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MATHEMATICAL REMINISCENCES

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Already in school I was interested in mathematics, but I found physics even more fun and exciting. The reason might have been that it was easier for a good student to go beyond the curriculum in physics than in mathematics, an observation that may have implications for mathematics teaching. For instance I remember reading piles of books on astronomy. After gymnasium I therefore went to a physics program at the Royal Institute of Technology, KTH, in Stockholm. After graduation from KTH in 1955 I began higher studies in mathematics at KTH; a number of inspiring mathematics teachers at KTH such as Åke Pleijel, Tord Ganelius, and Göran Borg may have contributed to my choice of mathematics. In 1959 I took the degree of Teknologie Licentiat[1] with mathematics as major subject. My advisor was Göran Borg, a former Beurling student who had made groundbreaking contributions to the inverse spectral problem for Sturm-Liouville operators.

In 1958 I visited the International congress in Edinburgh. It was a little too early for me to fully appreciate such an event, but I have very clear memories of two talks: Lars Gårding's talk on *Some trends and problems in linear partial differential equations,* because it made a deep impression on me and triggered my interest in partial differential equations, and Grothendieck's talk, because I didn't get the faintest idea of what it was about.

In 1956 Lars Hörmander was appointed professor at Stockholm University and shortly later I began to attend his lectures in distribution theory, Fourier analysis, and partial differential equations. Much of that material was later to become parts of Hörmander's 1963 book on Linear partial differential operators. In 1961 I moved to Stockholm University in order to continue my studies under Hörmander's guidance.

Hörmander suggested to me to prove a theorem on continuation of real analyticity for general partial differential operators. For the proof of my theorem it was necessary to use inequalities for compactly supported smooth functions. To tackle the difficulty that there exist no non-trivial compactly supported real analytic functions, Hörmander showed me that one can use compactly supported cut-off functions in an arbitrary non-quasianalytic Denjoy-Carleman class and use the fact that the intersection of all such classes is equal to the analytic class, a fact that had been proved in the 1940's by the Danish mathematician Thøger Bang. Since it was not possible to use *all* non-quasianalytic classes, a modification of the Bang theorem was needed; this led to [2], which became my thesis for the degree of Filosofie Licentiat at Stockholm University. When I had written the first version of the article on continuation of analyticity, Hörmander had learnt from work of Ehrenpreis about sequences ψ_k of smooth cut-off functions whose derivatives of order up to k obey bounds similar to those of real analytic functions. According to his suggestion I therefore rewrote the article using those functions, which made the intersection

[1] Before 1969, when the postgraduate education was reformed in Sweden and the requirements for the Doctor degree were reduced, the Licentiat degree was somewhat similar to the present Doctor degree, though the Licentiat theses were usually shorter.

theorem unnecessary. To my knowledge that article [1] was the first published use of those functions for that purpose. They were of course later to become crucial in Hörmander's definition of the analytic wave front set [29, ch. 8.4].

In 1962 the International congress was held in Stockholm. It became an especially memorable event for me since my advisor, Lars Hörmander, was awarded the Fields medal. I still remember very well Gårding's beautiful talk about the work of Hörmander.

Author in Paris 1959: note beard, scooter, and empty seat behind driver (see text)

During the academic year 1959/60 I had a scholarship from France and studied in Paris. I regularly attended seminaire Schwartz. I did not understand much, but I have a vivid memory of Laurent Schwartz and Jean Dieudonné sitting in the front row arguing energetically and taking over the show from the poor speaker. My parents visited me in Paris, which gave my mother two "shocks": I had grown a beard and I drove around in Paris on a scooter. My mother told me that she was afraid of the possible third shock that I would pick up a French girl friend, but this — unfortunately — never happened.

For the Licentiat degree at Stockholm University I was supposed to make an oral examination on the first five chapters of Hörmander's book that was about to come out. While I was preparing for the examination the proofs of the book were beginning to arrive from Springer in quantities of around 20 pages per week. I offered to help the author with the proof reading, which was gladly accepted. During several months we had weekly meetings where I got marvellous explanations of everything I did not understand. So, the examination on five chapters was replaced by private lessons on the entire book, an excellent deal for the student!

In 1964 Hörmander left Sweden for a position as a permanent member of the Institute for Advanced study in Princeton. This made me leave the area of partial differential equations shortly after I had started. Hans Rådström, a topologist and professor at Stockholm University, told me about his idea to develop analysis on very general sets by defining a real-valued function f on a set M to be smooth if the composition $f \circ \varphi : \mathbf{R} \to \mathbf{R}$ is smooth for a sufficiently rich set of functions φ from $\mathbf{R}$ to M. In the case $M = \mathbf{R}^n$ this condition meant that the composition $f \circ \varphi : \mathbf{R} \to \mathbf{R}$ is smooth for every smooth function φ from $\mathbf{R}$ to $\mathbf{R}^n$. It was of course expected, but not obvious, that this implied that f was smooth in the ordinary sense. I got interested, and I came up with a proof of this fact [4]. Perhaps I had stolen the problem from Rådström, but he never complained.

During the work with [4] I made the following observation. If a function $f : \mathbf{R}^2 \to \mathbf{R}^2$ has the property that $\mathbf{R} \ni t \mapsto \langle \xi, f(x + t\xi) \rangle$ is Lipschitz continuous (uniformly with respect to x) for every $\xi \in \mathbf{R}^2$, then f must be continuous and its modulus of continuity is $\leq C\varepsilon \log(1/\varepsilon)$. Examples show that f need not be Lipschitz continuous. I was intrigued by this phenomenon and was led to pose a more general problem as follows. Assume that a function $f : \mathbf{R}^n \to \mathbf{R}^m$ has the property that $\mathbf{R} \ni t \mapsto \langle \eta, f(x + t\xi) \rangle$ has modulus of continuity $\le \omega(\varepsilon)$ for a certain finite set Λ of pairs $(\xi, \eta) \in \mathbf{R}^n \times \mathbf{R}^m$. For which sets Λ does it follow that f is continuous, and for which Λ does it follow that f has modulus of continuity $\le C\omega(\varepsilon)$. The problem was solved in [3]. I consider this one of my best works: the problem was rather original, it was completely solved, and the solution can be said to have a certain elegance. However, this judgement seems not to have been shared by my readers, because 40 years after publication in Acta Mathematica the article had received four citations according to a citation index, and three of those were my own! By the way, if you are planning to promote research evaluation by citation counts, which is very fashionable now, please don't expect my support.

The main theorem in [3] will reappear with a simplified proof in [26], written together with a doctoral student from Cameroon, Dieudonné Agbor.

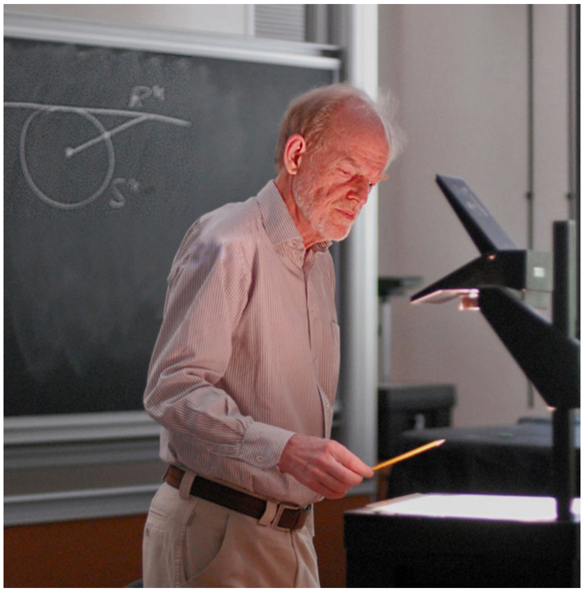
Articles [4] and [3] became my thesis for Doctor of Philosophy at Stockholm University in 1967.

During the 1970's I collaborated with Harold Shapiro concerning comparison between generalized moduli of continuity. This was a way to treat several kinds of problems in approximation theory in a unified way. The starting point was Shapiro's article [32]. Together we wrote [5] and [6], and later I wrote [8] with more complete and precise results. During this period I visited many conferences in approximation theory, mostly in Poland, Hungary, Bulgaria, and the Soviet Union. I was struck by the oldfashioned character of much of the research being done in this area at the time. Distribution theory, which had been successfully used for 25 years in the PDE community, was practically unknown in the field. I wrote a small note, *Saturation problems and distribution theory,* [7], which became perhaps my most widely read work so far. It was published as an appendix in Shapiro's *Topics in Approximation theory.*

I got tired of Approximation theory and looked for something else. Again Harold Shapiro gave me inspiration, this time by drawing my attention to a problem of closedness of the set of sums of a finite number of closed subspaces of $L^2(\mathbf{R}^n)$, which comes up in connection with description of the range of the Radon transform. This led to the article [9]. From then on Radon transforms has been my main theme of research.

I quickly noticed that tools and ideas from PDE theory that I had learnt from Hörmander in the early 1960's were quite useful in the study of Radon transforms. In the early 1980's I therefore began to study Hörmander's work again. For instance, I was fascinated by his big article from 1970 on Fourier Integral Operators. With Todd Quinto I wrote the two articles [11] and [14] which used the analytic wave front set for proofs of uniqueness results for generalized Radon transforms. This was the beginning of a long and fruitful collaboration with Quinto and many visits to Boston, a city that I like very much.

In Boston I also got the chance to meet Sigurdur Helgason, the founder together with Israel Gelfand of the modern theory of the Radon transform. I have met Helgason at many conferences on Radon transforms and tomography, and he and his work has been a great source of inspiration for me. Helgason's well known support theorem for the Radon transform says the following. If a continuous function f on $\mathbf{R}^n$ satisfies $Rf(L) = 0$ for all hyperplanes L not intersecting a compact convex set $K \subset \mathbf{R}^n$, and f decays at infinity faster than any negative power of $|x|$, then $f = 0$ outside K. In a new proof, [15], valid for weighted Radon transforms with real analytic and positive weight functions ρ, I showed that the role of the decay condition here was related to the fact that a real analytic function decaying infinitely fast at a point must vanish identically. In the proof the problem was transformed to projective space $\mathbf{P}^n$ using the standard imbedding $\mathbf{R}^n \hookrightarrow \mathbf{P}^n$.



Author lecturing at Luminy in April 2007

While visiting Frank Natterer and his group in Münster in 1985 I learnt about the attenuated Radon transform, a class of weighted Radon transforms that were inverted in SPECT, Single Photon Emission Computed Tomography. This method was already in use in hospitals, although there was no uniqueness theorem supporting the fact that the method could work. Since nobody had any idea about how to use the special properties of the attenuation weights in a proof of such a theorem, some mathematicians tried to prove that the weighted plane Radon transform $L \mapsto R_\rho f(L) = \int_L f(x)\rho_L(x)ds$ is injective on the set of functions f with compact support for arbitrary smooth and strictly positive weight function $(L, x) \mapsto \rho_L(x)$. The fact that $R^* R_\rho$ is an elliptic pseudodifferential operator may have appeared as a support for this conjecture. However, the fact that there are examples of elliptic differential operators with smooth coefficients that have non-trivial solutions with compact support made me sceptical, so I turned in the opposite direction and constructed an example of a smooth and positive $\rho_L(x)$ and a non-trivial function f with compact support for which $R_\rho f(L) = 0$ for all lines L, [10]. From then on it was clear that one needed to find conditions on a smooth or sufficiently differentiable weight function $\rho_L(x)$ which imply that R_ρ is injective and has a well-behaved inverse.

Talking about counterexamples, I think that trying to construct counterexamples is often a useful way to find proofs of theorems, because when you have located the obstacles to such a construction you may get ideas for a proof of your theorem. But in some cases work on counterexamples results in — a counterexample, for instance in [12], [13], [19].

For many years nothing happened on the problem of the attenuated Radon transform. A big step forward was taken by Roman Novikov in 2001 when he gave an explicit inversion formula for R_ρ for an arbitrary attenuation weight ρ, [30]. Since then several alternative proofs and slight extensions of Novikov's result have appeared, e.g. [22], but the class of weight functions ρ for which injectivity is proved has been only marginally extended (see also [31] and [27] in this issue). The problem to understand for which ρ the operator R_ρ is injective is still wide open.

The two crucial steps in my extension of Helgason's support theorem mentioned above were: (1) if a function on the sphere S^n has zero integral over all great spheres L in a neighborhood of some great sphere L_0, then $WF_A(f) \cap N^*(L_0) = \emptyset$, that is, each conormal to L_0 must be absent in the analytic wave front set of f, and (2) if a function on S^n satisfies the last mentioned wave front condition and also vanishes of infinite order on L_0, then f must vanish in some neighborhood of L_0. The statement (2) was easily proved using an argument [29, Theorem 8.5.6] from Hörmander's proof of Holmgren's uniqueness theorem [29, Theorem 8.6.4]. A *local* analogue of that statement was not quite so obvious, but turned out to be true; I called it *A vanishing theorem for distributions*, [16], and it says the following. Let S be a real analytic submanifold (of arbitrary codimension) of a real analytic manifold M. Assume that the distribution f on M is flat on S in the sense that the restrictions to S of all (distribution) derivatives of f vanish, and that f satisfies $WF_A(f) \cap N^*(S) = \emptyset$. Then f vanishes in some neighborhood of S. The reason why this relatively simple fact had not been observed earlier was probably that the experts in microlocal analysis knew that Mikio Sato had shown that it was not true for hyperfunctions, and so far Hörmander's theory of microlocal singularities for distributions had been quite parallel to Sato's theory for hyperfunctions. But I did not know about Sato's counterexample, so for me it was quite natural to try to prove the statement for distributions. So this may have been one of the rare occasions when ignorance was an advantage! Using the vanishing theorem I could prove an extension of Helgason's support theorem where rapid decay is assumed only in certain directions, [17]. The vanishing theorem directly implies a theorem on unique continuation of CR-functions that has been given by Baouendi and Trèves, and it has been used by Lebeau for proving a strong uniqueness theorem for solutions to wave equations. Due to my fascination with quasianalyticity, which began thirty years earlier with [2], I could not refrain from extending the vanishing theorem by allowing f to be a *non-quasianalytic* ultradistribution and by replacing the analytic wave front set by the wave front set with respect to an arbitrary *quasianalytic* class, [18].

After retirement from my position at Stockholm University in 1998 I have been very fortunate to be able to continue my work and keep contact with my many mathematical friends and colleagues at home and abroad, [21], [20], [22], [23], [24], [25], [26], [27], [28].

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Author performing a Mozart string quartet in his home together with friends in 2003 (celloist not visible)