established geophysical institutions: the Voeikov Main Geophysical Observatory (MGO), founded in 1849. Many prominent scientists – both men and women – worked at the MGO, including the well-known Ekaterina Blinova, who proposed methods of long-term weather forecasting (i.e., A Hydrodynamic Theory of Pressure Waves, Temperature Waves, and Centers of Atmospheric Activity, 1943). Moreover, I have not noticed a difference in a research performance or acceptance between men and women.

You have done a lot of work on global and regional climate change, and you are member of the Intergovernmental Panel on Climate Change (IPCC). Do you think that we currently have a solid picture of what is "climate change" versus "climate variability"?

These two terms are very closely related, but vary by time scale and point of reference. There are inherently variations in climate over any given period of time; the longer such variability is observed, the more concretely you can state whether change is occurring and make predictions. The smallest climate variability scale is a decade, whereas the minimum time scale for detecting climate change is over 3 decades. Therefore, I believe we do, indeed, have a solid grasp on the difference between the two terms.

What do you think is the most important achievement of your career?

I believe the recognition of a scientist's work is their most important achievement. By this standard, I consider my most-cited article, "Objective estimation of the probability density function for climate sensitivity", written in 2001, to be one of my important achievements.

You are also an Undergraduate Advisor at the University of Michigan and you mentor many students. If you were to make a comparison, how did their scientific interest changed throughout the years? Are they more knowledgeable about climate issues when they start?

Students usually come to our department with a great interest in and passion for climate science. In recent years, students have been growing increasingly interested in applying their hard-earned knowledge to real life problems.

What advice would you give a young student, who is passionate about atmospheric sciences?

Professors and advisors can only give you the tools you'll need, but keeping curious and motivated will light your way to success. Don't be afraid to ask questions.

Interview with Vicky Slonosky

Hans von Storch

Vicky in 2000.

Vicky Slonosky has graduated from McGill University with a MSc in Atmospheric and Oceanic Sciences where historical observations were a part of her thesis. More historical observations for precipitation in Paris, and the Gaultier observations for Quebec were uncovered during a post-doc at LSCE near Paris, France. A fellowship for Environment Canada allowed her to look for Canadian historical observations, and continue the analysis of circulation started earlier. In 2002 she took up a research position at McGill University and the Ouranos Consortium. The digitization of historical climate observations as a volunteer project has been Vicky's recent interest, along with an interest in the history and philosophy of climatology.

Vicky, please tell us about your way into and through Atmospheric Sciences.

In high school and college, most students with an interest in physics and mathematics were steered towards engineering, so I started at McGill University in Mechanical Engineering. Once there, I discovered I was more interested in pure research and in earth sciences. One professor pointed out a tiny line in the course catalogue which described a Faculty program in Climatology, with courses in all of the departments I was interested in, as well as math and physics. That summer I came across the works of Hubert Lamb, and once I read Climate: Past, Present and Future, studying the historical observations of past climate became what I wanted to spend my life on.

After graduating (when I was seated by myself, between Chemistry and Computer Science!), I spent two years working on sea-ice and atmospheric circulation for a Master's degree in Atmospheric and Oceanic Sciences, still at McGill, before spending three years at CRU on the project ADVICE. I was particularly interested in the reconstruction and analysis of the atmospheric circulation, since that is the starting point for many changes in climate: in the mid-latitudes especially, the climate changes because the circulation changes. The ADVICE project was a great experience, as it involved many researchers from different countries and institutions working on historical documents and historical instrumental data. Just about everywhere I went, I kept a look out for historical weather data. At CRU, I amused my office mates enormously by telephoning county archives and churches' offices, blithely asking if, by any chance, they had any records from, say, 300 years ago? No? How about 250 years ago? During a post-doc in France, I couldn't get into work one day because of a transit strike, so while working at home, I looked up the number of the Observatoire de Paris in my green Michelin tourist guide. It turned out that not only did they have records of precipitation going back to the 1600s, when the royal architects needed to know how much rain was falling around Paris so they could design the fountains of Versailles, but on learning that I was Canadian, the archivist showed me papers from 18th century Quebec. Those papers turned out to be some of the earliest known weather observations from Canada. I returned to Canada, to work for Environment Canada in Toronto, and later held a research position at McGill University and the Québec government-sponsored Ouranos consortium. I never got to go on any field trips to the Arctic or North Atlantic I had dreamed of as an undergrad: my scientific adventures turned out to be looking for treasure among basements full of old furniture, abandoned equipment, and dusty boxes.

In 2004, you were hit by a severe health problem – you got an auto-immune type of arthritis, which affected your hands badly, so that you could neither type nor hold a pen for several years. What did this mean for your professional life – how did your academic environment react and support you?

It meant that my professional life came to an end at the age of 31. This was very disappointing. For several years, I had virtually no contact with the academic community: it's as if I had ceased to exist! However, one colleague, Cary Mock, kept me on his email list and his updates were my only sustained link to the professional world of climatology. I spent several years in occupational therapy but ultimately my health was too unstable to be able to work consistently. This instability is still one of the main difficulties that I struggle to come to terms with. It makes it very difficult to commit to long-term plans such as meetings or conferences. On the other hand, forced inactivity has given me time to reflect and to read in other fields such as philosophy, history of science, anthropology, and cognitive psychology, which in turn has made me reconsider how I think about science.

Another thing that has been difficult about this

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[condition] is the lack of access to professional publications, which are out of the price range of individuals without professional associations.

What would you consider the most two significant achievements in your career?

One piece of work I'm quite proud of was the reconstruction of monthly rainfall in Paris back to 1688. This work, together with some previous analyses from my thesis, made me think that climate variability had changed a great deal over the past centuries. With several centuries of monthly data, I was able to see that there was much higher inter-monthly and inter-seasonal variability to the data in the centuries before the 20th. The 20th century, especially the second half of the 20th century, seemed to have been comparatively quiet but also comparatively smooth, with fewer drastic changes from one season to the next. This kind of change in the character of the variability is something that can only be resolved with daily or monthly data. I think this is related to the atmospheric circulation, particularly the degree of zonal flow, and so it would be interesting to try to investigate these circulation changes, rather than just to focus on the temperature record. The 2000s appear to have been somewhat a reversion to higher values of meridional flow, as were seen before the 20th century, and which have provoked some of the extreme events of the past few years that appear similar to those of previous centuries.

The second piece of work is the ongoing volunteer effort to collect and digitize historical climate data from Canada. After realizing I was unlikely to return to work, I became worried that the boxes of papers and data I collected would end up collecting dust in my attic, and could be lost altogether. Some of the records had been digitized with the support of Bill Hogg and Francis Zwiers at Environment Canada, and Liza Piper at the University of Alberta had scanned in some of the paper copies with the group NiCHE. Even so, I still had thousands of pages and images that needed to be converted to numeric data. While I had been unable to type, I started volunteering for the wiki project LibriVox, which creates audiobooks of out-of-copyright material sourced by the Gutenberg Project. LibriVox became the inspiration for my historical climate typing project, and I remain gratefully amazed at the people who continue to volunteer to type up 200-year-old manuscript weather diaries. It's taking me a while to compile and analyze them (see question 2!), but my aim is to eventually make all these data available on the web, as it was public volunteer participation which digitized them.

When you look back in time, what where the most significant, exciting or surprising developments in atmospheric science?

One of things I've found most surprising is the extremely high profile climatology now has.

Vicky in 2009.

When I first started to study climatology as an undergrad in 1992, the reaction of nearly everyone I knew was "what-ology?", followed by "so are you going predict the weather on the news?", followed by a change in subject. For much of the 20th century, even up to the 1990s, climatology was considered the most boring discipline in science. Today everyone knows what the climate is. It's become a cultural concept, with scarcely a day going by without some mention of climate change in the media. It's also very interesting to see the number of people who are passionately involved in writing or commenting about climate on blogs on the internet. There are many very interesting discussions going on about climate and climate change on the web. I'm mostly a reader of these sites: by the time I get my thoughts together and typed up, the topic has usually moved on or comments closed!

How would you assess the success, or failure, of efforts of bringing females into atmospheric science and of having females in leading positions?

I haven't been part of the professional climate establishment for close on a decade now, so my observations are based on personal stories of friends; for each statement made below I can easily think of counter-examples and exceptions. This is a very difficult issue, which has been discussed in many contexts and over several generations.

I myself have never felt discriminated against for being a woman, and have had very supportive mentors. What I have noticed is that parenthood is where differences start to occur, and this is true in many disciplines, not just atmospheric science. In academia and scientific research, for the more senior positions, the main consideration of merit is the number of publications over the past 5 or 6 years. A woman who has taken a recent maternity leave will have a reduced number of publications compared to what she would have had otherwise, and so be considered as less competitive for senior positions. A possible solution could be to weight the number of publications by the number of months actually worked in the past 5 years to give a better measure of productivity.

Another difficulty is economic: in some places the cost of daycare is so high that it isn't economically feasible to continue working on the salary of grad student or post-doc, which is at the point in life when many people have children. Some fathers who try to take paternity leave or work reduced or flexible hours have also faced discrimination; and mothers report not being taken as seriously once they have more than one or two children. It really seems to depend on the culture. Many jurisdictions are encouraging paternity leave, so the situation may become more equal as fathers share parental leaves and these choices are respected.

Based on the careers of friends and colleagues who studied in the sciences and engineering at university, the private sector, at least in the fields of engineering and software development, appears to be better at retaining talent in both genders and not discriminating against mothers. It might be worthwhile for the academic world to find out how and why women seem to be more successful and promoted to more senior levels in the private sector.

Is there a politicization of climate science?

Yes, certainly. The impulse towards recognizing limits, reducing our impact on the natural world and trying to reduce consumption is a good one and springs from good intentions. We can and should use reason and science to help us evaluate the best way to carry out the actions we undertake for ethical reasons. But science is about evidence, and to use scientific reasons to dictate ethical behavior does a disservice to both. Science is not fixed, but ethics are, or should be. If science becomes the reason which is used to induce ethical behavior, either the science cannot change, as it should be free to do, or ethics change with the science. If, for example, it's discovered one day that massive amounts of CO_2 in the atmosphere are actually beneficial for some reason, that shouldn't mean that greed and excessive consumption become ethically acceptable. It's also very important to examine the potential effects of our good intentions, and try to make sure our proposed solutions don't cause more unintended hardship than the dangers we're trying to avert. This is where the expertise of policy analysts comes in.

From a climatologist's point of view, I find it worrying that the social or political narrative about anthropogenic climate change has outstripped the scientific evidence. This enormous, and to many climatologists, unexpected social interest has placed climatologists in a very difficult position. They recognized and very properly warned society about a potential peril: imagine what the public reaction would have been if they hadn't issued any warnings, and it later came out that scientists had long known about this potential threat but kept quiet about it. But for reasons it would take a political analyst or sociologist to

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thoroughly explore, climate change morphed from a scientific issue about a possible threat (along with countless others which are either ignored or dealt with more or less quietly) to a dominating social narrative of our times. This makes it more difficult to undertake fundamental research into physical climatology and natural climate change and variability; the social message is that we know everything we need to know about climate already, and that anything that happens must be due to humans, so why waste time (and money) on studying the fundamental science or the past? It also makes it difficult to pass on messages of nuance and uncertainty.

I'm also concerned about how this social narrative is putting pressure on children and affecting their view of the world. We must of course teach them how to be good citizens and not to wantonly waste things or pollute their surroundings, but I find it worrisome that children are targeted to feel responsible for, or to change, things that are beyond their control. I find it very discouraging that before children are given a chance to feel wonder and curiosity about the natural world around them or to learn the basics of science, they are inundated with negative views of humanity's influence on the environment. Many museum exhibits, documentaries, and even school texts emphasize the problems that humans are causing. There is an entire wing of the Royal Ontario Museum called "Life in Danger", which causes many children (and adults) to feel anxiety and guilt; my daughter thinks "NOVA" (an American general science TV program) is one of the scariest program on television, and my 7 year-old nephew thinks the weather of the future will kill us all very soon. Giving children nightmares is not the right way to get them to be either good citizens or interested in science.

One aspect which combines questions 5 and 6, and which I haven't seen discussed much, is the impact that higher energy prices related to carbon taxes could have on domestic chores, which in many places are still largely performed by women. One of the factors in the late 20th century participation of women in the workforce was the increased affordability of domestic appliances, such as freezers and washing machines, which eased the burden of many domestic chores. If energy prices become higher as a means to curbing CO2, how will the decreased affordability of running domestic appliances affect women and the work-life balance with which both genders are struggling?

What constitutes "good" science?

My version of good science means on the one hand verifying theories with observations, and on the other, trying to understand what the observations are telling us. In this sense I disagree with Darwin, who said that all observations must be for or against something. Observations can also be independent entities. This is of course a very difficult challenge in climatology, where we don't have the option of running experiments and changing conditions and the variables. This is why I have a lot of respect for modelers; what they're trying to do is not easy and they're always trying to improve their estimations. But in the end, direct observations, flawed though they are, are the only information we have on what has actually happened on our actual planet, and this is why I decided to focus my work on observations rather than modeling.

Science should be discovery, it should be about what we don't know, it should be about trying to see things in different ways, and it should be fun. As Einstein said, "If we knew what we were doing, it wouldn't be called research". Good science should also be humble. What we know is certainly incomplete, and in some cases probably wrong. As science continues, new discoveries, new data, new events and new ways of looking at things will lead to new theories. If we're lucky, we'll have at least been on the right track; if not, our ideas will go the way of phlogiston and ether.

Western thought has a long history of the idea of human induced climate change, going back to shortly after Aristotle. We seem to like to have a narrative, especially one in which we are central, and we easily persuade ourselves that what we want to believe is what is true. Knowing all this, as scientists we have to keep trying to see beyond what we think we know or what we want to see. This is hard, but it's the constant struggle and delight that is science.

What is the subjective element in scientific practice? Does culture matter? What is the role of instinct?

Some histories of science suggest that western science came about in the western world because medieval philosophers in western medieval faiths assumed that the world was mostly reasonable and governed according to fixed laws, rather than by capricious forces common in, for example, the Greek or Babylonian myths. By pushing the limits of reason, they set the stage for using reason to explain the natural world, which eventually led to the development of science. According to this history, science itself only made sense in a certain cultural context: one is which the world was not capricious. Notwithstanding that I'm neither a political analyst nor a sociologist (see question 6!), I wonder sometimes if the anthropogenic climate change narrative is so compelling because it puts us in control, and not at vagaries of random chance (or erratic gods). Every time there is a letter to the editor or opinion on the radio saying something like "If we don't like the kind of weather we're seeing, we'd better get serious about controlling greenhouse gases", it seems to nebulously imply that the weather is within our control: if only we can reduce greenhouse gases, there will no longer be any droughts, floods, storms, or other unpleasant weather events.

It's hard for us to recognize that our surroundings, our peers, the received wisdom in a field of research and our culture do have an influence on our thinking and expectations. Developments in cognitive psychology described in recent books such as Mistakes Were Made (but not by me), by Carol Tavris and Elliot Aronson, The Righteous Mind by Jonathan Haidt, or Future Babble by Dan Gardner make it distressingly evident how easily we all fool ourselves, resolve contradictions and cognitive dissonance by suppressing uncomfortable ideas, and how we are often better at rationalizing than rational thinking. Science is the struggle to get outside ourselves, our biases, our errors in thinking, to try to apprehend the objective world outside ourselves. So there are many elements that are subjective and culturally-based, and the more we can recognize them, the more we can try to take them into account.

Science should be open and reasonably reproducible, but there are many subjective elements in any science dealing with large amounts of data and with measurement, so some aspects of climate science may never be perfectly reproducible. I have a few data points, and a strong correlation, but because the degrees are freedom are so low, it's not significant at the 95% level. Do I throw it away, even though it suggests a possible connection? Conversely, with a large number of data points, almost any correlation can be significant; do I take these seriously? How do I decide which variables are worth investigating for a correlation? Which data are not measuring the intended target, and how do I deal with them? There will always be an element of judgement, and things which are reasonable but which can't be proved. Reason itself and the existence of an objective world can't be proved, so we have to take these assumptions at least as our starting points.

Intuition, in an experienced scientist, is almost like integrated experience; after looking at hundreds of representations of a particular variable, we unconsciously build a picture of what to expect, and can detect an anomaly which might not be evident using automated, statistical screening. It's hard to find a balance between this kind of intuition and reproducibility, which is why details matter, and should be disclosed as much as possible. Intuition can be helpful and reproducibility is highly important, but they're not always compatible: you can't code a hunch.

The opinions expressed in this interview do not necessarily represents those of the reviewer or the AGU.