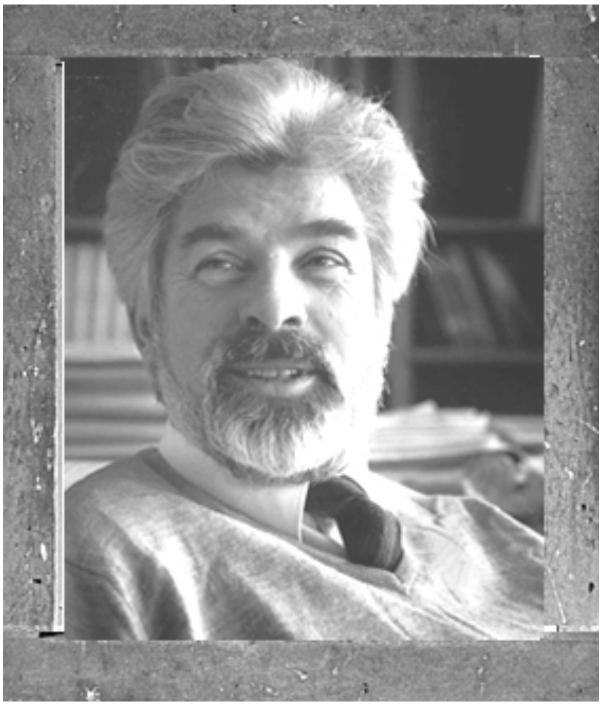


Interview with Klaus Hasselmann

prepared by Hans von Storch and Dirk Olbers
on 15 February 2006

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.

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.

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

Nevertheless you studied in Germany.

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, prenazi 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.

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

[1] Klaus Wyrtki has been interviewed in English earlier in this series, see von Storch, H., J. Sündermann and L. Magaard, 2000: Interview with Klaus Wyrtki. http://w3g.gkss.de/g/reports/interview_wyrtki.html
(GKSS Report 99/E/74, 41p p.)

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 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 offshore 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 in-

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.

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.

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

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.

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.

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

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 Sonderforschungsbereich 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 airsea 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

[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.)

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.

With Reimar Lüst, President of the Max Planck Society,
at inauguration ceremony of the Max Planck Institut, 1975

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.

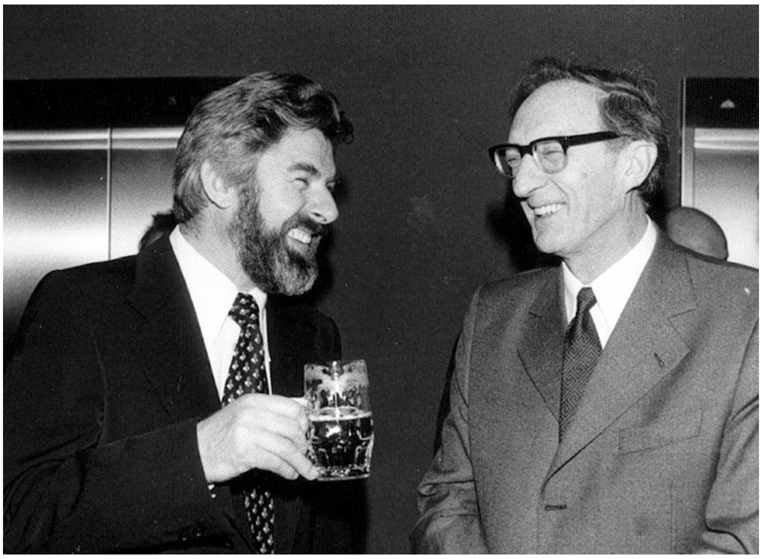
So I went off rambling about all sorts of vague things about ocean research in general. I finally tried to escape from this floundering by giving an example of research. I wanted to explain how the random-spectrum of ocean waves is generated by superimposing many different sinsusoidal waves. This part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

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.

> *So 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.



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

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, wo 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

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

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?

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

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.

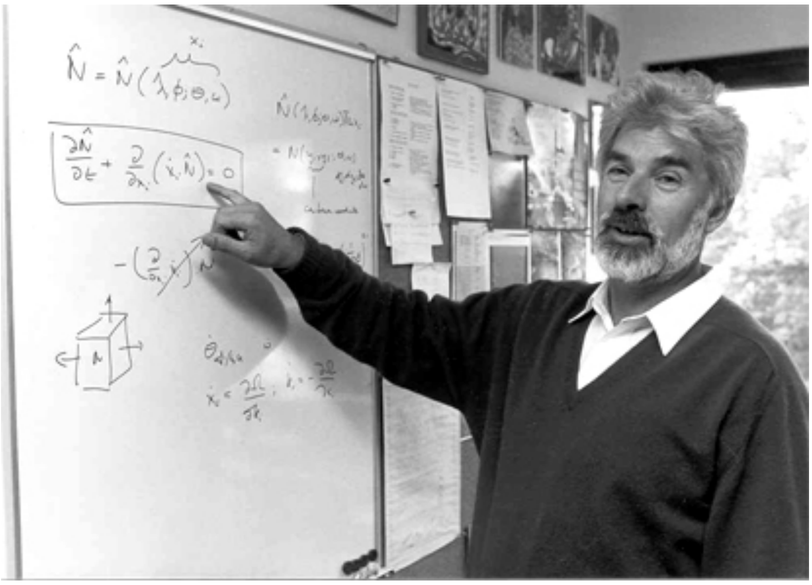
At the same time we were developing the global ocean circulation model, we were looking also at the carbon cycle. Maier-Reimer produced a first global carbon cycle model by incorporating the uptake and transport of CO_2 in the LSG ocean circulation model. This he successively extended in the following 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.



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?

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

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 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

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

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 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?*

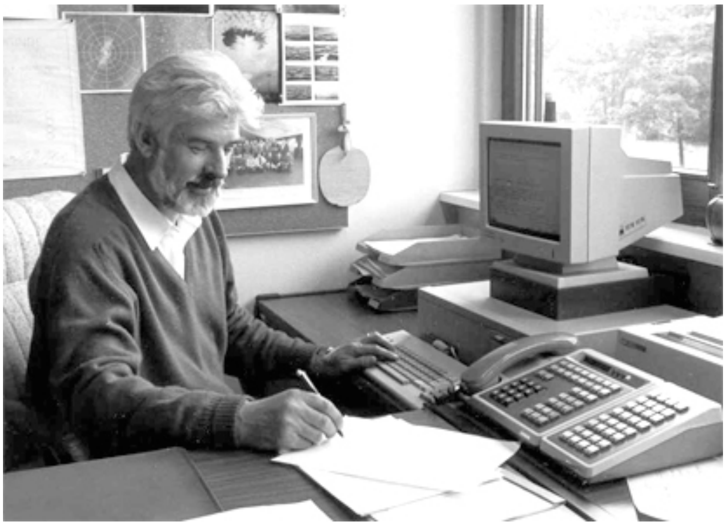
Hasselmann: It was clear at that time that we needed a good general atmospheric circulation model as part of the climate model. One needs a critically sized group to do this. The groups that had done this successfully were GFDL, NCAR in the US and – in particular – ECMWF in Europe. ECMWF was producing 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.


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

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.

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.

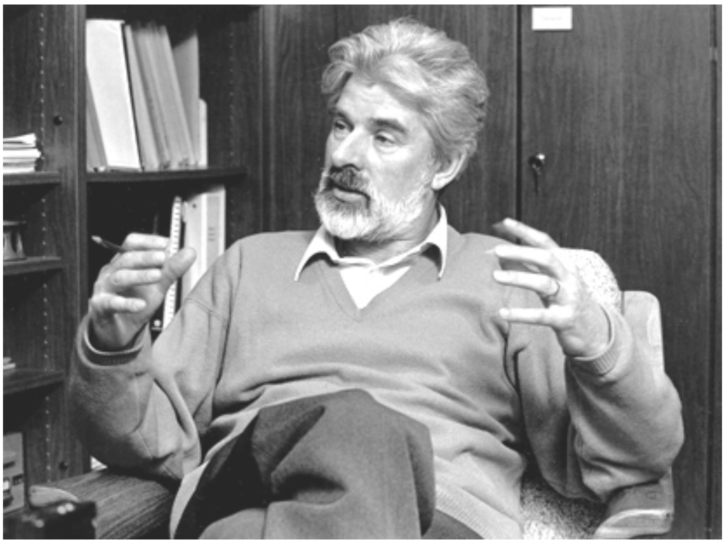
We found a good solution to both problems. I approached Reimar Lüst and reminded him of our second gentleman's agreement. I explained that the time had come when we really needed a third director to take care of the atmospheric modeling activities. His response was positive - in principle. I then approached 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.



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

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 stimula-ting 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.


Making a point, 1988.

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.

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.

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

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.

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.

With Wave modelling Group, Sintra, Portugal, 1992.

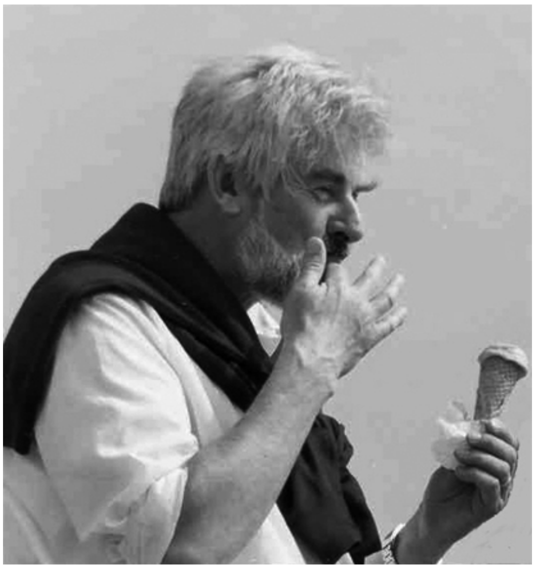
I heard stories that it was really tough for PhD students in the late seventies to work with you.

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.

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.



Enjoying an icecream in Sintra, 1992.

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 nonlinear 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.

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?

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 Hamburg, 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.

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).

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.

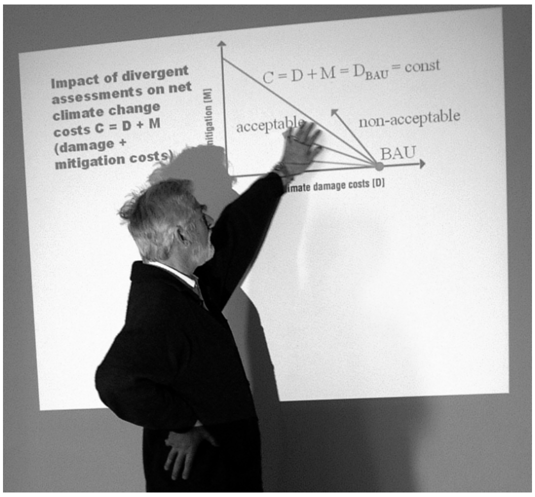
I followed up this work with somewhat more realistic but still relatively simple economic models based on non-equilibriium multi-agent dynamics. A few nice PhDs theses came out of this, by Volker Barth, Michael Weber and Georg Hooss [150,155]. As a side product, we created a climate computer game based 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.

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.

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 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.



Explaining the multi-agent aspects of a coupled climate-economy model, 2002

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 couhave 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]

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

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.

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

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 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?

60th birthday, Rissen 1991

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.

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

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.

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 – e-lementary particle physicists.

> *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.

With Hartmut Graßl, 1996.

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.

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

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.

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.

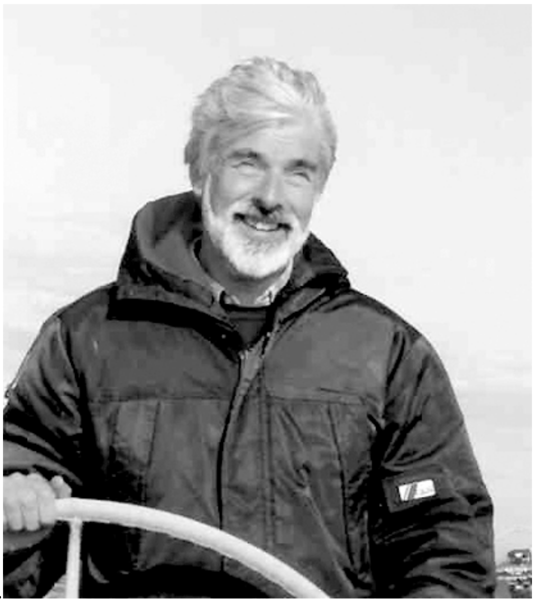
And I was enthusiastic when ERS-1 began providing ocean wave images with the SAR, from which we could retrieve two-dimensional wave spectra using the algorithm we had developed. When Patrick Heimbach compared the first three years of retrieved wave spectra in his thesis with the spectra produced with 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 tance that anthropogenic global warming was real and had been detected.

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

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.



Sailing in the Baltic, 1996.

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. 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 re-call 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
2. Fellow of the American Geophysical Union
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
6. Member of the Gesellschaft für Angewandte Mathematik und Mechanik

Books

Turbulent Fluxes Through the Air-Sea Interface, A. Favre and K. Hasselmann (eds.), NATO Conference Series, Series V: Air-Sea Interaction, Plenum Press, New York & London, 677 pp, 1978.

The Use of Satellite Data in Climate Models, Conference Proceedings, Alpbach, 10-12 June 1985, L.J. Bengtsson, H.-J. Bolle, P. Gudmandsen, K. Hasselmann, J.T. Houghton and P. Morel (eds.), ESA Scientific and Technical Publications, ESTEC, Nordwijk, 191 pp, 1985.

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

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