Photo courtesy of Stephen C. Porter

A TRIBUTE TO MINZE STUIVER UPON HIS RETIREMENT

Former *Radiocarbon* Editor and *INTCAL98* Guest Editor Minze Stuiver retired in October 1998 as head of the Quaternary Isotope Laboratory at the University of Washington, Seattle. The renowned lab, which Minze founded 29 years ago, will close when he leaves. Throughout his long and productive career, Minze's integrity and dedication to science have earned him the respect and admiration of his peers. We are pleased to present the following tributes from just a few of his many friends and colleagues.

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AUSTIN LONG

Whether you are a relative newcomer or a veteran to either the production side or as a user of radiocarbon measurements, the comments below from Minze's colleagues will give you personal recollections of a scholar and also an awareness of some less overt aspects of radiocarbon dating. If you are a new reader of *Radiocarbon*, you may notice what the veteran readers are aware of: the sense of international community that exists among those of us in the field. The remembrances below well illustrate this. The interesting aspect of this community is that its common thread is a technology rather than a discipline. Its triennial gatherings allow archaeologists to mingle with oceanographers, chemists and physicists. Happily, these scientific interfaces often are productive, and Minze's life illustrates this.

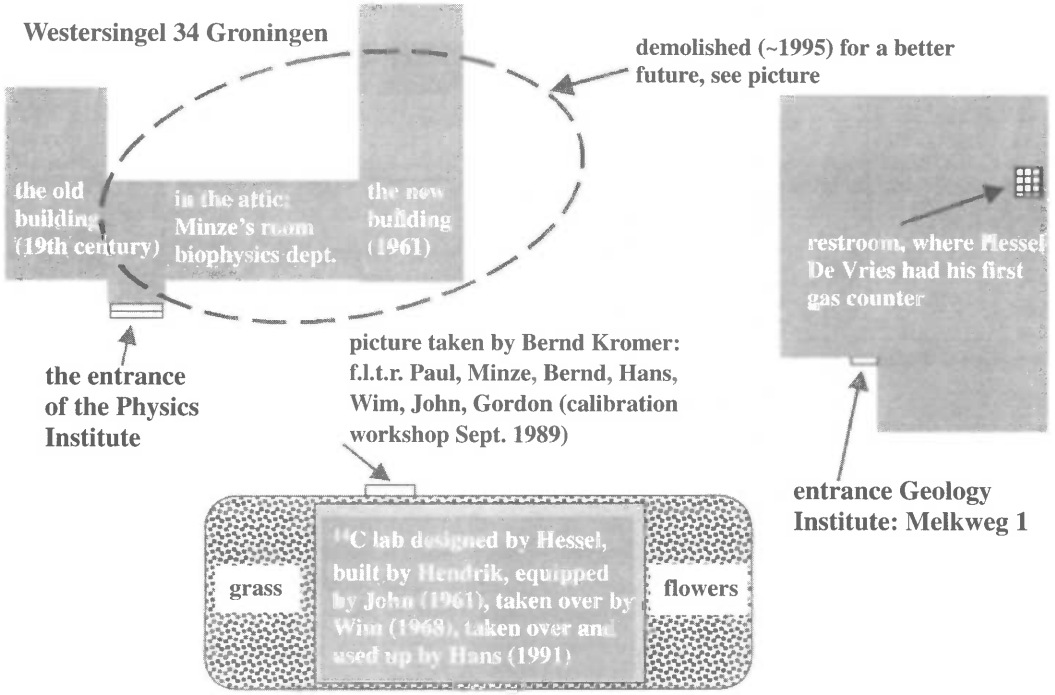
Minze began as a biophysicist, and applying his knowledge of physics and chemistry, he helped oceanographers understand oceanic upwelling and mixing, showed archaeologists how better to understand chronological uncertainties and apply corrections to their ^{14}C dates, and was involved in revealing the geophysical processes modulating the production rates and levels of radiocarbon in the atmosphere. Minze is a scholar who selects important problems, works meticulously toward their completion, and presents results in calm, dispassionate and thorough fashion. He edited this journal for 10 years until the heart surgeon demanded he cut back on his long work days. His editing has not stopped, as you saw from the 1993 calibration, and as you will see in this issue. Minze is not really retired, just redirecting his energy and talents.

Minze Stuiver in his Seattle ^{14}C lab. Photo by Jimi Lott/The Seattle Times.

Minze and Anneke Stuiver. Photo by Stephen C. Porter, University of Washington.

Minze in 1970 (left) and 1976 (right, in Seattle). Photos courtesy of Stephen C. Porter, University of Washington.

WIM MOOK AND HANS VAN DER PLICHT



As a student of Hessel de Vries, Minze wrote a thesis on "The Physics of the Sense of Smell". In his career, he proved to have developed a "good nose" for selecting his interests. One can wonder whether this "nose" led him not to continue working in Groningen on ^{14}C, considering the position of the first gas counter. He would have been involved in the demolition of 3 laboratories after all.

E. H. WILLIS

Hessel de Vries and the Groningen Laboratory: Recollections of Pioneers in Radiocarbon Dating

The radiocarbon community owes a very considerable debt to the early work at the University of Groningen, Holland. Throughout the fifties, it was arguably the leading radiocarbon laboratory in Europe. It also happens to be Minze Stuiver's Alma Mater so one has rummaged among the cobwebs in the attic of one's mind for some recollections of that most productive era. It is a story of imagination, improvisation, hard work, and dedication; the sort of qualities for which the Dutch are justifiably renowned. It was an environment of which Minze can be justly proud to have been part, as I am proud to have been but a witness. These recollections portray a world of difference between life in a laboratory in Minze's early days, and conditions in laboratories prevailing upon his retirement.

I first visited Groningen in 1953. It was as isolated a place as one can imagine; it was literally at the end of the line from Amsterdam, and although the German border was only about thirty kilometers away there was little or no cross border traffic. Later, Groningen was to become a center for the oil and gas industry, but for the moment it remained what it was, a parochial little town on the flat damp lands of northern Holland. World War II had been traumatic and the immediate postwar years had been protracted and difficult; bitter memories had been slow to fade. The main square of the town still bore the sad evidence of the savage fighting between Canadian contingents and the retreating SS, and pock-marks could be seen on some of the buildings that remained. Universities everywhere in Europe were, at best, still trying to claw their way back to the standards existing before 1939. Money was scarce beyond today's imagination, and old and dilapidated buildings had to serve for the moment. The science faculty of the University of Groningen was no exception. I well remember getting off the bus from the railway station on that cold and foggy December afternoon, and gazing somewhat despondently upon its somber and forbidding red brick exterior. Its uninviting interior proved faithful to its exterior promise, for it made no concessions whatever to either aesthetic taste or creature comfort. Fortunately, the downside stopped right there. Whatever its physical constraints, it had more than its share of bright people. For all the cruel legacies of the recent war, a positive one then on display in Groningen was a fervored drive to be a part of the new world being opened up by science. In this environment thrived Hessel de Vries, a single minded and energetic young professor of Biophysics. His interests ranged from the role of visual purple in sight, the phenomenon of smell and, of course, the exciting new technique of radiocarbon dating.

For all his biophysical interests, it was radiocarbon which was to consume most of his energies. Libby's screen wall Geiger counter had been adopted by each new radiocarbon laboratory in turn simply because it worked—no other redeeming feature comes to mind for it was extremely inefficient. The most obvious alternative, gas proportional counting, could not be made to work reliably. Acetylene proved to work quite well as a counting gas, but had the dismaying propensity for demolishing everything in sight at the slightest provocation, a fact that I was to prove conclusively at Cambridge. Carbon dioxide, the obvious gas to use, was thoroughly un-cooperating when it came to good counting characteristics—until De Vries that is. A letter in a relatively obscure journal, *Physica* (XVIII, p. 652), towards the end of 1952 by De Vries and his student Barendsen recorded on one half of the very last page the simple fact that when carbon dioxide was purified to a very high degree it provided excellent counting characteristics. They later published a more comprehensive report in *Physica* (XIX, p. 987) in 1953, after the first Groningen date was achieved on January 15th of that year. Shortly after, Fergusson, in New Zealand, published a similar finding in *Nucleonics* (13, p. 18,

1955). Groningen had taken a major leap forward in the accuracy in radiocarbon dating, and gas proportional counting was the technique embraced for the next decade or so.

Never was there a man so free as De Vries in his help for others in the field, and many of us owe him a special debt of gratitude. As a young graduate student assigned the task of creating the Radiocarbon Laboratory at Cambridge University I was eager to learn more, and was cordially invited to visit. In those days, such visits were arranged mostly by post card because the postage was cheaper—the terse "Please come" on a post card was all that was needed—that was cordiality! De Vries' intensity for the subject became legendary. Later, in 1956, when I boarded the SS *Nieuw Amsterdam* at Southampton bound for America, en route to the Andover, Massachusetts Radiocarbon Conference, I was unexpectedly hailed from the upper deck by a familiar voice—De Vries talked incessantly of radiocarbon from one side of the Atlantic to the other, and it was the only time I nearly gave up.

The shortage of space, and the ability to get the most from strained resources to pursue meaningful research, were very obvious in that old building at Groningen. The purification system for the carbon dioxide was arranged around the walls of a landing on a stairway between two floors of the building. The landing had normally housed a toilet, which was still there in its cubicle but surrounded now with glass tubing, John Vogel, a later Director of the Groningen Laboratory, tells me that De Vries got his high voltage apparatus as surplus from a Canadian military dump. One could only marvel at the ingenuity of these people in making the most out of so very little. This was a classic example of "if only you want the result badly enough you will find a way to achieve it, however inelegant the apparatus and constrained the resources". One might add that academic salaries at that time were invariably commensurate with the frugality of one's surroundings, and this was amply reflected in our life styles. As it turned out, our contemporary graduate students in the USA were living equally frugally, which was not what we had been led to expect since our views of Americans were conditioned by the apparent opulence of visiting professorial rank scientists, who naturally enough traveled by plane and not by ship.

Groningen was a welcoming community and made you feel part of the family. It was in the course of one of many visits that I was introduced to a young graduate student working with De Vries in a rather antiquated (if he will pardon that description) laboratory, trying to measure the threshold of smell—he was Minze Stuiver. It is a joy to me that Minze and I have remained friends ever since that first meeting. He was trying to determine with mounting frustration how many molecules of mercaptan remained in a flask after many many dilutions. It was no mean task, since the wretched things wanted to latch on to every surface they could find, and not obey neat dilution factors. I admired him then, as I do now, for his ingenuity, discipline, and tenacity. These qualities were to be the hallmark of his career.

In the late fifties, Groningen, De Vries and the Radiocarbon Laboratory were prospering. De Vries had established a national reputation in Holland, and had secured funding from the Dutch Government for research stimulated by the disastrous floods of 1952 which took hundreds of lives. Groningen had also a fine tradition of archaeology, first under the aging Van Giffen who had held the fort during the war, and later under the youthful and energetic Waterbolk. It had been Van Giffen who had given De Vries the first impetus to make his "machine". The Groningen Laboratory was thus in the right setting and with the right track record to receive what meager support was available at the time. This support enabled the laboratory to be moved in February of 1954 into new single story facilities which in those days seemed indescribably modern. De Vries was pursuing some new ideas with newer low-background counters, and later was to induce Minze Stuiver to join him in the expanded endeavor on conclusion of his thesis work—a perfect choice.

In 1958 there occurred a discovery which promised to rock the foundations of radiocarbon dating. Henrik Tauber of Copenhagen, myself, and De Vries were in Hamburg at a conference on pre- and protohistoric science. Karl Otto Munnich of Heidelberg had had some suspicions that some dates from tree rings of known age from an oak from the Spessart Forest were not in line with their dendrochronology. He sent the specimens to De Vries, who repeated the measurements, and with his greater counting precision extended them to other well-dated material. His first results seemed to suggest the unthinkable—radiocarbon years and sidereal years were not a one-to-one match over the last few hundred years. This was a seeming blow to the integrity of a method which had been touted as an absolute chronological tool. The Hamburg conference was to have been his opportunity to present his case publicly for the first time, and was thus of some importance. De Vries was easily bored by conferences, and a day not spent in the laboratory was a day wasted—he wanted to go back home before it ended, and quite unexpectedly asked me if I would give his paper for him. Since I had recently spent a fair bit of time editing it for him, I agreed, but not without some trepidation. Henrik and I did a lot of head-scratching over the next few days on how best to make the presentation. Based on this paper, Henrik and I, joined by Karl Otto Munnich, performed a three-way experiment with De Vries' enthusiastic blessing, using tree rings at fifty year intervals from a giant sequoia going back 1300 years. Each sample was measured independently by two out of the three laboratories. They confirmed De Vries' findings, and the results were presented at the Radiocarbon Conference in Groningen in 1959, and published in one of the early numbers of what was to become *Radiocarbon* (*American Journal of Science Radiocarbon Supplement*, Vol. 2, p. 1, 1960). Thus began a more intense effort, so ably refined by others with greater precision than we could muster, using the bristlecone pine to produce a reliable calibration of radiocarbon years with sidereal years. It is an irony of progress that these corrections are now taken for granted as part of the technique.

The Groningen Laboratory continued to prosper over the years ahead under Vogel and then Mook, but I draw these recollections to a close with De Vries' tragic and untimely death in December of 1959. This was a blow to us all working in the field at the time, but especially to those in Groningen not least of whom was Minze. His patient work under De Vries' tutelage should be recognized for the singular contribution that it was, only serving to reinforce the luster of his later achievements. In his indefatigable way Minze moved on to those other challenges which I shall leave to others to describe. But I shall always remember Minze admiringly for those early years when we shared so much and when a "brave new world" was no cliche—it was real.

PAUL DAMON

Reminiscence of Minze in the Early Days of ^{14}C and Solar Cycles

We first met when you came to Yale as a Postdoctoral Research Fellow to set up the Yale radiocarbon laboratory in 1959. I was there to visit Karl Turekian. Then you and Anneke visited us during your spring(?) vacation in 1960. Anneke was pregnant with Ingrid. You were soon to publish your paper, using De Vries' electrical analog computer model of the carbon cycle, demonstrating for the first time the correlation of ^{14}C variations and the sunspot cycle (*JGR* 1961; *Science* 1965). We had just built a proportional counter laboratory and we were in the process of independently confirming the *ca.* 1.5% increase in atmospheric $\Delta^{14}C$, which we renamed the De Vries effect (*Radiocarbon* 1962).

During your visit, we visited Andrew Ellicott Douglas, who was then 93 and fast declining, but with occasional flashes of energy and interest. Although dendrochronology and dendroclimatology had been so successful and took so much of his time, he had never lost interest in solar cycles. Our visit, telling him about our new approach to tree rings and solar cycles, seemed to evoke one of those

flashes of interest and energy. His wife said that he had been depressed but obviously perked up and showed keen interest in the conversation. She thanked us for our visit and remarked that he had got a little tired of only female company.

The GEOSECS program that helped build your laboratory absorbed your attention for a while. This was followed by high-precision calibration of the radiocarbon time scale in which you took the lead beginning about 1962, but you got back to solar cycles during the last two decades with outstanding contributions. I have enjoyed your work and your friendship. Best wishes for a happy, healthy and productive retirement with much more leisure time than you have allowed yourself during many years of hard and productive work.

REIDAR NYDAL

At an early stage in my childhood I regarded retirement as a kind of illness. In the small village where I lived there where only a couple of persons who had an official employment with a regular income. When some of them retired, they were normally observed slowly walking on the road or resting in a chair. Such sudden difference was generally not observed for other people on the small farms, who regularly were growing potatoes and fishing as usual all the time.

In your case, Minze, you have already done more than growing potatoes and fishing, and will certainly make further scientific contributions. Your activity has been successful in a large number of main topics in the field of radiocarbon, including counting technique, natural ^{14}C variation, calibration, and the study of ocean circulation by participation in the WOCE and GEOSECS programs for the Atlantic, Pacific and Indian Oceans. Your most important contribution is probably the high-precision calibration curve for radiocarbon dating, also in collaboration with Gordon Pearson, Belfast, and Henry Polach, Canberra. The whole radiocarbon community relies on this curve and also follows your advice in reporting data.

I believe that scientists in the future will mainly appreciate you for your scientific contribution, as you have not announced much of your other qualities. You have certainly been aware early that scientific activity and success are not enough. With increasing age we are gradually more focused on your friendship and kindness which have blessed us for more than 30 years. It has always been nice to meet you, but we should have had more time. Finally, I wish you and your family all the best for your future and hope to see you again in radiocarbon or private connection.

WALLY BROECKER

Minze Stuiver will always be remembered as "Dr. Radiocarbon." He not only set a standard for excellence of measurements, but he also delved into all the important aspects of radiocarbon science, calibration, ocean circulation, very old samples, sunspots...

In 1984 at Zurich when asked what would happen to the beta counting labs in light of the emerging accelerator method, I made the seat-of-the-pants prediction that they would likely be retired along with their mentors. Now, this prediction is becoming a reality. An era which lasted almost 50 years is coming to an end. Sad, yes in a way, but for those who developed the radiocarbon method over these five decades perhaps, it is a fitting honor. The great research done with these old war-horse counting cylinders will stand as a permanent symbol of those who slugged it out making sure the gas was pure and the radon was gone. Minze will surely stand out as the best of them all.

I remember well the first meeting of ^{14}C scientists of my generation. It was in Groningen in 1959. It was there that I became acquainted with K. O. Munnich, Eric Willis, Heinrich Tauber, and, of

course, Minze. There was also a Russian whose name I have long ago forgotten. We called him Shutka because of his broad smile and wonderful laugh. It's been many moons since those early days. Minze has remained a close friend and colleague. I wish him well in his retirement.

PAULA REIMER

Minze Stuiver has been a guiding light in science for those who know him and his work. His intuition is phenomenal, often predicting the results of much more complicated approaches. He never fails to go to the root of a problem. He has stubbornly remained ethical in all aspects of life. Those of us who were lucky enough to work with Minze have, hopefully, learned some of the more important lessons in life.

PIETER GROOTES

I first met Minze in Groningen, The Netherlands in 1975, during my Ph.D. study. He visited to compare notes on thermal diffusion enrichment of ^{14}C as a way to extend the ^{14}C dating range to 70,000 years and beyond. When, after finishing my Ph.D., I asked Minze whether he might have a postdoc opportunity, he answered with characteristic brevity that a one-year stay would not be a problem, but that I should not count on anything more. Yet one-year visits to the States may last a long time, as Minze demonstrated by going for such a visit to Yale in 1959! The Seattle thermal diffusion effort was successful and produced some of the oldest ^{14}C dates in the world (>70,000 BP).

Over the years Minze has had a lively interest in trees, which was decidedly unhealthy for the trees. Along the West Coast he collected sections of old-growth trees to create a tree-ring calibration for the last few thousand years independent of the bristlecone pines. After a field trip the lab would look like a wood workshop. Later, those trips extended farther afield from Kodiak Island to Patagonia and focussed on poor lonely trees from windy places. In the lab these told their story about their youth and their response to location and weather and to a slow change of CO_2 in the atmosphere. Minze's interaction with trees can also be seen around his home(s) in Bellevue and later Lopez Island. Minze and Anneke love the quiet of the forest, yet in his free time, Minze set to work to cut trees for a clearing to make a nice Dutch vegetable garden.

Visits to Minze's lab were common, and many visitors stayed at Minze's home in Bellevue. Yet quite a few he managed to confuse about the exact location of both home and lab by using, whenever possible, a different route between them every time.

Minze's long and prominent involvement in radiocarbon dating led at the Seattle Radiocarbon Conference to the pronouncement by a colleague that to be really "in" in radiocarbon dating one would have to learn Dutch.

Over these 17 years of collaboration, Minze has always been an innovative critical scientist, a great devil's advocate to test any plan or theory on, but also a generous and great colleague to work with.