

Helga Nowotny in Conversation with Elena Esposito

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Abstract

Helga Nowotny, Professor emerita of Science and Technology Studies at ETH Zurich, is a leading scholar in the social studies of science and technology. In her extensive publications she dealt, among other topics, with social and individual structuring of time, technological innovation, uncertainty, social effects of AI, and the interaction between biological life and social life. Always intensely engaged in research policy, Nowotny is one of the founding members of the European Research Council and was its President from 2010 to 2013. In this conversation with Elena Esposito, she talks about her scientific biography, the role of technologies in the experience of time, and the relationship between STS and sociology of science. Drawing on her experience in the organization and funding of science at EU level, she also reflects on the relationship between research and science policy and on the ongoing transformations in the way of doing research and in gender issues.

Keywords: Social time; STS; sociology of science; social impact of algorithms; science policy; gender.

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Elena Esposito: You have had a very diverse scholarly path, from legal studies to sociology, from Vienna to the US and then back to Europe. Has this combination of cultures and academic styles affected your research and approach?

Helga Nowotny: I would call my biography nonlinear. It had unexpected swerves, was uneven and certainly not planned this way. I switched from law to sociology because I went with my husband to New York. I had been an assistant professor for penal law and criminology at the university of Vienna — what to do with this in New York? A position closest to what I had done before was in Philadelphia, but commuting was out of question. From one day to the next, I decided to study sociology at Columbia University with Paul Lazarsfeld and Robert Merton. It was an exciting time with the ongoing war in Vietnam and students protesting. My Ph.D. defense took place in Lazarsfeld's living room as the university building was occupied. When I returned to Vienna I joined the Institute of Advanced Study as head of the Department of Sociology, a postgraduate institution founded by Lazarsfeld and Morgenstern to introduce the quantitative social sciences at their highest level to Austria. Later, I spent a sabbatical year in Cambridge, UK, hosted at King's College. Tony Giddens asked me to be a tutor for a remarkably well designed course: "Science, Knowledge and Belief". It broadened my knowledge of the sociology of science as taught by Merton to include anthropology and contributed to anchor me in what later would become STS. Back in Vienna I became the director of a UN-affiliated institute that was doing Social Research. There was no position for me at the university, as the system was very closed. It was a bit absurd: despite a Ph.D. from Columbia University, a doctorate from the University of Vienna and having been an assistant professor, I needed a *Habilitation* to apply for a professorship in Austria or Germany¹. And this brought me to Bielefeld which was a young so called, "reform university" in Germany. The academic environment was open and friendly and I was accepted for my *Habilitation* there. Niklas Luhmann was in my committee, along with Everett Mendelsohn from Harvard and Peter Weingart, who was the main person to sponsor me. It was a high level committee, and Luhmann was very supportive. I think what connected us was that he knew I had studied law before. After the *Habilitation* I was expected to teach in Bielefeld as part of my "*venia docendi*", so for a couple of years I took the night train to go there. In 1981, to my surprise, I received an invitation to join the newly founded *Wissenschaftskolleg*² in Berlin as a Fellow. Why me? I asked. Later I discovered that they had first invited Norbert Elias who declined and proposed to invite me in his stead. During my time in Bielefeld I had become a close friend to Norbert Elias. We both lived in the ZiF³ which had a swimming pool which Elias used every morning. As he was my neighbor and I had a car, I offered to buy whatever he needed for the weekend. He lived alone and after a bit of hesitation he gave me his shopping list. This is how it started. Soon he invited me to accompany him on his daily afternoon walks in the *Teutoburger Wald*⁴ behind the ZiF.

1. The *Habilitation* is an additional qualification at a higher level than the doctoral degree, required in Germany and Austria to achieve the rank of a Full Professor.
2. The *Wissenschaftskolleg zu Berlin – WiKo* (Institute for Advanced Study in Berlin) is an interdisciplinary institute dedicated to research projects in the natural and social sciences: <https://www.wiko-berlin.de/en/institute>. Helga Nowotny was a Fellow there in 1981-1982 (inaugural year) and in 2003-2004.
3. The *Zentrum für interdisziplinäre Forschung – ZiF* (Center for Interdisciplinary Research) is an Institute for Advanced Study in Bielefeld, Germany: <https://www.uni-bielefeld.de/ZiF/>. The ZiF promotes and provides premises for interdisciplinary and international research groups.
4. The *Teutoburger Wald* is a range of low, forested hills in the German states of Lower Saxony and North Rhine-Westphalia.

We had fascinating conversations about his life, his work and the world as he saw it. It was a very special experience for me to get to know this remarkable and very special man. Have you met him in person?

EE: Unfortunately not, I was in Bielefeld later. Was your connection with him only personal or does it also concern your work? In your books you quote him, but you deal with different topics.

HN: Yes, there was some overlap. We were both in the Editorial Board of the Yearbook in the Sociology of Science which was an early “scientific home” for me and he participated in some of our annual meetings. We also shared an interest in the topic of time, although my interest was still at a nascent stage. You know... sometimes people ask me who is my role model. I don’t think I ever had a role model, but the closest person would be Norbert Elias. I admired his life and his work and how he had achieved to merge the two: despite having lived through some of the horrors of the 20th century as a Jewish refugee in exile his intellectual trajectory was marked by a profound humanistic outlook and his persistent quest for understanding how humans could collaborate in the evolving “civilizing process” (Elias, 2000), yet fall back at any moment into committing the most atrocious crimes against each other. The range of his interests was phenomenal. After his retirement from the University of Leicester — his first secure academic post at the age of 57 — he went to teach at the University of Ghana. Let me return to the *Wissenschaftskolleg* as it is connected to the story of my *Habilitation*. In Austria my *Habilitation* in Bielefeld was not recognized and my entry to the university remained just as closed as before. However, when I received the invitation to Berlin, I decided to put exactly the same material that I had submitted for the *Habilitation* in Bielefeld into an envelope and sent it to the University of Vienna. I still remember that I had to paste *Stempelmarken* on the envelope, a kind of tax, for the then very high sum of 990 shillings. The *Stempelmarken* had to be bought in a kiosk operated by tobacconists, but none of them had so many stamps. I therefore had to make the rounds and collect *Stempelmarken*, which was just ridiculous. But I did it and left for Berlin. During my absence the University of Vienna granted me my second *Habilitation*.

EE: So you have two PhD’s and two *Habilitation*. A not linear trajectory also has burdens...

HN: Yes, but looking back I think that I had a much more interesting life and career than most of my former male colleagues who all had a very smooth career: they started working with their professor, in those times often a relationship involving dependence also in personal terms, then moved up the next career step and finally became professors for the rest of their life. Smooth, predictable, and often rather narrow. On the other hand — I enjoyed my independence, but there was a price to be paid. I knew that I could not expect to enter academic life in Austria or Germany based on intellectual merit and was not willing to compromise. I had to lead a kind of “double life”: as director of the UN-affiliated research center moving in an international policy environment while pursuing my academic work in STS in my “spare” time. I soon became a self-taught expert in time management. I also knew that I had to remain in the international sphere, as the national academic environment would be far too restrictive. I had always been international and felt very much at ease as part of the scientific community that sociology of science represented. It soon became known as STS — science and technology studies. As a new academic field it was still in its founding phase, distributed between Edinburgh, London, Paris, Bielefeld and Harvard. At one point I had to decide between keeping my contacts with the US or to concentrate more on Europe, which I did.

EE: This leads to my next question. You came from a background in sociology of science, in the tradition of Merton and Lazarsfeld, and now you are completely open to STS. How do you see the relationship between the two approaches? Many people say they are very different, others say that STS is exactly the development of the tradition of sociology of science. How do you see it? What are the elements of continuity and discontinuity?

HN: What happened represents a kind of epistemic watershed. Robert K Merton, the father of sociology of science, was predominantly interested in the analysis of social structures that constituted and upheld science and never touched the content of scientific knowledge. The switch came when Bruno Latour and Steve Woolgar published *Laboratory Life* based on their field work at the Salk Institute (Latour & Woolgar, 1986). The subtitle of the book — “The Construction of Scientific Facts” — says it all. By following the scientists, not the science, as Latour had admonished, the “deconstruction” of what before had seemed “off limits” to social scientists, began. Much of it was perceived as provocation by the natural scientists and the unfortunate “science wars” only underlined the many misunderstandings that persisted. But the gate had been opened and STS enabled us to ask new questions by empirically investigating practices and institutions, meanings and narratives as well as the impact of science and technology on the social worlds that people inhabit. Delving into this world where scientific knowledge was co-produced was new and fascinating. On the methodological side STS borrowed the anthropological practice of doing field work. Laboratory studies remained rare, as we soon realized that the social world had become a laboratory. But the question of the relationship between sociology and STS is still open. At ETH Zurich where I had been Professor for STS my collaborator and I published a piece on what STS and sociology could learn from each other (Guggenheim & Nowotny, 2003). Sociology had its ups and downs — right now I see it on the upswing, regaining visibility and relevance — but both can and should benefit from each other. Sociology has yet to come to terms with the role and future-shaping impact that science and technology have in contemporary societies. STS rightly speaks about “co-production” or “co-evolution”. On the other hand, STS has grown and diversified. Sometimes it risks to become too micro-oriented, losing itself in a “language game” that is applied indiscriminately to whatever it studies. An example for mutual complementarity would be one of the latest concepts that is gaining ground in STS: infrastructures and infrastructuring. We don’t know how Merton would have approached it, but STS looks at the *processes* of infrastructuring. This is a much wider and dynamic perspective as infrastructures are distributed across many domains and sub-subfields. They are planned, or not; need maintenance and repair; depend on a variety of different kinds of resources and their interconnections and so forth. However, one can easily fall into the trap where everything becomes infrastructuring and thus devoid of exploratory or explanatory value.

EE: It would be so interesting to go on talking about this, but we should also move to another topic, because I think that one cannot have an interview with you without mentioning time. You dealt with a lot of different topics, but time seems to be a sort of common thread. Some decades ago, you proposed the very influential concept of *Eigenzeit* (Nowotny, 1989), linked to the notions of extended present and of changing boundaries between past, present, and future. What role do technologies play in the experience of time? How did you move from *Eigenzeit* to predictive algorithms, and how are they connected?

HN: Let me take one step back. I started to work on time when I had a sabbatical in Cambridge, UK. My personal life was a bit turbulent at that time, and arriving in Cambridge meant that all of a sudden I had time to read, think and write. Practically, I had no obligation and

found myself confronted with an existential question: “how come that all of a sudden I have so much time, and what does it mean?” The Provost at Kings College was a famous anthropologist, Edmund Leach. We met and spoke about my interest in time. At one point he said: “look, if you want you can use my personal library, just tell my secretary when you want to come”. So, I started to read voraciously whatever I could find about time, mainly anthropological literature which was just fascinating. By chance I discovered the existence of an *International Society for the Study of Time*⁵ and that it planned a conference in Japan during the summer. I wrote to the organizers, explaining that I would like to attend and was invited to give a presentation. However, it was left to me how to get there. Not having much money, my friend and I took the Siberian railway to Vladivostok — via Moscow and Irkutsk. In Vladivostok we took an East German cargo ship that went to Osaka. I remember doing the last corrections of my lecture on the borrowed old typewriter of the captain — these were the days long before laptop or tablet. We were the only passengers. Altogether the travel with the train plus the cargo took about ten days, including a stop in Irkutsk, the capital of the “wild East” of the former Soviet Union. I did not like Moscow, while Irkutsk, at least in the summer, was beautiful, bathed in the light of the white nights. At the conference below Mount Fuji my talk was very well received. I spoke about the social dimensions of time measurement and how they shaped and were shaping social structures (Nowotny, 1975). The ISST turned out to be the most interdisciplinary group that one can imagine: there were sinologists studying time in China under the Ming dynasty; a number of serious theoretical physicists; musicologists and practicing musicians — time and music are intertwined —; historians of different epochs and continents; psychologists and literary scholars. The only ones conspicuously absent were the economists, for whom such interdisciplinary exchanges were obviously a waste of time.

EE: Was the discourse about time in such a group productive?

HN: Definitely, yes. I enjoyed talking to people from many disciplines and to see that the topic of time can be approached from so many different perspectives. So I kept in touch and later became one of the Presidents of the International Society for the Study of Time, involved in organizing the conferences held every couple of years. The founder of ISST, J.T. Fraser, was a charismatic personality. After his death, the nature of ISST changed, as it often happens. But the topic of time, once you begin to engage with it, never leaves you. It becomes part of you. The book I wrote on *Eigenzeit* is about social time, which includes chronopolitics, gendered time and how technologies impact the experience of time in post-modern societies (Nowotny, 1989). But there is a personal, hidden subtext: what time means to me, how I define and use my *Eigenzeit*, its creative potential and relevance. Even the setting of writing was very time-structured: I had only five weeks as a scholar in residence in Bellagio⁶ to finish it. It was gratifying but hard, like running a marathon.

EE: Did you have a deadline, or did you set yourself a deadline?

HN: I was still director of the institute, so I knew: either now or never. If I could not finish in five weeks, it would be delayed for a long time to come. The book was a big success in the German-speaking world and has been translated into several languages. I still meet people who have read it and tell me what it meant for them. Last year I was invited at the *Wiener Fest-*

5. Established by J.T. Fraser in 1966, the *International Society for the Study of Time (ISST)* (<https://studyoftime.org/>) is an interdisciplinary organization of professional scientists, humanists, and artists, exploring the multiple dimensions and perspectives of time across the disciplines.

6. At the Rockefeller Foundation Bellagio Center: <https://www.rockefellerfoundation.org/bellagio-center/>

*wochen*⁷ by the vice director of the *Burgtheater* for a session where people choose a discussion partner. She had read *Eigenzeit* and said it was still relevant for her. It made me curious: I went back to what I wrote more than three decades ago and must say — it stood up well.

EE: Of course, definitely. In hindsight you see that some topics became more and more relevant with the course of time. For example, the notion of the extended present.

HN: Yes, this is an important part of *Eigenzeit*. Now, with digitalization came also digital time. We live in a kind of digital time machine where the future has moved into the present. You can call it extended present or the future moving into the present, but it's exactly the same.

EE: That's how I understand the connection with the idea that the disappearance of the future is completely compatible with the new relevance of the future connected with new technologies.

HN: Absolutely. For me it is a kind of evolution, something that I anticipated without knowing how it would turn out. Nobody could foresee which role technologies would play in the future, and now with digital time, as you know from my book (Nowotny, 2021), a new strand of time has been added to physical, biological and social time. Technologies were always relevant to measure time, but especially during the period of industrialization clock time became such a dominant way of structuring the life of everyone. Many people don't think of it, because we are so Eurocentric, but countries around the globe had to adapt to the Western notion of clock time. They did not have clocks or, if they used them, clocks did not have the same function as they did in the West — structuring work, structuring life, structuring education and pension systems, thus penetrating every cranny of life in the industrial peril of modernity.

EE: This is impressive, but now of course the new technologies are different, the algorithms are different. You have this idea of the paradox of the future, connected with control and agency.

HN: Predictive algorithm are an integral part of extending the present, letting us glimpse a part of the — imagined? — future, something you are also interested in. No doubt, digital technologies and algorithms are powerful instruments to let us see further into the future (Nowotny, 2017). But the predictions they make are based on data from the past as the future has no data as yet. What I call the prediction paradox is that we leverage AI to increase our control over the future and uncertainty, while at the same time, the performativity of AI, the power it has to make us act in the ways it predicts, reduces our agency over the future. We transfer part of our agency to this technology. I am also concerned about the risks that come with this. Partly, they are the self-fulfilling prophecies, an old sociological concept, that now can be scaled up and hold many people and parts of society in their grip. But we also risk to fall back into a deterministic worldview which prevailed for the largest part of human history. For thousands of years, humans believed that gods or God or the higher powers, have decided already their fate, setting their destiny. In my view, it was one of the greatest social inventions of humanity to see the horizon of the future as open and, at least to some extent, humans are capable to shape it. This goes back to the work of Reinhart Koselleck (1979) but also to Norbert Elias (1939): given his own life experience, he knew that there is no inevitability of humanity progressing towards a higher — and better — state. Falling back into what he called barbarity is possible any time and I fear that right now we see signs of a dangerous regression in many

7. *Wiener Festwochen*: <https://www.festwochen.at/en/home>

domains.

EE: Sure. The open future does not necessarily mean progress, we know this very well. I find your reference to determinism really fascinating, because as you know I've been working on the idea that these algorithms have procedures that are really close to the ones of divination, and divination has been the technology for dealing with future for thousands of years (Esposito, 2022). Of course we cannot give up our open future, but there's a strange clash between these attitudes. Your paradox is a great way to express that we are moving in two directions that are not compatible, and we need both of them. The complexity of the open future is connected with this.

HN: This is what I very much like about your work. But we also tend to forget probabilities. Evolution has not equipped us mentally and cognitively to deal well with probabilities. You know the work of Gigerenzer et al. (1989), Daston (1988) and Porter (1986). Doctors often don't know how to tell their patients what probability actually means, and the patient leaves the doctor's office shattered and depressed. We know that — but the world is full with probabilities. This is why we have to learn to live with them. The future is inherently uncertain and instead of being afraid of uncertainty, we should embrace it (Nowotny, 2016).

EE: It is probably also connected with the open future: probability means that we don't know. It measures what we don't know, not what we know.

HN: Probabilities never can tell you anything about one individual.

EE: While the algorithms seem to do that.

HN: Yes, they *seem* to do that. We tend to forget that large parts of social behaviour *are* predictable. We are creatures of habit and without repetition and routines, without social norms that regulate social regularities, living together would be very cumbersome. But this can numb us into believing that “normality”, the belief that life will continue just as before, is to be taken for granted. The pandemic and its aftermath, and now the war in the Ukraine with its prolonged and unpredictable negative fallout, have shattered such assumptions.

EE: They actually do it, but what they say is not necessarily right. Algorithms work with individual data, but what they predict is not necessarily the right prediction. It is a really interesting debate, but there is at least one other completely different topic that I would like to address. I was always fascinated by the fact that you as a social scientist became so active and influential inside policy, as a founder and president of ERC, and in many other roles. What is remarkable is not only that they are such important positions, but also that a sociologist of science is holding them. As a sociologist, how do you see the connection between research and science policy? How do changes in the organization of science affect how people do science?

HN: Again, I did not plan it. I was always interested in speaking with scientists and learning more about what they do. Coming from a background in STS I found it easy. When I was at the ETH Zurich⁸, I often got my colleagues from the natural sciences to speak about their problems, and usually it went very well. They trusted me and thought that I'm competent enough to understand what they're talking about, so I learned a lot about the problems of working scientists — by listening, analyzing and elucidating. After all, science policy is about the organization and funding of science, but in order to do so you have to know what researchers are

8. *Eidgenössische Technische Hochschule* (Swiss Federal Institute of Technology).

actually doing, and what their problems are. They have a passion for science and are guided by epistemic norms, even if they do not know it. Most are truly fascinated by a research problem, but you have to know whether the goals they set themselves are feasible and how it fits into the specific scientific problem space in which they work in order to be able to make a real contribution to the production of new knowledge. When I was in Berlin at the *Wissenschaftskolleg*, a foundation was housed in the basement (the name escapes me now) that organized small meetings of selected scientists from different disciplines. In attending some of these meetings I was struck by differences in the dress code: for instance, biologists dressed differently from geologists. More importantly, I was impressed by a recurrent phrase that cropped up in the discussion: “I would like to know” which was followed by a concise definition of what the speaker identified as the problem to be better known. In other parts of the discussion participants asserted: “we will know this in five years”. So I asked them: “how do you know that you will know this in five years?” The explanation was: “this is where we are now, we have these new instruments and know they can deliver. Therefore, we can foresee where we will be five years from now, approximately”. I mention this because it taught me a lot about the value of instruments in the natural sciences while in the social sciences we mainly believe that scientific advancement is driven by new concepts. I continued to learn much by talking to practitioners, but I also realized how important it is to better understand how the system of science functions and the role played by funding and how to obtain it. In science, there exists a peculiar balance - or an inherent tension - between competition and cooperation. Scientists know that they need each other and they cooperate at different levels and in different forms. At the same time, they compete with each other, foremost in reputation, which is partly linked to success in funding. So, I became interested in the role of funding agencies. It all came together when I became active at the EU level. My first experience was as member of an EU Social-Economic Science Committee that evaluated and decided on grants. This was at an early stage in the Framework Programmes and I quickly noticed how successful our British colleagues were in obtaining funding. Why? Of course, you might think, their proposals were simply better, writing grants in their native language helped and so on. But I soon discovered that something else was involved. The British members of our Committee went home after each meeting and shared with their colleagues our mode of working. In other words, one of the advantages British applicants had over those from other countries was that they knew which criteria, emphasis on methods etc. mattered in the evaluation. In other countries, however, if you happened to be a member of such a committee, the tendency was to keep this kind of knowledge to yourself. As social scientists, we were far from “Open Science” and from sharing valuable information, working more like the alchemists in the Middle Ages. A bit later I was approached by a search committee of the European Science Foundation who asked me to become the Chair of the ESF Social Science Committee. Over a lovely meal in Paris I agreed. The only problem was that I had never met the Committee before and they only learned that I was their new Chair shortly before we met in Uppsala. As you can imagine the meeting started somewhat awkwardly. Looking back, I think this is when I learned on the spot how to chair a meeting, a kind of acid test, which I passed. Ever since, I have chaired a large number of meetings of different sizes and kinds. I have learned how to listen to what people say, interpret what they actually mean but don't say, combine the diverse inputs and in the end follow my own instinct where I want to lead the group.

EE: And they should recognize it as their own will.

HN: Yes, that's part of the secret. At the next meeting of the Committee I asked my assis-

tant to remove all the ashtrays on the table — back then, people still smoked heavily during meetings. Understandably, the smokers were surprised and upset. I briefly explained that we all know that smoking is not good for others in the same room and that there will be a break to smoke outside. Since then we had no-smoking meetings and no more complaints. Sometimes you just have to lead by doing. Through the European Science Foundation I got to know the European scientific establishment and the EU bureaucracy in Bruxelles dealing with research, technology and innovation. I took part in numerous “high level expert groups” and the conference circles in which RTI policy was discussed. At one point, a new advisory body to the Commission was set up, consisting of 45 persons, half of them from academia, the other half coming from industry. I was invited to become the chair, but had two conditions. First, I wanted to be able to select my own Committee Secretary. From experience I knew that if the Commission would appoint such a person for me, I would have had to rewrite the minutes of each meeting myself, as it would have been written in “Commission speak”. The other condition was addressed to the members of European Research Advisory Board. They were accustomed to meet as separate factions before, one for academia, one for industry. I told them that we needed to sort out differences not before, but during our joint meeting. If we wanted to become credible, we needed to speak with one voice and with every member behind it. I succeeded in getting a wonderful woman from Norway as Committee Secretary and EURAB members agreed that it was better to meet together if we wanted to make a difference. One of the most important and consequential recommendations was to strongly support the establishment of the European Research Council, ERC. It needed fresh money, at least 1 billion € to start. We were not the only ones. The Nordic countries has been strongly engaged in pushing for an ERC and so did many scientific lobby groups, organizing workshops and conferences. It was fascinating to be part of this collective effort that eventually led to the establishment of the ERC. Until the very end, two countries — Germany and the UK — opposed it with the argument that the Commission would not be able to set up such a funding agency guaranteeing the kind of scientific quality that prevailed in their respective national funding systems. A German law professor from a Max Planck Institute argued that on legal grounds all official languages had to be admitted, which clearly would have made the practical operation of the ERC impossible. There were also more political hurdles to be taken and at one point I sent my Scottish Vice-President to speak to Lord Sainsbury (at the time the UK Minister of science) before a crucial meeting. At the conference, Lord Sainsbury declared that he had come with a differently prepared speech but had been convinced that the ERC should be set up. This was a real occasion to celebrate!

EE: The issue of combining different national cultures is still here, but out of my experience I could observe that the way of doing research has dramatically changed in the last decades — and I think that the ERC and its fundings were part of it. It means doing research and academic activities in a deeply different way. How do you see it? You had a privileged standpoint to observe how this changed in the last decades.

HN: The ERC started with a Scientific Council of 22 members who had been chosen by a selection committee of five, chaired by Lord Patten (now Chancellor of the University of Oxford) out of a list of 450 names. The Selection Committee did a great job as we, the 22 founding members, were highly committed to establish a new European funding agency for frontier research (a code for fundamental research) on the basis of scientific excellence only. The ERC was to be “run by scientists, for scientists”. We quickly agreed on the principles in the very first meeting, including that two-thirds of the funding should go to younger researchers. There was

also immediate consensus that the social sciences and humanities would be part of the ERC and that the funding available should be allocated to the three domains according to demand. As Vice President I argued for 18% of the budget for the SSH which, as comparative figures from other international funding agencies were too few, was reduced to 15%. Now, the share is back to my original proposal. The European Commission deserves praise for the courage to take the radical step to leave the strategy for running the ERC programme entirely to the ERC Scientific Council. Most Commission officials expected the 22 members of the Scientific Council — all highly respected and busy people in their respective research fields — to become tired and gradually pull out. Indeed, the workload was huge. We had to set up 25 panels covering all fields of science and scholarship and appoint the best scientists we could think of as panel members. We had the privilege of building a quite unique funding agency at world-class level and were determined to live up to it. So, contrary to what some bureaucrats expected or hoped for, we did not become tired. We were a pioneering and cohesive cohort, determined to make the ERC the success story that it became. Problems arose from the fact that the implementation of the scientific strategy decided by the ERC Scientific Council was left in the hands of the Commission, first a small group, later the ERC Executive Agency. The implementation of our vision and our strategic decisions therefore had to meet many Commission rules not designed for such a purpose. Many obstacles had to be overcome, but in the end the quest for scientific excellence prevailed.

EE: In hindsight we can say now that what came out corresponded to the initial planning, with some adjustments. It was a big success.

HN: Yes, and it has been recognized.

EE: Did you expect you could do what you planned? And did you also expect it would be so successful?

HN: We worked in thus incredible pioneering spirit and we were a group of wonderfully dedicated people. Some of my colleagues eventually lost patience with the bureaucratic structures of the Commission. For the EU bureaucracy we, acting as independent scientists in the name of science, were a strange animal, almost an accident or perceived as a threat. My advantage coming from STS was to see the structures behind the hindrances we faced, while some of my colleagues from the natural sciences took much of it personally. Fotis Kafatos, the first ERC President, asked me to chair the Scientific Council meetings. Following Fotis' resignation in March 2010 my colleagues elected me as President in which function I served until the end of the Framework Programme in December 2014.

EE: That's certainly a lot of personal talent, but one also hears the sociologist speaking.

HN: Well, it helped me — because you don't take it personally.

EE: Yes, you have a sense of the institution.

HN: That's right, and it helps to see the people as they are. Of course, every person is different, but you also see where they come from. From this point of view, it helps to know that there is a background you can interpret and appreciate. Working in a European context also means it is good to know how different funding agencies function. Science is international, but scientific careers are still largely shaped by national science systems and the national contexts in which researchers work. Moreover, there are different scientific and disciplinary "sub-cultures" which may also somewhat differ from one country to another.

EE: A last question. You wrote very early about gender (Hausen & Nowotny, 1986), and now the issue is so relevant. How do you see the topic today? Is science still male, and in the same way? What role has gender played in your research and in your career?

HN: A lot has changed, and much for the better, I must say. But we are not there as yet. As you know, also in science unconscious bias is pervasive. At the ERC we set up a working group to look into how this could be mitigated. Did discrimination occur? Looking carefully at the figures available we wanted to know especially what happened in the last phase of the evaluation and whether women applicants were treated the same as men. It turned out that in physics, as expected, there were very few women. The women who made it to the end were usually very good, and there was no problem. In the social sciences almost as many women as men reached the final stage and, by and large, it also worked. The only domain where women seemed to fall off were the life sciences. The figures were roughly the same over years and the same pattern persists to this day. Why? My hunch — which I could not corroborate as the data were not sufficient — was that in the life sciences hidden hierarchies exist. For the ERC it is very important that the candidate comes with an original project proposal and above all, has an independent scientific mind. Apparently, in the life sciences and especially in medicine, work is organized in a more hierarchical way, with the director of the clinic or the big lab assigning who works on which problem. This means that young researchers have fewer opportunities to come up with their own ideas and that women are less able to resist. This is noticed in the interview by the panel probing the scientific independence and my hypothesis why more women failed. We also looked for unconscious bias in the deliberation processes of the panels. We asked the staff of the ERCEA who were present in the meetings to write down if a remark, however casual, strikes them as possibly pointing to unconscious bias. They showed us the results and we asked them to report back to the panel without revealing the who had said what. Most panel members were taken aback. They realized that also they were prone to unconscious bias. Being unconscious, it is difficult to say how long the effect of such a feed-back lasted, but it was a start. The book *How Male Is Science* (Hausen & Nowotny, 1986) was based on the talk I gave as a Fellow at the *Wissenschaftskolleg* in 1982. It was a reaction against the experience I had at WIKO during my stay. Last year, on the occasion of the 40th anniversary of WIKO, Barbara Stollberg-Rilinger, the current Rector, asked me to give the annual Welcome Address in which I also spoke about how it felt then to be a woman among the many *Germanisten*⁹ who were the Fellows during the first year. We were only two women and my colleague, a literary scholar from Israel, announced that she was pregnant. I was deeply shocked to hear these scholars discuss whether a pregnant woman can be a fellow and decided to change the topic of my Fellow lecture. It was a memorable event. The lecture was public and most men in the audience kept silence, either because they were clever or indifferent. Those who spoke ran into fierce opposition from the feminists who were present. My WIKO co-fellows, however, thought that the topic was neither scientific nor of any concern to them. Later, Karin Hausen approached me to organize a conference on the topic which was turned into a book. A last little anecdote about this conference. We had invited women from different scientific disciplines, but also four or five men whom we asked to comment the papers given by women. They all were sympathetic to feminism. However, during the week two of them approached me separately and told me that only now they realized what it meant to be in a minority as a man in a women's conference, and how women must constantly feel when they are in a conference dominated by men. Intellectually they could analyze it, but emotionally they felt unable to cope and told

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me that they would leave. Maybe this is a reminder how much of “society”, cultural norms, socialization and feelings are involved when doing science which is a wonderful, but and above all, a *social* endeavour.

EE: Thanks so much for your time. I have a lot of really interesting materials.

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