February 2017

Hartmut Heinrich – The unknown world-famous climate researcher of Hamburg

An interview by Hans von Storch and Kay Emeis
with a preface by Wally Broeker

Preface

When you talk with climate researchers about **Heinrich Events**, they usually have at least some idea what they mean. Only few, however, know who is concealed in the name “Heinrich”. Many think of a discovery from the early twentieth century, or perhaps even earlier, and that this Heinrich person here hasn't been active for quite a long time. This is certainly not the case—this Heinrich is an active individual living and working in Hamburg. Who is this person who, on the one hand, is so famous that an important geological event has been named after him, but who has remained rather unknown within his own working environment? This interview aims at commemorating this individual in Hamburg and Germany for his scientific discovery and interpretation, a recognition that internationally exists since long time.

It is also an attempt to convey this recognition to Hamburg’s “climate community” so that it can reflect on the scientific achievements arising from its own midst. Science is rightly part of our culture because scientific insights help better orient society in a complex world. This gain in scientific knowledge contributes to quality of life, as it permits to deal with positive as well as negative prospects. The acquisition of knowledge through the work of Heinrich has enabled to recognize and assess new and interesting—even exciting—perspectives in climate dynamics and in climate change. These insights have actually penetrated far into our society, right down to popular entertainment – it brings to mind the film *The Day After Tomorrow*.

Hartmut Heinrich’s research is what one calls “small science” today; it did not arise from deep speculation during important committee meetings addressing the main challenges of the future. Hartmut Heinrich stumbled on a detail that made him curious. It is science in its purest form. It is clearly in contrast with what we refer to today as "big science", in which billions are set out to address predetermined questions. A scientist is, first and foremost, successful in “big science” if he or she convinces bureaucrats to invest a great deal of money in scientific endeavors; but in small science, the insights gained occupy the forefront.

This interview makes something else clear that basic research (whatever that may be) is not deeper or more challenging and therefore not more valuable than “applied" research (whatever that may be). That apart from questions relating to Atlantic circulation stability, the issue of the fate of dredged material in the Hamburg Harbor is also important.

February 23rd, 2017, Hans von Storch and Kay Emeis

Preface

In 1989 I came across a paper sole-authored by Hartmut Heinrich. It astounded me. He identified six debris layers in a core from the eastern north Atlantic. This core was taken as part of a program designed to study the stability of sediments onto which low-level radioactive waste was being dumped. The presence of those unusual layers in normal foraminifer ooze was apparently a cause for concern. Hartmut set out to determine their origin and in doing so he got it 99 percent correct. He proposed that these layers were formed by the melting of armadas of icebergs which drifted across the Atlantic from the Hudson Straits lobe of the North American Laurentide ice sheet. His student Rüdiger Jantschik confirmed this by showing that the ^{40}K-^{40}Ar ages for the debris layers were Archean while that for the normal sediment was Paleozoic. The sharp bases and the absence of foraminifera shells indicated to him that these layers were rapidly deposited.

With no success I tried to interest John Imbrie's 'Specmap' group in these layers. But as they were obsessed with Milankovitch cycles they didn't want any discord in their planetary symphony. As part of the previous CLIMAP program Ruddiman and others had studied cores from the northern Atlantic. Blinded by Milankovitch they put aside the layers free of foraminifera as times when it was too cold for forams to survive. My only ally in this was George Denton. The late Gerard Bond, by chance, came across a deep-sea drilling core from the northern Atlantic which had strange white layers. When he showed me the photo I got excited and said, "Those are the layers found by Heinrich!!" Indeed, they were. Working together we duplicated Hartmut's record.

This led to a meeting on these layers held at Lamont. It featured Hartmut and led to my putting his name on these layers. But they remained largely curiosities until abrupt hydrologic changes in the tropics were shown to have occurred close to the times of these ice armadas. It appears that winter freeze-ups of the northern Atlantic associated with slowdowns of deep water formation pushed the thermal equator and the tropical rain belts to the south. So Heinrich events became a very big part of abrupt climate change research.

I reunited with Hartmut only a handful of times over the years. During this period I was introduced to his baby daughter and then some years later, I saw her again as a beautiful young lady. At one point Hartmut told me that at a reception he attended someone pointed to his name tag and said, "You have the same name as the famous Heinrich." Hartmut said that he blushed and admitted,

"Alas, I am that man."

It is unfortunate for those of us in paleoclimate research that Hartmut opted for an alternate career. Had he not done so I'm sure he would have astounded us with other discoveries. However, he can take great pride that he not only found these debris layers but that he also figured out how they formed and how they might have impacted ocean circulation. Perhaps in his retirement Hartmut will spend time pondering as yet unsolved mysteries regarding 'his' layers!

Wally Broecker, January 2017

Can you explain to us what Heinrich events are and their significance?

Heinrich events were catastrophic collapses of continental ice sheets during the last glacial period. At that time, vast quantities of icebergs more or less periodically calved into the North Atlantic, causing an interruption of the oceanic heat conveyor belt, a rapid increase in sea level and, dramatically changed the climate on a global scale for a period of several hundreds of years. They're not only an interesting paleo climatological phenomenon but also a perfect stratigraphic and diagnostic tool for climate questions.

What do you mean by tool?

Sediment layers are usually deposited over an extended period and, often the deposition areas migrate in space. If sea level rises, for example, beach sands will increasingly be deposited further inland over the course of time. We say that such beach sands are diachronic. A large problem is therefore finding sediment forming a time-stamp meaning that has been deposited within a short time span, such as ash from volcanic eruptions do; we call this synchronic. The ice drift events and their spatially extensive climatic effects are global synchronous time-stamps, as they occurred during a -in geological terms- relatively short time span. If I can now temporally assign certain geological phenomena to the ice drift events, it is then possible to connect their genesis with the ice sheet collapses and to reconstruct their climatic conditions of origin. I received a letter in the early nineties from a Chinese geologist. He wrote that fragmentary knowledge about the Weichselian glaciation in China could now be knitted together into a consistent spatial climate history. That has since been applied worldwide.

You were the one who discovered this. When was that? And how and where did it happen?

In the mid-eighties, the German Hydrographic Institute (today known as the German Maritime and Hydrographic Agency) carried out an environmental impact assessment on the deposition of intermediate to high-level radioactive waste in the Iberian deep sea. The study was primarily oceanographic and radiochemical in nature. Geologically, a very accurate bathymetric survey of the area was to be carried out as well as an assessment of the characteristics of the topmost sediment layers.

A pensive Hartmut Heinrich gazes at a sediment core. Circa 1985

You then went to sea.

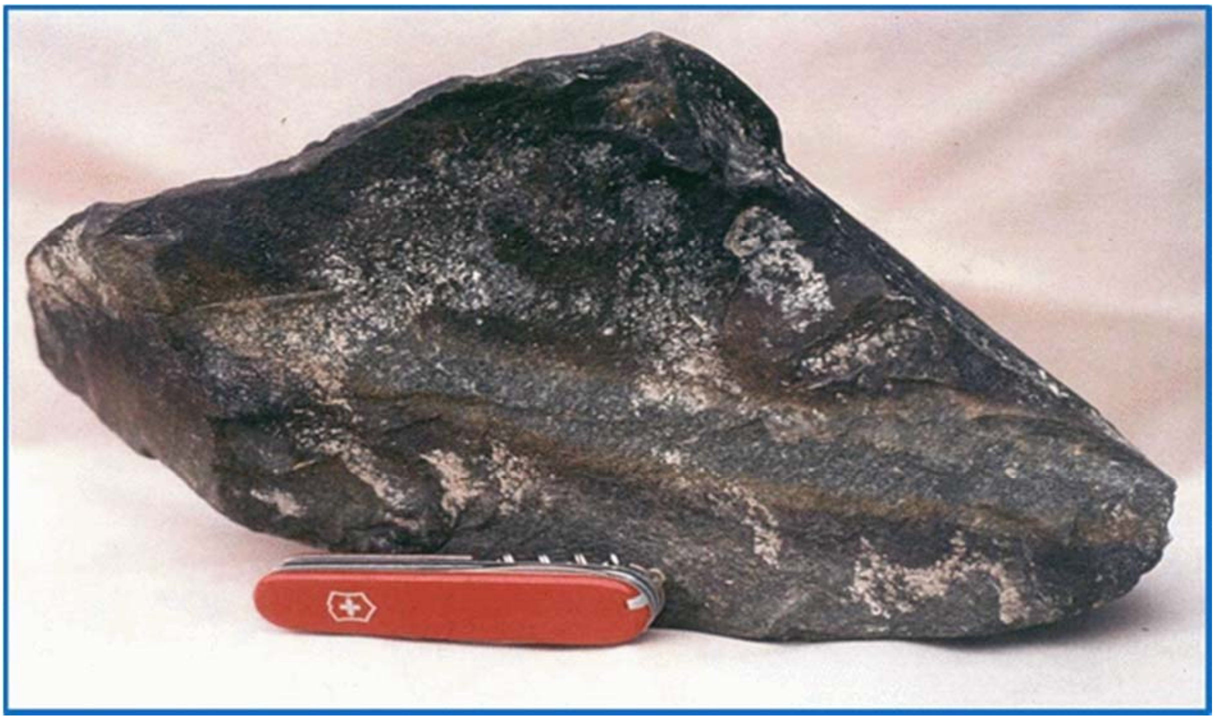
Right. The assessment region lay in the eastern foothills of the Mid-Atlantic Ridge possessing a lively relief of ridges running north-south and valleys running east-west, and with water depths ranging from 3700 to 4500 meters. It *soon became apparent that the valleys are episodically traversed by powerful turbidity currents of Icelandic origin, which can cut into the seabed up to several tens of meters. This could cause a risk to nuclear waste stored in the seabed if releasing radioactive substances into the seawater. This result contributed to the fact that the idea of radioactive waste disposal in the sea was wasted.*

For radiochemical studies box corer samples were taken from the upper fifty centimeters of the seabed. A chunk of basalt measuring twenty centimeters was randomly found in one of the box corers on board. It immediately aroused my interest because it looked unusual. It was a glacigenically faceted Icelandic basalt.

Why did you immediately take notice of the stone?

The upper and lower parts were covered with black manganese oxide and in the middle was a bright strip two to three centimeters in width; above and below was a band of reddish brown iron oxide. This is a typical phenomenon that one observes on stones that are partially embedded in oxygen-free (anoxic) sediments and partially protrude into oxygen-containing (oxic) bottom water. The bright stripe therefore should represent a layer of oxygen-free (anoxic) sediment. The sediment layer in this corer, which bordered against this bright strip, differed from the under and overlying brown sediment due to its very bright color. This whole situation surprised me because I wasn't aware of anoxic sediments from the deep Ice Age Atlantic.

And then I pursued the mystery



The stone

What did you discover?

My suspicion was confirmed in the laboratory; we were dealing with an anoxic sediment. However, it was surprising that the content of organic carbon, which normally leads to oxygen-free conditions in the sediment was only half as high in this "ominous" sediment layer as in the over and underlying layers of oxic sediment. My conclusion was that northeastern Atlantic bottom water must have been anoxic during the deposition of this layer. There was nothing to indicate such a thing in the literature though.

The petrographic analysis of the bright sediment layer told me that the sand fraction in this layer consisted nearly exclusively of crystal-clear, sharp-edged quartz grains and that the few foraminifera found belonged to a species that today only exists north of the polar front. In addition, the layer was somewhat diagenetically solidified by a mineral that often forms in anoxic situations, namely dolomite.

Obviously, this stone sunk from a thawing iceberg, which likely had drifted from Iceland to the site where we discovered it. Sometime later, the deposition of the "ominous" layer then took place within a relatively short period, followed again by the "normal" ice age conditions in the northeastern Atlantic as before.

You then used further findings for interpretation.

I then went searching for more layers in the sediment cores and was successful. In what appeared to be the main Weichselian glacial period, there were six of them, each measuring a few centimeters. A further five layers were found in the period leading up to the preceding glacial, but they were markedly weaker. Thanks to additional resources from the BMFT, I had been able to arrange for stable oxygen and carbon isotopes measurements to be taken in foraminifera. The oxygen isotope ratios in the polar foraminifera showed that the surface water temperature must have been very low and clearly the salinity was considerably reduced.

Could you determine the ages of the events and, where did the sediment come from?

With the help of certain types of foraminifera, which usually are found in defined water temperature conditions, a rough temporal classification of the eleven layers could be made. Because there were no resources available for dating the layers, I tried it with Milankovitch and his astronomical cycles. From the end of the Saalian glaciation till the end of the Weichselian glaciation there were eleven cold phases caused by the alternating summer and winter radiation minima in the Northern Hemisphere, controlled by the precession of the Earth's axis. I therefore concluded that the layers could be ten to eleven thousand years apart. After the first carbon-14 datings from the Weichselian Pleniglacial were available it turned out, at least in this period, that the ages were somewhat different.

To figure something about the origin of the sediment in the ice drift layers, I carried out an analysis of the clay fraction using X-ray diffraction. These showed that layers three and six, counting from above, contained smectite, a mineral that arises from basalt weathering. Their origin, therefore, must have been Iceland or further north. The sediment of the other layers likely were from Greenland or North America. My doctoral student Rüdiger Jantschik, who wrote his dissertation on the material in Neuchâtel (Switzerland), confirmed the sources.

And what have you concluded from this?

A model for the origin of the sediment layers then emerged. Collapsing ice sheets around the North Atlantic suddenly discharged enormous amounts of ice. The "normal" thermohaline circulation that leads to the formation of oxygenated bottom water in the North Atlantic was likely interrupted by the freshened surface layer. "Old" Antarctic bottom water could therefore penetrate farther north along the North Atlantic seafloor, which was low in oxygen (layers three and six) and even anoxic at times (layers one, two, four and five). Worth *noting was that directly beneath the layers, on occasion, planktonic foraminifera were found that indicated somewhat warmer water shortly before an ice drift event.*

This was a new concept.

There was absolutely nothing on the books regarding the matter. These horizons popped up occasionally and sporadically in publications on North Atlantic glacial sediments but no attention was given to them. The reason for these occasional mentions was very simple: the overly sparse sampling intervals. The layers that were only a few centimeters thick virtually slipped through the fingers of those who analyzed the samples at that time; for me, however, this didn't happen because I had sampled in very narrow spaces.

Had such hypotheses already been speculatively expressed at the time or did your interpretation contradict the usual concept?

The occurrence of rapid climate change during the glacial period was already assumed due to the Dansgaard-Oeschger cycles, which had been identified in ice cores. Furthermore, Scandinavian archaeological finds also indicated that the ice age was likely more dynamic than expected.

You then published your findings – when was that? And how did the peer reviewers and publishers react?

There was nothing left to do but publish. I decided on Quaternary Research in 1988. The journal seemed thematically suitable. One of the peer reviewers was Sir Nicolas Shackleton. He found the article interesting and the English somewhat "special". The other one was Bill Ruddiman. We had an intense exchange via airmail, but at some point he couldn't come up with any more counterarguments. When I met him some time later, he said the work had shocked him. He himself had been working extensively for far more than ten years on glacial sediments from the North Atlantic and was forced to admit he hadn't recognized something extremely important.

As someone who introduced a new perspective in geologic history, you're part of tradition like Alfred Wegener. For him, his explanation of continental drift didn't initially go so well. How was it for you – with the acknowledgment that you had discovered and correctly interpreted an important element of geologic history?

Well, I at least still haven't been kicked out of any geoscience society. Wally Broecker once said that I was geology's revenge for Alfred Wegener. Gerard Bond, a colleague of Wally Broecker, had received the American Science Award because he proved I was right.

How did it go with the name? In the US, England and France, the name was quickly accepted without issue, but this acceptance came only much later in Germany.

In 1989—that is, one year after the publication in QR—I discovered one of my graphics in a Canadian publication. The author was making fun of the fact that now someone else was raising Milankovitch again. That spring was when I came into contact with Wally Broecker (LDEO). He wanted to know if perhaps these layers could be turbidite layers, but that was impossible. He visited me on September 24th, 1991, in Hamburg, the day before my daughter was born. At the Kiel Paleoceanography Conference in the autumn of 1992, the Americans, British and French showed up with more than a dozen presentations on what they called Heinrich events. It took a long time before people became "accustomed" to the term in Germany. Terrestrial geology is obviously still somewhat reluctant in in this country.

Do you call them Heinrich events yourself?

Sure, of course. A Heinrich event, in German known as a "Heinrich Ereignis", is an internationally established term. The term Heinrich event was first mentioned in a publication by Broecker et al. in 1992. The one who coined the term was Jerry McManus from LDEO. He was inspired by the medical term "Heimlich maneu*ver". This is a special grip used when a foreign object is lodged in the airways. The object is ejected when you cough. In addition to Heinrich events, we now also have the scientific terms Heinrich layers and Heinrich stadials. An American climate blogger had even gone so far as to use the term Heinrich monster—due to the fear that climate warming could trigger a Heinrich event. There's even a British artists' group called the Heinrich Event because the phenomenon had made such a strong impression on them.*

How did those in your own life react to your achievement?

The reaction of colleagues in my agency? There was almost none. This was surely because they weren't working in my field. The topic also doesn't fall under the purview of the BSH. My wife was naturally proud and my daughter somewhat timid when the topic came up during a class; she studied environmental sciences.

And how did the geology community at German universities and establishments such as the GFZ react?

If you mean in the form of medals or prizes from the academic establishment, then there's been nothing like that so far. On the contrary, I've even been maligned. I had allegedly stolen the samples "in a cloak and dagger operation" from a contractor's sample cellar. There was, however, no cloak and dagger involved. We had given parallel samples to a university institute for certain tests. These analyses had nothing to do with the events. It was probably jealousy.

How did the "top dogs" react to this development?

One of the top dogs alleged that he had made the discovery himself, which no one believed. I recently found out that he has been trying for fifteen years to prove that there was no substance to it. Otherwise, the contribution has largely been ignored in Germany. It took off like wildfire abroad though. There was a great deal of international approval; at the time, this all occurred through airmail. The events are a worldwide component of geoscience curricula today and many students are surprised when they find out I'm still alive.

Let's talk about recent climate change. Are these events merely a paleoclimatic detail or are they a climate aspect relevant in the current discussion about climate change and climate policy?

Current anthropogenic climate warming has the potential to trigger a Heinrich event or something similar. Warmed ocean water melts the contact between ice and stone (the grounding line) on the shelves of Greenland and the Antarctic so that the buttressing effect is weakened, and could even fail. It is then like pulling a cork from a bottle, after which the contents are released unhindered, rapidly flowing out. In addition, melt water flowing through the glacier increases slippage at its base. There are increasing signs in both Greenland and in the Antarctic that large glaciers are not only increasing in speed due to this effect but also that their massive ice release can no longer be halted. Such reports are accumulating. The effect could work for a long period of time because seawater has an exceptionally large heat capacity. The probability that the sea level will rise considerably more than predicted by the IPCC by the end of the 21st century seems very high. It will also probably continue to rise for centuries.

Did the discovery lead to a paradigm shift only in marine geology and paleoclimatology, or in our understanding of the dynamics of the coupled Earth system? How were the concepts before your discovery – and how were they after?

The term paradigm change is perhaps not completely applicable. In any case, the discovery provided new details and brought some order globally to the sequence of events during the last ice age as well as to understanding connections of climate phenomena, their causes and their spread. It should be emphasized that freshwater input into the North Atlantic is responsible for weakening the global heat conveyor belt and that ice sheets are much more dynamic than previously assumed. The American ice physicist Douglas McAyel wrote me at the time that there was decreasing research support in the late eighties in the US for ice physics due to the assumption that everything about the behavior of ice sheets was already known. "And then someone came along and wrote that everything was completely different. It was a miracle for our science." I think that the current interest in the Antarctic and in Greenland regarding climate warming was promoted by the discovery because of the comparable situation with the HEs and the possible effects on sea level rise. There are, by the way, also other scientific branches that have been influenced by the existence of Heinrich events, such as anthropology. This rapid climate change during the last ice age obviously had a decisive impact on the genetic development from Homo sapiens to Homo sapiens sapiens as well as on their spread to Asia and Europe. We here in Europe are all ancestors of African climate refugees, so to speak. History is likely to repeat itself.

Your name is known worldwide today. And you are telling its story.

In 2015 I was persuaded by the Dresden geographer Dominik Faust to tell the story of the discovery at an Iberian geography conference in Seville. Later I received some more invitations: to the Environmental Physics Department in Heidelberg, in 2016 to the Freie Universität Meteorology Department in *Berlin and in September 2016 to the DEUQUA, which is mostly visited by geographers.*

Would you have thought that your name would one day become world-famous?

Oh, no, I wouldn't have. I initially completed junior high school with a Secondary Technical School certificate, subsequently barely managed to finish high school (Gymnasium) with a diploma. During my work at the Federal Border Police in Duderstadt, I began studying geology in Göttingen, where I discovered my love of limestone and climate. The idea to study geology was an extremely fortunate coincidence. It just fell from the sky—right in front of the career advisor's door at the high school in Herzberg am Harz. For me, geology was and is the most exciting and multi-faceted science.

The young Hartmut Heinrich in the port of Lisboa in 1985, participating at the NOAMP project

There's a rumor circulating that your discovery and interpretation was part of your doctoral dissertation in Kiel. This is incorrect. You earned your PhD in Kiel (under Prof. Seibold), but in a topic that had nothing to do with Heinrich events. The work on the Heinrich events arose later in Hamburg during your time at the BSH, known as the DHI at that time.

After I earned my master's degree (on the Mesozoic *of the Northern Göttingen Forest*), *I transferred my attention for my doctorate to marine geology*. *My dissertation advisor was Prof. Eugen Seibold. The topic I undertook was a biofacies analysis of a Bermuda lagoon (Harrington Sound) and its development since the last glacial period. I became acquainted with the entire spectrum from pore water chemistry, subtropical fresh and saltwater fauna and flora, to isotope analysis. And I dived a bit for a friend who was working on manganese nodules in the Baltic Sea, hence my experience in anoxic sediments.*

Where did you start working after you finished your university studies?

I first worked through an employment creation scheme at what was then the Schleswig-Holstein State Office for Geology, where I explored sand and gravel in Segeberg County. That lasted six months, until September 1983. Then I transferred through the employment agency to the DHI, known today as the German Maritime and Hydrographic Agency (BSH). After the NOAMP project, when I happened to make the Heinrich event discovery, I went on an odyssey at the BSH through the geology of the North Sea, pollutant geochemistry, environmental management in national and international marine protection, biology within the framework of the offshore wind energy authorization process, then from November 2006, taking over the "Physics of the Seas" unit. Now my focus lies in physical ocean observation, such as in the United Nations Argo program, and recently in climate change adaptation. The climate issue has become full circle for me. At the beginning of my professional life, I investigated paleo climate, and at the end of my professional career, which will be in September 2017, I'm looking at climate of the future. In between, I'd taken an exciting tour through the breadth of marine sciences.

You then also carried out practical management tasks such as about the EU Water Framework Directive. Is that right?

In the nineties, I was coordinator of German Marine Environmental Monitoring. I had been delegated as the German representative by the German Environmental Ministry to the European group for implementing the EU Water Framework Directive. I presided over this group with a British colleague. One of my tasks consisted of developing a classification of European coastal habitats as the foundation for an ecological evaluation. The biological experts had proposed a classification with more than one hundred species-based habitat types, and for each, a five-step evaluation would have been necessary, which was technically unfeasible. I was then struck with the idea to use the ecological function as the basis of the classification system. For example, surf zones are generally populated by hard-shell mollusks, from the North Cape to Greece. This means that all European coasts could fit into a system of a good twenty habitat types. I was familiar with the topic of ecological function through the work on my dissertation.

A by-product of my work in the Water Framework Directive (WFD) was suggesting to the Hamburg Port Authority to deposit only weakly contaminated harbor silt at the mouth of the Elbe, namely where the Elbe itself unloads its sediment. I believe they were very happy about that suggestion at the time. Hamburg should also make the Elbe residents more intensely aware that, as those who profit from the harbor, they should take a special interest in its competitive position by strong implementation of the WFD.

Your reinterpretation of the role of the Atlantic in climate events reminds us somehow a little of Einstein's early life as a young civil servant who made grand suggestions, ones that were greater and more important than those of prominent scientists at universities. So, we have a few questions about the relationship between universities and government bodies.

The comparison with Einstein is daring.

Yes, that's true, but we want to switch the topic to the relationship between governmental research and academic research: How did you feel about research within the framework of government bodies and independent research in the context mainly of universities? How is scientific practice different in such varying institutions?

I can only describe my experience at the BSH. Until about 2007, governmental research was viewed suspiciously by universities as a competitor for funding. This changed suddenly at the BSH's Marine Research Department as climate modeling increasingly demanded systematic ocean observation data, which the universities couldn't produce because it is too expensive. The global Argo program as well as other climate projects at universities are examples. A state institution is, however, in the position to undertake this work because regular observations are what we do. We therefore became interesting for this community and symbioses formed over the years. We have since also been working on issues in the applied sciences that arise from our endeavors, such as on climate change and adaptations to climate change.

You have surely often seen that government bodies have assigned research projects to universities and research institutes such as the Helmholtz Association. Have the universities understood the issues put forth by the government bodies and have they worked toward those issues? Furthermore, were the results provided by the universities of use to the governmental agencies? Are they speaking the same language?

The universities and government agencies have, speaking diplomatically, sometimes different methods of working and aims. We have discovered that regular work meetings are beneficial to both sides and to the matter at hand. Initially, we needed to guide university colleagues more concretely. We have, in the meantime, been able to work well together.

Just as universities harbor prejudices against governmental research, there are also surely government agency prejudices against universities and research institutes. Can you fill us in?

I can only speak for my working group. Occasionally we sense an attitude of arrogance towards us, but we're above such things. We recognize our own value, both scientifically and monetarily.

Is there a need for improvement or intensification in cooperation between universities and government bodies and non-academic research institutions? What prevents this?

Governmental research is, in principal, applied research. This is often impossible without basic research. Applied research occasionally fails to get the recognition it deserves. But experience shows that both types of research mutually inspire each other. Sometimes a bit more humility would be appropriate at universities. In terms of climate research, university research should focus more on the needs of those who must adapt to climate change. "Bottom up" is somewhat missing in this field. Furthermore, the connection between the natural sciences and the social sciences needs considerable improvement. University research also has a societal responsibility. It is, after all, financed by society.

Are there concrete topics in connection to climate research?

I have the feeling that improvement of climate models needs to be undertaken more extensively. I am skeptical that uncertainty reduction in climate projections is really advanced by using the most refined statistics. A successful example for me is the recent development in ice modeling that can better deal with the conditions in Greenland and the Antarctic. Here it's about a pressing need for improved sea level rise projections. This is a topic of extraordinary importance for the planet.

You're professor at the University of Hamburg, right?

I am not a professor at any university. In my position as head of a scientifically active organizational unit, a unit within a government agency, I possess the title of "Director and Professor". My work differs little from that of colleagues with the academic title of "professor". It is, perhaps, not as research-intensive.

Climate research in Hamburg sets high standards for itself: to be a – or perhaps be the – center of German climate research. What is your opinion of the Hamburg location, which is expressed mainly through its KlimaCampus?

I think Hamburg is at the forefront. The KlimaCampus is a meaningful establishment because it also integrates non-university institutions that work on the topic of weather and climate and thus benefits from their insights. I hope things will continue this way. It would be ideal if climate research in Hamburg would be heavily supplemented by climate impact research to make it a well-rounded institution.

You're involved in CLISAP. How did the BSH and its associated ministry react to that?

My unit is actively involved in the sea level subproject. This strongly affects our official interests. We can also bring a great deal of our own expertise. In this regard, our participation is welcomed.

In addition to Klaus Hasselmann and Vladimir Köppen, you belong to a group of scientists who have provided something lasting in terms of insights into climate dynamics. Do you get the impression that this has been recognized, for example, in CLISAP? Are people in Hamburg proud to know you here?

No one has communicated that to me yet.

Maybe we'll do that here then. What role and responsibility do you see for yourself and for science to advise society on how to deal with climate change caused by humankind?

I fear that with the warming climate a Heinrich event could potentially be triggered in Greenland and the Antarctic. If I look at the current literature at the turn of 2016/2017, I could see the experiment to trigger the event could succeed—to put it sarcastically. It already seems sufficiently warm in the ocean to trigger such a thing. The consequences will, indeed, be dramatic. In any case, we need to reduce the CO_2 emissions as quickly as possible and think of how we can deal with the topic of climate refugees. Unfortunately, the population is exploding in the most vulnerable regions. There will be climate refugees within Germany as well, on our flat coasts in Schleswig-Holstein, for example, when the sea level rises considerably more than predicted, which looks likely. We must not forget that even if it is possible to reduce greenhouse gas emissions, CO_2 content will continue to rise and have an impact for many centuries.

When you retire in a few months, what will you do then? Will you continue your climate research in Hamburg?

You need to ask climate research that question. I have not been an active researcher in recent years, but rather a scientific provider of ideas and advice as well as a research manager. I don't know what I'll do yet. There are so many *interesting things to do. Every ten years or so I started something new. Another ten years are now up again.*

And what will you discover then?

A Danish colleague once said that if you take Hartmut on board, then expect the world to look different afterwards. And Wally Broecker thought I had the ability to see things and connections that others do not see. Heinz Glindemann, a former department head at the HPA, even went so far as to say that I could not only look past my own nose, but that I could even see beyond the horizon. There's a bit of truth surely to it when I think back on my professional life. So, we'll see what the future brings.

In front of the Vatnajökull, the source of the turbidity currents studied in the NOAMP project.

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Concluding Remark

This interview was carried out in writing, from November 2016 to January 2017. A group of friendly assistants have supported us in this endeavor, some of whom prefer to remain anonymous.

We thank Anupa Srinavasan for translating the interview from German.