BECOMING AN OCEANOGRAPHER FORTY YEARS AGO

By Klaus Wyrtki

Editor's Note: *The following is a slightly abbreviated transcript of an informal seminar given by Professor Wyrtki to oceanography graduate students in September 1988.*

MEINE DAMEN UND HERREN, Ladies and Gentlemen:

Today I'm going to tell you something different. Not about sea level, not about new instruments, not about a new solution of an old equation that you know more about than I do. I'm going to tell you how it was to be a student forty years ago, and you may relate your experience right now to the things I am going to tell you. I could start "once upon a time," but I don't want to do that because I don't want to tell you a fairy tale. I could also tell you about the "good old days," but I think the term is an illusion. The "good old days" is when you're young and you're vigorous, when you have a lot of energy, when you have little responsibility and few obligations. And, by the way, your "good old days" may be right now.

There is of course a reason for giving this talk. The reason is that one year ago I got on a committee of NOAA that would decide, and subsequently oversee, how NOAA is structuring its program on climate and global change. Here we come to change. After the first few sessions, I was somewhat disappointed, because all the discussion in the panel was about climate and not about global change. I believe if you are thinking of global change then you are thinking of pollution, of deforestation, of shortages in water, and first of all, of course, the population explosion. I think this is the worst of all problems.

In 1925, when I was born, we had less than two billion people on this earth, and during the short time of my life so far, a little over sixty years, we have increased to five billion, a factor of 2.5. Now *this* is global change, please remember that. I think climate change is a very minor part of global change, probably rather insignificant.

I will tell you how it was to be a student some forty years ago. When I was fourteen, I knew exactly what I would be and what I would be doing. I wanted to become a naval architect, and I wanted to design aircraft carriers. It was as clear as anything. It didn't happen that way. Since my good mother was a widow—my father died when I was four years old—and not of very great means, I chose the simple way out. I enlisted with the Navy and hoped that the Navy would send me to a university where I would study shipbuilding and then design the aircraft carriers. When I was seventeen years old, we were called to the Navy before we got our high school certificate, so I didn't have to make final examinations in high school. We were given a little piece of paper that said you have passed your high school obligations, and the teachers thought, "Well, most of them aren't going to come back anyway." Let me skip a little bit of time and tell you what happened in 1945, after the war.

In late fall of that year, many German universities started to open their doors again. We young people were running up and down the Rhine searching for admission to one or the other university because we wanted to get in. So I traveled from Heidelberg to Frankfurt to Giessen to Marburg to Göttingen and back down to Karlsruhe and Darmstadt to find an admission at a university. By that time, I knew that aircraft carriers were out, so I changed my plans. I wanted to study harbor engineering or waterway engineering or something like that. In Germany, there are the known universities and there are what we call "technical" universities that in earlier time were called "technical high schools." I didn't get into Karlsruhe or Darmstadt, but I was admitted in Marburg. Now, how did this happen? Well, at that time you went to the registrar and said you wanted to study. You showed him the little slip telling him you had passed high school and he would say, "Yes, what do you want to study?" I said "Mathematics and physics," and he gave me the names of two professors to see. I went to these two professors and they asked me in about fifteen minutes a few questions, and they found that I should be admitted. A very subjective process.

The little town of Marburg had 30,000 inhabitants before the war. Right after the war, it had about 60,000, swelled by the refugees from the east, and the town had a university of 3,000 students. Three thousand was a full size university at that time. The university had five faculties—Theology, Philosophy (what you know as Arts and Sciences), Medicine, Law and Economics. People who wanted to become elementary school teachers, accountants, nurses and so on, did not go to university. They went to professional schools that gave them their education in that particular profession. Only students who really wanted to earn an M.D., Ph.D. or doctor of law went to a university. When you sign up you get what is called a study book. You go to the registrar and you enter in the book the classes you want to take and you pay your fees. In my first year, I paid 180 marks; but that doesn't tell you much. I could live at that time under rough conditions for about 150 marks a month, so you paid for your university tuition about one month's worth of your living expenses per semester.

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Curriculum for Klaus Wyrtki

	hours
Winter Semester 1945/46	(17)
Calculus I	(4 + 2)
Geometry	(4)
Physical Chemistry	(2 + 1)
High Frequency Physics	(2)
German Literature	(2)
Summer Semester 1946	(37)
Calculus II	(4+2)
Partial Differential Equations	(4)
Differential Geometry	(4)
Algebra	(4)
Theoretical Mechanics	(4 + 2)
Experimental Physics	(4 + 1)
Electronic Tubes	(2)
Physics Lab	(6)
Winter Semester 1946/47	(36)
Statics	(4)
Projective Geometry	(2)
Applications of Calculus	(2)
Theoretical Mechanics	(4 + 2)
Experimental Physics II	(4 + 2)
Wave Mechanics	(2)
Physics Seminar	(2)
Physics Lab	(6)
Climatology	(2)
Physical Geography	(2)
Philosophy	(2)
Summer Semester 1947	(46)
Statistics	(2)
Math Seminar	(2)
Differential Equations of Physics	(3)
Electrodynamics	(4 + 2)
Theoretical Physics Seminar	(2)
Geometrical Optics	(2)
Atomic Physics	(2+1)
Experimental Chemistry	(5 + 4)
Advanced Physics Lab	(8)
Electronic Tubes III	(2)
Physical Chemistry	(3)
Chemical Lab	(4)

	hours
Winter Semester 1947/48	(26)
Math Seminar	(2)
Math Upper Seminar	(2)
Theory of Functions	(5)
Vector Analysis	(2)
Physical Geography	(3)
Geography Seminar	(2)
Mineralogy	(3+2)
Crystal Structures	(3)
Men and Cultures	(2)
Summer Semester 1948	(22)
Physics of the Oceans	(2 + 2)
Meteorology	(2)
Zooplankton	(1)
Fishery Biology	(1)
Resources of the Sea	(1)
Limnology	(1)
Ocean Colloquium	(2)
Landforms	(2)
Geography Seminar	(2)
Cultural Geography	(2+2)
Geology of NW Germany	(2)
Winter Semester 1948/49	(11)
Marginal Seas	(1)
Ocean Colloquium	(2)
Vectors and Tensors	(4)
Geography Seminar	(2)
Physics Seminar	(2)
Summer Semester 1949	(17)
Wave and Tides	(2)
Advanced Meteorology	(1)
Chemistry of the Sea	(1)
Phytoplankton	(1)
Ocean Colloquium	(2)
Thesis Research	(8)
Physics Seminar	(2)
Winter Semester 1949/50	
Thesis Research	(8)
MAY 1950, Ph.D.	

Table 1: Curriculum for Klaus Wyrtki at Marburg University and later at the Institut für Meereskunde. Figures in parentheses show total hours of instruction per semester, as well as lecture and laboratory hours for each course.

Table 1 gives you an idea of the courses I took, and you see that all the same things that you have now were offered then. The first semester was in the winter of 1945-46. I took what one usually does; this was all that was offered. In the summer semester of 1946 you see a greater number of courses, and you may think, "How in the world can you get thirty-seven hours of commitment in a week?" Well, easy enough. First of all, the week was six days long, so the physics lab was all of Saturday. And on Wednesday, there were two other labs. The classes were held on Monday-Tuesday and Thursday-Friday, and you went to every one.

The real difference is that we didn't get any grades. We didn't have any examinations. I have never in my whole university career made an examination except the one for my Ph.D. And that gives you the freedom to learn, whenever you feel like learning, whenever you like to learn, whenever you don't. This is a big difference, a fundamental difference. All that we got was a signature from the professor at the beginning and a signature at the end. In the first year of my studies it was essentially all mathematics and physics; I took many courses that were way over my head. I think these were the most valuable courses I ever took, because I learned something about what I might learn later on. These courses gave me ideas about which way I really should go or not. Why not listen to something you don't understand about atomic physics if it makes you interested in the subject? I think there's nothing wrong with that.

All the time I was studying physics and mathematics during 1946, I was really looking for something different, and you see from the table that I started to become interested in climatology and geography. I took a few classes and in doing so I found that a field called meteorology existed. By reading a few textbooks on meteorology, I discovered oceanography. That was about in 1947. There were nice books like *Physical Oceanography* by Defant that came out just a few years before the war, and Ekman's theory, and similar things. They were all in the library and I really got interested in oceanography.

There's one other item that I'd like to tell you about. There were of course a number of student clubs that were springing up, and I happened to join one about German-Canadian things—probably because there were a few nice girls there, but that may not have been the only reason. We got into correspondence with a few Canadians, and these people asked us what we would like to have. It was just the time when the "Bible of Oceanography," *The Oceans*, by Sverdrup, Johnson and Fleming, came out in 1946, and I asked for a copy. This book arrived in Marburg one day, sent by a gentleman, Mr. Lawrence, from Saskatchewan, and I still have it here.

Look through the table, and you will basically find all the courses that belong to a mathematics/physics curriculum. In the second year I was already taking advanced courses at a rapid rate. The reason is that I didn't have to go to exams: you can study the way you wish to and not be forced into a rigid curriculum and into working for examinations. In the summer of 1947, I went up to Kiel, on the Baltic. When I was looking for the Institut für Meereskunde, I came to a little villa that was the Institute of Geology, and it also housed the Institut, which actually was Professor Wüst's little apartment, just under the roof.

When he let me in, I told him my story, that I wanted to study oceanography. He just had a little bedroom, a little study, and a small kitchen, and that was the whole Institut in 1947. I told him my story and he said, "Oh, how nice, now I have a student." He

arranged for a student exchange—at that time one could do that, one could switch admission places—and I went from Marburg up to Kiel the following summer.

In the university there an institute was the same as a department here. When I arrived in 1948, there were four professors: Wüst was the physical oceanographer; Hoffman was a botanist; Friedrich, a zoologist; Kandler, a fishery biologist. We had one docent, which you would call here an associate professor, and that was Krey and he was a chemist. We had one assistant, which you would call an assistant professor, and he was a meteorologist. There were about four or five students in 1948. Other things were a little different too: On the door of your professor you would see a sign that said "Visiting Hours—Thursday, 11:00-12:00." The rest of the week—no way, unless he calls you. In my sixth semester, the summer of 1948, I took all the things that are familiar to you: Physics of the Ocean, Meteorology, Zooplankton, Fishery Biology. There were four professors and you went to everyone's lectures. There were also Resources of the Sea, Limnology, Ocean Colloquium, and I took some Geography because I was still interested in that subject in many ways. And then came the winter semester, when apparently nobody wanted to teach very much, and what you see in the table was all that was offered.

One day you have to go up to your professor and ask whether you may become a doctoral candidate. And what usually happened is that the good old man will look at your study book, open it, and say, "Oh, I think you should take this and that and in a year, come again." That can happen; it happens very often. It is in some way or another a rather arbitrary decision, because if he doesn't take you, you'd better go to another university. There's no way you can complain. If he doesn't like you, you'd better move on and find the next one who might take you.

But, I was accepted and Wüst, being very pragmatic, told me, "Well, we have to find a subject for your thesis." He said, "This is easy." During the war, the people at the German Hydrographic Institute were designing instruments to measure light in the ocean. A fair amount of research was being done and instruments were developed to measure light in the sea. Wüst told me that the German Hydrographic Institute would loan us one of these instruments and I could take that instrument and go out to sea and measure. He was very pragmatic about it and he said, "Well, Dr. Krey, who is a chemist in the Institut and has worked with these instruments, will explain how to use them." So, I went to Krey and he gave me a two-hour lecture on colloid chemistry, about which I didn't have the slightest idea. He gave me also three books, each one bigger than a telephone book, and I walked out of his office somewhat depressed. But I took rapid action—I put these three big books in the lowest drawer of my desk, and I never opened them until after I had my Ph.D. And I think that was a good move, because the whole thing would have sidetracked me. I did not want to know the chemistry of the dirt in the ocean, but I wanted to know how the dirt behaves in the ocean environment.

Klaus Wyrtki reading instruments as a post-doctoral researcher in Kiel.

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Wüst arranged for me to go to a light vessel that was anchored in about 30 m of water at Fehmarn Belt—that's a strait between Germany and Denmark, which has very strong currents and a very strong thermohaline structure. This part of the ocean is unique, there is no doubt. At the surface, you have water of 10 ppt, in the lower 10 m, of 25 ppt. At the surface, the temperature is 15 °C and in the lower layer 4 °C. The upper layer moves with 2 knots one way, the lower with 1 knot the other way. So this is quite a structure.

Since decades before the war, light vessels in Danish, German and English waters had taken daily observations of temperature and salinity, sometimes of currents, and a huge observational data set was available. But—the old story—you measure more closely in time and space and you will find something new. So we measured every four hours, and every meter instead of every five. And we had a new instrument that would measure the transparency of the water. It was quite a big piece of equipment, and we rigged up the winch on the light vessel so that one man could operate it. I could myself operate the whole thing slowly up and down. I was reading a little voltmeter every meter of depth as the meter wheel was ticking away and I made my observations. I did that for four weeks, came back, evaluated, and went out a second time. I went to other light vessels, together with a biology student, Max Gilbrecht. He was studying detritus in the sea by means of an inverted microscope and he found out that most of the stuff is dirt and not life—that wasn't known at that time. And we tried to put together counts of the particles, counts of the plankton with the vertical distribution of turbidity, and we found that in this rapidly moving current system there were water bodies that were only a few miles long, a few meters high, that were rushing by—clear water, turbid water, clear water again—and so on. We observed that storms would stir up the dirt into the water column and that the settling would happen in the discontinuity layer, and all such things. It was a rather exciting time.

I don't think we get students that are any brighter when we keep them three years longer.

Instruments were, of course, a problem at that time. Have you ever had a sensitive voltmeter, an ordinary primitive voltmeter, and tried to read its needle on a moving boat?—it's impossible. We didn't have digital displays. We tried to get companies to design instruments that would work at sea. I remember one case where an engineering company from Hamburg said they had the right instrument. It was a big machine that was writing with a stylus on wax paper, giving a continuous record, and the stylus was held by fist-sized magnets so that it wouldn't move too much. It was very hard to govern that stylus with an electronic tube amplifier, and very primitive by our standards. And I remember they tried it out in the lab, decided it worked perfectly, until we took them out into 15-foot waves in a small research vessel that rolled heavily, and they were totally seasick. Four weeks later, we had a working instrument. That gave them a good lesson: they knew then what the environment for this instrument really was.

Now, on the matter of publications. When I was a student, every few months a journal came in. It was very exciting to see, for instance, Stommel's theory of 1946 on the general ocean circulation and other things. But the journals came in at such a slow rate that as a student I was reading every paper in oceanography that was being published. Today you can read perhaps 2% in your particular field. At that time, you did read everything that was published. The *Journal of Marine Research* came out every four months or so, and there was of course the German *Hydrographic Journal*, there was a British journal, there was the *Journal de Conseil* from Copenhagen, but this was all. Now, of course, we have the information explosion. How much can you possibly read? I don't know.

I always admired Wüst. When you came to him and talked about something—Indian Ocean, south of Madagascar—"Oh yeah," he would say and he would pull out the drawer in his desk in which he had file cards, on which all the deep hydrographic stations ever taken in the world were registered. He would say, "Oh, this is the station south of Madagascar." That was it, and he had it. There were only about 600 to 800 deep stations around the world, and he knew every one and could dig it out of the file. So, one might say with fewer data more thought was probably devoted to the interpretation of the data and more care was given to make data valid and to get something out of them; a real effort was made to interpret the data. Today, if somebody gets a thousand XBTs in the Pacific in a month, he says, "Oh, that's not enough, I should have 4,000." And he doesn't make an effort to get something out of the thousand first; that's very often the case.

I told you already a little bit of what I did for my Ph.D. Now comes the next step, and like you do it today, you would write a Ph.D. thesis that had to go through the committee. The committee at that time consisted of three professors, so I had Wüst for oceanography, a physics professor, and a mathematics professor. That was standard at that time. You had to have three subjects and they gave you about an hour of examination each, after they had approved your thesis. And when it was over, you had your Ph.D. and were ready to be released to the world. In my case, from entering the university to the Ph.D., it was five years, and that was not unusual at the time. That's not an unusual case, I'd like to emphasize that. I have the feeling that today we are going too slowly. The demands on the student are just too big. I don't think we get students that are any brighter when we keep them three years longer. They probably can acquire some knowledge or some skills later on in their profession. They need to learn how to do it, how to think, how to formulate scientific problems. They don't have to solve all the problems before their Ph.D.

Now, let me close and give you a summary of what I have been saying today. First of all, we started with the problem of change. I don't think the students have changed. Many other things have changed. The university environment has changed. Most of all, I think the universities are too large. We are withdrawing into a sphere in which we are really only concerned about our own institute, no longer with the university. In Marburg, with 3,000 students, a professor of philosophy gave an introduction to philosophy twice—Monday and Tuesday—from 8:00 to 10:00 in the evening in the biggest auditorium—because there were a thousand students in each session who wanted to hear her. And afterward, after her lectures, we went to the nearest wine cellar or beer cellar and had a night-long discussion. There was another year in which a young associate professor gave a lecture with the title *The Struggle for God in German Literature*. And it was so fascinating that again every student at the university was there from 8:00 to 10:00 in the evening. This is something that I feel is a little bit missing today.

On the other hand, you have enormous advantages over what we had. The first thing I want to mention are computers. I think your ability to handle problems, data crunching, data processing and mathematics, in general, is probably—if I should give an estimate—increased by three orders of magnitude. When it comes to instrumentation—meaning the precision and the value that you get out of measurements, the ease of effort—then I would say that probably you have an improvement of one and a half orders of magnitude in this short time-span of forty years. When it comes to the availability of data, things have totally changed; we are accumulating data at such an enormous rate that in my time we couldn't even dream about it.❑

Acronyms: NOAA, National Oceanic and Atmospheric Administration; XBTs, expendable bathythermographs.