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Uta Frith began her science career studying experimental psychology in Germany. She continued her training in clinical psychology at London University. Her doctoral thesis on pattern detection in typically developing and autistic children started her lifelong fascination with the study of neurocognitive mechanisms, which can fail, and if so, result in developmental disorders, such as autism or dyslexia. Uta Frith and her collaborators developed and tested innovative theories to explain the characteristic behavioral features of both these conditions, such as "mentalizing problems" and "weak central coherence" to explain the social and nonsocial features of autism, and phonological processing problems in dyslexia, respectively. Mentalizing, the ability to attribute mental states to others, such as intentions and beliefs, proved to be a particularly fruitful notion, and her group was the first to identify the circumscribed brain network that underpins this ability. Uta Frith is currently writing a book with Chris Frith on "What makes us social" and a graphic novel on the same topic. She is concerned about the future of science and is advocating for Slow Science. Her aim is to promote a research culture that values quality more than quantity, with the motto "less but better." She is passionate about the role of women in science and science communication and has presented a number of BBC Horizon documentaries.

Uta Frith

I accepted the invitation to narrate my life for this volume with apprehension, knowing that I would be in the company of celebrated psychologists and neuroscientists. This is the company of dreams. But I have qualms that are hard to shake off. Who am I? My story is a story that I believe to be true, and that I would like the reader to believe in, too. Still, I am bound to be biased by a natural wish to present myself in a good light. As a scientist, I am skeptical of autobiographical accounts. Nevertheless, I have found them invaluable in helping me understand the mental world of autistic individuals. With this preamble off my chest, it turns out that I have always wanted to tell this story about my life, my favorite ideas, and my favorite people.

A Kind of Fairy Tale

Nothing predestined me to become a scientist, and the environment of my childhood was as far removed as possible from where I am today. I can't help thinking that the unpromising beginning, the unexpected turns, and the happy ending are marks of a fairy tale.

I was born in Germany in the middle of World War II, in Rockenhausen, a very small town in the Donnersberg region of Rhineland-Palatinate. I had an idyllic childhood. It seems a miracle that it was possible for me to live untouched by the dreadful events happening at the time. My mother managed to keep me and my younger sister safe, and I well remember her singing a popular song with the refrain "Keine Angst, keine Angst, Rosmarie." I grew up in my grandparents' house, playing all day in the street or garden, and roaming the surrounding fields with the children from the neighborhood. My mother ran her father's shop, known in the region as Zigarren-Gödel. From a young age, I fetched bread from the bakery that also belonged to the Gödel family.

When I was four years old, I was surprised that a strange man, my father, suddenly arrived and stayed. I learned much later that his experiences of the war had been horrific and deeply traumatizing. Wilhelm Aurnhammer was an artist and art teacher. As part of a group of expressionist artists in Munich, he was forbidden to exhibit. He trained as a teacher and was lucky to get a job in a village school in the back of beyond, not far from Rockenhausen, where he met my mother. His father had been an accountant with the BASF chemical industries in Ludwigshafen am Rhein. My Aurnhammer ancestors came from Augsburg in Southern Germany. My two grandmothers were quite different from each other, one associated with town, the other with the country.

Rockenhausen Streetview, etching by Wilhelm Aurnhammer, 1947.

The little town where I grew up was embedded in a soft landscape with meadows, hills, woods, orchards, and vineyards. I was proud to know that Hildegard von Bingen, the 12th-century polymath, was a child of the region. On each hill, there would likely be the ruin of a medieval castle. For each castle, there was a story, and the ones I remember had a heroine who withstood a siege and saved everyone by a clever ruse. Who told the stories? Mainly my mother. She also had a special love of poems, which I picked up without effort. I fondly remember an old relative, known as fairy-tale aunt, who lived on a remote farm. She had a knack for entrancing us children with her wonderfully elaborated versions of Grimm's fairy tales.

I started primary school in 1947, but it might as well have been 1847. Horse and cart, endlessly reworked clothes, and oil lamps were part of daily life. There were old books and old toys, and not enough paper for drawing and scribbling. We learned to write using a slate, with a sponge attached to wipe it clean. My father produced a children's book, making wood cuts for the illustrations, which accompanied verses by my uncle, and were colored thanks to the help of a local printer. A funny incident occurred on my first day at school, for which I have no memory, but a family friend, the mother of another child, told me about many years later. All the mothers were present, except mine. She had sent me along on my own, in line with her principles

of instilling independence. At the time, I was unaware that this was a bit unusual. A school inspector welcomed the new intake and asked the rhetorical question: "Which of you children is the cleverest?" Apparently, I stepped forward without hesitation. Everyone laughed, but this startling self-confidence was a gift that never left me, even though it went underground on many occasions. Maybe it was a self-fulfilling prophecy, enhanced by my mother's claim that, as I was born on a Sunday, I was bound to be lucky. It so happened that throughout my school years, I got top marks.

When I was eight years old, we moved to the nearest big town, Kaiserslautern. My father taught art at several of the secondary schools, as there was a shortage of teachers. There was also a shortage of school buildings and of everything else. Children and teachers brought logs of wood to heat schoolrooms on the coldest days. Bombing raids had destroyed much of the town, as was the case with any other town I visited. I later realized that my concept of town was of a gigantic building site of rubble and ruins. As a teacher, my father obtained a coveted apartment, when most apartments in the area were shared by several families. Large numbers of refugees had to be accommodated. We children benefited directly from the Marshall Plan as hot meals were given out at schools. I remember everyone being inventive with scarce resources. This was the best of times, as many said afterward. The terrible war was over, and the 12-year reign of terror of the Third Reich had ended, although there would be lasting wounds and lasting guilt. Remarkably, within a few years, the economic miracle converted West Germany into a modern democratic society.

The ruins disappeared gradually under rebuilt houses, but in the meantime, they furnished a tempting playground. Despite barbed wire and broken glass, every unsafe step was a thrill. There were other exciting places I was eager to explore. The woods were only minutes away from where we lived, woods that quickly turned into deep dark forest. Unsupervised, my sister and I went there carrying little baskets to bring back blueberries. We climbed up steep paths through mossy valleys and over pink sandstone rocks. I still hanker after these woods. I had started a nature diary, where I entered "observations," with little drawings, each ending with the question, "Why was it interesting?" Was I aware that only some things drew my attention and other things didn't? Perhaps it was the start of a phase in my own cognitive development in which I realized that it was thrilling to observe nature, and even more thrilling to observe the observer.

I fondly imagine that I reached peak energy when I was between 10 and 12 years old. Learning and memory came easily to me, and I could count on friends to play with and go on adventures. In my limited environment, I felt I could take on anything. I did not mind that other people called me ambitious, because I wanted to excel, and I craved approval. Soon enough, I found my limits and discarded various interests, such as sports, drawing, and chess, all very much to my father's regret. Instead, I preferred reading

and looking at art, listening to music, and joining in the conversation with eccentric artists and colorful relatives, when they visited the family. I was becoming "encultured" in a bourgeois-cum-bohemian home.

My parents allowed me immense freedom. They agreed readily to my wish to change from the modern secondary school where all my friends were, to the "Altsprachliches Gymnasium." My choice was unusual because this particular school had a long tradition of preparing boys for university and was attended by only a handful of girls. I was befriended by one of the older girls, who went on to read classics at Heidelberg. One day she invited me to get a taste of what it was like to be a student. I loved everything about it. But something took me aback. She told me that she considered doing a doctorate, like her older sister before her. However, her admired professor advised her against it, as 'she wasn't the type', which, he stressed, she should take as a compliment. Suprisingly, she was flattered by this response. She became a classics teacher, a job thought to be highly suitable for a clever girl, and as it turned out, she was very good at it. But somehow, I knew that Heidelberg had to go from my list of desirable universities, and classics from my list of desirable subjects.

Little did I know that my willful change of schools was tantamount to challenging well-established gender roles. Gradually, I realized I had traded in traditional femininity to become an intellectual bluestocking. What on earth made me do this? Well, I did enjoy showing off with recondite knowledge and getting better grades than the boys. I also liked the fact that the curriculum was so wide that I did not have to make any decision on whether I would take up arts or sciences at university. I was curious about everything and soaked up an indiscriminate mixture of information, especially when it went against expectations and overturned previously unquestioned beliefs. Disenchantment was enlightenment. It felt far more exciting than the enchantment of earlier times. Fairy tales were lies! There was an exhilarating frisson in the possibility that everything might be questionable. This was getting nearer to a scientific mind-set, but I still had no idea of what I was going to do when I grew up.

Into the Big Wide World

What could be more splendid than being hostess of a salon in 19th-century Paris? Drifting among intellectuals and artists for perpetual entertainment was my idea of winning life's lottery. It was a fabulous aim to strive for, if somewhat unrealistic, but being a student at the Sorbonne seemed the next best thing. My parents accepted this plan with their customary laissez-faire. But first, I reckoned, a university near home, yet close to France, the University of the Saarland at Saarbrücken, would be a more practical idea. I knew lots of people who studied there. It was a modern university with a distinct French presence. I was 20 when I left school, but I was still a

provincial girl, far more backward and naïve than young people today would be able to imagine.

What was it like to be a student with no fixed aims? I sampled lectures and seminars in a whole range of subjects. I was thrilled to get to know professors who had written books that you could find in a bookshop. I was dreaming of being an eternal student, sampling all of the disciplines on offer. Still, I focused on history of art, which quickly became my favorite subject. And no wonder. Those students were interesting, and moreover they went on trips to see art and architecture at first hand. There were trips to Paris and visits to the churches of Lorraine and of Champagne. I recommend study tours! They are far more instructive than lectures, especially about the people who study with you, not to mention the senior academics. Professor Schmoll genannt Eisenwerth lectured on captivating topics, such as the idea of the torso in Rodin's work, the history of photography, or exquisite 14th-century sculpture. I tried to impress him with seminar presentations and preparing slides for his lectures.

Gradually, my attention was drawn to students who were following a structured career, doing exams and doctoral dissertations. Slowly, I realized that they were the ones to emulate. It was a random piece of luck that this got me into psychology. Out of curiosity, I attended a lecture, given by an earnest professor, about factor analysis and intelligence. Despite the dry delivery my interest was captured. I felt there were serious questions, and serious methods to answer them. What actually was intelligence? Did it change during development? Why were there individual differences? I eagerly offered to take part in psychometric tests, which reminded me of party games. It was fun going to the Psychology Department: it was attractively furnished, and there were original works of art on the walls. The people were cool, too. Furthermore, there was a proper course structure, very unlike what I had seen in the other subjects I had sampled.

The course involved learning about anatomy and physiology as well as statistics, which to me only made it more attractive. Ernst Boesch, a Piaget pupil, was head of department and gave lectures on development and cultural anthropology; these subjects were totally new to me and utterly absorbing. Paul Baltes was already an established student and teaching assistant and encouraged me to continue in psychology. He later became well known for his studies in life-span psychology. The course work was all novel, and some of it slightly horrifying, like the lab class that involved killing frogs to record spinal nerve activity using smoked drums. But none was as fascinating as the grand ward rounds in psychiatry. They were given by an inspiring professor, who presented patients suffering from schizophrenia, depression, phobia, obsessive-compulsive disorder (OCD), and other disorders. I could not wait for these presentations, which were by far the most mindboggling thing that I had ever come across. I desperately wanted

to know what caused these disorders of the mind and, all of a sudden, I knew that I had to become a researcher in clinical psychology.

There was one big hurdle. To follow the course in experimental psychology I had to use a textbook, a standard American textbook, in English! I decided to go on an immersive language course for students in London. It was tough. But London enchanted me. Much to my own surprise, I liked it even better than Paris, and I began to wonder whether I should go to London University instead of the Sorbonne. I had read *Uses and Abuses of Psychology* (Eysenck, 1953) and *Sense and Nonsense in Psychology* (1958)—but in translation. These books had been quite a shock to me, because at that time, few people had dared to criticize psychoanalysis, and Eysenck's books promised a better way to treat patients by applying the principles of learning theory. I was thrilled to learn that Behavior Therapy was being practiced and taught at London's Maudsley Hospital. Clearly, this was the place for me! I applied for an internship during the summer vacation and was lucky to be taken on. I was able to help on a project on OCD led by Reg Beech, working out correlations on a massive calculator. I learned about the diploma course in abnormal psychology, which provided a wonderful and unique opportunity to train alongside clinicians and researchers. I so wished I could get on that course. As my time ended and my suitcase was already half packed, I found out that somebody had dropped out, and a place had become vacant. I will always be grateful to Monte Shapiro, who was head of the course, for his faith in me and taking me on as a trainee.

Happy Ending

This stroke of luck was nothing compared with the miraculous good fortune that led me to my future husband, Chris Frith. He was a trainee in the year before and had just started on a doctorate. It did not take us long to know that we were made for each other, a couple of days at most. The similarity of our interests, tastes, likes, and dislikes astonished us. But, perhaps these wonderfully coinciding interests were actually created and shaped by our interactions. For example, we both developed an interest in British constructionist art and enjoyed meeting the artists, to whom we were introduced by our art historian friends. One of the earliest works we acquired, a white circular relief by Susan Derges, can be seen in the background of our double portrait, which is shown at the end.

In this painting, you also can glimpse some of the influences that made me "me," but that had nothing to do with science. They have everything to do with the beautiful things in life. I have an irrepressible desire to collect things that touch me as especially pleasing, from Persian rugs to Chinese porcelain, from Old Master prints to anonymous alphabet samplers. They fill our large Victorian home. Everything is intermingled, perhaps reflecting my omnivorous mind. As I am writing this piece, I am listening to classical

music on BBC Radio 3, something I cannot do without. My favorite composers are as predictable as my favorite colors. I enjoyed talking about them on the BBC's legendary *Desert Island Disks* program.

I adapted to English culture fast, but I had a lot to learn, and not just the language. I was delighted to be welcomed into the Frith family, as Chris was welcomed into the Aurnhammer family. We were married in 1966 and remain harmoniously united to this day.

Uta and Chris Frith, 1966.

We had two sons: Martin, born in 1975, and Alex, born in 1978. Neither of them took up psychology—in case you are curious, Martin is a computational biologist living in Tokyo, and Alex is a children's book editor living in London. With three wonderful grandchildren, I cannot imagine a happier ending.

For my story to end happily, some crucial foundations had to be laid, and it was Chris who laid them. He taught me about information processing and about neuropsychology and he tutored me in statistics. He continuously kept me up to date with all the exciting developments in methods, from the very first crude computers to the very first positron emission tomography (PET) scanners. His career in research was independent of my own. We specialized deliberately, thereby enlarging our research activities, rather than doubling up skills. We were lucky enough to pursue research in distinct areas, and we avoided working in the same department. This provided enough room for dissent, as well as common ground for productive argument and discussion. We were always curious about each other's work and talked about it constantly. We read each other's drafts with a critical eye. I believe the continuous exchange of new information and willingness to give and take criticism was by far the most important factor in our apparently successful scientific career. At first, we only occasionally wrote a paper together, and it was not until later years that this became our favorite mode. Looking back, I feel we could feature in an advertisement for scientific collaboration. It is perhaps no coincidence that we recently have

been writing mainly about social communication, cooperation, and the role of diversity in making joint decisions.

After my last Medical Research Council (MRC) program grant finished, having reached the then-normal retirement age of 65, I was touched that my colleagues and students put together a *Festschrift* with a meeting and subsequent volume of the papers presented (Bishop & Snowling, 2008). But now a new exciting phase of life started. Both Chris and I had been offered visiting professorships at the University of Aarhus, which we held until recently. We regularly drove to Harwich and crossed the usually stormy North Sea by ferry. For our extended stays in Denmark, we loved living in the Scandinavian style. Being able to take walks in stupendous beech woods on the Baltic provided a refreshing contrast to our London life. The Interacting Minds Centre (IMC) with Andreas Roepstorff at the helm, still serves as a multidisciplinary hub with many offshoots. Now that our positions have officially ended, we continue to visit Aarhus every year.

Breakfast of the Interacting Minds Group at the Yellow Villa at Aarhus University, ca. 2007. From left to right: Ethan Weed, Peter Vust, Giacomo Rizzolatti (invited seminar speaker), Chris Frith, Uta Frith, Kristian Tylen. Andreas Roepstorff is outside the picture.

I will now leave behind the true tale of the country mouse who ventured into a foreign land, grew fat and famous, and was even invited to one of the Queen's garden parties. I next try to give a condensed account of the main preoccupations that dominated my work—dyslexia and autism. I will mention only some of the people who have influenced me, because if I mentioned all of them, this section would turn into a long, long list. There

is more information about my life, interests, and passions on the frithmind .org website.

Obsessed with Literacy

The paradox that there are individuals who are excellent readers but atrocious spellers, excited me because it was something very dear to me: Chris was one of these individuals. I was amazed by the speed of his reading. He solved fiendishly difficult crosswords, yet he often asked me how to spell a word when entering it into the grid. How could it be that my own still quite shallow command of English included orthographic knowledge, without even trying? I made it my project to find out.

It was not long before I found similar people, whom I termed type B spellers, and I developed tests to find out whether memory representations for written words were used differently for reading and for writing. I was following the notion that we read by eye but write by ear, and there was no single lexicon in the brain that efficiently bridged both channels (Frith, 1979). The reading lexicon could be built with incomplete spelling patterns, allowing very fast recognition, but the writing lexicon needed the complete word forms, given that writing is a slow sequential process. Other researchers were interested in similar questions, and I thought it would be fun to edit a book on spelling that brought this new work together (Frith, 1980). And it was fun. First, because I was in close contact with the process of production with the long-defunct publisher, Academic Press. Apparently, it was one of the last books to be typeset and printed by an artisan printer. Second, it was great to meet up with the authors to talk about their chapters, accompanied by strawberries and fizz in the summer and gingerbread and mulled wine in the winter. Oh for the good old days, when we were happy with such simple pleasures.

Although there were plenty of books on teaching reading, there was not a lot of theory about the development of reading and spelling. I thought of it as a stepwise process, with the use of three different strategies (Frith, 1986). First, I proposed, reading would be accomplished by holistic (logographic) impressions; second, this strategy would be opposed by another (alphabetic) one, which would automatically arise from learning to write, a sequential process; third, both strategies would be integrated in a mature (orthographic) strategy of the skilled normal reader. The type B spellers would manage to get to only the second step. The dyslexics (i.e., those who were poor readers as well as poor spellers) presumably would not manage step two. This may all sound simpleminded, but it served as a framework to carry out experiments that addressed the question of how children acquire literacy, and sometimes fail to do so.

Letter reversals that are typical of beginners, were of particular fascination to me (Frith, 1974), and I first thought of reading as part of visual

perceptual development. However, I completely changed my mind about this. And this dates from a summer school run by the Society for Research Child Development at the University of Delaware in 1974. I applied and was amazed and delighted to be accepted. The experience was truly "formative": my first time in the United States, first time being among like-minded young researchers, and first time meeting famous psychologists. Lila Gleitman left an indelible impression, and it was her presentations that convinced me that learning to read and write had far more to do with language development than with visual perception. However, the most lasting influence on me were my fellow students, with Linnea Ehri, George Marsh, Rod Barron, Rob Kail, and Lynn Waterhouse among them.

Listening to Isabelle Liberman and Don Shankweiler I remember the frisson of insight, when I realized that the way I thought about spoken language had been indelibly "corrupted" by my own literacy. Words and the sounds of letters are rather unnatural segments to cut out from the natural flow of speech, and yet we feel them as natural segments. I took this as a striking lesson in how culture shapes our thought processes.

In the 1960s and 1970s, many people claimed that dyslexia had no biological basis and that a medical model for the condition only played into the hands of middle-class parents trying to explain away their children's poor school achievements. I was not convinced. The children's difficulties were "specific," that is, their other cognitive abilities were seemingly intact. The contrast would be general intellectual impairment, where difficulty in learning to read (as difficulties in other areas) was only to be expected. Furthermore, there were strong indications that dyslexia ran in families and a genetic predisposition seemed likely. I had an inkling that some unknown and possibly subtle brain abnormality, dating from before birth, could have long-lasting but circumscribed effects. This was somewhat at odds with the prevailing assumption that the heightened plasticity of young brains would readily enable reorganization and compensation. It was very much at odds with the then-popular assumption that psychosocial factors caused disorders, such as dyslexia and autism.

My interest in dyslexia was fired up by the arrival of my first doctoral student, Maggie Snowling, in the 1970s. Maggie's research was foundational in putting grapheme-phoneme skills at the center of investigations and putting the focus on phonological deficits. Later, we set up a study with children at genetic/family risk for dyslexia. We found telltale signs of phonological problems even at the preschool age (Gallagher, Snowling & Frith, 2000). Franck Ramus, of the next generation of dyslexia researchers, joined me as a postdoc in 2000. He systematically compared the three then prevailing theories of dyslexia (Ramus et al., 2003). It was a tense contest, but the phonological deficit theory won. Sadly, we still don't know precisely what phonological deficit, at a cognitive and neural level, actually means. More work is needed.

I believe that my forays into literacy development and disorder were hugely propelled by the possibility of brain imaging. This was an exciting moment in my personal history, and a turning point in my growing collaborations with Chris. Could we see what was wrong in the brain of dyslexics? This was at the time when PET scanning had just become available. Unfortunately, it was suitable only for adults given the potential risks of this method. This meant finding adult dyslexics when previously I had studied only children. The same applied to autism. It was clear to me that dyslexia and autism did not disappear in adulthood, even if behavioral symptoms improved markedly. Dyslexics, in particular, tended to achieve "average" scores on standard reading and spelling tests. For a behaviorist it was unthinkable that they could still be dyslexic. Poor reading and spelling were the very definition of dyslexia, and this was the model I resolved to overturn.

The argument was won with the demonstration that dyslexia manifests differently in behavioral tests in different languages but bears the same brain signature (Paulesu et al., 2001). Italian, for example, is a language with a transparent orthography, so that almost anyone with knowledge of the grapheme-phoneme rules, can pass a reading test, even dyslexics. Guided by the phonological deficit theory, my colleagues and I developed tests that tap into phoneme manipulation (think "pig Latin") and naming speed (e.g., colors or numbers) and thus could act as behavioral markers of dyslexia, in Italian as well as in French and English. The cross-language work in Italy was led by Eraldo Paulesu and, in France, by Jean-François Démonet, with a grant from the European Union. As a result of this project, we were able to show that culture-specific writing systems not only shape the way we read, but also shape the relevant regions of the brain (Paulesu et al., 2000).

My interest in reading, spelling, and dyslexia ran along a parallel track to my interest in autism (Frith, 2013). I justified these parallel research streams with the firm belief that I might be able to understand cognitive development better if I understood what could go wrong in development, and that I might be able to do this by comparing the apparent fragility of different cognitive abilities, both resulting in lifelong, if sometimes subtle, impairments. But how did this way of thinking start? How did I get there?

One inspiration was the ground-breaking work by Elizabeth Warrington and Tim Shallice with neuropsychological patients. For example, they studied in depth two patients recovering from encephalitis and found significant discrepancies in their ability to identify or define different categories of objects (Warrington & Shallice, 1984). Remarkably, they were much better at identifying inanimate than animate stimuli, whether from pictures or words. They were also better at defining abstract words (e.g., caution) compared with concrete words (e.g., cabbage). I was struck that it was possible to find such dissociations and eager to learn if it was possible to find them also in children with developmental disorders. My ambition was

nothing less than to "cut nature at the joints" (Frith, 2012) to identify "modules of the mind."

But it was not just the fantastic idea of circumscribed mental modules that might be glimpsed when they have faults. I also believed that such modules could relate to very abstract capacities. I was never drawn to investigate low-level acoustic processes in dyslexia, and likewise, low-level sensory processes in autism. Perhaps under the nostalgic influence of Gestalt psychology, I was not convinced by a simple forward processing model, and in order to explore cognitive impairments I did not feel I had to start at the bottom level of the processing hierarchy. Instead, I was excited by the idea that primary problems might originate at quite high levels with top-down effects on lower-level perception.

It might have seemed obvious, but it took my brilliant young colleague, Sarah-Jayne Blakemore, to make me think about implications of my work for education. Our book (Blakemore & Frith, 2005) was the result of an intensely enjoyable collaboration.

Incurably Drawn to Autism

I now rewind back to 1965–1966 when any of this was still far from my mind. I was training in clinical psychology and eager to do research. I had already earmarked OCD as a possible topic for a doctorate. But the unexpected happened. As part of the rotation to different departments, I found myself on the Children's Ward. Michael Rutter led a ward round discussion of an autistic child whose parents had come to consult him from as far away as South Africa. I was immediately struck by the little five-year-old boy. He had a bright expression, but did not talk, and I was deeply puzzled about what tests to use to find out what he was or was not able to do. I found an article about perceptual abilities of autistic children by Beate Hermelin and Neil O'Connor (1964) and was astonished to learn that the authors were right there at the Maudsley Hospital. Someone pointed them out to me in the lunch queue of the canteen. They stood out by their almost exotic chic. With great trepidation I approached them, and to my amazement, they invited me not just once but twice to discuss some of their work. I reveled in the privilege. I learned that they had already started on a whole series of psychological experiments on autism that they were to summarize in a monograph (Hermelin & O'Connor, 1970).

This encounter changed everything. The glamorous duo, Beate Hermelin and Neil O'Connor, were to be my most important role models. I compared them to the Avengers, heroes of a then-popular television series (Frith, 2009). I have to talk about them together because they carried out all their research in collaboration and alternated authorship of their papers.

I was overjoyed when Beate, known as Ati, offered to take me on as a doctoral student. She later told me that she had made the decision on the

Beate Hermelin and Neil O'Connor, ca. 1980.

grounds that I had been very critical of one of her experiments. She was unconventional in many ways and never went along with received opinion. I, on the other hand, was shocked that I could have been so forward when I had been overawed by her. The most thrilling part of our discussion was not about autism at all, but about the possibility of a back door into understanding intelligence, precisely by studying individuals who were of very limited intelligence. For example, by comparing children who either did or did not speak, it might be possible to find out something about the role of language in learning and memory.

I started as an apprentice, observing Neil and Ati, while doing a replication and follow-up of an experiment that contrasted recall of random and meaningful sequences (O'Connor & Hermelin, 1967). The astonishing finding was that there was no advantage in meaningful sequences for autistic children. They could repeat back as many of the random as of the meaningfully arranged words. If their memory did not take advantage of meaning, did they not get the meaning?

My doctoral thesis was meant to be about problems of motor coordination in autistic children, but I felt so passionately about the recall paradigm and the promise to find out something about the secret of "meaning" in the brain, that I begged to be allowed to continue along these lines. I embarked on a series of experiments that used binary patterns as stimuli, words and colors. This work gave rise to the rather vague notion of a drive for meaning or "central coherence," which I proposed was weak in autism. I did not know about Bayes at the time, but if I had, I would have used the term "weak priors."

I did not get any further however, until Amitta Shah, one of my early doctoral students, came up with the idea of testing autistic children's

Uta Frith and Neil O'Connor, ca. 1971.

ability to find hidden figures (Shah & Frith, 1983). Much to our amazement, they were very good at it, far better than the comparison group of nonautistic intellectually impaired children. This allowed us to put the spotlight on cognitive strengths in autistic children. Their excellent attention to detail might explain why they paid more attention to an earring than to the person wearing it. It might also explain savant skills, such as excellent rote memory or absolute pitch. These ideas were taken up and refined by a later doctoral student, Francesca Happé (Happé & Frith, 2006). More recently, this work has been extended in terms of Bayesian models (Lawson, Mathys & Rees, 2017). Even before I finished my thesis, Neil O'Connor asked me if I would join him at his research unit, funded by the MRC, and to be located at University College London (UCL). This was my dream job, and I didn't even have to apply for it.

Neil also hired psycholinguist Rick Cromer, fresh from his doctoral work with Roger Brown at Harvard. We bonded straight away. I was incredibly sad that Rick became an early victim of the AIDS epidemic without having had a chance to fulfill his promise. The MRC Developmental Psychology Unit, to give it its full name, was highly productive given its tiny size with two senior and two junior researchers, a research assistant, a technician, and a secretary (Bryant, 1983). Rick and I were given utmost freedom to do the research we wanted to do. We benefited from visitors who stayed for sabbatical periods, such as Jacques Mehler and Jerry Bruner. Rick, Neil, and Ati had a huge influence not only on my work but also on my outlook

Rick Cromer, ca.1985, demonstrating a psycholinguistic experiment: "The wolf is eager to bite" versus "The wolf is easy to bite. Who does the biting?" Young Sarah-Jane Leslie has to act out what the sentences mean and demonstrates who bites whom.

on life. The four of us went to lunch together every day, and our conversation was mostly about art, theater, and literature. There was always time to visit one of the major London museums and browse antiquarian bookshops. It now seems a golden age in which pressure to publish was felt far less than pressure to cultivate style.

Theory of Mind (Mentalizing)

Neil O'Connor retired in 1982 and John Morton became director of the MRC Cognitive Development Unit. This was a much larger unit, with a distinctive ethos that emphasized theory over data collection. In intense discussions, new ideas emerged, and arguably the most startling of these was "Theory of Mind" as an explanation of the social difficulties in autism.

Mentalizing was the name we coined for the awkward "attributing mental states" and for avoiding the awkward expression "having a Theory of Mind." How do we best characterize and explain this novel ability? Is there a brain basis for mentalizing? When does mentalizing originate in development and in evolution? Does a fault in the mentalizing system lead to autism? We tried to answer some of these questions (Frith, Morton & Leslie, 1991). Actually, they are so hard that they are still waiting for answers more than 30 years after Alan Leslie and I discussed them over endless cups of tea. Alan was the key person in proposing a putative mechanism that might underlie this ability. His innovative and ingenious idea was that the ability to process mental states presupposed the "decoupling" of representations of the world from their actual state in the physical world (Leslie, 1987): why does John take an umbrella? Because he *thinks* "it is raining," not because it is actually raining.

Alan Leslie and Uta Frith, ca. 1985, in front of Wilhelm Aurnhammer abstracts.

Alan had observed his own baby daughter laughing with delight when her mother picked up a banana to use it like a telephone. He had something like a lightbulb moment. Little Sarah-Jane, who, after all, relied on getting reliable information about the world from her parents, did not learn that bananas are for speaking into, nor that telephones are for eating. Even at age 18 months, she understood make-believe actions. By inference, she was able to distinguish physical reality from what Alan called "having an attitude to reality." (I cannot resist mentioning that she is now a prominent philosopher at Princeton).

Now, I knew of a study that found that autistic children did not show pretend play (Wing et al., 1977). Perhaps there could be a failure somewhere along the complex path of having and recognizing an attitude to reality, perhaps a decoupling failure. So far, so circular. To get further, we needed to test this hypothesis with a novel task that required mentalizing, which if the hypothesis held up, autistic children should be unable to perform. Probably, it was part of the Zeitgeist that other groups were also interested in testing the development of "Theory of Mind" in children, and its presence or absence in other animals. Premack and Woodruff (1978) had led the way with their paper "Does the Chimpanzee Have a Theory of Mind?" We wanted to ask, "Does the Autistic Child Have a Theory of Mind?" and chose it as the title of our first paper (Baron-Cohen, Leslie & Frith, 1985). We were extremely fortunate to be able to use the false belief task invented by Heinz Wimmer and Josef Perner (1984). Heinz and Josef were to become both lifelong friends and collaborators, cemented by mutual sabbatical visits and co-supervised students. Thus, I was able to work with Josef on theory of mind (Perner et al., 1989) and with Heinz on cross-language studies of literacy acquisition (e.g., Landerl, Wimmer & Frith, 1997).

Simon Baron-Cohen was the exceptional doctoral student who did the testing and later developed his own influential theory of "mindblindness" in autism (Baron-Cohen, 1995). He used the Sally-Ann task, which goes like this: Here are two dolls, Sally and Ann. Sally has a basket; Ann has a box. Sally has a marble; she puts it into her basket. Then she goes outside to play. While she is outside, Ann takes the marble out of the basket and places it in her own box. When Sally comes back, she wants to play with her marble. Where will Sally look for her marble? Typically developing five-year-olds immediately grasp the situation and can explain readily that Sally will look in her basket because she couldn't know that Ann put the marble into her box. She wasn't there when the marble was moved. The interpretation is that they take into account Sally's false belief when they predict where she will look. Amazingly, even quite able autistic children did not reliably predict Sally's behavior on the basis of her belief.

Our hypothesis that autistic children lacked the ability to predict behavior on the basis of mental states did not rely on a single experiment, but was tested with a wide variety of paradigms. For instance, we used a picture-sequencing task, contrasting events involving mental states, and events involving physical states (Baron-Cohen, Leslie & Frith, 1986). Consistent with our claim, autistic children were superior at reconstructing physical cause-and-effect sequences. A favorite task of mine brought out the dark side of mentalizing: Beate Sodian and I contrasted (physical) sabotage with (mental) deception (Sodian & Frith, 1992). Deception, but not sabotage, depends on the ability to attribute beliefs to others and to manipulate them for one's own advantage. Autistic children turned out to be well able to put a lock on a box to prevent a thief from stealing the contents, but they were pretty much unable to keep the thief away by (falsely) stating that the box was locked.

The comparison of good understanding of physical cause and effect and, at the same time, poor understanding of mental cause and effect is an example of "cutting nature at the joints." These are dissociations that lay bare the circumscribed nature of a cognitive problem, and that, following Shallice and Warrington, may help us begin to understand the architecture of the mind. I felt incredibly excited that a fault in a cognitive mechanism could explain a range of diverse and demonstrably impaired behaviors in autism at one fell swoop: poor joint attention, delayed language learning, and failure in conversational turn taking are all behaviors that critically depend on taking into account others' intentions. Other explanations of these features were either too piecemeal, or too sweeping to be useful.

Francesca Happé propelled the research on mentalizing by designing "Strange Stories," which contrast content that does and does not require mentalizing. These stories were the basis of the first functional imaging PET study of Theory of Mind (Fletcher et al., 1995) and the first imaging study with adults with Asperger syndrome—that is, autism without language or

intellectual impairment (Happé et al., 1996). Subsequently, we developed short videos of animated triangles, who interacted either in a way that provoked mentalizing (e.g., tricking one another) or else were perceived as being merely goal directed (e.g., copying) or random. This idea was based on the famous Heider and Simmel animation (1944), which had demonstrated the overpowering urge of observers to attribute desires and intentions even to geometric shapes, as long as they moved interactively. Fulvia Castelli, for her doctoral project, conducted PET scan studies with adults with Asperger syndrome who watched these videos (Castelli et al., 2000, 2002). The imaging studies highlighted a distinct network of brain regions involving medial prefrontal regions as well as regions at the junction of temporal and parietal lobes. We suggested that our results with able autistic adults showed weaker connectivity between these regions. There have been a number of similar studies in other labs, but we still do not know precisely what is different in the brain activity during mentalizing in autism. Nor do we have a computational model for the cognitive mechanism. There is lots more work to do.

But oh—the problems that met my attempts to convey this exciting science story. Even now, I despair at the misunderstandings of mentalizing that stubbornly resist attempts at clarification. I need to make clear, over and over again, that failing or passing a test is not by itself informative, because there are different ways of achieving the same behavior. It is for good reason that we need to keep our focus on cognition and not on behavior. I will come back to this point.

After several years, I realized that it was not just other people who had problems understanding mentalizing, but that I myself had a problem. A very big problem. All the mentalizing tasks we produced, in principle, could be solved within the realm of conscious deliberate thought. So, they could be solved slowly by logic and not necessarily by an automatic process served by a presumably innate mentalizing module. Intellectually able autistic people would be able to "hack" the tests. Indeed, they often performed perfectly, if somewhat slowly, suggesting that they might well apply conscious problem-solving strategies. The question was whether they would be able to show implicit (i.e., automatic) tracking of mental states.

Only after infant researchers had developed ingenious tasks that relied on automatic processes, such as eye gaze, did it become possible to answer this question. We selected autistic adults whose performance on false belief type tasks was perfect. Amazingly, they did not show the systematic anticipatory looking pattern that was shown by ordinary adults (Senju et al., 2009). These are tantalizing results, and it remains to be seen how they hold up and what they tell us. I would not be surprised if only a subgroup of individuals currently diagnosed autistic displayed a profound inability to track mental states. Would this mean the death of the mentalizing hypothesis for autism? Not just yet. If mentalizing isn't what it used to be, autism isn't what it used to be either.

The Changing Concept of Autism

For all questions about autism Lorna Wing and Margaret Dewey, both mothers of autistic children, were my gurus. Lorna convinced me with her arguments that led to the concept of the autism spectrum. Margaret acted as a vital advisor for my book (Frith, 1989) since I wanted to share what I knew about autism with parents like her. My book turned out to be only the first of a flood of books that are now on the market. *Rainman*, the popular film with Dustin Hoffman's well observed portrayal of an autistic adult, may well have been one of the drivers of the surge in autism awareness.

At around this time, people became first aware that there were not only children but also adults with autism. Adults had been ignored by clinicians as well as researchers, and there were hot arguments about whether a person who was verbally fluent and intellectually able could have a diagnosis of autism. Lorna Wing had no doubt that they could, and she encouraged me to look at Hans Asperger's early descriptions of such cases. It was a belated but exciting discovery, which led me to edit a book (Frith, 1991). It contained my annotated translation of Asperger's seminal paper (1944) as well as contributions from the few people I knew who had experience of such individuals, that is, Lorna Wing, Christopher Gillberg, and Francesca Happé. Digby Tantam, a young psychiatrist whose doctoral thesis I was supervising, was another contributor, interested in both autism and schizophrenia. I was also able to persuade Margaret Dewey to contribute a chapter that, through the use of vivid vignettes, laid bare the shattering social problems of even highly intelligent autistic people.

The public response to the concept of Asperger syndrome was overwhelming. Everyone seemed to know someone who was "on the spectrum." This trend is still continuing, but I have to confess that I am not entirely sanguine about it. It is hard for me to look in the same way at the very disabled children who participated in the early experiments and the amazingly accomplished people who are today's favorite experimental subjects. Are they even autistic? I often wonder.

Undeterred by the fact that able autistic people can pass explicit false belief tasks, I remain attached to the hypothesis that there is at least a subgroup, whose social interaction problems are due to implicit mentalizing failure, independent of intelligence. My belief is undiminished that this is the basis for a cognitive phenotype and free from the vagaries of constantly changing diagnostic criteria (Frith, 2012). But for this to be tested robustly, we need psychometrically validated tests of mentalizing. What a pity that such tests don't exist—yet.

Cognitive Neuroscience: A Revolution and a Revelation

Historical change is neatly marked in the names of the three centers where I was fortunate enough to have worked. The information processing

revolution, which changed the course of experimental psychology had deeply influenced my work at the MRC Developmental Psychology Unit. The influence of the cognitive revolution was evident in the MRC Cognitive Development Unit. Finally, I moved to the Institute of Cognitive Neuroscience, which, as the name implies, was part of the neuroimaging revolution.

Throughout my career, I was always able to carry out my own research without any interference. However, it was only between 1998 and 2006, that I had a research group of my own, with two postdocs, a research assistant, and several doctoral students.

Some members and friends of the Institute of Cognitive Neuroscience Developmental Group, ca 2000. Front row from left to right: Francesca Happé, Tim Shallice, Uta Frith, Maria Tallandini-Shallice. Back row from left to right: Eamon McCrory, Frances Abell, Lauren Stewart, Fulvia Castelli.

What was new? We strongly felt a new beginning: a systematic approach to linking brain and mind was surely facilitated by the location of the new institute. Tim Shallice, the founding director, had managed to persuade the provost of UCL to acquire the lease of a substantial building in Queen Square, opposite the National Hospital for Nervous Diseases and Institute of Neurology, and almost next door to the innovative and already world-leading Functional Imaging Laboratory (FIL), equipped with a state-of-the-art PET and magnetic resonance imaging (MRI) scanners. We shared the building with the Gatsby Centre for Computational Neuroscience, which allowed for many fruitful interactions.

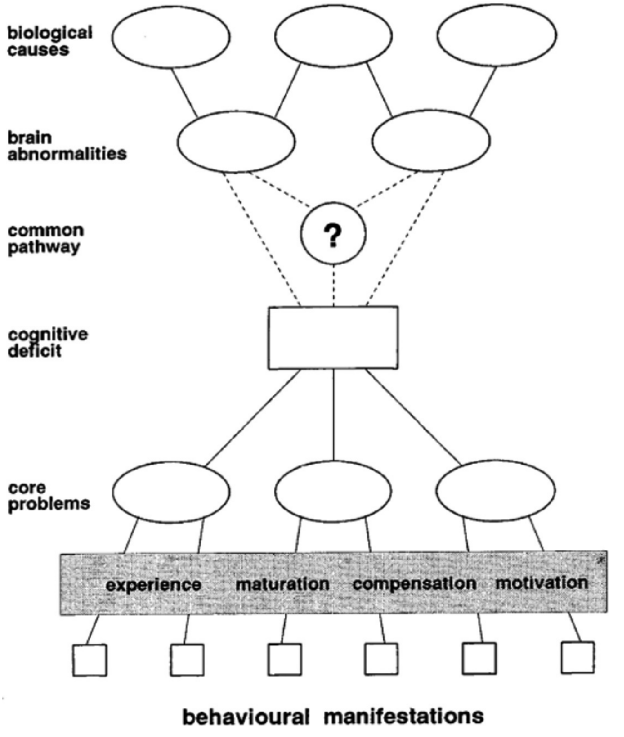
The founding members of the institute included John O'Keefe, Jon Driver, Paul Burgess, and Patrick Haggard, all of whom had their own research groups. All collaborated with the members of the FIL, which included Richard Frackowiak, Chris Frith, Karl Friston, and Ray Dolan, among others. I believe that this intense collaboration resulted in an innovative strand of work with many "firsts." It was inspired by rigorous psychological experiments, animal and human, as well as by neuropsychological case studies. It was only natural that my collaborator in all of the imaging experiments was Chris. A constantly replenished and diverse pool of talented postdocs and doctoral students, many from countries outside the United Kingdom, was an essential ingredient in our success.

Not every cognitive scientist was happy to embrace brain imaging as the method of choice to advance our understanding of cognitive processes. I, on the other hand, could not wait to try out and learn about this new method. The rewards dazzling before my eyes were nothing less than solving the paradoxes of dyslexia and of autism, and perhaps even understanding how the mind creates meaning from information. My enthusiasm was still some years away from the inevitable disenchantment, when it became clear to me that the questions I was asking were simply too difficult to expect to result in quick answers. Worse still, perhaps I didn't have the right questions. Nevertheless, to be able to be among the pioneers of the field was a priceless experience.

John Morton, ca 1998.

John Morton was highly skeptical of what brain imaging could contribute to cognitive models and had prepared me well for disenchantment. Already when he started his unit, he managed to shake me out of my happy-go-lucky attitude to theory and experiments. For a long

time, I had been content to be led by strange phenomena, such as the paradox of being a good reader but a poor speller. I claimed that theories were airy-fairy things and that all I wanted to do was to follow up the phenomena to be rewarded by glimpses of a hidden truth. Eventually, I had a revelation. I suddenly could see that trusted data can turn out to be ephemeral and unreliable, while theories, even if proved wrong, can be important and memorable. Through marathon lab meetings that John had instituted, it had become clear to me that theories and testable hypotheses would make or break the new field of cognitive developmental neuroscience. It was exciting to think that hypotheses created by different theories could be tested against each other, even if it was hard to find the right way to test them. In particular, I was taken by the strikingly simple but controversial idea of innate mechanisms that might be the causes of specific impairments in developmental disorders. It testifies to the strong influence of Chomsky's idea of an innate capacity for language development, which I had absorbed from Rick Cromer. Of course, it is still a bold claim that needs to be questioned continuously (Frith, 2013).



Framework for explaining developmental disorders.

John Morton and I tried to find the simplest possible framework that we could use to evaluate such radical claims and speculate about causes of developmental disorders (Morton & Frith, 1995). It was true back-of-the-envelope stuff. Basically, at the top is the level of the brain. In the middle is the level of cognition. At the bottom is the level of behavior. We suspected that there were many different, presumably genetic, causes lurking at the top level and influencing brain development, but we assumed that they converged on a bottleneck, the place of cognitive mechanisms. It seemed okay to speculate about one mechanism at a time and how it affected a whole range of behaviors. In fact, if you draw this out, it makes an X-shape.

This simple framework turned out to be useful. This was mainly because drawing lines and pointing to spaces was a way of avoiding words, which are often the main cause of misunderstandings. For example, mentalizing is just a node you can point at, occupying a particular space in the model. Of course, we'd like to have a computational model eventually, but we are not anywhere near that yet. Nevertheless, the node becomes more tangible when it is assayed by tailor-made tasks at the behavioral level and physiological measures at the brain level. I feel happy and hopeful when I can fill the ovals with putative content, starting from cognition and tracing lines in both directions, linking brain and behavior

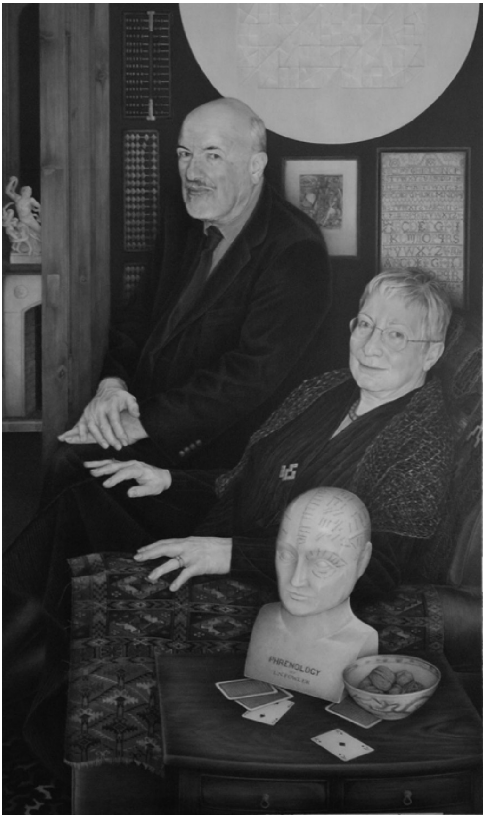
The framework made it apparent that the distinction between cognition and behavior was often deceptively difficult, because behavioral descriptions disguise themselves as explanations. For example, the explanation of poor reading test performance by poor reading ability! Poor reading can have many causes—think of poor teaching or truancy. We saw examples of circularity everywhere. The trick was to take a counterfactual stance: If a key mechanism was missing, what kind of seemingly unrelated behaviors would be affected?

Sunset

Now I am retired and I can do what I like best: read the latest research papers on cognitive development, autism, and dyslexia. I am also feeling passionate about diversity and promoting women in science. Worried by the current replication crisis, I started campaigning for Slow Science (Frith, 2019). By this I mean upholding quality over quantity and standing up against the publish-or-perish culture. In the spirit of this, I am taking time to think and write. Jointly with Chris, we have supplied the content for a graphic novel—a nonfiction book on social cognitive neuroscience. This was then scripted by our son Alex and illustrated by the artist Dan Locke. In a parallel project, we are slowly writing an academic monograph, which is meant to provide the scientific backstory.

I spend most of my time in front of my computer. I check Twitter on my favorite topics. I follow up on new research papers. I have a to-do list that always includes writing a reference, reviewing a paper, preparing a talk, writing a blurb, or arranging speakers for a conference. It is a nice life. I recommend it to those of my colleagues, who wish to never retire. The best is the luxury to critically reflect on research I did in the past, when I did not take enough time to think. The worst is failing health, the inevitable burden of aging. There remains one important goal, and that is a good death. I would be very happy if neuroscience research could be creatively extended to obtain more information on what this means.

If I were to sum up my life in a sentence: I changed country, language, and culture and found happiness in marriage and in work far beyond anything that the fairy tales of my childhood could have prepared me for.



Portrait of Chris and Uta Frith, painted by Emma Wesley, 2008 (https://emma-wesley.co.uk/section/429750-Portraits.html).

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