GKSS FORSCHUNGSZENTRUM

Interview with Klaus Wyrtki
25 February 1999

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44 pages with 17 figures

Abstract

For five decades, Prof. Klaus Wyrtki was part of the illustrious group of leading oceanographers. His scientific career began in Kiel, where he got his PhD under the supervision of Prof Wüst, lead him to Indonesia, Australia and California, until he finally came to Honolulu, where he stayed until his retirement in the early 1990s. On the occasion his 75th birthday and 50th PhD anniversary, Jürgen Sündermann, Klaus Magaard and Hans von Storch spoke with him about his experiences and insights of a long, successful and exciting scientific life.

Interview mit Klaus Wyrtki, 25 Februar 1999

Zusammenfassung

Prof. Klaus Wyrtki hat über fünf Jahrzehnte zu den weltweit hervorragendsten Ozeanographen gehört. Nach Stationen in Kiel, wo er 1950 bei Prof. Wüst promovierte, in Indonesien, Australien und Kalifornien, führte ihn die Wissenschaft schließlich nach Honolulu, wo er sich Anfang der 1990er Jahre aus der Wissenschaft verabschiedete. Aus Anlaß seines fünfundsiebzigsten Geburtstages und des fünfzigsten Jahrestages seiner Promotion befragten ihn Jürgen Sündermann, Klaus Magaard und Hans von Storch über seine Erfahrungen und die Einsichten eines langen, erlebnis- und kenntnisreichen Lebens.

PREFACE

On 25 February 1999 Lorenz Magaard (University of Hawaii), Hans von Storch (GKSS Research Center Geesthacht) and Jürgen Sündermann (University of Hamburg) have interviewed a famous "grand old man" of physical oceanography: Dr. Klaus Wyrtki, Professor emeritus of the University of Hawaii. The interview is printed underneath. For introduction a short curriculum vitae of Klaus Wyrtki is given.

J. Sündermann

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

I.

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?

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 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

[1] Institute of Oceanography.

[2] See also page 10.

[3] Wyrtki, K., 1950: Über die Verteilung der Trübung in den Wassermassen der Beltsee

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.

almost 50 years later

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 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.

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?

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 en 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

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und ihren Zusammenhang mit den hydrographischen Faktoren., Ph.D. dissertation, Univ. Kiel, FRG, 49 pp.

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?

Klaus in 1953

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.

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?

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.

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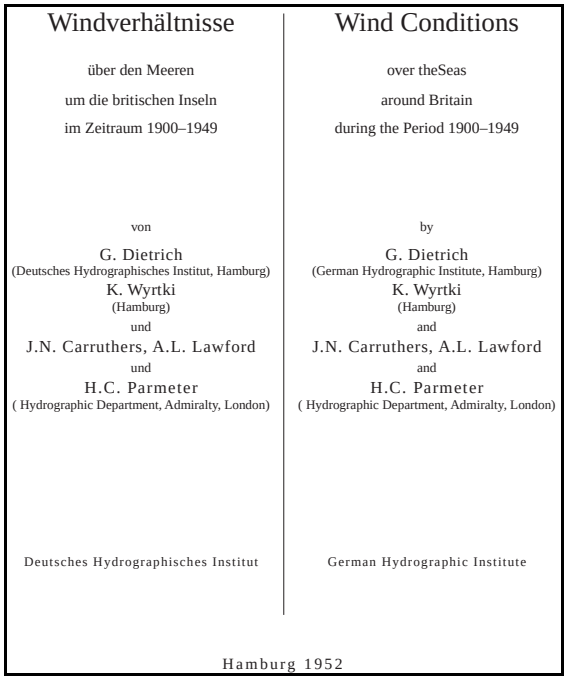
[4] Here, Wyrtki changed spontaneously into German: "come down"

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



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

Honestly speaking, I think it was done on the recommendation of Wüst. He was the professor at that time. There were no reviewers. Reviewing was an unknown matter, also reviewing for journals was not in existence. When a senior professor told a journal to publish something it was published.

Was that only so in Germany?

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?

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

[5] a research grant from the German Science Foundation

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

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 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?

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.

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

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

[8] See page 19

[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.

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.

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?

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

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[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.

and are counteracted by vertical diffusion. All these problems were at that time addressed.

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.

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?

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.

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

Was that the time when you started using a computer?

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.

Did you yourself any programming?

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 email?

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?

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?

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 at Columbia, Lamont, New York

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 magnetic

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 mill

Indian

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.

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?

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

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-Perio de.[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 Fragestel-

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[12] see page 1.
[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."
[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.

Kieler Nachrichten Jan.1954

Wie kam es zu der großen Flut ?

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

Von GERD SCHARNHORST

Eine Sturmflut von 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 ahzuschätzen.Sturmflut - das ist ein Wort, vor dem nicht nur die Menschenan der Küste erzittern. Auch tief im Binnenland weiß, man von dessen gefahrbringender Bedeutung. Und wenn man geneigt war, katatstrophale 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 mitStärken zwischen 8 und 10 gegendie Küste. Mit großer Gewalt langte die See in die Buchten von Lübeck, Kiel, Eckernförde,Flensburg und in die Schleimündung, die genau in der Nordost richtung 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. Bereits vor den Auswirkungen des Sturmes 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: " Ineinem abgeschlossenen Seeqebiet, 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 Fak tor hinzu: Die Wassermassen der Ostsee führen als Ganzes Schwingungen aus, die durch den Wind hervorgerufen werden.

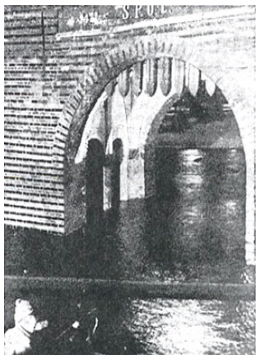
Einen ähnlichen Vorgang kann man In einer Badewanne leicht nachahmen. Auch hier befindet sich das Wasser ohne äußere Einwirkung zunächst in Ruhe. Macht man jedoch mit der Hand periodische Bewegungen, beginnt das Watser mitzuschwingen. An den Enden der Badewanne sind die Wasserstandsschwankungen am größten, 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 Lüberker 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 Tiedruckgebietes 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 ‚Überschwappen' 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 desDeichbaues (die übrigens die Anlieger zu tragen haben) übertroffen 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 nachfolgende 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 annimmt In den Ostseebuchten rechnet man 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 einer ausgedehnten Sturmflut trotz aller menschen möglicher Anstrengungen nicht in der Lage gewesen wäre, die Küste zu halten. An der Osiküste 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.

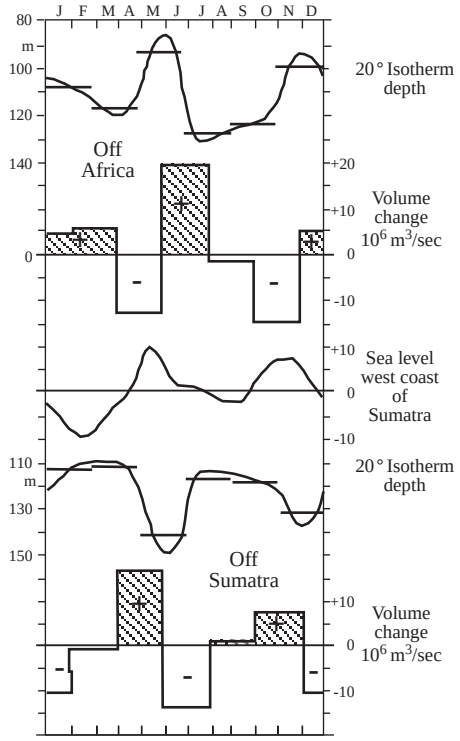


TRAVEWASSER AN DEN FUNDAMENTEN DES HOLSTENTORES

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

lung.[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° 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 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.



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

Did you make your own measurements in the Indian Ocean?

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

[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.

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

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 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.

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?

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..

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.

Definitely.

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

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?

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.*

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

[22] Departing from science.

and there is a time to retire when you have deserved it. There are lots of young people who 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?

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?

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

II.

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

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.

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.

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?

The eddies ... of course, biochemical cycles.

Already in the fifties or sixties?

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

[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.

[24] How long were the skirts in science?

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?

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?

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

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

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?

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?

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?

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?

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 observe the atmosphere or the ocean or the

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

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?

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...

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?

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

You are not talking about models like Stommel's?

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.

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?

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.*

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.

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

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?

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?

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?

Eventually TOPEX and the altimeters. I did not participate in the use of altimeters anymore. We had younger people who

[25] Statement by Jürgen Sündermann

were doing an excellent job at that. It is not necessary that you do everything.

III.

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

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?

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?

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?

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 throughflow 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.

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

What about international organizations?

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 d nd productivity without knowing about circulation, mixing and other processes.

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

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.

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?

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?

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?

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?

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 ..."?

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.

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?*

I personally prefer to be in a small university, in a small institute that is relatively independent. I do see the need

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

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.

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?

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?

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?

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?

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?

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?

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?

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.

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.

IV.

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?

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?

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?

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

[29] The dear colleagues

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.

V.

What are your forecasts of the future of science.

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.

Taping the interview: Hans von Storch and Klaus Wyrtki

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]

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?

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

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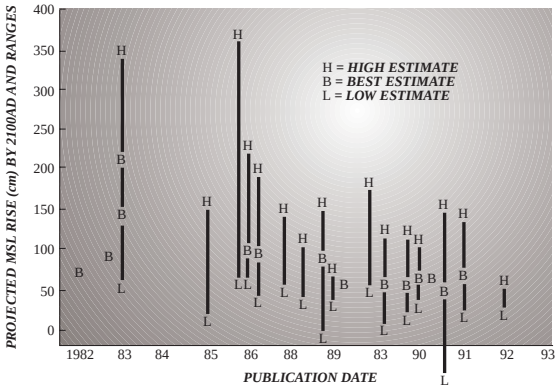
[30] see page 7.
[31] Gary Mitchum.

made a very interesting plot. It starts with the first prediction of three meters over one



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

century, 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".

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

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?

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

Have you anything to do with paleoclimatology?

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?

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

Will it become fashion?

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?

It depends what you call sudden.

Within decades of years.

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?

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?

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.

That's what Wüst said and I demonstrated it.

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

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.

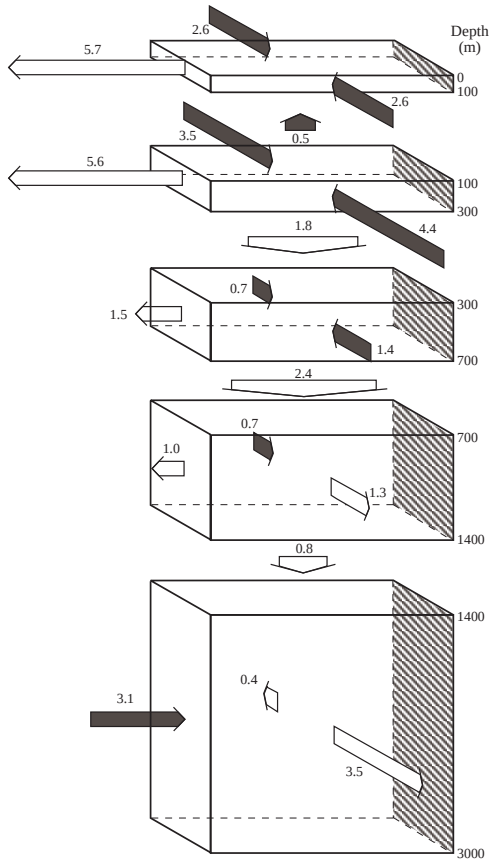
I said that sea level rise will be a picnic compared with the population explosion.[32]

VI.

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 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]

Which are your favorite own publications?



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.

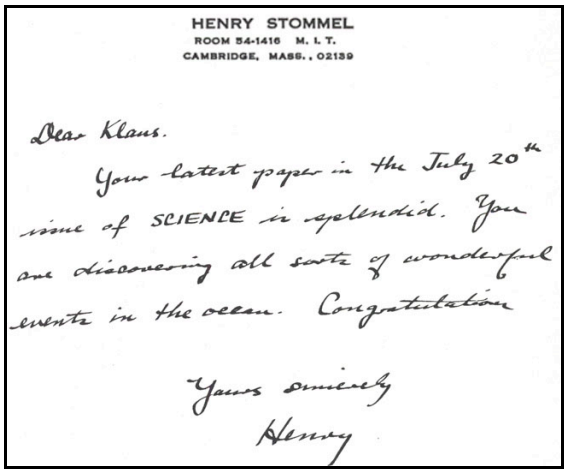
These are the thermohaline circulation from 1961[34], and the deep sea basins, the oxygen

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

[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.

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



HENRY STOMMEL
ROOM 54-1416 M. I. T.
CAMBRIDGE, MASS., 02139

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

What about your Baltic studies?

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?

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 summarized 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?

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.

Have you thought of writing a book?

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[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.

[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.
Wyrtki, K., 1954: Die Dynamik der Wasserbewegungen im Fehmarnbelt II. *Kiel. Meeresforsch.*, **10** (2), 162-181.

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

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.

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.

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.

Klaus in February 1999

The interview was conducted in 25 February 1999 in two sessions of about 2 hours each – the first in Peter Müller's lab in the Marine Science Building in Honolulu, the second in Klaus Wyrtki's apartment. Participants were Hans von Storch, Jürgen Sündermann and Lorenz Magaard. Two tapes were transcribed by Ilona Liesner and edited by Klaus Wyrtki and Hans von Storch.

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

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

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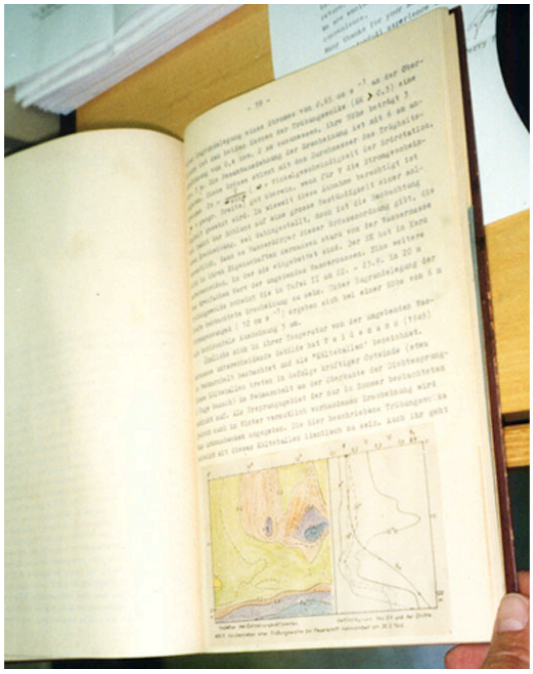

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