Bratteli–Robinson 1974–2014

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Those were the days my friend, I thought they'd never end.

Gene Raskin 1962

My intention is to describe some details of the nearly forty years that I collaborated with Ola. The talk will be largely historical rather than mathematical although it will be difficult to avoid some mathematics. Over the period that we collaborated we spent surprisingly little time in the same place although we met many times in many different places. So things were complicated. But first a warning. Since I am not a historian what follows will be an informal colloquial description of years gone by, not necessarily in the order they occurred.

Early in my career I learnt that there were three essential things to know about history. The first is that 'it is all behind us', which is certainly true of my collaboration with Ola. Truly and very regrettably. Initially neither of us ever envisaged that we would form an enduring research partnership and I regret that it came to a premature end two years ago. I also deeply regret losing so many other friends and colleagues in the last two or three years, friends and colleagues who played a role in our collaboration: Rudolf Haag, Daniel Kastler, Bill Arveson, Uffe Haagerup, John Roberts, Oscar Lanford, Alan McIntosh. Clearly I have entered the 'Four funerals and a wedding' phase of life.

The second point is that 'history has no plot' and that can certainly be said of the collaboration with Ola. When we met we had quite different backgrounds, very different personalities and totally different educations. None of these differences affected, however, our ability to collaborate efficiently and it was always a great pleasure working with Ola. Our collaboration often appeared to progress by osmosis rather than direct interaction.

I have forgotten the third all important historical rule. But maybe it was that history is unreliable because it largely depends on the memories of the protagonists and these memories are often coloured by subsequent reconstructions and interpretations. Certainly the following account depends on my memory of distant events and the dates and details should not be totally trusted. There are alternative histories but not alternative facts.

I do not remember exactly when I first met Ola, it was sometime at the beginning of 1974. I nevertheless remember exactly where I met him. It was in the foyer, if you could describe the bottom of a staircase as a foyer, in the then home of the CNRS Department of Theoretical Physics in Marseille. This was a small building just off the Boulevarde Michelet and not far from the famous Corbusier Unité d'Habitation. At that time I was Professor of Physics at the Luminy Campus of the Université d'Aix-Marseille. This campus had been founded shortly after 'les événements de mai' in 1968. In fact it only came into being in 1969 some six months after I had been appointed. Hence there were no offices initially, in fact there were very few buildings, and that was why the theoretical physics group was housed at the CNRS site.

Ola was introduced to me by Trond (Digernes) whom I had met earlier in 1971 in Los Angeles. Our meeting in the CNRS was to be a pivotal moment in each of our lives, although we had no premonition of this at the time. There were many unpredictable consequences: a few years later Trond was to meet his wife Halle at an open air opera in Sydney, Ola became the owner of a mushroom farm in Northern Thailand and Ola and I were to write a book that is still bought, read and regularly cited 35 years later.

Let me start the story in 1971 when I first met Trond. In fact it was a good year from many points of view. Ola was a graduate student in Oslo with Erling (Størmer) in 1971 and completed his now famous, influential and often cited work on AF-algebras. It appeared in the September 1972 edition of the Transactions of the American Mathematical Society. He then continued his graduate studies at NYU. Trond at that time was a graduate student in Los Angeles with Masimichi (Takesaki) working on operator algebras. I, on the other hand, was entangled in several different areas of mathematics and physics, e.g. quantum field theory, particle physics, operator algebras, quantum statistical mechanics, spin systems etc. During the 1971 trip to America when I met Trond I also wrote a paper on AF-algebras with Elliott Lieb. In contrast to Ola's work our paper passed without notice. But 35 years later it suddenly obtained recognition and is now very frequently cited in the physics literature. I will return to that later.

You might well wonder what two Norwegian graduate students of mathematics and an English theoretical physicist were doing together in Marseille at that time in 1972 and how did they come to have common interests in operator algebras. To explain that I have to jump back a few more years. The starting point for our common interest in operator algebras and theoretical physics began for me in the late 50s when I was a graduate student in Oxford working on nuclear physics. I read a lot during that time and as a consequence my personal interests turned toward quantum field theory. One paper I read, in the Journal of the Danish Royal Society, which particularly fascinated me was by Rudolf Haag. It considered representations of the algebra associated with fields satisfying the canonical commutation relations. A key abstract idea was that the representation describing interacting particles could not be unitarily equivalent to the representation describing free particles. This clarified an earlier, more specific, result of van Hove. It was the bud that bloomed slowly into algebraic quantum field theory. Altthough I tried to understand these developments I had no opportunity to work in this area until I had completed my rather mediocre thesis on the structure of deformed nuclei. That was in 1960. But I then had the good fortune to spend two years as a NATO postdoctoral fellow at the Eidgenössiche Technische Hochscule in Zürich. In 1960–61 there was a small but active group headed by Res Jost working on quantum field theory. David Ruelle also had a postdoctoral position at the ETH during the same period and in 1961 his interest turned to obtaining precise mathematical results in statistical mechanics. Simultaneously Rudolf Haag had also ventured into this area and had given a precise analysis of the Bardeen–Cooper–Schrieffer model of superconductivity based on algebraic representation theory. At that time there no particular interest in algebraic methods in Zürich although this changed in 1961–62 when Huzihiro Araki visited Zürich and gave a lecture series on von Neumann algebras. Araki completed his doctoral work with Rudolf Haag in 1960 at Princeton and his thesis contained a detailed development of Haag's 1955 theorem giving a precise link between different interactions and distinct representations. The exposure to these various sources was a strong influence on my subsequent work.

After the completion of my NATO fellowship in 1962 I was fortunate to meet and work with Rudolf in Illinois. Quite coincidentally Araki also spent the year 1962–1963 in Rudolf's group as did Daniel Kastler. Daniel had a sabbatical year from his position as Professor of Theoretical Physics in Marseille. He had met Rudolf at the 1958 Varenna Summer School and was impressed with the idea of exploiting algebraic methods to understand the local structure of quantum field theory. As a consequence he had invested considerable time and energy reading the Murray–von Neumann papers and learning all he could about the theory of C^*- and W^*-algebras. These papers were one of the few sources of information on operator algebras, together with the book by Gelfand and Naimark and the books of Dixmier. The fruits of Daniel's labours were then harvested in the 1964 paper with Rudolf 'An Algebraic Approach to Quantum Field Theory' which attracted a great deal of attention from both the physics and mathematics communities. This paper had a significant impact which went beyond its mathematical content since it reinvigorated Daniel's efforts in developing mathematical physics in Marseille. A development which led ten years later to Ola and I meeting at the CNRS.

Let me at this point leap forward a bit in time, until 1968 when I moved to Marseille for the beginning of a ten year stay. In 1965 I visited Marseille for a couple of days and then later that year I began a six month visit. I had started to make some progress in the algebraic approach to statistical mechanics developing the notion of quasi-free states and I was interested in their application to superconductivity etc. Sergio Doplicher also visited Marseille in 1965 for the first three months I was there and we worked on covariance algebras, now more commonly known as crossed products, and asymptotically abelian systems. Our visit was sufficiently successful that Daniel started a campaign to appoint me to a permanent position. This took two years. In fact before 1968 it was not legally possible for a foreigner to be appointed to a tenured position in a French University but this changed with the reforms following the uprisings of 1968 and in September of that year I moved to Marseille. To be exact I moved to Bandol, the small coastal village 45kms east of Marseille where Daniel and his family lived. Subsequently my wife, Marion, and I built a house outside of Bandol on a hill overlooking the mediterranean. It was an ideal setting for mathematics. By 1968 Daniel had been successful in establishing his visitors program and there began to be a regular flux of visitor from all parts. Erling was one of the earlier ones, in 1967 I believe. By good fortune I was also visiting at that time from my then position at CERN. (I still had an interest in particle physics.) In fact I met Erling, and many other people, for the first time at the Baton Rouge conference on Operator Algebras. This was a seminal meeting as it brought together for the first time many of the mathematical physicists working in the area with the establishment mathematicians. It was the first time that many of the new elements of the burgeoning theory were brought together, from the theory of KMS states, asymptotically abelian systems, invariant states, non-isomorphic Type III factors, etc. But back to Marseille.

Organizing a visitor's program in the French system in the 1960s presented many practical problems in those years, and possibly this has not changed. All paperwork had to go through Paris and that led to unpredictable delays and errors. (When I was in Marseille for six months of 1965-66 as a Professeur Associé my salary did not get paid until four and a half months had passed.) But as time went by Daniel learnt to game the system and there were an increasing number of longer term visitors on sabbatical leave or travelling scholarships and fellowships. Ola and Trond fell into the latter category with support from the beneficent Norwegian government.

At the time I first met Trond after his arrival in Marseille in 1974 I was thinking about derivations of C^*-algebras as these appeared significant for the description of physical symmetries and time evolution. The main problem was that the only general mathematical theorems for derivations were, at that time, for bounded derivations. These were of little interest in the context of symmetries since the corresponding generators were analogues of differential operators and consequently were unbounded. Hence I was trying to understand what were the general features of unbounded derivations that could be useful. I knew that densely-defined symmetric operators on Hilbert space were automatically closeable and I thought that there was possibly an algebraic analogue. Could it be that a densely-defined symmetric derivation on a C^*-algebra is automatically closeable? I thought that this was not unreasonable and asked Trond if he knew the answer. After assuring himself that it was not obvious he said he would think about it. The following week I ran into him in the foyer I mentioned earlier in the company of another person whom he introduced as Ola Bratteli. Trond also immediately followed up this introduction by mentioning that he had referred my question to Ola who had constructed a counterexample. This had me excited, not a lot but a little, since I was expecting a positive rather than a negative example. Up to this point Ola had said very little but then he broke into an explanation which I did not fully understand, it certainly involved Cantor sets, of which I had scant knowledge, and also seemed to be for abelian algebras. Somewhat later that afternoon he explained things in a bit more detail but since I was primarily interested in non-abelian algebras, even relatively simple ones such as UHF-algebras, I pressed him about more complicated situations. That was the beginning of our collaboration.

Things went quickly. In a few weeks we had written most of our first paper on unbounded derivations of C^*-algebras. One of the themes of the paper was closeability and we gave a couple of criteria and also examples of non-closeable derivations. The principal example we gave was on a UHF-algebra generated by an increasing sequence of matrix algebras on which the derivation was identically zero although the derivation itself was not zero. This was constructed by extension of Ola's original argument for abelian algebras. Several years later Ola mentioned to me that he regretted that he had not written a more comprehensive description of derivations on abelian algebras. It was certainly something he understood well and which various other authors wrote about some years later. I regret that I did not encourage him more but I was focused on non-abelian situations and quantum mechanical applications.

Our first paper also gave criteria for a derivation to generate a strongly continuous group of *-automorphisms. The latter were in large part adaptations of standard results of semigroup theory. The special feature which, in hindsight, I realize we did not fully appreciate was positivity. In fact semigroup theory which was widely viewed as a well-developed and largely complete theory was severely deficient in respect to the analysis of positivity properties. The one notable exception was a 1962 paper by Ralph Phillips published in the Czechoslovakian Mathematical Journal and which had apparently lapsed into obscurity. This paper gave a very nice version of the Hille-Yosida theorem tailored to positive semigroups. We only discovered this paper in 1980 and analyzed its implications for C_0-semigroups on C^*-algebras in a paper in Mathematica Scandinavica. In fact this small project was prototypical of much of our collaboration. I discovered the Phillips paper just before leaving Sydney for a two week visit to Zürich and Ola, who was in principle in

Oslo but was actually visiting Marseille for four weeks, drove up and stayed for six days. That was sufficiently long for us to write our paper. Somewhat later in the 1980s there were many other developments in the analysis of positivity properties. In particular it was realized that a 1973 inequality of Kato for the Laplacian could be adapted to give a criterion of positivity for quite general semigroups. Moreover, the theory of Dirichlet forms and submarkovian semigroups really began to develop in the 80s and 90s.

We were not the only people interested in unbounded derivations in the 1970s. Powers and Sakai were working in this area but mainly on UHF algebras. Their belief was that each symmetric derivation on such algebras was the limit of bounded derivations. Alternatively stated the derivation was the asymptotic limit of inner derivations. This is a topic that Akitaka will talk about after lunch so I will not go into further details. But the principal method that was used, at least initially, was to analyze the functional properties of the domain of the derivations. Our first paper also addressed this problem and we showed that if $A = A^*$ was in the domain of a closed derivation δ on an abelian C^*-algebra then $f(A)$ was also in the domain for each continuously differentiable function f on the real line. In our second paper we gave a somewhat weaker statement that was valid for general C^*-algebras with identity and was based on the observation that the identity was automatically in the domain of δ and $\delta(\mathbb{1}) = 0$. In the interim Bob Powers, who had gained fame with the construction of a one-parameter family of non-isomorphic Type III factors in his PhD thesis, had extended our abelian result to general C^*-algebras but unfortunately his proof was wrong. I was able to construct a counterexample to one vital step in his proof but two years later, on my first visit to Australia, I mentioned the problem to Alan McIntosh and he gave a counterexample to the general statement. Fortunately Bob forgave me pointing to the error of his ways and we later became good friends and collaborators.

In the period 1974–1976 that Ola and I wrote our first three joint papers we were both in Marseille, but more in theory than in practise since we were often travelling. I remember that a significant part of our second paper was developed when I was in California visiting Berkeley and Ola was in Marseille. It was during this period I met Bill Arveson and we also became good friends. It was then that I met Tosio Kato for the first time. His book on Perturbation Theory had been very influential for me. In particular I learnt almost all that I then knew about semigroup theory from this book. It was also the key resource on quadratic forms and positive self-adjoint operators. In those days, of course, there were no computers and no email so the only means of communication that Ola and I had was ordinary mail. This certainly slowed things down but it was to become useful experience for us when we later wrote the second volume of our book. At that point I was in Australia and Ola was in Norway. But I will come to that a bit later. Another thing that slowed us down in those early days was an accident that I had in which I squashed three vertebrae in my back. This put me in hospital for a short period but kept me in bed, supine, for several weeks. The hospital in Toulon wanted to fit me with a jacket that would keep my back rigid but it turned out that I was too tall for all the jackets available. Therefore they constructed a plaster cast which cased the whole upper part of my body. As it was seriously warm in the South of France they left a large round aperture in the front of the casing for cooling purposes. I resembled a frontloader washing machine with legs. I had to wear this construction for five months and the weeks in bed were decidedly uncomfortable. But as Autumn arrived I could move around and conduct business as usual. It was during the period I was bedridden that Ola and I worked on our fourth paper. He often visited

Marion and I in the period I was confined to bed and although I was limited in movement we found that was no impediment to doing mathematics.

By Autumn 1975 I was essentially recovered, my cast was removed and I could resume a normal life. It was at this point I received an invitation which was to change my life and to a lesser extent those of Trond and Ola. The invitation was to lecture at a Summer Research School organized by the Australian Mathematical Society in Adelaide. The prime mover behind this invitation was Angas Hurst whom I had met at a similar summer meeting in 1961 in Yugoslavia. Since I had never been to Australia, although my parents flirted with the idea of emigrating there in the years after the second world war, I was very excited by the prospect of the trip. I was also apprehensive of sitting in an aeroplane for 24 hours so soon after my accident. As it turned out the total journey from Marseille to Adelaide took 42 hours as I had to detour via London and Sydney. In Adelaide I lectured on various topics including the work with Ola on derivations but mainly on the more physical aspects of my work. I returned to Marseille in February 1976 excited by all the new experiences and also seriously overweight partly as a consequence of my prolonged recuperation period but also because of the excellent Australian food and wine. The latter was a particular surprise as Australian wine was not at that time widely known in Europe and definitely not in France. At that period I believe that I was probably heavier than Ola although the French food and wine helped him forge ahead and subsequently I never challenged again in this domain.

Two things happened after my return from Australia. First, full of enthusiasm for Australia, Marion and I began to think of moving there at some point in the future and I wrote to a couple of people about possibilities. This was foreseen as a longterm project. Secondly, after my lectures in Adelaide Angas had begun to encourage me to write something more extended about operator algebras and mathematical physics. In fact I had already attempted that during a months stay in Groningen in 1972, a month during which Marion and I simultaneously gave up smoking. It was a memorable month for the latter but not for the former. I did fill three exercise books with draft material for a book but I realized that I was not sufficiently prepared to write the mathematical background. Then around June 76 I suggested to Ola that we should write something together. The suggestion was a bit of a surprise to him but within a few days he agreed. This was intended as a relatively short term project. My idea was to write something about 3-400 pages with a couple of chapters on mathematical background and a couple of chapters on applications to physics. Since the major applications were at that point to models of statistical mechanics the book would be Operator Algebras and Quantum Statistical Mechanics. We started work in September 76. But the best laid plans of mice and men oft go awry, to paraphrase Robbie Burns.

In October 76 the University of New South Wales, in Sydney, advertised a Chair in Pure Mathematics. I applied and in February 77 flew out to Sydney for an interview. Shortly thereafter I was offered the position. So the longterm project of moving to Australia became a very short term project and we started to plan our move for January 78. During this period Ola and I were working and writing and by September 77 we had the equivalent of 500 printed pages of material which exceeded somewhat our estimated length. That was the good news. The bad news was that we were only half way through the planned material. So the short term project turned out to be a long term project and the book changed from one volume to two. It also meant that the second volume was largely written with Ola in the Northern Hemisphere and me in the Southern Hemisphere.

Now I will try to explain how we planned, organized and wrote the book. First we quickly agreed on the outline as far as individual chapters were concerned. The final plan was for six chapters, one a brief introduction on the background of the material to be covered, three on mathematical topics and two on the more physical aspects. We started immediately with the second chapter on the general theory of C^*-algebras and von Neumann algebras. We then made a tentative sketch of the intended sections. Next we each took primary responsibility for one or other of the sections. Then we would discuss the general presentation of the material in each section. After these preliminaries we would begin to write drafts independently. For example, I would draft the first section on general algebraic structure and Ola would draft the second section on representations of algebras. Then we would swap the drafts and each edit the others work. This process would be repeated until we were each satisfied with the outcome. I am not sure whether this is a standard procedure with coauthors of books but it worked well with us. The editing was not a superficial process since we often had different notions of the relative significance of the material and the emphasis to be given to various statements and results but this would be ironed out in the various exchanges. At times my first draft would be completely changed by Ola and vice versa. Somehow the process always reached equilibrium after a reasonably short time, with one exception. This procedure also had various advantages. It naturally introduced a uniformity of style. It also gave a fairly foolproof method of avoiding error, although we were not completely successful in that respect.

There were two style decisions that we agreed on at the beginning. The first was to eschew footnotes. This artifice is used by many authors as a means of citing the relevant literature. We, however, decided that we would not give references in the main text but add notes on the background and references in sections at the end of each chapter. This also allowed us to introduce some digressions from the main themes. The problem with this method is that one can easily offend colleagues by false attribution of priority and just by oversight of their contribution. I am not sure whether we did offend anyone but we never had any complaints so I think our 'history' was probably reliable. The second decision was to include as many examples as we could envisage and construct. This developed into some serious work at times because we often found that there were illuminating aspects of specific examples that had not been explored.

The section which caused the most difficulty was the section on Tomita–Takesaki theory. This theory had its origins in a paper by Tomita which he tried unsuccessfully to talk about at the Baton Rouge conference. It was clarified in the years after that meeting by Takesaki but it was still a very opaque theory. Ola made a brave attempt to describe it in a transparent way. Unfortunately I did not find the end result at all transparent so I completely rewrote it. Then Ola was dissatisfied with my presentation and rewrote it once more. Finally we had seven drafts before we came to an agreement. We were both pleased a couple of years later to hear that Alain Connes was recommending our description as the best introduction to the material.

The collaborative exchange process we developed for the chapter on the general algebraic theory was continued for all the other chapters. It meant that all parts of the book, with a couple of exceptions were really written by both of us. It also meant that we were not guilty of just transcribing proofs of other authors or copying text directly out of research papers. But we did run into some problems of presentation even where least expected. For example our third chapter was on the theory of one-parameter groups and semigroups of operators. This theory was well developed for strongly continuous groups and was immediately applicable to the C^*-algebra theory. But for W^*-algebras one needed to consider weak*-continuous semigroups. The change of topology affected several of the standard results and we had to sort this out. Ola then made the suggestion that we should develop the theory in the language of dual topologies and this gave a unity to the description. It is only in the last five years that I have seen another genuine attempt at unification. In addition Ola suggested we should consider Jordan algebras and positive maps. I think he had learnt about such things through his days as a student of Erling. He was well versed in this theory but it was a topic I learned in the writing.

A different type of problem occurred with Chapter 4 on Decomposition Theory. The theory of decomposition of representations was well established but the theory of decomposition of states of operator algebras was much more recent. There had been lots of developments in the late 60s and early 70s and I had been heavily involved with several of them. The literature was a melange of Choquet theory, operator theory, functional analysis, ergodic theory etc. involving central, subcentral, extremal and ergodic decompositions. Christian (Skau) had written a very nice paper on the subject but we had to try and unify results by many authors who had used disparate notations, definitions and techniques. Both Ola and I were very satisfied with the end result and am far as I am aware that is still the final definitive description of the theory.

The theory of decomposition of representations was a different kettle of fish. This originated with von Neumann in the late 40s and had undergone very little change. I was not sure whether it was worthwhile including a description as it was already well covered in other books such as those of Dixmier. But Ola bravely volunteered to write a version and although I read it and made a few minor changes it is all his own work.

We had finished writing Chapters 2, 3 and 4 by August 77 and then Ola went on holiday. So in his absence I also did a bit of privateering and wrote Chapter 1 on the motivation for the theory. When Ola returned and I gave him my draft manuscript he let out an enormous sigh of relief and admitted that it was the one thing he had not looked forward to writing. He did suggest a few small changes but other than that I can claim it as a personal contribution. These were the only two deviations from our combined, unified, writing method.

We realized that summer that we had begun to write the book with no specific plans for its publication so we began to discuss possible publishers. But any potential difficulties were overcome by a visit of Walter Thirring to Marseille. He apparently heard about our project and immediately came to my office and suggested he could have it published in the Springer series, Texts and Monographs in Physics, of which he was an editor. Since he had not seen any of the manuscript this was a very flattering offer that provided an easy solution to the publication problem. In fact we subsequently had a couple of difficulties with the publishing house which left us doubting whether we had made the correct choice. Anyway we did proceed and submitted the first four chapters as the first volume of the book.

In September 77 we began to see the problems ahead. I was due to leave Marseille at Christmas and so that only left us a bit over 3 months to write second volume. On top of that I had to teach full time for two of those months. (One of the reforms introduced in Luminy after the 'revolution' of 1968 was to introduce block teaching. A typical course would be taught for sixteen hours a week over two months instead of four hours a week over eight months.) But we decided to do as much as possible in the time remaining.

Chapter 5, the first chapter of Volume 2, was a mixture of well-established material, i.e. the free bose gas and the free Fermi gas, and new material only developed in the immediately preceding years, i.e. the structure of KMS states. Somewhat miraculously we managed to write the major part of this chapter before I was due to leave. One thing that helped was the arrival of Akitaka Kishimoto. He was an enormous help with the analysis of stability properties of KMS states. This led to a separate research paper the main results of which were integrated into the book. In retrospect I am amazed that we accomplished so much in such a short time but we nevertheless were far from finishing the whole book. Parts of Chapter 5 and all of Chapter 6 remained.

My recollections of the first half of 1978 are rather blurred. Marion and I, together with our two young daughters, arrived in Sydney late in January and the first semester of the Australian academic year began toward the end of February. I think the book writing came to a halt. But I also believe the proofs of Volume 1 arrived in this period and they provided a considerable amount of work. They also provided one major surprise. Springer had unilaterally decided that all proofs would be in a different smaller typeface than the general text. Unfortunately they had never asked us to mark the ends of proofs. Therefore the decision on the points at which the typeface should change had been made by some member of the Springer editorial staff. They clearly did not understand the text. There were the inevitable errors to correct in the proofs but large expanses of typeface had to be changed. This was still in the era that the typesetting was done in the traditional Gutenberg manner so this involved a considerable amount of extra work and cost. Springer expected us to pay for it. This was the first point that I had doubts about our choice of publisher. I remember corresponding with Ola about the problem and he agreed that I should try and negotiate with Springer. I was not in the mood for any form of friendly negotiation and my letters to Springer and the entire editorial board of the Texts and Monographs series reflected my vehemence. Fortunately Springer accepted that the error was on their part and waived the costs. There was one other unfortunate consequence of the typeface incident. Reading so much small type in such a short period caused my eyes to deteriorate and for the first time in my life I needed glasses.

The next time I saw Ola was in the Australian midyear break, which would have been late June, early July 78. I returned to Marseille for a month and our collaboration started up again. I recently checked that the paper with Akitaka and Ola on stability and the KMS condition was submitted for publication at the beginning of March 78, after I had moved to Australia, so I assume the version that appears in Chapter 5 had been written earlier or that we wrote it that month. Akitaka was still in Marseille and the three of us wrote a paper on the ground states of quantum spin systems. Ground states were a topic of Chapter 5 and a section of Chapter 6 dealt with ground states of spin systems. So I infer that we were completing Chapter 5 and thinking ahead for Chapter 6. Certainly after my return to Australia we began to seriously write about spin systems. About the time I returned to Australia Ola and Akitaka also left Marseille. Ola returned to Oslo and Akitaka moved to Ottawa.

The next eleven months Ola and I continued our collaboration regularly by mail. Remember that there were no convenient personal computers at that time and certainly no email. Although Steve Jobs and Steve Wozniak, unbeknownst to us, were producing the first prototype of the Apple computer in a garage in Colorado and the revolution in communication was on the horizon. Unfortunately it was a bit too late to help us with the task of completing Chapter 6. Fortunately there was a a reliable airmail system between Australia and Norway. Typically one of us would write a section of manuscript mail it to the other who would edit it and mail it back. The whole exchange took two weeks. It was not ideal as there was no opportunity to discuss the editing face to face. At times it was also very frustrating as it broke up the continuity as topics changed every fortnight. Nevertheless we were able to proceed with this system and we adapted to the routine. Then, however, there was a disaster. International airmail passing through Sydney was handled at a postal exchange in the centre of the city and at the beginning of 1979 the workers all went on strike. I have no memory of their grievances, justified or not, but the strike lasted two months and during this period there was simply no overseas mail. Again our collaboration ground to a halt. But life went on.

After the end of the Sydney postal strike our collaboration rebooted and we made serious progress on the final chapter of the book. Then in the midyear break we both returned to Marseille and began the final work. In fact we set up in Bandol in one of the katikias apartments. We had four weeks to complete the book and it took us three and a half. Although the book was in some sense completed there remained a great deal more drudgery. There was no TEX in those days and typing mathematics depended on the now archaic typewriters with interchangeable balls which allowed for a wide variety of mathematical notation. It was impractical to prepare any kind of usable draft by this method and all manuscripts had to be written by hand and then typed by an expert typist, an expert with a great deal of patience. I believe the text we wrote that summer was eventually typed back in Sydney. Anyway later in the (northern) Autumn of 79 the completed manuscript was submitted to Springer and the second volume finally appeared in 1981. So the total operation of writing and publishing the 1000 page two volume book took about three and a half years. But that was not the end!

There was one topic in Chapter 6 that I referred to earlier in the talk and which provides a salutary warning to the research managers who rely on citation indices to assess performance. Toward the end of Section 6.2.1 we derive and discuss a property of finite speed of propagation for a large class of quantum spin systems. This material was an expansion of the paper I wrote with Elliott Lieb in 1972, the week after I met Trond for the first time, and a subsequent paper of mine in 1976. These papers went down like the proverbial lead balloon. Other than self-citations and a mention by Park they were totally ignored until 2006. Since then the paper with Lieb has been cited approximately once a week and it even has its own Wikipedia page. The explanation–the development of the theory of Quantum Information Theory. So don't despair if your work is not cited immediately.

Although the manuscript of the book was finished in 1979 that did not slow down the collaboration with Ola and Akitaka. They both visited Sydney for extended periods and we wrote several papers on the properties of positivity preserving semigroups. This topic was to receive a great deal of attention during the 80s and it is still a fruitful area of research. Of course the generators of positive contractive semigroups are determined by Dirichlet forms and the theory of the latter expanded rapidly in the same period and continues to expand. From 1979 onward Ola was a regular visitor the Australia. He realized that by timing the visits correctly he could ensure that it was always summer.

In late 1981 Marion and I were on the move again but this time it was only a short (300 kilometers) hop to Canberra where we have remained ever since. In the first years in Canberra we had many visitors beside Ola and Akitaka and work continued on aspects of operator algebras and somewhat less on continuous semigroups. By 1985 Ola and I had recovered from writing the book, which was still selling regularly after the first major sales to university libraries. But then Ola and I independently noticed that the sales of Volume 1 ceased. We did not understand why until Bill Arveson's house in the hills overlooking Berkeley burnt down. You might well think that these events were unrelated but Bill had lots of his mathematics books in the house and when he tried to buy a replacement for our book he was told that Volume 1 was not available. He then wrote to Ola asking for his assistance in locating a copy, Ola informed me and we decided to write to Springer asking why the volume was out of print. The explanation we received again gave us doubts about our rapid choice of publisher. Apparently Springer decided to close their New York office and also decided to destroy all copies of books in underperforming series which were stockpiled in New York. Our book which was the best performing in the Physics series was a victim of the group action.

Ola and I were both horrified by this revelation and wrote that we would like Volume 1 to be reprinted. Springer then made a counter offer that they would publish a revised and updated version as a second edition. So we had to start again. In some ways it was a good thing. It gave us an opportunity to correct two small errors and also to add some discussion of new developments. The latter was not difficult as not much had changed in six years that had intervened. The subject matter was in a stable state. It was also not difficult since we were both still working in the same general area. But things were somewhat different in 1995 when Springer asked us to produce a second edition of Volume 2.

By the mid 90s I was principally working on elliptic operators and subelliptic operators and Ola, Palle (Jørgensen), Charles (Batty) also participated on occasions. Our interests were far from models of quantum statistical mechanics. But we were fortunate in three respects. First Ola was aware of the work of Bost and Connes on KMS systems and this allowed us to include a description of their theory. Secondly I had followed some of the developments in the theory of phase transitions in spin systems so that allowed us to improve our original description of these topics and move the original emphasis away from classical systems to quantum systems. We also added some discussion of aperiodic lattice systems. Thirdly, MathSciNet arrived on the scene. As Ola was arriving in Canberra for a month's stay to revise Volume 2 the university obtained a free trial access to MathSciNet. I did not realize the significance of this development but I mentioned it to Ola and he immediately started searching for all work that had been published in the preceding 15 years which might be of interest. We were amazed how helpful the new technology could be. Although we started to prepare the second edition of Volume 2 with great trepidation we were finally satisfied with our efforts. A few years later when a new editor with Springer suggested we write a third volume we did not hesitate in refusing.

At this point I will break off from the temporal description of our collaboration. Ola continued to visit Australia in his attempt to make life a continuous summer and he often visited our beach house. Canberra is about a two hour drive from the Pacific coast and the nearest coastal town, Bateman's Bay, lies at the mouth of the Shoalhaven River. The latter is renowned for its oysters and they were one of Ola's main delights. He became a well-valued customer at the main oyster outlet in town. He also was happy to spend days on the beaches which by any European standard were almost deserted. He enjoyed swimming and would cover much longer distances than I ever fancied. After we sold the beach house we rarely visited the coast but Ola would drive down for the day to have fish for lunch and to bring back a bag of oysters. The last time I saw him was at Oslo Airport and I suggested that he might be able to visit Australia and Bateman's Bay once again but he shook his head wryly. Unfortunately it was never to be.