

**International Association of Mathematical Physics**



# **News Bulletin**

**April 2015**



# International Association of Mathematical Physics News Bulletin, April 2015

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## Contents

ICMP2015 Approaching	3
Interview with Yakov Sinai	5
Obituary: Edward Nelson	21
Ed Nelson's Work in Quantum Theory	22
News from the IAMP Executive Committee	40

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*Cover picture:* Santiago de Chile, site of the ICMP 2015

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## ICMP2015 Approaching

by RAFAEL BENGURIA (Congress Convenor, ICMP2015, Santiago de Chile)

The International Congress on Mathematical Physics, ICMP2015, is rapidly approaching. Every three years the mathematical physics community participates in the ICMP's in different parts of the world, and we are happy to host the XVIIIth ICMP in Santiago de Chile.

The International Scientific Committee, headed by Antti Kupiainen, selected 16 plenary speakers and 20 session organisers of the ten Thematic Sessions which cover most of the current topics in Mathematical Physics. Overall, in the 10 Thematic Sessions there will be 64 invited talks and many contributed talks. The opening ceremony of ICMP 2015 will take place on Monday July 27, 2015, at 9:00 AM at the Hotel Intercontinental, the venue of the Congress, and will be presided by Robert Seiringer, the president of IAMP, and Francisco Brieva, the President of CONICYT (CHILE). As has been the tradition by the ICMP's, during the opening ceremony the Henri Poincaré Prize, the IUPAP prizes, and the IAMP Early Career Awards will be given. In the evening of the inaugural day the participants are cordially invited to a cocktail at the Museo Chileno de Arte Precolombino. The museum is housed in an old colonial building of Spanish Style in the heart of Santiago's downtown.

Santiago, on the foothills of the Andes is an attractive modern city. The conference site is in the "El Bosque Norte" quarter, a lively neighborhood which is a mix of business, commercial and residential area. Within five blocks of the conference site one can find numerous restaurants, banks, and shopping areas. In less than 10 minutes by car one can reach the San Cristobal Hill or the Parque Bicentenario, for nice walks in a hilly or flat (Bicentenario Park) area. Buses and a subway line give easy access to downtown and other neighborhoods.

Following a tradition started at the ICMP2000 in London, the weekend before the ICMP we will have the Young Researchers Symposium, where recent Ph.D.'s, postdocs and young faculty will give talks in the many different topics of the Congress. The YRS will take place on the Campus San Joaquín of the Pontificia Universidad Católica de Chile on Friday July 24 and Saturday July 25. The Campus is located next to the Subway Station "San Joaquín" on Line 5.

Four satellites have being organised around the ICMP 2015:

1. *Disordered models of mathematical physics*, will be held in July 21–24, 2015, in Valparaíso, Chile,
2. *Operator Algebras and Quantum Physics*, organized at the University of São Paulo. The workshop will concentrate on new developments in the theory of operator

algebras and their applications in quantum statistical mechanics and quantum field theory. Applications of the renormalization group as well as convex analysis will be further topics covered. This workshop will begin on Friday, July 17, 2015 and end on Thursday (at lunch time), July 23, 2015.

3. *Summer school on current topics in mathematical physics*, Universidad Técnica Federico Santa María, Valparaíso, Chile, August 3–7, 2015,
4. *IP-Phys2015 “Inverse Problems in the Physical Sciences”* will be held at the Center for Mathematical Modeling in Santiago, Chile, August 3rd–5th, 2015.

We encourage participants of ICMP2015 to consider their participation in one or more of these satellite meetings. For information on the ICMP2015, please visit the official web page of the Congress at: <http://www.icmp2015.cl>.

We are thankful to the many institutions that are supporting ICMP2015. The Iniciativa Científica Milenio (ICM Chile), CONICYT (Chile), the National Science Foundation (US), the International Union of Pure and Applied Physics (IUPAP), the Mathematics and Physics Departments of the Pontificia Universidad Católica de Chile, the Centro de Modelamiento Matemático (CMM) of the Universidad de Chile, The Clay Mathematics Institute, The Daniel Iagolnitzer Foundation, CIMPA, The American Institute of Physics (AIP), the Journal of Mathematical Physics (JMP), Springer Verlag, the Swiss Embassy in Santiago, Antofagasta Minerals, BHP Billiton, and several others have generously supported ICMP2015.

In the name of the Local Organizing Committee, we cordially invite you to participate in the XVIII International Congress on Mathematical Physics.

Looking forward to seeing you next July in Santiago,

Rafael Benguria  
Congress Convenor,  
on behalf of the Local Organizing Committee of the ICMP2015.

## **Interview with Yakov Sinai**

MARTIN RAUSSEN (Aalborg) and CHRISTIAN SKAU (Trondheim)

### **The prize**

**Professor Sinai — first of all we would like to express our congratulations. You have been selected as the 12th winner of the Abel Prize and you will receive the prize tomorrow. We are curious; did you have any expectations beforehand? How did you receive the information?**

In early March this year I got to know that the Abel Committee were interested in taking my photograph. A friend of mine told me this and I thought this must mean something because this had never happened before. And then there was a telephone call from the Norwegian Academy of Science and Letters informing me about the prize.

**And this was on the same day that the prize was announced here in Oslo?**

Yes. That happened on 26 March.

### **Youth**

**You were born in Moscow in 1935 into a family of scientists. Your parents were both biologists and your grandfather was well known in mathematics. We suppose this had important consequences for the development of your interests?**

Definitely, yes. How could I say no to this question? Everything was about mathematics and mathematical events. But, at that time, I preferred to play volleyball.

The influence of mathematics was not as direct as you may think. I participated in many Olympiads in mathematics during my school years but never had any success and never won any awards. I say this to young people who have never won in Olympiads; there may be compensation in the future. At this time, my grandfather was of a great age and he did not have the energy to push me into mathematics. And I also had a half-brother C. I. Barrenblatt who worked at Moscow State University and who was convinced that I should pursue a career in mathematics.

**Do you remember when you found out that you had an exceptional talent for mathematics?**

If at all, it happened very late. I was a graduate student when I brought my paper on entropy to my advisor A. N. Kolmogorov and he said: “At last you can compete with my other students.” But I am not sure that he was right and that I have an exceptional talent for mathematics.

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**You must have entered school at about the same time that Nazi Germany invaded Russia. How did the war influence your first years at school?**

I entered school in 1943 after my family returned from the evacuation of Moscow. At that time boys and girls studied separately; at the end of each year, we had about 10 exams. Before the evacuation, life was different. It was forbidden to leave windows open in the apartments in Moscow because it had to be dark. In 1943, windows were allowed to be open again. In Moscow there were no clear signs of war. But life was hard because of the time of Stalin. People had to behave in a special way.

**And that also influenced life at school?**

It was everywhere; you could be expelled from school or even sent to prison for being controversial.

**Were there teachers with a lot of influence on you, in particular in mathematics?**

We had a very good teacher in mathematics at our high school. His name was Vasily Alekseevich Efremov and he was a great old-style schoolteacher. He always brought us his problems in accurate handwriting on a piece of paper which he distributed among the students. Because of the well organised and inspiring work, mathematics was very popular among us. We discussed and tried to solve his problems. At this time I was not among the best in the class. There were definitely other students who were much better than me.

**What was your age at this point?**

This was still in high school just before I entered university. Thus, I was probably 16- or 17-years-old.

**Student at MSU-Mech-Mat**

**You entered the Faculty of Mechanics and Mathematics at Moscow State University in 1952 still a teenager. How was it to study at this famous institution as such a young student?**

We had a number of very good professors there. For example, the lecture course in analysis was given by M. A. Lavrentiev, who was a very famous scientist at the time. He was also involved in administration but was a great teacher and his lectures were very interesting. We also had a very good lecturer in classical mechanics, Chetaev. I was his student in the second year. Moreover, we had lectures of geometry given by Bachvalov, who was famous in Russia but not so much known in the West. There is a story about him: When we entered the university on 1 September, he came into the room and said: "Let's continue." And that was the beginning of his lecture course. In algebra, we had lectures by Dynkin, who was an excellent teacher for those who had started to study. These were lectures at a very high level. Dynkin used to hand out interesting problems for

the enthusiastic students. Among such students in my year I could particularly mention I. Girsanov, who became a famous probabilist, and L. Seregin.

**Was it Dynkin who inspired your first paper in mathematics?**

Yes. I was a student of Dynkin during the second and third years, and I wrote the first paper under his supervision. I solved a problem that he formulated for me; this became my first published paper when I was a student in the third year. I loved the work I did and still do.

Dynkin wanted me to work on problems on Markov processes in the style of Feller. The papers by Feller became very popular in Moscow at that time and Dynkin suggested that I should continue along this line. However, I was not very excited and interested in it.

**To what extent were mathematics and mechanics integrated in the curriculum?**

There were independent parts of the curriculum. Everybody could attend lectures within each branch. I was attending lectures on mathematics and mechanics but also, to a minor degree, some lectures in physics. But on the whole it was mainly in the mathematics department.

**We imagine that besides Dynkin, Kolmogorov and Gelfand must have been very important figures for you?**

Kolmogorov had many students and I became one of them. His students had complete freedom to work on any problem. Kolmogorov loved to discuss with them their results. There were several cases when Kolmogorov wrote their papers in order to teach them how to write mathematical texts. Kolmogorov organised a seminar, which was initially a seminar on random processes and later became a seminar on dynamical systems and ergodic theory. I began to attend, together with other mathematicians like Arnold, Alekseev, Tikhomirov and others. Later I became a student of Kolmogorov. At that time he was also interested in problems of entropy in different compact and functional spaces. The questions of this type were very much discussed at that time.

**But Gelfand tried to recruit you as a graduate student as well?**

Yes. Gelfand organised his famous seminar, which was attended by many mathematicians of different generations. I took part in it for many years. It happened, if I remember it correctly, in 1955 when Gelfand was writing a famous volume of his series of books on distributions. Gelfand was interested in probability theory and he wanted me to become his student. We had some discussions about it and I told him that I wanted very much to work on problems related to probability theory but I had already written a paper under the supervision of Dynkin. He asked me: "Do you want to have probability theory as an appetiser or as a dessert?" I answered: "I want it as a main course." That was the end of the story . . .

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This did not mean that our contact came to an end; we met many times, especially when he worked on problems in representation theory, which were connected with problems in ergodic theory, like the theory of horocycles and others. We discussed this many times. I attended Gelfand's seminars for many years because Gelfand had the remarkable ability to explain difficult mathematical topics in a clear and simple way.

## **Dynamical systems. Entropy and chaos**

### **Could you explain what a dynamical system is?**

We understand dynamical systems as objects that describe all types of evolution. The most interesting case is that of non-linear dynamical systems, that is, when the formulas for dynamics of the evolution are non-linear. There can be many different phenomena, which require deep analysis.

### **And among these dynamical systems, what is an ergodic system?**

I have a very good example for an ergodic system which I always explain to my students. Suppose you want to buy a pair of shoes and you live in a building that has a shoe store. There are two different strategies: one is that you go to the store in your building every day to check out the shoes and eventually you find the best pair; another is to take your car and to spend a whole day searching for footwear all over town to find a place where they have the best shoes and you buy them immediately. The system is ergodic if the result of these two strategies is the same. The entropy characterises the growth of the number of possibilities in dynamics. I heard the first explanation of this role of entropy from I. M. Gelfand.

### **Ergodic theory originally came from physics, in particular from the study of Hamiltonian equations. Can you explain in general terms what chaos is and how one can measure it?**

This is the subject of my lecture, which I will give the day after tomorrow, but I can summarise it briefly here. The main question concerns the difference between chaos theory and probability theory. In probability theory you have statistical experiments, say you toss a coin 100 times. One can have many different series resulting from this experiment and study the result.

If you consider the problem of chaos and for example want to measure the temperature at the same point you make the measurement during the year, you now have only one realisation of the temperature. You cannot have a hundred realisations of the temperature at a given place and at a given time. So the theory of chaos studies the series when the results of measurements have a limit as time tends to infinity and how to describe this limit. The existence of the limit actually follows from some hypothesis about the equations of motion. This gives the existence of the distribution, which determines the value of all kinds of averages (or, it is better to say, the existence of the averages and also finding their values).

Then, the question is: what are the equations of motion which determine the distribution and these averages? The basic statement in chaos theory is that the dynamics must be unstable. Instability means that small perturbations of the initial conditions lead to large perturbations in the dynamics after some time.

Then there is a mathematical theory that says that if the system is unstable then the time averages exist and there is a possibility of calculating them. This is the general description of what is done in chaos. A more precise description requires some more mathematics.

### **How do you measure chaos? Does entropy come into the picture here?**

If we understand chaos as mentioned already, i.e. as the existence of time averages and also properties related to mixing, then there is a natural description of chaos in terms of some special distribution. Entropy is used in the theory of unstable systems and it characterises how many types of dynamics a given system can have. It is certainly a very useful notion because the positivity of the entropy determines other properties of the systems that can be studied.

Physicists always expressed their hope that entropy would allow them to understand turbulence (see, for example, the paper by B. Chirikov and the books by G. Zaslavski, R. Sagdeev and others). On the other hand there are many situations in physics where systems have small entropy.

### **Definition of entropy for dynamical systems**

**Kolmogorov had come up with the definition of entropy for Bernoulli shifts but then he changed it to a definition that was not invariant. Then you came with the correct definition. What is now called the Kolmogorov-Sinai theorem gives an efficient way to compute the entropy.**

Kolmogorov started his seminar with the von Neumann theory of dynamical systems with pure point spectrum, which he explained in a purely probabilistic way. Later I found this approach in the book by Blanc-Lapierre and Fortet. Everything in Kolmogorov's seminars was very exciting. At that time we believed that the main problem in ergodic theory was to extend the theory of von Neumann to systems with continuous spectrum that can be constructed in terms of the second homology group of the spectrum with coefficients in the ring of bounded operators. It did not work, but the idea remained.

At that time, Kolmogorov spent his time primarily on problems of information theory and the concept of dimension of linear spaces. I do not know how it happened, but one day Kolmogorov came to his lecture and presented his definition of entropy. Using modern terminology, one can say that he gave the definition of entropy for Bernoulli shifts and thus proposed a new invariant for this class of dynamical systems. It was certainly a great result. Kolmogorov wrote his text. He submitted it for publication and left for Paris for a whole semester. As is known, the text that was submitted for publication was different from what he explained in class. In his paper he introduced a new class of systems which

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he called quasi-regular. Later they were called K-systems (K for Kolmogorov). For this class of systems he introduced the notion of entropy. While Kolmogorov was away, I was thinking about a definition of entropy that could work for all dynamical systems. Later it appeared in my paper on entropy.

At that time, there was a clear feeling that for dynamical systems appearing in probability theory, the entropy is usually positive, while for dynamical systems generated by ODEs it should be zero. Thus, there seemed to be a possibility to distinguish dynamical systems in probability theory from dynamical systems in analysis.

### **How about your connection with Rokhlin?**

The story about my connection with Rokhlin, who later became a close friend of mine, started when Kolmogorov's paper on entropy appeared in 1958. At that time, Rokhlin lived in a small provincial town, Kolomna, not far from Moscow. He had a very good graduate student named Abramov. There are several general theorems that were proven by Abramov, like the entropy of special flows, and other things like Abramov's formula, etc. When Rokhlin heard about the paper by Kolmogorov, he sent Abramov to Moscow to find out what had been done, what was the situation, and if possible to see the text.

When Abramov came to Moscow, he found me, we talked a lot and I taught him everything I knew. Abramov then invited me to Kolomna to talk to Rokhlin and I accepted the invitation. I remember my first visit to Kolomna very well. Rokhlin had an apartment there, which was very orderly; everything was very accurate and he was dressed very well. We began to talk and he made a very strong impression on me.

Rokhlin had great experience in ergodic theory because he had already published several papers in this field. His doctoral thesis was also about this subject. Rokhlin formulated a number of interesting problems in ergodic theory. Some of them were connected with Rokhlin's theory of measurable partitions. This theory became very useful in ergodic theory because through it one can understand conditional probabilities in probability theory much better.

One of the problems that I began to work on under the influence of Rokhlin was the calculation of entropy for group automorphisms of the two-dimensional torus. At that time it was not known that Kolmogorov's definition had to be modified; the analysis was rather difficult and I could not achieve anything. Following the ideology of that time, I tried to prove that the entropy was zero but all my attempts failed. Then I visited Kolmogorov and showed him my drawings. He said that it was clear in this case that the entropy must be positive. After that I proved the result.

At that time there was no question about publication of my paper because Kolmogorov's paper on entropy had been published and it was not clear why another definition of entropy was needed. However, after some time, Rokhlin pointed out his result about the deficiencies in the definition by Kolmogorov. It became clear that I had to publish my paper with the definition and the calculation of the entropy for the automorphism which I had already done at that time.

This was the beginning of my contact with Rokhlin. After that, he organised a seminar on ergodic theory in Moscow, which was attended by Arnold, Anosov, Alexeev and others. In parallel, he had a seminar in topology where Novikov was the central figure.

Later Rokhlin moved to Leningrad (Saint Petersburg) and I used to go there to give talks at his seminar on later results.

## Billiard systems

**You then came up with an extremely interesting example of an ergodic system: the so-called billiards. Can you explain what these are?**

A billiard, as people know, is the motion of a ball on the billiard table. An interesting mathematical theory arises if you allow the table to have a more or less arbitrary form. A natural question, which was actually raised by the Russian physicist Krylov long before the theory of entropy appeared, was: Which billiard systems have the same instability as the dynamics of particles moving in a space of negative curvature? Particles moving in a space with negative curvature yield the best example of unstable systems. The theory of billiards says that if the boundary of the table is concave then the system is unstable in the sense we previously described. If we consider two initial conditions with different values of the velocities then the corresponding trajectories diverge exponentially. Apart from that, if you consider a square billiard table from which an inner circle is removed the regular billiard is due to the fact that the trajectories come to the holes much faster.

**This may become a little technical now. You proved a very important result about systems with positive entropy. Given a system with positive entropy you can find a Bernoulli shift — which is a so-called factor — with the same entropy. This implies that if you have two Bernoulli shifts with the same entropy, they are what is called weakly isomorphic. Ornstein proved later that entropy is a complete invariant for Bernoulli shifts. It follows then from the work of Ornstein that the billiard example is the most chaotic system and is actually a Bernoulli flow, right?**

From Ornstein's theorem it follows that if we have two ergodic billiard systems with the same value of entropy then they are isomorphic. This is a remarkable and great result.

So coin tossing is, in a sense, similar to the deterministic billiard system — an amazing fact.

My result says that if you have a system with positive entropy there could be subsystems that move like Bernoulli shifts.

**What about billiard systems in higher dimensions? Is anything known there?**

A lot of things are known. We have, for example, the result from the Hungarian mathematician Nándor Simányi who is in Birmingham, Alabama, now. He studied multi-dimensional dynamical systems that eventually become unstable and have positive entropy and are ergodic.

**You introduced Markov partitions in your study of Anosov diffeomorphisms. This led to what later became known as the Sinai-Ruelle-Bowen measure, also referred to as the SRB-measure. Would you please explain?**

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First of all, there was my paper where I constructed this measure for the case of the so-called Anosov systems, or just hyperbolic systems. Then there was a paper by Bowen and Ruelle where they extended this construction to systems considered by Smale, that is, Axiom A systems with hyperbolic behaviour.

These measures are important if you study irreversible processes in these systems. Suppose you start with some non-equilibrium distribution and consider the evolution and you ask how a non-equilibrium distribution converges to the equilibrium one. The result of the theory says that if the evolution is in a sense very non-uniform, then along some directions the expansion is very small and all the time averages behave very well and converge to a limit. But along other directions this convergence is very erratic and hence it can only be studied using probability theory. So the measures, which are called SRB-measures, are the ones which are smooth along some unstable directions and that are very irregular along the other directions. This is a class of measures that appears in the theory of evolution of distributions in the case of chaotic systems.

### **Are the SRB-measures related to Gibbs measures?**

Yes. These measures are examples of Gibbs measures. But the Gibbs measure is a much more general object.

## **Mathematics and Physics**

**Let's go back to more general questions, starting with the interplay between mathematics and physics. May we begin with the physicist Eugene Wigner, who in 1960 published the paper "The unreasonable effectiveness of mathematics in the natural sciences", in which he gave many examples showing how mathematical formalism advanced physical theory to an extent that was truly amazing. Do you have a similar experience?**

My impression is that this effectiveness of mathematics is no longer a surprise for people. There are so many cases, for instance the fact that string theory is practically a mathematical theory for physics. Some time ago Joel Lebowitz organised a discussion about this phrase of Wigner — in particular how it can be that mathematics is so effective. The conclusion was that this is just a well-established fact.

In my generation, there was a group of young mathematicians who decided to study physics seriously. However, there were different points of view of how to do mathematical physics. F. A. Berezin always stressed that mathematicians should prove only results that are interesting for physicists. R. L. Dobrushin and I always tried to find in physical results some possibilities for mathematical study.

**On the other hand, there seems to be influence going in the opposite direction. Physicists have had a noteworthy impact on questions in quantum geometry and sometimes even in number theory. They have come up with formalisms that were not really developed in mathematics but nevertheless led to correct**

**predictions which could be verified only after lengthy mathematical development.**

So mathematics is effective but you can say that it is not effective enough.

**You published in 2006 an article with the title “Mathematicians and Physicists = Cats and Dogs?” What is the main message of that paper?**

I wanted to show examples where mathematicians and physicists look at the same problems differently. One example for this is the following story: my student Pirogov and I worked on problems in the theory of phase transitions in statistical physics. We proved several theorems and I went to meet the famous Russian physicist Ilya Lifshitz to show him our theory; Lifshitz replaced Lev Landau when Landau had his car accident, severely incapacitating him. When I presented the theory he stopped me and said: “It’s very simple what you are talking about.” He started to write formulas which eventually gave our results. I left him very much embarrassed and I started to think why this had happened. I realised that the final result of our theory was an obvious statement for him. He certainly did not know how to prove it but he did not need the proof. He just used it as an obvious fact.

**There is a famous quotation of the great Gauss: “Now I have the result. The only thing remaining is the proof.” So intuition does play an important role in mathematics . . .**

I can also tell the following story, again connected to Gelfand. I explained to him a theorem, which we obtained together with Robert Minlos. And Gelfand said: “This is obvious. All physicists know this.” So we asked him if it was so obvious, should we write a text of 200 pages with complete proofs? He looked at us and said: “Certainly, yes!”

## **A Jewish mathematician in the Soviet Union**

**May we continue with a political question? You mentioned that being at school in the time of Stalin was not easy; life was still difficult for you when you entered university and started your career. You came from a Jewish family; in the Soviet Union, at least sometimes, a latent anti-Semitism prevailed . . .**

I would like to mention three cases in my career which were connected to anti-Semitism. The first one was the entrance examination, which I failed the first time. The influence of my grandfather, who was Head of the Chair of Differential Geometry, was needed in order to give me the possibility of being admitted to another exam. This was a clear sign that things were not simple.

The other case arose with my entrance examination to graduate school. This exam was about the history of the Communist Party; I was very bad in this topic and failed the exam (I don’t want to discuss the details). But P. S. Alexandrov - who was Head of the Mathematical Department at Moscow State University - together with Kolmogorov, visited the Head of the Chair of the History of the Party and asked her to allow me to

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have another attempt. She gave permission and I got a B on the second attempt, which was enough to enter the graduate school. The result was not clear a priori and it could have gone either way.

The last case I would like to mention is about the letter in support of Alexander Esenin-Volpin, which was signed by 100 mathematicians. As a result, the opportunities for many of them changed. For example, I could not even travel abroad after that for 17 years. When I applied for a position as full professor at Moscow State University, I was denied and became a full professor only 17 years later.

Much later I could participate in three of the International Mathematical Congresses. These congresses were always very useful because they gave me a chance to meet more or less simultaneously the best mathematicians in each field.

**In spite of these obstructions, it is quite obvious that many famous Russian mathematicians were and are of Jewish origin. This is quite amazing — can you offer any explanation?**

First something trivial: Jews had more traditions in learning than other nations. They study the Bible, the Talmud and other religious books and spend a lot of time doing this, which is conducive to learning. At that time, following the Jewish religion was strictly forbidden. People still did, however, but under very high pressure. If you do something under pressure you work more. There is some kind of conservation law. This is my opinion of why Jews could succeed.

**You had to be much better in order to get the same opportunities?**

I think it would be wrong to say that we had this feeling. We certainly tried to prepare for all exams and competitions; the result was not clear a priori but there was always a hope that something could come out of it.

**Perhaps another reason is (especially under Stalin but also later) that a lot of very intelligent people were attracted to the natural sciences because there were fewer restrictions than in, say, history or political science . . .**

That is certainly true. I can give you one example: at the time, Mech-Mat, the Faculty of Mechanics and Mathematics, had many graduate students that came from other countries. The rule was that they could only have advisors who were members of the Party. But there were students who wanted to work with Arnold, with me or perhaps with some other people. The way out of this situation was the following: there were a number of people in the Party who became the students' official advisors but the students actually worked with professors and mathematicians who were not members of the Party.

## **East and West**

**You told us that you were not allowed to travel for many years, and this happened to a lot of Russian mathematicians at the time. Did these obstruc-**

**tions hamper or delay progress in science? Did it have the effect that Russian mathematics did not get recognition in the West that it deserved?**

It is very difficult to answer your question because you are asking what would have happened if something didn't happen. It is just impossible to say. It certainly caused harm but it is not clear how big it was.

Arnold was rather adamant about the lack of recognition. As a consequence of bad communication between East and West, results by Russian mathematicians during the isolation period were sometimes later rediscovered in the West. Therefore Russian mathematicians did not get the credit they deserved.

I have perhaps a special point of view concerning this. The question is whether some results can be stolen or not. My point of view, to which many people probably won't agree, is that if a result can be stolen, it is not a very good result.

**Tell us about the Landau Institute for Theoretical Physics in the Russian Academy of Sciences — your workplace for many years.**

For many years the Landau Institute was the best institution in Russia. It was organised after Lev Landau's untimely death as a result of a car accident. Its director I. M. Khalatnikov had a remarkable talent to find gifted people all over Russia and to invite them to the institute. After several years, the Landau Institute had a very strong group of physicists like Abrikosov, Gorkov, Dzyaloshinski, A. B. Migdal, Larkin, Zakharov, Polyakov, A. A. Migdal and many others. The group of mathematical physicists was headed by S. P. Novikov and was much smaller.

It turned out that there was a big area of theoretical physics in which mathematicians and physicists could understand each other very well. They could even work on similar problems. Among these mathematicians I can name Novikov, Krichever, Khanin, Shabat and Bogoyavlenskiy. Sometimes we invited physicists to explain to us their results in our seminars. The tradition of discussing problems of mutual interest still prevails.

**You moved in 1993 from the Landau Institute to Princeton University while still maintaining your position in Moscow. Why was it so attractive for you to go to the USA?**

That is an easy question. First of all I had many friends at Princeton. When we met we always had many points for discussion and common interests. Another reason was that many people had escaped from Russia so the situation there was no longer what it was before. In previous times everybody was in Moscow and St. Petersburg and you could call everyone to ask questions or to have discussions. Now that became impossible. The working conditions were better in the West and in particular at Princeton.

**You have now been in the USA for more than 20 years and you must know the American system almost as well as the Russian one. Could you tell us about how they compare from your perspective?**

Concerning academic lives, it seems to me that they are quite similar. However, I must stress that I was never a member of any scientific committee at Mech-Mat at Moscow State

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University and I was never invited to participate in any organisational meetings. Now I am Chairman of the Scientific Council at the Institute of Transmission of Information.

## **Teaching and collaboration**

**You have been teaching courses and seminars for almost all your career. Do you have a particular technique or philosophy?**

First of all, I like to teach undergraduate courses rather than graduate courses for the following reason: when you teach undergraduate courses you can easily see how your students become cleverer and more educated as they absorb new notions and connections and so on. When you teach graduate courses, the subject matter is usually a narrow piece of work and students are mostly interested in some special issues that are needed for their theses. For me, that is less attractive.

My basic principle is as follows: if people do not understand my explanations then this is my fault. I always ask students to ask questions as much as possible. Students who have asked me many questions during the lecture course have better chances for a good mark.

**You have an impressive list of students that have done well after graduation under your supervision. Grigory Margulis, just to mention one name, won the Fields medal in 1978 and he will give one of the Abel lectures related to your work later this week . . .**

I think the reason for this is not because of me but because of the types of problems we worked on. We did very interesting mathematics and formulated interesting problems that students were attracted to. This is my explanation. Many students preferred to work independently and I was never against this.

**You are a very good example of the fact that mathematicians can flourish in late age as well. We came across a paper on number theory that you published this year together with two of your students. You have also published other papers related to number theory so you must have kept an interest in that aspect of ergodic theory?**

Yes, definitely. In the field we are working in there are many problems that are more natural for ergodic theory than for number theory. I don't want to be specific but we had a paper that was more natural for an ergodic theorist than for a number theorist, so we could get the results more easily.

**Many joint papers appear on your list of publications. Apparently you like to have a lot of collaborators.**

Well, I would say that they like it! And I'm not against it. It has never happened that I have asked someone to be my co-author. I can only talk about some problems and explain why they are interesting.

But you are right, I have had many co-authors. I very much liked collaborating with Dong Li, who is now a professor at the University of British Columbia. When we work on the same problem we call each other many times a day. There are many others of my students with whom I liked to work. It's different to work with different people. Certainly I can work with Russian mathematicians as well as with mathematicians from other countries. Sometimes I like to work alone but with age, I need co-authors.

**You have only published one paper with Kolmogorov but you have mentioned that you would have liked to publish more papers with him.**

At a certain time, Kolmogorov decided that the Soviet Union did not have enough applied statistics. He worked on theoretical statistics and found many beautiful and deep results but he was not satisfied with the fact that the theorems in applied statistics were not used for practical purposes. He found a problem related to the motion of the rotational axis of the earth that could be studied with the help of mathematical statistics. French observatories published data about the axis of rotation every two weeks and Kolmogorov wanted to construct statistical criteria that could predict this motion. He wanted us to work on this problem and invited a very good geophysicist Yevgeny Fyodorov, who was one of the main experts in this field. We were sitting there — Kolmogorov and Fyodorov were present. Kolmogorov said: “Look at these people; they prefer to write a paper for *Doklady* instead of doing something useful.” (*Doklady* was the leading Russian journal.) In our joint paper (by M. Arato, A. Kolmogorov and myself) written on this occasion, practically everything was done and written by Kolmogorov. Later, M. Arato wrote a big monograph on that subject.

In other cases, I often tried to explain my latest results to Kolmogorov. Sometimes his reaction was unexpected: “Why did you work on that problem? You are already a grown-up?” But usually his reaction was very friendly. I regret very much that we never worked together; perhaps the reason is a difference in style.

**Wasn't it Kolmogorov who said that he spent a maximum of two weeks on a problem?**

Kolmogorov used to stress that he did not have papers on which he worked for a long time. He mostly prepared his papers, including the proof and the text, in just two weeks and this was a major difference in our approaches. Kolmogorov was a person with a strong temperament and he could not do anything slowly. I worked on some of my papers for years.

**He was a towering figure, not only in Russian mathematics but worldwide in the 20th century.**

Yes, definitely. Can I tell you one more story about him? It was when Kolmogorov was close to 80. I asked him how it happened that he was a pure mathematician, even though he worked on concrete physical problems like turbulence. He answered that he was studying the results of concrete experiments. He had a lot of papers with results

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from experiments lying on the floor. He was studying them and in this way he came up with his hypotheses on turbulence.

**So his intuition was motivated by physical considerations?**

Yes. He subscribed to physical journals and one could say he was into physics in a big way.

**Is that also true for you? Do you think mainly in terms of algebraic or analytic formulas? Or is it geometric intuition or even a mixture of all of that?**

It depends on the problem. I can come to the conclusion that there is a problem that must have an answer. I just told a story to a journalist about a problem in which I knew there should be a definite answer. I worked on this problem for two years and at the end of that time I discovered that the answer was one-half!

In general, I probably prefer to develop theories, sometimes to find the right concepts rather than solving specific problems.

**Have you had what we sometimes call a Poincaré moment, where all of sudden you see the proof?**

Ideas often come unexpectedly, sometimes like revelations. But it happens only after a long period, maybe years, of difficult work. It did not happen while trying to find a taxi or something similar. It was very hard work for a long time but then suddenly there was a moment where it became clear how the problem could be solved.

**If you yourself made a list of the results that you are most proud of, what would it look like?**

I like all of them.

## Mountaineering

**You mentioned Arnold who died four years ago, an absolutely brilliant Russian mathematician. Arnold is, among many other things, known for his contributions to the so-called KAM theory. You both followed Kolmogorov's course and seminars in 1958. You told us that there was a close friendship already between your grandfathers. Both you and Arnold loved the outdoors and hiking. You once went to the Caucasus Mountains together and you have to tell us the story about what happened when you stayed in the tents with the shepherds.**

That is a very funny story. The weather was very bad; there was a lot of rain. We came to the shepherds' tent and they let us in and we could dry our clothes. We had lost our tent in the mountains so we decided to go back to try to find it. We started to walk back but these shepherds had some very big dogs — Caucasus dogs, a really big race. The shepherds weren't there any longer and when the dogs found out that we were leaving,

they surrounded us and started to bark ferociously. Arnold began to yell back with all the obscenities he knew and the dogs did not touch him. But they attacked me. They didn't touch my skin but they ripped my trousers apart. Finally, the shepherds came back and we were saved.

**We would like to ask one final question that has nothing to do with mathematics: you have certainly focused on mathematics during your life but surely you have developed other interests also?**

I was interested, especially in previous years, in many different sports. I was a volleyball player and I liked to ski, both downhill and cross-country. I also liked mountaineering but I cannot say I was a professional. I climbed often with a close friend of mine Zakharov, who worked on integrable systems. We were climbing in the mountains together and once we were on a very difficult 300 metre long slope, which took us four hours to get down from! We had to use ropes and all sorts of gear. Nowadays, my possibilities are more limited.

**Thank you very much for this most interesting conversation. We would like to thank you on behalf of the Norwegian, the Danish and the European Mathematical Societies.**

Thank you very much.



Yakov Sinai (Photo: John Jameson/Princeton University)

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## Edward Nelson (1932–2014)



Edward Nelson, Professor emeritus of Mathematics at Princeton University, passed away on September 10, 2014.

Born in Decatur, Georgia, USA on May 4, 1932, he received his Ph.D. from the University of Chicago in 1955, then spent the years 1956-1959 at the Institute for Advanced Study, Princeton. He became Assistant Professor of Mathematics at Princeton University in 1959, was promoted to the rank of Professor in 1964, and stayed there until his retirement in July, 2013. Nelson made fundamental contributions to various fields of mathematics, including probability, logic, foundations, mathematical physics and analysis. In 1995, he won the Steele Prize for research of seminal importance for his contributions to constructive quantum field theory in mathematical physics.

*Tribute of Princeton University to Edward Nelson*

Photo courtesy of Sarah Nelson

## Ed Nelson's Work in Quantum Theory

by BARRY SIMON (Vice President of the IAMP, Pasadena, USA)

Edward Nelson passed away on September 10, 2014 at age 82. He and Arthur Wightman were my mentors. Ed taught me not only mathematics but how to be a mathematician. Ed was both a gentle man and a gentleman and a remarkable innovator in mathematical physics. In June of 2004, there was a conference in honour of Ed's work in Vancouver. I prepared an article reviewing Ed's work in quantum theory (B. Simon, Ed Nelson's work in quantum theory, in *Diffusion, Quantum Theory, and Radically Elementary Mathematics*, pp. 75–93, *Mathematical Notes*, 47, Princeton University Press, Princeton, NJ, 2006) which the editor of the IAMP News Bulletin asked to republish in Ed's memory. This is the output of a .tex file as submitted — it may have slight difference to the published version. It is © Barry Simon, 2004. By the time this appears, I will have given a talk at Princeton in Ed's memory. Slides of that talk with some overlap but also some differences should be available at <http://www.math.caltech.edu/simon/biblio.html>.

### 1 Introduction

It is a pleasure to contribute to this celebration of Ed Nelson's scientific work, not only because of the importance of that work but because it allows me an opportunity to express my gratitude and acknowledge my enormous debt to Ed. He and Arthur Wightman were the key formulative influences on my education, not only as a graduate student but during my early postdoctoral years. Thanks, Ed!

I was initially asked to talk about Ed's work in quantum field theory (QFT), but I'm going to exceed my assignment by also discussing Ed's impact on conventional nonrelativistic quantum mechanics (NRQM). There will be other talks on his work on unconventional quantum theory.

After discussion of NRQM and the Nelson model, I'll turn to the truly great contributions: the first control of a renormalization, albeit the Wick ordering that is now regarded as easy, and the seminal work on Euclidean QFT.

Many important ideas I'll discuss below involve crucial remarks of Ed that — in his typically generous fashion — he allowed others to publish.

### 2 NRQM

Ed has very little published specifically on conventional NRQM but he had substantial impact through his lectures, students, and ideas from his papers that motivated work on NRQM. In particular, two of my own books [68, 73] have subject matter motivated by what I learned from Ed.

(a) **Quadratic Forms.** Reed-Simon [64] call the perturbation theorem for closed quadratic forms the KLMN theorem for Kato, Lions, Lax-Milgram, and Nelson. Ed

was not the first to prove the KLMN theorem nor was he the first to use the scale of spaces that lies behind rigged Hilbert space theory, but so far as I know, he is the first to use scales in the context of studying selfadjointness of operators associated to quadratic forms, not only in the KLMN theorem but in the selfadjointness theorem [57] I'll discuss below.

(b) **Path Integrals.** Ed's best known published paper on path integrals deals with Feynman path integrals [53], that is, for  $e^{-itH}$  where he uses the Trotter product formula to write  $(e^{-itH}\varphi)(x)$  as a limit (in  $L^2$  sense in  $x$ ) of Riemann integral approximations to a formal path integral. One cannot take the limit inside the integral and get a well-defined measure in the conventional sense, so these ideas have had limited use as an analytic tool. Still, they have conceptual uses and have been the starting point for other work on Feynman path integrals [4, 18, 24, 79].

From my point of view, the most significant contribution of [53] is the idea of using the Trotter product formula to prove Feynman-Kac-type formulae, an idea which is now standard.

Even more, Ed was a strong proponent of using path integrals in NRQM, an attitude which permeated Princeton in the 1970's, for example, Aizeman-Simon [1], Carmona [12], and Lieb [45].

(c) **Selfadjointness Theorems.** Proving (essential) selfadjointness of unbounded operators on a suitable domain is a basic part of mathematical quantum theory. Besides the KLMN theorem already mentioned, Ed is responsible for two general theorems and played a role in a third.

If  $A$  is a Hermitian operator, an analytic vector for  $A$  is a  $\varphi \in \cap_n D(A^n)$  so that for some  $t > 0$ ,

$$\sum \frac{t^n \|A^n \varphi\|}{n!} < \infty.$$

In [52], Ed proved that if  $D(A)$  contains a dense set of analytic vectors, then  $A$  is essentially selfadjoint. This is basic to representation theory. For extensions of [52], see Nussbaum [62] and Masson-McClary [51].

In [57], Ed proved a result that essentially says that if  $N \geq 1$  is a second operator which is selfadjoint and

$$\begin{aligned} \pm A &\leq c_1(N + 1) \\ \pm i[N, A] &\leq c_2(N + 1), \end{aligned}$$

then  $A$  is essentially selfadjoint (I suppress the technical issue of what  $[N, A]$  means; see [57] or [64, Section X.5]). Ed applied this to selfadjointness of time-smearred quantum fields, a result that Glimm-Jaffe [28] also proved using commutator estimates on the operator, commutator, and double commutator.

While Ed didn't apply his commutator theorem to NRQM, Faris-Lavine did [20].

Finally, I should mention the dog that didn't bark [19]: selfadjointness and hypercontractive semigroups (the later are discussed in Section 4 below). Segal [67], following up on Ed's work in [56], proved that if  $H_0$  generated a hypercontractive semigroup on

$L^2(M, d\mu)$  and if  $V \in L^2$ ,  $e^{-tV} \in L^2$  (for all  $t > 0$ ) for some function,  $V$ , then  $H_0 + V$  is essentially selfadjoint on  $e^{-H_0}[L^\infty]$  (see also [41, 70]). Rosen [65] in a concrete setting had a similar idea to Segal [67].

(d) **Diamagnetic Inequalities.** These inequalities state that if  $H(a, V)$  is a quantum Hamiltonian (any number of dimensions or particles, any masses, and any magnetic vector potential,  $a$ , and scalar potential  $V$  with enough regularity to define  $H$ ), then

$$|(\exp(-tH(a, V))\varphi(x))| \leq (\exp(-tH(0, V))|\varphi|)(x). \quad (1)$$

I named them diamagnetic inequalities since they imply a finite temperature analog of the fact that  $\inf \text{spec}(H(a, V)) \geq \inf \text{spec}(H(0, V))$ , an expression of the fact that in the absence of spin (i.e., of magnetic moments) and/or fermi statistics, energies increase in a magnetic field. One author tried to name them Nelson-Simon inequalities but the name didn't stick, so I guess I should apologize to Ed for coming up with a name that had such a nice ring to it.

What was Ed's role in this? The story begins with two of the selfadjointness results of the last section. Before 1972, the conventional wisdom was that selfadjointness results for  $-\Delta + V$  on  $L^2(\mathbb{R}^\nu)$  required  $V$  to be at least locally  $L^p$  with  $p > \nu/2$  (and  $p \geq 2$ ). Since  $-\Delta - c/r^2$  for  $c$  large and  $\nu \geq 5$  is not essentially selfadjoint on  $C_0^\infty(\mathbb{R}^\nu)$ , this condition would seem to be close to optimal since  $\int_{|r| \leq 1} (r^{-2})^p d^\nu r < \infty$  if  $p < \nu/2$ . What I discovered is that the "correct" conditions are asymmetric — positive singularities need only be  $L^2$ . I proved that if  $V \in L^2(\mathbb{R}^\nu, e^{-x^2} d^\nu x)$  and  $V \geq 0$ , then  $-\Delta + V$  is essentially selfadjoint on  $C_0^\infty(\mathbb{R}^\nu)$  for any  $\nu$ .

The proof [70] went as follows. By Ed's result on hypercontractivity of the fixed Hamiltonians [56],  $H_0 = -\Delta + x^2$  generates a hypercontractive semigroup after translating to  $L^2(\mathbb{R}^\nu, \Omega_0^2 d^\nu x)$  with  $\Omega_0$  the ground state of  $H_0$ . By Segal's theorem and a simple approximation argument,  $N = H_0 + V$  is selfadjoint on  $C_0^\infty(\mathbb{R}^\nu)$ . Now use  $[N, -\Delta + V] = [x^2, -\Delta + V]$  to verify the hypotheses of Nelson's commutator theorem [57] to conclude that  $-\Delta + V$  is essentially selfadjoint. Actually, in [70], I used a different argument from the Nelson commutator theorem, but I could have used it!

I conjectured that the weak growth restriction implicit in  $\int V(x)^2 e^{-x^2} dx < \infty$  was unnecessary and that  $V \geq 0$  and  $V \in L_{\text{loc}}^2(\mathbb{R}^\nu)$  implied  $-\Delta + V$  was essentially selfadjoint on  $C_0^\infty(\mathbb{R}^\nu)$ . Kato took up this conjecture and found the celebrated Kato's inequality approach to selfadjointness [43]. This is not the right place to describe this in detail (see [43] or [64, 75]), but what is important is that between the original draft he sent me and the final paper, he added magnetic fields and that he used as an intermediate inequality

$$|(\nabla - ia)\varphi| \geq \nabla|\varphi| \quad (2)$$

pointwise in  $x$ . Formally, (2) is obvious; for if  $\varphi = |\varphi|e^{i\eta}$ , then  $\text{Re}(e^{-i\eta}(\nabla - ia)\varphi) = \text{Re}((\nabla - ia + i(\nabla\eta))|\varphi|) = \nabla|\varphi|$ .

What I realized two years later was that by integrating (2) in  $x$ , one has

$$(|\varphi|, H(0, V)|\varphi|) \leq (\varphi, H(a, V)\varphi), \quad (3)$$

which implies the diamagnetism of the ground state. The analog of (3) for finite temperature is

$$\mathrm{Tr}(e^{-\beta H(a,V)}) \leq \mathrm{Tr}(e^{-\beta H(0,V)})$$

and this led me to conjecture the diamagnetic inequality (1).

At the time, every Thursday the mathematical physicists at Princeton got together for a “brown bag lunch.” During 1973–78, the postdocs/assistant professors included Michael Aizenman, Sergio Albeverio, Yosi Avron, Jürg Fröhlich, Ira Herbst, Lon Rosen, and Israel Sigal. Lieb, Wightman, and I almost always attended, and often Dyson and Nelson did. After lunch, various people talked about work in progress. I discussed (3) and my conjecture (1), explaining that I was working on proving it. After I finished, Ed announced: “Your conjecture is true; it follows from the correct variant of the Feynman-Kac formula with a magnetic field.” So the first proof of (1) was Ed’s. Characteristically, he refused my offer to coauthor the paper where this first appeared with another semigroup-based proof [72].

I should mention that the simplest proof of (1) and my favorite [75] is very Nelsonian in spirit: One uses the Trotter product formula to get the semigroup  $(e^{-tH(a,V)})$  as a limit of products of one-dimensional operators  $e^{+t(\partial_j - ia_j)^2/n}$  and uses the fact that one-dimensional magnetic fields can be gauged away. This is Nelsonian for two reasons. The use of Trotter product formula in such a context is due to Ed, but also the proof is a poor man’s version of Ed’s original proof: The gauge transformations are just a discrete approximation to an Itô stochastic integral.

(e) **Point Interactions.** The subject of point interactions has been heavily studied (see, e.g., [3]). So far as I know, Ed was the first to study point interactions as limits of potentials with supports shrinking to a point. He presented this in his courses; an extension of the ideas then appeared in the theses of his students, Alberto Alonso and Charles Friedman [22]. The basic points are:

- (i) If  $\nu \geq 4$  and  $V_n$  is any sequence of potentials, say, each bounded (but not uniformly bounded in  $n$ ), supported in  $\{x \mid |x| < n^{-1}\}$ , then  $-\Delta + V_n \rightarrow -\Delta$  in strong resolvent sense.
- (ii) If  $\nu \geq 2$  and  $V_n \geq 0$ , (i) remains true.
- (iii) If  $\nu = 1, 2, 3$ , there are special negative  $V_n$ ’s that have strong limits different from  $-\Delta$ , many with a single negative eigenvalue. These are the point integrations.

(i) is an immediate consequence of the fact that  $\{f \in C_0^\infty(\mathbb{R}^\nu) \mid f \equiv 0 \text{ in a neighborhood of } 0\}$  is an operator core of  $-\Delta$  if  $\nu \geq 4$ . While (ii) can be obtained by similar consideration of form cores (and a suitable, somewhat subtle, limit theorem for quadratic forms), in typical fashion, Ed explained it not from this point of view, but by noting that in dimension 2 or more and  $x, y \neq 0$ , almost every Brownian path from  $x$  to  $y$  in fixed finite time  $t$  avoids 0. Thus, in a Feynman-Kac formula, if  $V_n$  has shrinking support, the integrand goes to one (i.e.,  $\exp(-\int_0^t V_n(\omega(s)) ds) \rightarrow 1$ );  $V_n \geq 0$  is needed to use the dominated convergence theorem in path space.

### 3 The Nelson Model

A search in MathSciNet on “Nelson model” turns up nineteen papers, many of them recent [2, 5, 6, 7, 8, 9, 11, 13, 14, 23, 25, 37, 38, 39, 40, 47, 48, 49, 78], so I’d be remiss to not mention the model, although I’ll restrict myself to describing the model itself and noting that Ed introduced it in [54] and studied it further in [55].

The nucleon space  $\mathcal{H}^{(N)}$  is  $L^2(\mathbb{R}^{3n})$  where  $n$  is fixed (most later papers take  $n = 1$ ) with elements in  $\mathcal{H}^{(N)}$  written  $\psi(x_1, \dots, x_n)$  and free nucleon Hamiltonian

$$H^{(N)} = - \sum_{j=1}^n \Delta_j.$$

The meson space is the Fock space,  $\mathcal{H}^{(M)}$ , on  $\mathbb{R}^3$  with creation operators  $a^\dagger(k)$  ( $k \in \mathbb{R}^3$ ). The meson has mass  $\mu$  (Ed took  $\mu > 0$ ; many applications take  $\mu = 0$ ) and free Hamiltonian

$$H^{(M)} = \int \omega(k) a^\dagger(k) a(k) d^3k,$$

where

$$\omega(k) = (k^2 + \mu^2)^{1/2}.$$

One defines the cutoff field for fixed  $x \in \mathbb{R}^3$  by

$$\varphi_\chi(x) = 2^{-1/2} (2\pi)^{-3/2} \int \omega(k)^{-1/2} (a(k) e^{ik \cdot x} + a^\dagger(k) e^{-ik \cdot x}) \chi(k) dk.$$

Ed took  $\chi$  to be a sharp cutoff (characteristic function of a large ball); some later authors take other smoother  $\chi$ ’s. One defines

$$\mathcal{H} = \mathcal{H}^{(N)} \otimes \mathcal{H}^{(M)}$$

and on  $\mathcal{H}$ ,

$$H_I = g \sum_{j=1}^n \varphi_\kappa(x_j),$$

where  $g$  is a coupling constant and now  $x$  is the nucleon coordinate. The Nelson model is the Hamiltonian

$$H^{(N)} + H^{(M)} + H_I.$$

This has been a popular model because it is essentially the simplest example of an interacting field theory with an infinite number of particles.

### 4 Hypercontractivity

The next two sections concern outgrowths of Nelson’s seminal paper [56]. This paper of only five pages (and because of the format of the conference proceedings, they are short

pages; in J. Math. Phys., it would have been less than two pages!) is remarkable for its density of good ideas. The following abstracts a notion Ed discussed in [56]:

**Definition.** Let  $H_0 \geq 0$  be a positive selfadjoint operator on the Hilbert space  $L^2(M, d\mu)$  with  $d\mu$  a probability measure. We say  $e^{-tH_0}$  is a *hypercontractive semigroup* if and only if

- (1)  $\|e^{-tH_0}\varphi\|_p \leq \|\varphi\|_p, 1 \leq p \leq \infty, t > 0$
- (2) For some  $T_0$  and some  $C < \infty$ ,

$$\|e^{-T_0 H_0}\varphi\|_4 \leq C\|\varphi\|_2. \tag{4}$$

Here the bounds are intended a priori on  $\varphi \in L^2 \cap L^p$ . Ed's key discovery in [56] is that if  $V$  is a function with  $e^{-V} \in \cap_{p < \infty} L^p$ , then  $H_0 + V$  is bounded below (to define  $H_0 + V$  in reasonable cases, one usually assumes also  $V \in L^2$  but for any  $V$ , and each  $k < \infty$ ,  $V_k = \max(V, -k)$  allows  $H_0 + V_k$  to be defined as a form sum, and one has  $\inf_k \min(H_0 + V_k) > -\infty$ ).

The simplest proof of this boundedness result follows from the formula

$$\|e^{A+B}\| \leq \|e^A e^B\| \tag{5}$$

for selfadjoint operators  $A$  and  $B$ . This formula is associated with the work of Golden, Thompson, and Segal (see the discussion of Section 8a in Simon [74]). It is proven by a suitable use of the Trotter product formula and the fact that  $\|CD\| \leq \|C\| \|D\|$ . Typically, in Ed's application, he appeals to a Feynman-Kac formula which has the Trotter formula built in and a use of Hölder's inequality which can replace  $\|CD\| \leq \|C\| \|D\|$  because in the path integral formulation, the operators become functions.

I'd like to sketch a proof of (5) since it is not appreciated that it follows from Löwner's theorem on monotonicity of the square root ([50]; see also [36, 42]). We start with

$$C^{1/2}\varphi = \frac{1}{\pi} \int_0^\infty w^{-1/2}(C+w)^{-1}C\varphi dw \tag{6}$$

By the functional calculus, it suffices to prove (6) when  $C$  is a number and by scaling when  $C = 1$ , in which case, by a change of variables, it reduces to an arctan integral. Since  $C(C+w)^{-1} = 1 - w(C+w)^{-1}$ , we have

$$\begin{aligned} 0 \leq C \leq D &\Rightarrow (C+w)^{-1} \geq (D+w)^{-1} \\ &\Rightarrow C^{1/2} \leq D^{1/2} \end{aligned} \tag{7}$$

which is Löwner's result.

Let  $A, B$  be finite Hermitian matrices. Since

$$0 \leq C \leq D \Leftrightarrow \|C^{1/2}D^{-1/2}\| \leq 1$$

(7) can be rewritten

$$\|C^{1/2}D^{-1/2}\| \leq 1 \Rightarrow \|C^{1/4}D^{-1/4}\|^2 \leq 1$$

which, letting  $C^{1/2} = e^A$ ,  $D^{1/2} = e^{-B}$ , implies

$$\|e^{A/2}e^{B/2}\|^2 \leq \|e^A e^B\| \quad (8)$$

Iterating (8) implies

$$\|(e^{A/2^n} e^{B/2^n})^{2^n}\| \leq \|e^{A/2^n} e^{B/2^n}\|^{2^n} \leq \|e^A e^B\|$$

Taking  $n \rightarrow \infty$  and using the Trotter product formula implies (5) for bounded matrices, and then (5) follows by a limiting argument.

Once one has (5), one gets lower boundedness by noting

$$\begin{aligned} \|e^{-TV} e^{-TH_0} \varphi\|_2 &\leq \|e^{-TV}\|_4 \|e^{-TH_0} \varphi\|_4 \\ &\leq C \|e^{-TV}\|_4 \|\varphi\|_2 \end{aligned}$$

so hypercontractivity and  $e^{-4TV} \in L^1$  implies  $\|e^{-T(H_0+V)}\| < \infty$ .

The term “hypercontractive” appeared in my paper with Høegh-Krohn [41], which systematized and extended the ideas of Nelson [56], Glimm [27], Rosen [65], and Segal [67]. The name stuck, and I recall Ed commenting to me one day, with a twinkle in his eye that many know, that after all “hypercontractive” was not really an accurate term since the theory only requires (4) with  $C < \infty$ , not  $C \leq 1$ ! That is,  $e^{-TH_0}$  is only bounded from  $L^2$  to  $L^4$ , not contractive. We should have used “hyperbounded,” not “hypercontractive.”

Ed was correct (of course!), but I pointed out (correctly, I think!) that hypercontractive had a certain ring to it that hyperbounded just didn’t have. There was, of course, a double irony in Ed’s complaint.

The first involves an issue that wasn’t explicitly addressed in [56]. What Ed proved, using  $L^p$  properties of the Mehler kernel, is that for the one-dimensional intrinsic oscillator,  $H_0 = -\frac{1}{2} \frac{d^2}{dx^2} + x \frac{d}{dx}$  on  $L^2(\mathbb{R}, \pi^{-1/2} e^{-x^2} dx)$ ,  $e^{-tH_0}$  is bounded from  $L^2$  to  $L^4$  if  $t$  is large enough with a bound on the norm between those spaces of the form  $1 + O(e^{-t})$  as  $t \rightarrow \infty$ . Ed then applied this to a free quantum field in a box with periodic boundary conditions. Because the eigenvalues of relevant modes go  $\omega_\ell \sim \ell$ , one has  $\prod_{\ell=0}^{\infty} (1 + e^{-\omega_\ell t})$  convergent, so this application is legitimate — [56] does not discuss anything explicit about the passage to infinitely many degrees of freedom, but this step was made explicit in [21]. (I thank Lenny Gross for making this point to me at the conference in Vancouver.) To handle cases like  $H_0$  in infinite volume, it is important to know that for  $t$  large enough,  $e^{-tH_0}$  is actually a contraction from  $L^2$  to  $L^4$ , so the discreteness of modes doesn’t matter. This was accomplished by Glimm [27] who showed that if  $H_0 1 = 0$ ,  $H_0 \upharpoonright \{1\}^\perp \geq m_0$  and (4) for some  $C$ , then by increasing  $T$ , (4) holds with  $C = 1$ .

The second irony concerns Ed’s second great contribution to hypercontractivity: the proof in [60] of optimal estimates for second quantized semigroups — exactly the kind of special  $H_0$  in  $e^{-tH_0}$  he considered in [56]. He proved such an operator from  $L^p$  to  $L^q$  was either not bounded or it was contractive!

His precise result is if  $H \geq a \geq 0$ , then  $\Gamma(e^{-tH_0})$  is a contraction from  $L^q$  to  $L^p$  if  $e^{-ta} \leq (q-1)^{1/2}/(p-1)^{1/2}$  and is not bounded otherwise. Here  $\Gamma(\cdot)$  is second quantization of operators; see [71].

Ed's work in these two papers on hypercontractive estimates spawned an industry, especially after the discovery of log Sobolev inequalities by Federbush [21] and Gross [31]. Brian Davies, in his work on ultracontractivity [17] and on Gaussian estimates on heat kernels [15], found deep implications of extensions of these ideas. While I dislike this way of measuring significance, I note that eighty papers in MathSciNet mention "hypercontractive" in their titles or reviews and Google finds 269 hits. See [16, 32] for reviews of the literature on this subject.

## 5 Taming Wick Ordering

There was a second element in [56] besides hypercontractivity, namely, the control of  $e^{-tV}$ . I want to schematically explain the difficulty and the way Ed solved it. In adding the interaction to a free quantum field, one might start with a spatial cutoff and want to consider

$$V_{un} = \int_{-L}^L \varphi^4(x) dx,$$

where  $\varphi$  is a free field. If  $g_1, \dots, g_8$  are Gaussian variables, then

$$\langle g_1 \dots g_8 \rangle = \sum_{\text{pairings}} \langle g_{i_1} g_{j_1} \rangle \dots \langle g_{i_4} g_{j_4} \rangle \tag{9}$$

over all 105 pairings of  $1, \dots, 8$ . Thus, in computing  $\langle V_{un}^2 \rangle$ , one gets  $\langle \varphi^4(x) \varphi^4(y) \rangle$  and the pairings  $\langle \varphi(x) \varphi(x) \rangle$  are infinite, since they are  $\int \frac{dk}{\sqrt{k^2 + \mu^2}} = \infty$ .

The solution is the very simplest of renormalizations, Wick ordering. If  $g$  is a finite Gaussian variable, one defines

$$: g^4 := g^4 - 6\langle g^2 \rangle g^2 + 3\langle g^2 \rangle^2. \tag{10}$$

The constants are exactly chosen, so in using (9) to compute  $\langle : g^4 :: h^4 : \rangle$ , all cross terms involving  $\langle g^2 \rangle$  drop out and

$$\langle : g^4 :: h^4 : \rangle = 24\langle gh \rangle^4 \tag{11}$$

which allows one to prove that  $V = \int_{-L}^L : \varphi^4(x) : dx$  makes sense and defines a function in  $L^2$ , indeed, in  $\cap_{p < \infty} L^p$ . The difficulty is that (10) says

$$: g^4 := (g^2 - 3\langle g^2 \rangle)^2 - 6\langle g^2 \rangle^2 \tag{12}$$

is no longer positive, so since  $\langle \varphi^2(x) \rangle = \infty$ ,  $V$  is no longer bounded below. What Ed realized is that it was still true that  $e^{-V}$  is integrable (and that using hypercontractivity,  $H_0 + V$  is bounded below).

To prove  $e^{-V}$  integrable, Ed made a momentum cutoff in  $\varphi$  to get a  $\varphi_\kappa$  with  $\langle \varphi_\kappa^2 \rangle \sim \log(\kappa)$  realizing the rate of divergence. He then wrote  $V = V_\kappa + V'_\kappa$  where  $V_\kappa$  is just  $\int_{-L}^L : \varphi_\kappa^4(x) : dx$  and  $V'_\kappa$  is the remainder. By using (12),  $V_\kappa$  is bounded below

$$V_\kappa \geq -C(\log \kappa)^2 \tag{13}$$

Moreover, it is easy to bound

$$\langle (V'_\kappa)^{2j} \rangle = \frac{(8j)!}{2^{4j}(4j)!} \langle (V'_\kappa)^2 \rangle^j \quad (14)$$

using Gaussian variable calculations. Here  $\langle (V'_\kappa)^2 \rangle$  goes to zero as a negative power of  $\kappa$ . Since (13) implies

$$\begin{aligned} \text{Prob}(V \leq -c(\log \kappa)^2 - 1) &\leq \text{Prob}(|V'_\kappa| \leq 1) \\ &\leq \langle (V'_\kappa)^{2j} \rangle \end{aligned}$$

for any  $\kappa$ , one can choose  $j$  using the explicit formula in (14) to optimize this bound and find

$$\text{Prob}(V \leq -x - 1) \leq \exp(-c_1 \exp(c_2 x^{1/2})),$$

which implies  $\langle e^{-V^p} \rangle < \infty$  for all  $p$ .

This general idea of decomposing the interactions, undoing the renormalization to control one piece, and using  $L^p$  estimates on the other piece became a standard tool in much later work [29] in constructive quantum field theory.

## 6 Euclidean Quantum Field Theory

I've saved the best for last. In 1971–72, Ed, with an important boost from Guerra [33] (also [34]), in part following up on work of Schwinger [66] and Symanzik [76, 77], caused a revolution in mathematical quantum field theory, at least the model-building side. Ed's Euclidean Field Theory and the lattice approximation of Guerra-Rosen-Simon [35] that it motivated totally changed the objects studied. The structure of QFT in 1973 looked very different from 1971, although it looks very similar in 1973 and 2003! The way high energy physicists looked at quantum fields also changed to Euclidean and lattice models during this period. I'm not enough of a historian of the subject to know to what extent mathematicians' work had an impact on them.

Ed developed his ideas in the first part of 1971, gave a few lectures at Princeton (I only heard the first and didn't understand where it was heading or what it was good for!), and a lecture in Berkeley [58]. In retrospect, it is remarkable that — despite Ed talking in Princeton at a lecture attended by all or almost all the local experts and in Berkeley to many of the other workers — there was almost no reaction. I don't remember the details of the Princeton talk, but I assume it was close to the published Berkeley lecture [58], and rereading it, I can perhaps reconstruct my own reasons for not catching on.

I think Ed partly had me in mind when he stated in [58]: “Probabilistic methods ... have been used in quantum field theory particularly by Glimm, Jaffe, Rosen and myself. The usual reaction of workers in the field is to recoil in horror and to attempt to find alternate methods.”

It's true that until Euclidean Field Theory changed my tune, I tended to think of probabilists as a priesthood who translated perfectly simple functional analytic ideas into a strange language that merely confused the uninitiated. In [71], the dedication

says: “To Ed Nelson who taught me how unnatural it is to view probability theory as unnatural.”

Ed's lecture has one result that should have made everyone sit up and take notice — a really simple proof of the linear lower bounds on the cutoff vacuum energy. But by dressing the proof in layers of functors, Markov properties, multiplicative functionals, and only sketching it (my brief sketch below is much more detailed than his!), he perhaps obscured its great simplicity. Because I had then recently found my own simple (but it turns out not nearly as simple as Ed's!) proof [69], I put his work aside.

Francesco Guerra was visiting Princeton at the time and was rather quiet and unassuming. At Guerra's request, Arthur Wightman set up a meeting with Lon Rosen and me in January 1972. Guerra began by listing on the blackboard what he was going to prove. It was as if he were from another planet. His first result was the linear lower bound that Ed and others had proven. All his other results (starting with existence of the limits) were beyond anything that the then current technology could prove. I remember thinking to myself: “Yeah, sure, you're gonna prove all that.” He proceeded to say he needed Nelson's ideas and, in particular, something he called Nelson's symmetry. I'd seen this on the blackboard during Ed's talk eight months before, but thought it a curiosity. Within fifteen minutes, Guerra had explained his proofs of the results that I'd found impossible to believe! It was like a thunderbolt.

Within a week, Guerra, Rosen, and I had used these ideas to recover [34] some bounds of Glimm-Jaffe [28] whose proofs were regarded as very hard. The next week, Glimm visited to give a talk on this bound, sketching the strategy of their proof. After his talk, Lon and I cornered Glimm and described the new proof. He seemed to have the same jaw-dropping reaction I'd had. Euclidean Field Theory had arrived, but at least six months late. Six months may not seem like a lot, but in the next six and twelve and eighteen months, there was a flood of new results that came from exploiting the Euclidean point of view (the breathless pace is described in some detail in the introduction of [71]).

This is not the place to give a minicourse on Euclidean QFT, but I'd like to make some general remarks to explain what Ed did.

Prior to Ed's work, the usual way path integrals came in was to cut off the field theory to get a finite-dimensional system, write down a path integral for that, estimate, and try to get results that survived removal of the cutoff. From my point of view, what Ed did first of all was to view the free field semigroup as a positivity-preserving operator on an infinite-dimensional space. Such a positivity condition allows one to build up an abstract path integral which, for the free field, could be written as an explicit Gaussian process. This process is formally an analytic continuation of the quantum field from real time to imaginary time, and the continuation of Minkowski invariance to the Gaussian process is Euclidean invariance, that is, the process covariance  $C(x-y)$  is invariant under rotations. Indeed,  $C$  is the integral kernel (Green's function) for  $(-\Delta + m^2)^{-1}$ .

It turns out that the process Ed wrote down had been written down somewhat earlier by Pitt [63] who discussed its properties as a multiparameter Markov process but didn't consider any connection to QFT.

Ed's proof of the linear lower bound depended on this Euclidean invariance. First, let

we describe the Feynman-Kac-Nelson formula, the Feynman-Kac formula for this case.

Let  $H_0$  be a free quantum field Hamiltonian,  $\Omega_0$  its ground state, and  $V \equiv V(\varphi(x, 0))$  as functions of the time zero fields. Then

$$\begin{aligned} & \langle F_1(\varphi(x, 0))\Omega_0, e^{-t(H_0+V)}F_2(\varphi(x, 0))\Omega_0 \rangle \\ &= \int F_1(\varphi(x, t))F_2(\varphi(x, 0))e^{-\int_0^t V(\varphi(x, s)) ds} d\mu_0 \end{aligned} \tag{15}$$

where  $\mu_0$  is the Gaussian measure for the free Euclidean field. The proof is by the Trotter product formula method of Nelson [53].

In particular, if  $V_\ell \equiv \int_{-\ell/2}^{\ell/2} F(\varphi(x, 0)) dx$ , where  $F(\varphi)$  is a local function of the field (think :  $\varphi^4(x, 0)$  :) and  $H_\ell(\lambda) = H_0 + \lambda V_\ell$ , then Euclidean invariance and (15) imply Nelson's symmetry

$$Q_{t,\ell}(\lambda) \equiv \langle \Omega_0, e^{-tH_\ell(\lambda)}\Omega_0 \rangle = \langle \Omega_0, e^{-\ell H_t(\lambda)}\Omega_0 \rangle. \tag{16}$$

To get the linear lower bound on

$$E_\ell(\lambda) = \inf \text{spec}(H_\ell(\lambda)),$$

we need to use hypercontractivity, Ed's earlier idea, and Hölder's inequality in the FKN formula (15):

$$\begin{aligned} |\langle F_1\Omega_0, e^{-tH_\ell(\lambda)}F_2\Omega_0 \rangle| &\leq \langle \Omega_0, e^{-tH_\ell(4\lambda)}\Omega_0 \rangle^{1/4} \langle |F_1|^{4/3}\Omega_0, e^{-tH_0}|F_2|^{4/3}\Omega_0 \rangle^{3/4} \\ &\leq Q_{t,\ell}(4\lambda)^{1/4} \|F_1\|_2 \|F_2\|_2, \end{aligned} \tag{17}$$

where  $t$  is chosen so  $e^{-tH_0}$  is a contraction from  $L^{4/3}$  to  $L^4$  (if  $e^{-sH_0}$  is a contraction from  $L^2$  to  $L^4$ , by duality, it is a contraction from  $L^{4/3}$  to  $L^2$  and so  $e^{-2sH_0}$  is a contraction from  $L^{4/3}$  to  $L^4$ ).

(17) says that

$$\begin{aligned} e^{-tE_\ell(\lambda)} &\leq Q_{t,\ell}(4\lambda)^{1/4} \\ &= Q_{\ell,t}(4\lambda)^{1/4} \quad \text{by (16)} \\ &\leq e^{-\ell E_t(4\lambda)/4}. \end{aligned}$$

Thus

$$E_\ell(\lambda) \geq \frac{\ell}{4t} E_t(4\lambda),$$

which is the linear lower bound.

This is actually the hardest application of Nelson's symmetry. What Guerra shocked us with is actually much simpler! For any selfadjoint operator,  $A$ , bounded from below, and any unit vector  $\varphi$ , we have

$$\langle \varphi, e^{-tA}\varphi \rangle = \int_\alpha^\infty e^{-tx} d\mu(x)$$

for some  $\alpha$  ( $= \inf \text{spec}(A)$ ) and probability measure,  $d\mu$ . Hölder's inequality thus implies

$$t \rightarrow \log \langle \varphi, e^{-tA} \varphi \rangle$$

is convex. Since  $E_\ell = -\lim_{t \rightarrow \infty} \frac{1}{t} Q_{t,\ell}$  (we henceforth set  $\lambda \equiv 1$  and drop it!), we see, by Nelson's symmetry, that  $\ell \rightarrow E_\ell$  is concave. Concave functions,  $g$ , with a linear lower bound always have that  $g(\ell)/\ell$  has a finite limit,  $e_\infty$ , and  $g(\ell) - \ell e_\infty$  is monotone, and so it also has a limit! In this way, Guerra showed that the energy per unit volume and the surface energy actually converged!

There is a postscript to Ed's breakthrough, a final significant contribution. Guerra, Rosen, and I realized that (15) made Euclidean QFT look like classical statistical mechanics (at least for bosons). A key tool in the rigorous statistical mechanics of the time were correlation inequalities, starting with Griffiths [30]. It was natural to try to prove them in EQFT by approximating with a discrete system. Since the free EQFT was the Gaussian process with covariance  $(-\Delta + m^2)^{-1}(x, y)$ , it was natural to use a Gaussian lattice theory with covariance  $(-\Delta_\delta + m^2)_{ij}^{-1}$  where  $\Delta_\delta$  is a discrete Laplacian. In the Gaussian measure, the inverse of covariance matrix appears, so the Gaussian measure here has  $\Delta_\delta$  in it, that is, nearest neighbor interactions. So was born the lattice approximation [35]. Using ideas of Ginibre [26] for general classes of spin systems, we could get correlation inequalities for EQFT!

In the applications though, there was one catch. One of the nicest applications of Griffiths' work was the existence of the infinite volume limit. A system of spins  $\sigma_j$  in volume  $\Lambda$  with no spins outside  $\Lambda$  has correlations monotone in  $\Lambda$ , so a limit existed. Our problem was that the  $\delta \downarrow 0$  limit we could control was to the free, infinite volume  $(-\Delta + m^2)^{-1}$  Gaussian process. The  $\Lambda$  dependence was where the local interaction was turned on. There was no monotonicity in  $\Lambda$  in this limit.

We could follow Griffiths and take a sharp cutoff lattice theory, take the limit for that as  $\Lambda \rightarrow \infty$ , and then hope to take  $\delta \downarrow 0$ , but the lattice cutoff destroyed rotation symmetry.

Ed made a crucial remark. The finite  $\Lambda$  theory with sharp cutoff had a limit as  $\delta \downarrow 0$ . It was just a theory where the  $(-\Delta + m^2)^{-1}$  Gaussian process was replaced by  $(-\Delta_\Lambda + m^2)^{-1}$  with  $\Delta_\Lambda$  a Dirichlet Laplacian! One needs to take a  $\Lambda$ -independent local interaction so Wick order is done relative to  $\Delta$  not  $\Delta_\Lambda$  ([35] calls this "half-Dirichlet" boundary conditions). With this remark, it was easy to get a Euclidean invariant infinite volume limit, and so, using other ideas of Nelson [59], the first interesting field theory obeying all the Wightman axioms except perhaps uniqueness of the vacuum.

Many would have insisted on publishing this crucial remark in their own name, but Ed urged us to use it and we, of course, acknowledged his contribution. [61] does include this remark, but it is unclear from the discussion there that the remark is due to Ed and not to GRS!

Kuhn [44] regards the hallmark of a scientific revolution as a change of paradigm. The only way to think of the change in rigorous QFT produced by Ed's introduction of EQFT ideas is as such a change of paradigm.

## 7 Some Concluding Remarks

Lipman Bers in the introduction to Löwner’s “Complete Works” [10] described Löwner in a way that so accurately describes Ed Nelson that I’ll quote it here: “. . . was a man whom everyone liked, perhaps because he was a man at peace with himself. He conducted a life-long passionate love affair with mathematics, but was neither competitive, nor jealous, nor vain. His kindness and generosity in scientific matters, to students and colleagues alike, were proverbial. He seemed to be incapable of malice. His manners were mild and even diffident, but those hid a will of steel . . . But first and foremost, he was a mathematician.” Contemplate how many really first-class mathematicians for whom one can say they are “neither competitive, nor jealous, nor vain” and appreciate Ed for who he is!

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## News from the IAMP Executive Committee

### New individual members

IAMP welcomes the following new members

1. DR. BASTIEN FERNANDEZ, CNRS, France
2. PROF. ABDELMALEK BOUMALI, University of Tebessa, Algeria
3. DR. ADRIAN SOTOMAYOR, Department of Mathematics, Antofagasta University, Chile
4. PROF. JAN DE GIER, Department of Mathematics and Statistics, University of Melbourne, Australia
5. DR. MATTEO SOMMACAL, Department of Mathematics and Information Sciences, University of Northumbria at Newcastle, UK
6. DR. HISHAM SATI, Department of Mathematics, University of Pittsburgh, USA
7. DR. MARCIN NAPIORKOWSKI, IST Austria
8. ROBIN REUVERS, Department of Mathematical Sciences, University of Copenhagen, Denmark
9. DR. GERHARD BRÄUNLICH, Institute for Mathematics, Friedrich-Schiller-University Jena, Germany
10. HANNE VAN DEN BOSCH, Facultad de Física, Pontificia Universidad Católica de Chile
11. DR. ZHIGANG BAO, IST Austria
12. DR. GUSTAVO DE OLIVEIRA, Departamento de Matemática, Universidade Federal de Sao Carlos, Brazil
13. DR. GIUSEPPE DE NITTIS, Departement Mathematik, Universität Erlangen-Nürnberg, Germany
14. DR. SÖREN PETRAT, IST Austria
15. DR. DEREK WISE, Department of Mathematics, University of Erlangen-Nürnberg, Germany
16. DR. MARTIN FRAAS, Institute of Mathematics, LMU Munich, Germany
17. PROF. HUGO DUMINIL-COPIN, Département de Mathématiques, Université de Genève, Switzerland

18. PROF. ANTOINE GLORIA, Department of Mathematics, Université Libre de Bruxelles, Belgium
19. DR. JULIEN SABIN, Laboratoire de Mathématiques d'Orsay, Université Paris-Sud, France

## **Recent conference announcements**

### **The Dynamical Systems, Ergodic Theory, and Probability Conference**

May 18-20, 2015. University of Alabama at Birmingham (UAB).

Organized by Alexander Blokh, Paul Jung, and Lex Oversteegen.

Dedicated to the memory of Nikolai Chernov.

### **NEEDS (Nonlinear Evolution Equations and Dynamical Systems)**

May 24-31, 2015. Santa Margherita di Pula, Sardinia, Italy.

Organized by Francesco Demontis, Sara Lombardo, Marta Mazzocco, Barbara Prinari and Matteo Sommacal.

This conference is partially supported by the IAMP.

### **Master Class on Quantum Mathematics**

May 26-29, 2015. University of Copenhagen.

Organized by the QMath group (led by Jan Philip Solovej, Matthias Christandl and Bergfinnur Durhuus) at the Department of Mathematical Sciences at the University of Copenhagen.

### **Thematic semester on AdS/CFT, Holography, Integrability**

June 2015-January 2016. Centre de Recherches Mathématiques, Montréal, Canada.

Scientific committee: M. Bertola, R. Brandenberger, F. Cachazo, J. Harnad, A. Its, V. Kazakov, J. Maldacena, A. Maloney, R. Roiban, J. Walcher.

### **Constructive Renormalization Group: A conference in memory of Pierluigi Falco**

June 9-11. INFN - Laboratori Nazionali di Frascati (Rome).

Organized by G. Benfatto, M. Lombardo, V. Mastropietro, A. Pizzo.

Dedicated to the memory of Pierluigi Falco.

### **Workshop on Quantum Spin Systems**

June 22-24, 2015. Cergy-Pontoise University.

Organized by Laurent Bruneau, Flora Koukiou, Bruno Nachtergaele, Robert Sims.

### **Operator Algebras and Quantum Physics**

July 17-23, 2015. USP - Sao Paulo, Brazil.

Satellite Conference to the XVIII International Congress on Mathematical Physics.

Organized by J.C. Alves Barata, R. Bissacot, M. Forger, C. Jäkel, D. Marchetti, C.R. de Oliveira, W. de Siqueira Pedra, P. Lauridsen Ribeiro, P.A. Faria da Veiga.

## **Open positions**

### **Graduate Student Position at Memorial University**

The Department of Mathematics and Statistics at Memorial University of Newfoundland, Canada, invites applications for a graduate student position at the Master's or Ph.D. level, in the field of mathematical physics / open quantum systems. The starting date is September 2015 or January 2016. Please contact Marco Merkli at merkli@mun.ca if interested. The deadline of application is May 31st, however, the position may be filled before this date.

The deadline of application is May 31st, 2015.

### **Associate Professor of Statistics or Probability Theory**

The Department of Mathematical Sciences at the University of Copenhagen is looking for an expert in Statistics or Probability Theory. The successful applicant has to be an internationally renowned researcher at a very high level. The position is open from 1 January 2016 or as soon as possible thereafter.

The deadline for applications is September 25, 2015.

More information and instructions to apply can be found at

<http://www.math.ku.dk/english/about/jobs/sandsynlighedsregning/>

More job announcements are on the job announcement page of the IAMP

[http://www.iamp.org/page.php?page=page\\_positions](http://www.iamp.org/page.php?page=page_positions)

which gets updated whenever new announcements come in.

**Benjamin Schlein** (IAMP Secretary)

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