International Association of Mathematical Physics

News Bulletin

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Contents

Bulletin editor

Valentin A. Zagrebnov

Contacts

http://www.iamp.org and e-mail: bulletin@iamp.org

Cover photo: "O Tiger-lily," said Alice, addressing herself to one that was waving gracefully about in the wind, "I wish you could talk!" "We can talk," said the Tiger-lily: "when there's anybody worth talking to." (According to a modern Quantum Information casting: Alice is the sender/encoder, Bob is her favourite recipient/decoder and Eve is the eavesdropper.)

Before passing the helm

by Pavel Exner (IAMP President)

This editorial is going to be longer than usual for the obvious reason: the term of the present Executive Committee is drawing to its end and by the time you will get the next issue, the Association will already have new leadership for the next three-year period.

A short time ago you all received an invitation to vote with the information required by the Statutes, in particular, a list of candidates suggested by the EC who are eligible and agreed to stand the election and serve if elected. Let me add in passing that the invitation came later than expected due to technical problems related to the database transfer from Mainz to Braunschweig. Although we initiated the process early enough it was not under our control and we have to apologize for any related inconveniences.

Since IAMP has many new members and we cannot longer suppose that everybody knows everybody, most of these candidates provided short electoral statements which you will see on the ballot page. Please pay attention to them. Furthermore, one has to stress that you can vote not only for people on the list; the ballot form contains empty slots into which you can fill the name of any other member provided he or she is eligible being in good standing at the beginning of the year.

What is most important, of course, is that each of us takes part in the elections and casts the ballot within the indicated period, before the end of October, thus giving the new IAMP executive a strong mandate.

The end of the EC term is also an opportunity to look back and summarize what we have achieved — and where we have failed — in this three-year period. I have to start with personal thanks the Executive Committee members for the friendly and efficient way we have been able to work, mostly through wires with the exception of one meeting at the last ICMP and various *ad hoc* encounters in various places of the world. In one breath with the EC I have to name Jean Downes who supported us with the secretarial work.

My most sincere thanks go to my fellow officers in this three-year period, Bruno Nachtergaele as vice president, Jan Philip Solovej as secretary and Lászlo Erdös as treasurer — they have been a great team.

The Association changed more than a little on this EC watch. At a glance the list of members shrunk as result of our decision to improve the way the dues are collected. One has to stress that while we implemented the rule contained in the Statutes by which the membership is terminated if the dues are not paid for more than a year, we declared a generous amnesty allowing people to restore their good standing by paying for just the last two years; recall that many on the list were in debt for a much longer time or even never paid anything. During a year we repeatedly addressed those in arrears and said good-bye only to those who ignored all those invitations; we believe that such a step was necessary for the health of the IAMP.

To keep the Association open to people from poor countries, we honoured the tradition that their dues are waived, however, we made more precise the eligibility rules for this "reduced dues" status.

At the same time we were persistently trying to attract new members and to some degree we have succeeded. Since January 2009 over 150 colleagues joined our ranks, so at present the IAMP membership is over 640. We believe that the Association has still a significant potential to grow and that our successors will continue our efforts. We have reviewed also the status of our Associate Members, and what is important, we have acquired several new ones, specifically American Institute of Physics, Cambridge University Press, Center for Mathematical and Theoretical Physics (in Rome), Pacific Institute for Mathematical Studies, and Steklov Mathematical Institute.

The life of a mathematical physicist is never free of dangers. A fresh illustration was provided by the story of another IAMP Associate Member, the Erwin Schrödinger Institute in Vienna, which appeared suddenly at the brink of disappearance following a dubious decision of an Austrian politician. We were a part of the broad international protest, and it is good to know that the community voice was strong enough to achieve the survival of ESI as a part of Vienna University.

The crucial factor in attracting new members is to make the Association visible and interesting for people in the field. Speaking figuratively, the scene is now much more crowded than it was in the seventies of the last century when the IAMP was born. With this fact in mind we decided to reshape and revive our main communication tool, the News Bulletin. Since October 2009 it has appeared in a new form containing not only dry official announcements but also articles, interviews, opinions, and other interesting reading. I want to express my heartfelt thanks to Valentin Zagrebnov and members of his editorial team who spent a lot of time and energy to make this possible.

It is my sincere hope that this important work will continue and the editorial team will function independently of the IAMP three-year cycle, similarly as the webmaster does in the present. Speaking about the web page, it is appropriate to note that we have changed its design hoping that the new one is more user-friendly and pleasant to look at.

One more thing which deserves to be kept and developed further is the working group we instituted to look into the problems mathematical physics is facing in less fortunate parts of the world and into the ways we can, with the means at our disposal, help our colleagues there. The initiative is fresh and not very visible so far, but its goal is important and we should remember that life does not end up at the borders of affluent countries.

Turning to our traditional activities, the first thing to mention is our congress. In Prague we decided that the next one will be held in Aalborg. Hardly any of you would doubt that bringing such a meeting to life means a lot of hard work and I want to thank the Local Organizing Committee headed by Arne Jensen for all they have done so far and what they would do before we will meet next August in the north of Denmark. Also the International Scientific Committee was not idle and a look at the congress page, http://www.icmp12.com/, shows that the programme is taking shape.

There are two important things to be recalled in connection with the congress. As usual we will distinguish there the best achievements in our field, so do not forget to propose candidates while the calls for nominations are open. In Aalborg we will also decide about the site of the next ICMP, to convene in 2015, and I encourage all of you in the position to make a bid to think seriously about it. Organizing a congress is a lot of hard work, of course, but it is a distinguished service to the community and the effort will be highly appreciated. With four of the last five congresses, the next one being taken into account, held in Europe, it is desirable to have proposals from the other parts of the world; none of us wants mathematical physics to belong to a single continent.

The congress is the largest and most important meeting, but not the only one. In the last three years the IAMP financially supported ten conferences and schools. You could find reports about most of them in the News Bulletin. Given the Association budget, the support was limited and the organizers had to seek other and more substantial resources, nevertheless we hope our contribution helped. In addition to that, there were 28 conferences we recognized and supported morally by spreading the information about them. Speaking about meetings, it is also appropriate to recall that we agreed on common summer schools with the European Mathematical Society; we are working on having the first one in the next summer.

The list of our achievements could be complemented by that of our failures. Should I mention just one then it is the fact that a large part of the mathematical-physics community still stays outside the IAMP; if you want a proof, just compare the list of participants, and even speakers at an ICMP with the Association member database. I am afraid that it will take a longer time to correct the situation and to secure for the Association a truly dominant position. We made steps towards that goal, but our successors should not relax and have to go on with the effort.

To close this overview where it began, let me repeat an invitation to all the members to cast their ballots. The future of the IAMP is in your hands.

An EMS interview with Yves Meyer, part II

The IAMP News Bulletin wishes to extend to the IAMP membership this reprint made earlier for EMS members. We publish here (together with the previous issue) a complete version of an interview, originally taken by Ulf Persson (Göteborg, Sweden) for the European Mathematical Society, whose excerpt has appeared in the EMS Newsletter, June 2011, issue 80. The IAMP News Bulletin is very thankful to Yves Meyer for the complete original file of his interview as well as to Ulf Persson for authorisation to reproduce all his questions.

(*. . . continuation from the previous issue of the bulletin. . .*)

(. . .) Concerning wavelets, the story is quite strange. At École Polytechnique where I was teaching, mathematicians and mathematical physicists were sharing the same Xerox machine. Jean Lascoux, a physicist, was making copies of every paper he would receive. If you needed to make a copy, you had to wait until he had finished. I was never irritated. I was happy to discuss half an hour with Jean while he was making his too many copies. Once he said: "Yves, I am sure this preprint will mean something to you." It was the first Grossmann-Morlet paper on wavelets. I recognized Calderón's reproducing identity and I could not believe that it had something to do with signal processing. I took the first train to Marseilles where I met Ingrid Daubechies, Alex Grossmann and Jean Morlet. It was like a fairy tale. This happened in 1984. I fell in love with signal processing. I felt I had found my homeland, something I always wanted to do.

My move to Navier-Stokes equations occured ten years later and was initiated by a demand by Jacques-Louis Lions. I was still working on wavelets when Paul Federbush published a paper with a provocative title: "Navier and Stokes meet the wavelets". Navier-Stokes equations govern fluid dynamics. Jacques-Louis Lions was puzzled and irritated both by the title and by the contents. He asked my opinion about the relevance of wavelets in this approach to Navier-Stokes equations. I had no experience of this subject but I was fortunate to welcome Marco Cannone, a student of Carlo Cercignani. Marco had acquired training on nonlinear PDEs in Milano. With the help of Marco, I could understand what Federbush had in mind. As I expected, wavelets were irrelevant in Ferderbush's paper and a conventional Littlewood-Paley approach worked better. With Marco we went further. Fabrice Planchon joined us. We proved the global existence of a Kato solution when the initial condition is oscillating. A Kato solution to the three-dimensional Navier-Stokes equations is continuous in time with values in $L^3(\mathbb{R}^3)$. This result was a complete surprise since it says that arbitrarily large initial velocity generates a global solution as long as this initial velocity has enough oscillations. Oscillations are measured with a Besov norm which is much weaker than the L^3-norm. Here Kato is the same Tosio Kato as the one we will meet below in relation to Calderón's conjectures. The best results in that direction were eventually obtained by Herbert Koch and Daniel

Tataru. Pierre-Gilles Lemarié-Rieusset proved the uniqueness of Kato's solutions. I did not attack Navier-Stokes equations in a joint work with a specialist. I did it to answer a question by Jacques-Louis Lions. Here I would like to say how much I am grateful to Jean-Michel Bony, Jean-Yves Chemin, Patrick Gérard, Guy Métivier and many others who helped me with care and patience.

As I will develop later on, it often happens in mathematics that a problem cannot be solved inside the field where it has been formulated. One first needs to recast this problem and insert it in a completely new circle of ideas. Then a solution can emerge. I will say more on this theme after evoking Antoni Zygmund. Before meeting Zygmund, I had read with extreme pleasure his book "Trigonometric Series". The first edition, published in 1935, was the only one which was available in Strasbourg's mathematical library. The second edition would probably have been discouraging to me. This happened in 1964 when I was preparing my Ph.D. To someone used to the Bourbaki style, reading Zygmund's treatise was as refreshing as giving up "L'être et le néant" by Jean-Paul Sartre and switching to Anna Karenina by Tolstoï. Zygmund's treatise was decisive in my early scientific orientation. "Trigonometric Series" was a collection of fascinating problems about Fourier series expansions. I truly fell in love with Zygmund's mathematical style. I was thirty years old when I first met Antoni Zygmund. Zygmund treated me as a child who still needed advice. I loved this attitude. He told me that a problem should always be given its simplest and more concise formulation. Whenever it is possible Zygmund would advise to treat an illustrative example before attacking the general case. This attitude was strictly forbidden by Bourbaki. Instead the Bourbaki group ordered to raise a problem to its most general and abstract formulation before attacking it. Let me mention that the proof of Ramanujan's conjecture on the growth of coefficients of the Taylor expansion of the τ-function was obtained by Pierre Deligne and used a reformulation by André Weil. Today I know that there exists a third attitude which unfortunately cannot be systematic. This third approach consists in translating a problem into the language of a completely distinct branch of mathematics. That is the way I could solve the problem of the boundedness of the Cauchy kernel on Lipschitz curves. This problem was raised by Alberto Calderón in the late sixties and was formulated in the language of complex analysis, more precisely of holomorphic functions of one complex variable. It is the problem Coifman urged me to attack when I visited Wasington University in 1974. Alan McIntosh discovered that Calderón's problem could be reformulated inside a program in abstract operator theory proposed by Tosio Kato. More precisely Kato raised the issue of the symbolic calculus on accretive operators. McIntosh understood two fundamental facts. He discovered that Kato's conjecture could not be true at the level of generality it has been formulated. In the concrete setting of differential operators in one real variable, McIntosh proved that Calderón's problem was identical to Kato's conjecture. That vision led to a completely new approach to Calderón's conjectures. Then I could solve these conjectures.

I met Alan in a strange way. In 1980 he was awarded a sabbatical year and was visiting the University of Orsay. I was already teaching at École Polytechnique and I was giving a graduate course at Orsay. In 1980 my colleagues at Orsay were refusing to teach graduate courses for obscure political reasons. Polytechnique did not have a graduate school at that time. Alan was sitting silent in the last row of the classroom. I was intrigued by this man who obviously was not a student and I invited him to lunch. Then he revealed what he was trying to do. As I said above Alan had discovered that Calderón's problem was a corollary of a conjecture raised by Kato. We then were on the good track. Six months later I gave the final assault and reached the summit after a visit at Yale and some important discussions with R. Coifman. Tosio Kato was unaware of Calderón's work and vice versa. Kato's full conjecture on the domain of the square root of accretive second order differential operators in n real variables could then be attacked and solved using some improved real-variable tools. This was achieved by Pascal Auscher and his collaborators. Unfortunately Calderón was dead when Pascal Auscher did it.

Another example illustrating my point is the solution by Pierre Deligne of Ramanujan's conjecture on the τ-function. The methods came from algebraic geometry and used Grothendieck's program. Ramanujan's conjecture was viewed by many mathematicians as a problem in analytic number theory. Solving a problem through a rephrasing in a new mathematical language always gave me an intense feeling of happiness. Then people with distinct culture are able to communicate and understand each other. I enjoyed the same pleasure in my work on wavelets when I understood that my Calderón-Zygmund expertise was useful in signal processing. An unexpected happiness.

When I switched from singular integral operators to wavelets, my students Guy David and Jean-Lin Journé did not follow my move to signal processing. Instead they made a major breakthrough in the theory of singular integral operators with their celebrated $T(1)$ theorem. Later on Guy David and Xavier Tolsa solved the famous Painlevé problem. Painlevé wanted to characterize the compact subsets K of the complex plane such that every function F which is holomorphic and bounded in $\Omega = \mathbb{C} \setminus K$ is a constant (Liouville theorem for Ω). Similarly when I gave up wavelets for studying Navier-Stokes equations, my work on wavelets was completed by the fantastic discovery by Ingrid Daubechies of orthonormal bases of compactly supported wavelets and by the spectacular achievements of Albert Cohen. It is as if my students were saying: "Do not worry, feel free to travel, we will take care of everything at home". What I did in mathematics is negligible as compared to what was achieved by my students. This statement is the true meaning of my mathematical life.

Applied versus pure.

I would like to evoke an everlasting memory. In 1984, after the brutal death of my friend Charles Goulaouic, I was so depressed that I was unable to do mathematics. I was in charge of the Goulaouic-Schwartz seminar, a seminar launched by Charles Goulaouic and Laurent Schwartz at École Polytechnique. Jacques-Louis Lions was then the head of the French Space Agency (CNES). He accepted to give a talk at the Goulaouic-Schwartz seminar. The main issue of his talk was the control and stabilization of some dangerous oscillations which might occur on the Space Laboratory, which was under construction. This control was expected to be made possible by firing some tiny rockets installed at some given positions on the Space Lab. Jacques-Louis Lions had the magic power to translate this control problem into a question in pure mathematics. A week later I solved his problem. Jacques-Louis Lions was extremely happy. I understood for the first time in my life that what I was doing in pure mathematics could be used in applied mathematics. Up to that time I was thinking that pure mathematics and applied mathematics were distant and isolated continents. I could not believe that some communication with applied mathematicians was possible to me. Jacques-Louis Lions told me it was the case. I am so grateful. He helped me at a crucial moment of my life. A year later I moved to signal processing and I discovered the existence of orthonormal wavelet bases belonging to the Schwartz class.

Any good (and bad?) advice you would like to give to young mathematicians?

When I was preparing my Ph.D. a faculty member of the University of Strasbourg, Peter Gabriel, who is now emeritus at the University of Zürich, told me the following: "Yves you should give up doing classical analysis and switch to algebraic geometry instead. The language of algebraic geometry has been put upside down by Grothendieck and people above 40 are completely lost now. Young people can freely work in this field without being challenged by the old generation. In classical analysis (what I was doing) you are fighting against all the older specialists who have accumulated so much training and experience." I did not follow Gabriel's advice and, as he predicted, my Ph.D. was immediately defeated by Elias Stein who was attacking the same problems at the same time with more powerful methods. Stein used Calderón's work. Calderón proved the equivalence between the L^1 norm of the square function of a holomorphic function $F \in \mathcal{H}^1$ and the L^1 norm of F. Then E. Stein could apply to the operator theory on Hardy spaces the tools that Hardy, Littlewood and Marcinkiewicz have been using in the context of L^p spaces. I was not familiar with this piece of mathematics. Let me mention that my thesis was not directed by a supervisor. Studying operator theory on the Hardy space $\mathcal{H}^1$ was my decision. I brought the manuscript to Jean-Pierre Kahane when the eleven chapters of my thesis were written and typed by my wife. My Ph.D. was in some sense a failure and a success. It was a failure since I did not obtain the results I wanted to prove. But my guesses on the future devolpments of harmonic analysis were the correct ones. Moreover I was going to collaborate with Calderón eight years later. Indeed Coifman and I proved the continuity of the second commutator, a problem raised by Alberto Calderón. Let me insist that I did not fight against Calderón. Instead he became my friend as I will tell elsewhere.

My advice to young mathematicians is to disobey and follow their inclination, whatever the advice of older people. Indeed you need to dig deeply in your own self to do something as difficult as research in mathematics. You need to believe that you possess a treasure in the very bottom of your mind, a treasure which has to be unveiled.

A last advice to young mathematicians is to simply forget the torturing question of the relevance of what they are doing. It is clear to me that the progress of mathematics is a collective enterprise. All of us are needed.

Would you think of mathematics as part of natural science or as part of the humanities? (Or straddling the middle ground?) In other words, if you had not chosen to become a mathematician, would you have worked as

an engineer, or a physicist, or a chemist ? Or would you have become a philosopher, a historian or perhaps a writer ?

I am happy to answer this question. In the fifties the best students of the French high schools were studying humanities. I took Latin and Greek. I was and I am still completely fascinated by the personality of Socrates. I am reading Plato again and again. To me Socrates is an older brother. In Phedon, the argumentation of Socrates is often questioned by Cebes or Simmias. When it breaks down the argument needs to be repaired by Socrates. This explains my first fascination for mathematics. In mathematics and only in mathematics a child can discover something by himself. For instance when I was forteen, I solved by myself the Diophantine equation $a^2 + b^2 = c^2$. This seems ridiculous, as compared to Gauss, but it was a beginning. As a student, I could say to my teacher that he was wrong or that I had a better proof. Like Simmias criticizing Socrates' argument. In physics you have to believe. You cannot tell your teacher that the experiment by Michelson and Morley is wrong. Believing this experiment had the same philosophical status as believing in God. You believe something you cannot check by yourself. It is pure faith.

It means something very important. The old tradition in mathematical education stressed the importance of correct proofs. Mathematics was identified with geometry. I was in love with Euler's nine points circle. The bad side effect of this old tradition is that almost all students believed that mathematics was a field far from the scientific and industrial development of a country. Mathematics was identified with dead knowledge. In France we recently moved from one extreme to another one. For reasons that are grounded in demagogy and populism, there is a tendency to give up proofs in high schools and to teach mathematics as a natural science. It is a difficult choice.

Would you be able to identify any interesting parts of mathematics that have problems that do not require much machinery and thus prior education and which hence can be attacked by elementary techniques ?

This is a crucial question. Let me tell the method I used with my graduate students. I received them every week and they were given then a problem to be solved along the line of what I hoped to be a good direction of research. The problem I was giving then was slightly harder than the one I gave the preceding week. During the first three months I knew how to solve the problem but I concealed my knowledge to the student in such a way that she/he could be extremely happy with her/his first discovery. But very soon we were both lost in the wilderness and we exchanged ideas. My point in telling that story is that I never asked the student to read five or six books and to come back six months later. I wanted the student to trust and develop his own strength. In France it was completely unusual to proceed that way. This way of supervising a thesis does not depend on your field of interest in mathematics. To faithfully answer your question, problems with simple formulation might be awfully difficult and require elaborated theories to be solved. An example is Fermat's last theorem and its solution by Andrew Wiles. It is also true that most of the problems in a newly born chapter of mathematics are quite natural and do not require sophisticated tools. That explains why Peter Gabriel advised me to switch to algebraic geometry "à la Grothendieck".

How important would you say that social interaction is to mathematicians ? (So that a good advice to young mathematicians would be to try an co-operate with as many as possible.) In many other fields co-operation with a host of other co-authors is the norm, and many people feel that if they are not part of a group, they are completely lost. In mathematics, most co-operations are made with just one co-author, we are normally quite suspicious of papers written by three or more people. Mathematics is also a lonely pursuit, because when it comes to think through a problem and understanding it you cannot continually interact with another individual. The idea of say being given a specific problem and to solve it in continuous tandem with another through conversation seems to me painfully suffocating. After all the process of mathematical thinking is very inchoate, and the need to actually try to formulate your vague process, seems to be almost as hard as solving the problem itself. But of course inputs from others are invaluable. In your own co-operations what are you looking for, what do you find most helpful?

I remember with nostalgia my first international conference in Oberwolfach. It happened in August 1965. I was 26 years old. The old castle still existed. My wife Anne was pregnant. We met some marvellous mathematicians who were going to play such an important role in my life. Let me single out Guido Weiss. He was there with his wife Barbara. Anne and Barbara used to enter the dining room five minutes before the dinner and to reorganize the seats in order that we would be seated together every day. The organizing committee wanted the people to be seated randomly in order that every participant would have a chance to meet all the other mathematicians. Anne and Barbara were cheating with this rule. Guido was enjoying a sabbatical at the University of Geneva where he had delivered a graduate course. He met Raphy Coifman there. Raphy was just finishing his Ph.D. and Guido convinced him to go with him to the States. Coifman did not participate to this Oberwolfach meeting. Coifman and I began our joint work nine years later. I have been working with Coifman for thirty five years since. We complement each other marvellously. Raphy is a visionary. Compared to him, I am a skeptical technician. Raphy is a dreamer while I often do the dirty work of trying to find a proof which is consistent with Raphy's vision.

I remember Carleson once discouraging a student from writing his thesis in co-operation with somebody else. In mathematical work, it is the sup-norm which is relevant not the integral-norm, he claimed. This might be true on the thesis level, but surely would it be true otherwise, few joint papers would be written. In your opinion and experience are joint papers more of the nature of two separate contributions joined (a division of labor) or a truly joint effort, in which in a long chain of arguments, each author provides every other one. (I once had that experience with a joint paper, that a problem was solved exactly that way, and that consequently had we been alone, we would only have climbed one step each on the ladder, getting nowhere.)

When I was preparing my Ph.D. at Strasbourg, I was given the following advice by the established professors: "Do not publish your first paper, you will regret it. Wait a few years more until it is perfect." At that time there were only a few mathematicians all over the world. Therefore everyone knew what the others were working on. It meant, following them, that the risk of being passed by another colleague was low. Communicating was achieved by writing letters to the colleagues you were knowing and respecting. Fair game was the rule. I was very impressed by this advice. Jean-Pierre Kahane told me another story: "Yves, publish your first paper, unless you want to spend the rest of your life improving it, which would be so stupid, since it is just above average." Both bits of advices were right. Indeed I published my first paper, and I am so ashamed of this poor paper now that I suppress it from my publication list. Kahane thought that getting a Ph.D. is the beginning of a story, not the end. A Ph.D. does not need to be a masterpiece, it is a kind of driving licence allowing you to make a splendid journey in the world of mathematics. I liked this idea.

Talking about bad papers, Antoni Zygmund once told me the following: "Yves, to judge someone you should integrate f^+". Here $f(x)$ is a real valued function and $f^+(x) = \sup(f(x), 0)$. Let me explain this sentence. Zygmund thought I was too critical of my colleagues and he was saying that, before judging a person, I should wipe out his negative aspects and only retain the best of him. What a splendid advice!

As I noted in other fields, such as big science, there are very big projects involving hundreds of people and large sums of money. (Funding agencies prefer to give out money in large chunks, this is understandable, because the more money involved, the decision process becomes less involved.) Should mathematics be more like that? A few visionary leaders identifying the problems to attack, and then a whole army of people doing the work. Thus instead of figuring out yourself what to do, the mathematicians are being told (by other mathematicians) what to do. This would make social interaction automatic, and many people might actually be relieved to no longer have the responsibility to be self-driven. (It reminds me a little about arranged marriages, more people than one would suspect, might actually like it, as it likewise removes the personal responsibility and the need to take initiatives.). In big science it is inevitable, making the life of a scientist rather different from that of a mathematician, who in many ways have more in common with people working in the humanities.

This is a remarkable question. Yes there are big programs in mathematics. Alberto Calderón raised a series of problems which reshaped harmonic analysis and PDEs. Solving these problems took about ten years and involved more than a hundred people. There was no formal funding devoted to Calderón's program but the NSF knew it was crucial and gave money generously. The Painlevé conjecture could not have been solved outside Calderón's program. The Langlands program is another example of a huge endeavour which is well accepted by funding agencies. Programs are more distinct than conjectures. A program is conveying a spirit. A conjecture without a program might remain an enigma. Algebraic geometry and number theory were reshaped by Alexander Grothendieck, Jean-

Pierre Serre and André Weil. Ramanujan's conjecture on the growth of the coefficients in the expansion of the τ-function would not have been solved by Pierre Deligne without this reshaping.

Finally one of the most impressive big program in mathematics has been the complete classification of finite simple groups. The boss was Daniel E. Gorenstein and, unlike what happened in the preceding programs, the boss told everyone what he had to do, what piece of work he should achieve. And it worked. Now the proof of the fact that the classification is complete is six thousands pages long and no one can say he has mastered this entire proof. his example challenges our ideal to base our knowledge on reading and understanding proofs. At a philosophical level, the solution of the classification problem has the status of a rumor.

Mathematical applications to physics are of course well-known. But physics has also applications to mathematics (just think of string theory, which has no physical applications whatsoever from what I have understood). There is a two-way street. Physics gives rise to very central mathematical problems, and a physicist's intuition could be very helpful in solving purely mathematical problems. There are also applications of mathematics to biology and some of the social sciences, especially economics. But here there seems to be no two-way street. Mathematics could certainly be helpful, but not the other way around. Economical and biological intuition seem not to give clues to the solution of mathematical problems. And if say a biological problem stimulates a purely mathematical problem, its solution seems not very relevant to biology, unlike the case of physics. To me many of the applications of mathematics to the real, messy world, seem rather messy and ad-hoc and deprived of the beauty and simplicity that characterizes the interaction with physics, Would you like to expound on this ?

I can answer this question since I worked ten years at Paris-Dauphine in a department where the interaction between mathematics and finance is highly developed. I can attest that some fascinating mathematical problems are coming from finance. Moreover finance provides a kind of intuition that simply does not exist in other scientific fields. This was stressed by Ivar Ekeland. Roland Glowinski was invited to a prospective meeting on the future of non-linear PDEs. People were surprised to discover that three quarters of the relevant problems originated from finance.

Quasicrystals

I would like to end this interview with quasicrystals. The jury of the Gauss prize liked the absence of clear frontiers between pure and applied mathematics in my research. An illustration is my discovery of some geometrical configurations of points in $\mathbb{R}^n$ which anticipated quasicrystals. Quasicrystals were later found as specific organizations of atoms in certain alloys in chemistry. I will comment now on these findings.

After completing my Ph.D. I became fascinated by number theory and by the work achieved by Tirukkannapuram Vijayaraghavan. T. Vijayaraghavan (1902-1955) was an Indian mathematician from the Madras region. He worked with G. H. Hardy (a famous

English mathematician) on what is now called Pisot-Vijayaraghavan numbers. A Pisot-Vijayaraghavan number $\theta \in \mathbb{R}$ is an algebraic integer of degree n all of whose conjugates $\theta_2, \ldots, \theta_n$ satisfy $|\theta_j| < 1$. We obviously except $\theta_1 = \theta$ which is larger than 1 (without losing generality we can assume $\theta > 0$). A Salem number is defined the same way with $|\theta_j| < 1$ being replaced by $|\theta_j| \leq 1$. T. Vijayaraghavan did this work when he went to Oxford in mid-1920s. Vijayaraghavan was a fellow of the Indian Academy of Sciences, elected in the year 1934. Charles Pisot (1910-1984) was a French mathematician whom I knew and admired.

In 1969 I designed some new configurations of points in $\mathbb{R}^n$ which are nowadays named "Meyer sets" or almost lattices. An almost lattice $\Lambda \subset \mathbb{R}^n$ is defined by the three following conditions

(a) There exists a positive number r such that every ball with radius r whatever be its center contains at most one point in Λ.

(b) There exists a positive number $R > r$ such that every ball with radius R whatever be its center contains at least one point in Λ.

(c) There exists a finite set F such that $\Lambda - \Lambda \subset \Lambda + F$.

Here $\Lambda - \Lambda$ is the set consisting of all differences $\lambda - \lambda'$, $\lambda, \lambda' \in \Lambda$. If $F = \{0\}$ then Λ is a lattice. For many problems in harmonic analysis ordinary lattices can be replaced by almost lattices. The relation between almost lattices and Pisot-Vijayaraghavan numbers is quite exciting. If Λ is an ordinary lattice and if $\theta \in \mathbb{R}$, then the dilated lattice $\theta\Lambda$ is contained in Λ if and only if θ is an ordinary integer. For almost lattices we have something similar: if Λ is an almost lattice and if $\theta\Lambda$ is contained in Λ, then θ shall be a Pisot-Vijayaraghavan number or a Salem number. Conversely if θ is a Pisot-Vijayaraghavan number or a Salem number, there exists an almost lattice Λ such that $\theta\Lambda \subset \Lambda$. These almost lattices have been rediscovered independently by Roger Penrose (Penrose pavings, 1976) and next by D. Shechtman, I. Blech, D. Gratias and J.W. Cahn in chemistry (1984). The positions of atoms in certain alloys exactly obey the mathematical rules I discovered fifteen years before Shechtman's work. This striking example proves that mathematicians can be prophetic. It suffices to type "quasicrystal" or "Meyer sets" in Google to admire some of these beautiful configurations. The story does not end there. Indeed it seems that quasicrystals have been used as decorative patterns in medieval Islamic art. Peter Lu of Harvard University made this outstanding discovery while he was visiting a madrassa located at Boukhara, Ouzbekistan. More details and amazing pictures can be found if you type "Quasicrystals and Islamic art" in Google. I love this story where number theory, pavings, chemistry and Islam are reconciled. It is like returning to Tunis at the end of my journey.

Comment on the interview with Yves Meyer

by David Ruelle (Rutgers, USA & Bures-sur-Yvette, France)

Dear Yves Meyer,

I was very pleased to read the text of your EMS interview as published in the IAMP News Bulletin of July 2011, especially what you say about the French Grandes Ecoles. I am, as you are, in favor of the elitist system of the Ecole Normale and the Ecole Polytechnique, but against the lifelong privileges given to their students. The latter means indeed that there is lifelong discrimination against those who did not graduate from one of the Ecoles. Amusingly (or sadly) many left-wing French intellectuals, who are ready to fight every sort of discrimination, make an exception for that one.

When I came to France in 1964 (after working in Belgium, Switzerland, and the US), the IHÉS was not yet called IHÉS, and it was outside of the French system. Discussions about people to invite were based on scientific quality at the international level; only later would one hear things like: "He is one of the best mathematicians from the Ecole Normale in the last ten years". After my arrival at the IHÉS, my new colleague Louis Michel took time to tell me who in the French Mathematics and Physics community was from the Ecole Normale, who was from the Ecole Polytechnique (Michel became furious when I got it wrong). I was neither Normale nor Polytechnique and, after a while, I came to realize that this was important for many French people I interacted with (there were some visible exceptions like René Thom, who couldn't care less, and also, curiously, Jacques-Louis Lions). Later, in the Physics Section of the Académie des Sciences, I was surrounded by two blocs: Saclay-Polytechnique headed by Anatole Abragam, Normale-Ulm headed by Jean Brossel. At one point, Jacques Friedel made a list of "grand prix" awarded in Physics, showing a fair equilibrium between the two blocks. But, as Friedel pointed out, very little was left to people outside the blocks...

Discrimination against various human groups is widespread, and often has tragic consequences. The discrimination in France against those who are not from the Grandes Ecoles may be seen as a rather mild affair, and one is tempted to simply shrug it off. One shouldn't. I think that Alexander Grothendieck's tragic story is strongly related to the fact that he was not from the Ecole Normale. If he had been, the IHÉS Director Léon Motchane would have been much more prudent in dealing with him, and he might not have left. Or after he left the IHÉS, probably more effort would have been made by the community to find a suitable position for him.

Thank you again then for publishing your interview. This gave me a chance to repeat things that are fairly well known, but deserve to be discussed rather than just being silently accepted.

David Ruelle — Bures, July 26 2011.

Selected advances in Quantum Information Theory

by Nilanjana Datta (Cambridge, UK)

Nilanjana Datta is an affiliated lecturer of the Statistical Laboratory in the Centre for Mathematical Sciences of the University of Cambridge, and a Fellow of Pembroke College. She completed her PhD in mathematical physics in ETH Zurich in 1996. She has been working on quantum information theory since 2001 and has written papers on various topics in this field. These include quantum channels and the additivity problem, quantum data compression, perfect transfer of quantum states and entanglement theory. In the last few years her research has focussed on developing one-shot quantum information theory, which concerns the analysis of information processing tasks involving finite, correlated resources.

The last three years have witnessed various significant advances in quantum information theory. In this article we briefly review the salient features of three of them. These are, namely, a counterexample to the additivity conjecture, superactivation of the quantum capacity of a channel, and one-shot quantum information theory. The first two pertain to information-transmitting properties of quantum channels, whereas the third applies to a plethora of information-processing tasks, over and above information transmission.

The biggest hurdle in the path of information transmission is the presence of noise in communications channels, which can distort messages sent through them, and necessitates the use of error-correcting codes. There is, however, a fundamental limit on the rate at which information can be transmitted reliably through a channel. The maximum rate is called the capacity of the channel and is evaluated in the limit of asymptotically many, independent uses of the channel.

For the case of a classical channel, this was derived by Claude Shannon in his seminal paper of 1948 [1], which heralded the birth of the field of classical information theory. His Noisy Channel Coding theorem gives an explicit expression for the capacity of a discrete memoryless channel $\mathcal{N}$. For such a channel there is no correlation in the noise acting on successive inputs, and the channel can be completely described by its conditional probabilities $p_{Y|X}(y|x)$ of producing output y given input x, with X and Y denoting discrete random variables characterizing the inputs and outputs of the channel. Shannon proved that the capacity $C(\mathcal{N})$ of such a channel is given by a unique formula

$$C(\mathcal{N}) = \max_{\{p_X(x)\}} I(X:Y), \tag{1}$$

where $I(X:Y)$ denotes the mutual information of the random variables X and Y, and the maximisation is over all possible input probability distributions $\{p_X(x)\}$.

In contrast to a classical channel, a quantum channel has many different capacities. These depend on various factors, for example, on the type of information (classical or quantum) which is being transmitted, the nature of the input states (entangled or not), and whether any auxiliary resource is available to assist the transmission. Auxiliary resources like prior shared entanglement between the sender and the receiver can enhance the capacities of a quantum channel. This is in contrast to the case of a classical channel where auxiliary resources, such as shared randomness between the sender and the receiver, fail to enhance the capacity.

Let us briefly recall some basic facts about quantum channels. For simplicity of exposition, we refer to the sender as Alice and the receiver as Bob. A quantum channel $\mathcal{N}$ is mathematically given by a linear, completely positive trace-preserving (CPTP) map which maps states (i.e., density matrices) ρ of the input quantum system A to states of the output system B. More generally, $\mathcal{N} \equiv \mathcal{N}^{A \to B} : \mathcal{B}(\mathcal{H}_A) \to \mathcal{B}(\mathcal{H}_B)$, where $\mathcal{H}_A$ ($\mathcal{H}_B$) denote the Hilbert spaces associated with the system A (B), and, in this article, are considered to be finite-dimensional. By Stinespring's dilation theorem, any such quantum channel can be seen as an isometry followed by a partial trace, i.e., there is an auxiliary system E, usually referred to as the *environment*, and an isometry $U_{\mathcal{N}} : \mathcal{H}_A \to \mathcal{H}_B \otimes \mathcal{H}_E$, such that $\mathcal{N}(\rho) = \mathrm{Tr}_E\, U_{\mathcal{N}} \rho U_{\mathcal{N}}^\dagger$. This in turn induces the complementary channel $\mathcal{N}_c \equiv \mathcal{N}_c^{A \to E} : \mathcal{B}(\mathcal{H}_A) \to \mathcal{B}(\mathcal{H}_E)$ from the system A to the environment E, given by $\mathcal{N}_c(\rho) = \mathrm{Tr}_B\, U_{\mathcal{N}} \rho U_{\mathcal{N}}^\dagger$. Physically, the complementary channel captures the environment's view of the channel. A quantum channel is said to be anti-degradable if there exists a CPTP map $\mathcal{E} : \mathcal{B}(\mathcal{H}_E) \to \mathcal{B}(\mathcal{H}_B)$ so that the composition of the maps $\mathcal{N}_c$ and $\mathcal{E}$ satisfies the identity $\mathcal{N} = \mathcal{E} \circ \mathcal{N}_c$. So an eavesdropper (Eve), who has access to the environment of the channel, can simulate the channel from A to B by applying the map $\mathcal{E}$. An anti-degradable channel has zero quantum capacity since otherwise it would violate the so-called *no-cloning theorem* which forbids the creation of identical copies of an arbitrary unknown quantum system. This can be seen as follows. Suppose there is an encoding and decoding scheme for Alice to communicate quantum information reliably at a non-zero rate over such a channel. Then by acting on the output that she receives by the CPTP map $\mathcal{D} \circ \mathcal{E}$, where $\mathcal{D}$ is the decoding map that Bob uses, Eve could obtain the quantum information sent by Alice. However, the ability for both Bob and Eve to obtain Alice's information violates the no-cloning theorem. Hence the quantum capacity of an anti-degradable channel must be zero. In contrast, a quantum channel is said to be degradable if there exists a CPTP map $\mathcal{E}' : \mathcal{B}(\mathcal{H}_B) \to \mathcal{B}(\mathcal{H}_E)$ so that $\mathcal{N}_c = \mathcal{E}' \circ \mathcal{N}$. So in this case Bob can simulate the complementary channel from A to E by applying the map $\mathcal{E}'$.

Determination of the different capacities of a quantum channel have only been partially resolved, in the sense that the expressions obtained for most of them thus far are asymptotic in nature and hence cannot be used to determine the capacities of a given channel in any effective way. If entanglement between multiple inputs to the channel is not allowed, then the capacity of a channel for transmitting quantum information is given by an entropic quantity $\chi^*(\mathcal{N})$ called its Holevo capacity [2]. The general classical capacity of a quantum channel, in the absence of auxiliary resources but possibly using entangled inputs, is given by the regularized expression

$$C(\mathcal{N}) = \lim_{n \to \infty} \frac{1}{n} \chi^*(\mathcal{N}^{\otimes n}) \tag{2}$$

i.e., the limit for large n of the capacity when we permit the input to be entangled over blocks of n independent channel uses denoted by $\mathcal{N}^{\otimes n}$. Similarly the capacity $Q(\mathcal{N})$ of the channel for transmitting quantum information (in the absence of auxiliary resources) is also known [3] to be given by a regularized expression:

$$Q(\mathcal{N}) = \lim_{n\to\infty} \frac{1}{n} I_c(\mathcal{N}^{\otimes n}), \tag{3}$$

where for any quantum channel $\widetilde{\mathcal{N}}$, $I_c(\widetilde{\mathcal{N}})$ is an entropic quantity referred to as its coherent information.

Another important capacity of a quantum channel is its private capacity $P(\mathcal{N})$, which is the maximum rate at which it can be used to send classical information which is secure against an eavesdropper, Eve, who has access to the environment of the channel. The private classical capacity $P(\mathcal{N})$ of a quantum channel is also given by the regularization of an entropic quantity, $P^{(1)}(\mathcal{N})$. Unfortunately, these intractable, regularized expressions are in general useless for computing the actual capacities of a channel. As regards quantum capacity, an exception to this is provided by degradable channels, for which the coherent information is additive and so the quantum capacity reduces to a single-letter formula. Other than the Holevo capacity, there are only a few other capacities which have a single-letter (and hence not-regularized) expression for any arbitrary quantum channel. The most important of these is the entanglement-assisted classical capacity [4], which is the maximum rate of reliable classical communication when Alice and Bob are allowed to make use of entangled states which they initially share.

An important property of the capacity of a classical channel is its additivity on the set of channels. Given two classical channels $\mathcal{N}_1$ and $\mathcal{N}_2$, the capacity of the product channel $\mathcal{N}_1 \otimes \mathcal{N}_2$ satisfies $C(\mathcal{N}_1 \otimes \mathcal{N}_2) = C(\mathcal{N}_1) + C(\mathcal{N}_2)$. In fact, many important questions in information theory can be reduced to the purely mathematical question of additivity of certain entropic functions on the set of channels. In particular, the regularized expressions for the classical, quantum and private capacities of a quantum channel $\mathcal{N}$ would reduce to tractable single-letter expressions if its Holevo capacity, coherent information and $P^{(1)}(\mathcal{N})$ were respectively additive. However, it has been proved that the coherent information and $P^{(1)}(\mathcal{N})$ are not necessarily additive for all channels. It was conjectured that the Holevo capacity of a quantum channel is indeed additive, i.e., for any two quantum channels $\mathcal{N}_1$ and $\mathcal{N}_2$,

$$\chi^*(\mathcal{N}_1 \otimes \mathcal{N}_2) = \chi^*(\mathcal{N}_1) + \chi^*(\mathcal{N}_2).$$

This conjecture is directly related to the important question: *Can entanglement between input states boost classical communication through a quantum channel?* The answer to this question is "no" if the Holevo capaciy of the channel is additive, since in this case $C(\mathcal{N}) = \chi^*(\mathcal{N})$, i.e., the general classical capacity reduces to the classical capacity evaluated under the restriction of unentangled input states. The additivity conjecture had been proved for several channels (see e.g. [5] and references therein), however proving that it is true for *all* quantum channels had remained an important open problem for more than a decade. Shor [6] provided useful insight into the problem by proving that the additivity conjecture for the Holevo capacity was equivalent to additivity-type conjectures for three other quantities arising in quantum information theory, in the sense that if any one of these conjectures is always true then so are the others. One of these conjectures concerns the additivity of the minimum output entropy (MOE) of a quantum channel, which is defined as

$$H_{\min}(\mathcal{N}) = \min_{\rho} H(\mathcal{N}(\rho)),$$

where for any state σ, $H(\sigma) := -\operatorname{Tr}(\sigma \log \sigma)$ is the von Neumann entropy. The additivity conjecture for the MOE is that for any pair of quantum channels $\mathcal{N}_1$, $\mathcal{N}_2$, the minimum entropy of the product channel $\mathcal{N}_1 \otimes \mathcal{N}_2$ satisfies

$$H_{\min}(\mathcal{N}_1 \otimes \mathcal{N}_2) = H_{\min}(\mathcal{N}_1) + H_{\min}(\mathcal{N}_2). \tag{4}$$

Note that we always have $\leq$ in (4), by considering the product state $\rho_1 \otimes \rho_2$, where ρ_1, ρ_2 are the minimizers for $\mathcal{N}_1$ and $\mathcal{N}_2$ respectively. The conjecture amounts to the claim that we cannot get a smaller MOE by entangling the inputs to $\mathcal{N}_1 \otimes \mathcal{N}_2$.

These longstanding additivity conjectures were finally resolved in 2008 by Hastings [7], who built on prior work by Hayden and Winter [8]. He proved the existence of a pair of channels for which the above conjecture is false. By Shor's equivalence, this in turn implied that all the additivity conjectures (including that for the Holevo capacity) are false. Hence, we can conclude that there exist quantum channels for which using entangled input states can indeed enhance the classical capacity.

The product channel considered by Hastings has the form $\mathcal{N} \otimes \overline{\mathcal{N}}$, where $\mathcal{N}$ is a special channel called a *random unitary channel*, and $\overline{\mathcal{N}}$ is its complex conjugate. This means that there are positive numbers $\nu_1, \nu_2, \ldots, \nu_d$, with $\sum_i \nu_i = 1$, and unitary $n \times n$ matrices $U_1, U_2, \ldots, U_d$, chosen at random from the Haar measure, such that for any input state ρ,

$$\mathcal{N}(\rho) = \sum_i \nu_i U_i \rho U_i^\dagger \quad ; \quad \overline{\mathcal{N}}(\rho) = \sum_i \nu_i \overline{U}_i \rho \overline{U}_i^\dagger,$$

The probabilities ν_i are chosen randomly and depend on the integers n and d, where n is the dimension of the input space of the channel and d is the dimension of the environment. Hastings' main result is that for n and d large enough, there are random unitary channels for which $H_{\min}(\mathcal{N} \otimes \overline{\mathcal{N}}) < H_{\min}(\mathcal{N}) + H_{\min}(\overline{\mathcal{N}})$, thus disproving (4).

A key ingredient of the proof is the relative sizes of dimensions, namely $n >> d >> 1$. The details of Hastings' original argument were elucidated in detail by Fukuda et al [9] who also derived explicit lower bounds to the input, output and environment dimensions. A simplified proof of Hastings' result was given by Brandao and Horodecki [10] in the framework of concentration of measure. They also proved non-additivity for the overwhelming majority of channels consisting of a Haar random isometry followed by partial trace over the environment, for an environment dimension much bigger than the output dimension, thus extending the class of channels for which additivity can be shown to be violated. Remarkably, in 2010 Aubrun et al. [11] proved that Hastings' counterexample can be readily deduced from a version of *Dvoretzky's theorem*, which is a fundamental result of Asymptotic Geometric Analysis – a field of mathematics concerning the behaviour of geometric parameters associated with norms in $\mathbb{R}^n$ (or equivalently, with convex bodies) when n becomes large. However, the violation to additivity in Hasting's example is numerically small, and the question of how strong a violation of additivity is possible, is the subject of much research.

2008 was also the year which saw the discovery of a startling phenonmenon in quantum information theory, again related to the question of additivity of capacities. Smith and Yard [12] proved that there are pairs of quantum channels each having zero quantum capacity but which have a non-zero quantum capacity when used together. Hence, even though each channel in such a pair is by itself useless for sending quantum information, they can be used together to send quantum information reliably. This phenomenon was termed “superactivation”, since the two channels somehow “activate” each other's hidden ability to transmit quantum information. Superactivation is a purely quantum phenomenon because classically if two channels have zero capacity, the capacity of the joint channel must also be zero. This follows directly from the additivity of the capacity of a classical channel, which in turn ensures that the capacity of a classical channel is an intrinsic measure of its information-transmitting properties. In the quantum case, in contrast, the possibility of superactivation implies that the quantum capacity of a channel is strongly non-additive, and does not adequately characterize its ability to transmit quantum information, since the usefulness of a channel depends on what other channels are also available. A particular consequence of this phenomenon is that the set of quantum channels with zero quantum capacity is not convex.

Superactivation of quantum capacity is currently the subject of much research and is still not completely understood. However, it seems to be related to the existence of channels, called “private Horodecki channels” which have zero quantum capacity but positive private capacity. The key ingredient of Smith and Yard's proof of superactivation was a novel relationship between two different capacities of a quantum channel $\mathcal{N}$, namely, its private capacity $P(\mathcal{N})$ and its assisted capacity $Q_A(\mathcal{N})$. The latter denotes the quantum capacity of the product channel $\mathcal{N} \otimes \mathcal{A}$, where $\mathcal{A}$ is a *symmetric channel*. Such a channel maps symmetrically between the output and the environment, i.e., for any input state ρ, the joint state $\sigma_{BE} := U_{\mathcal{A}} \rho U_{\mathcal{A}}^\dagger$ of the output and the environment after the action of the channel $\mathcal{A}$, is invariant under the interchange of B and E. A symmetric side channel is anti-degradable and hence has zero quantum capacity. Smith and Yard proved that

$$Q_{\mathcal{A}}(\mathcal{N}) \geq \frac{1}{2} P(\mathcal{N}).$$

This in turn implies that any private Horodecki channel, $\mathcal{N}_H$, has a positive assisted capacity, and hence the two zero-quantum-capacity channels $\mathcal{N}_H$ and $\mathcal{A}$ exhibit superactivation:

$$Q_A(\mathcal{N}_H) = Q(\mathcal{N}_H \otimes \mathcal{A}) > 0.$$

The particular symmetric side channel that Smith and Yard considered was a 50% *erasure channel*, which faithfully transmits the input state half of the time and outputs an erasure flag in the rest of the cases.

Recently, Brandao, Oppenheim and Strelchuk [13] proved that superactivation even occurs for pairs of channels $(\mathcal{N}_H, \mathcal{N})$, where $\mathcal{N}$ is anti-degradable but not necessarily symmetric. Specifically, they proved the occurrence of superactivation for two different choices of $\mathcal{N}$: (i) an erasure channel which outputs an erasure flag with probability $p \in [1/2, 1)$, and faithfully transmits the input state otherwise; (ii) a depolarizing channel which completely randomizes the input state with probability $p \in [0, 1/2]$, and faithfully transmits the input state otherwise. It is known that the output of any arbitrary quantum channel can be mapped to that of a depolarizing channel by an operation known as "twirling". The latter consists of Alice applying some randomly chosen unitary on the input state before sending it through the channel and informing Bob as to which unitary operator U she used, with Bob subsequently acting on the output state of the channel by the inverse operator $U^\dagger$. This special feature of the depolarizing channel and the fact that it can be used for superactivation, suggests that superactivation is a rather generic effect. Recently, superactivation has also been proved for other capacities of a quantum channel (see e.g.[14] and references therein), namely its zero-error classical and quantum capacities, which are, respectively, the classical and quantum capacities evaluated under the requirement that the probability of error is strictly zero, and not just vanishes asymptotically.

All the capacities mentioned above, were originally evaluated in the limit of asymptotically many uses of the channel, under the assumption that the noise acting on successive inputs to the channel is uncorrelated, i.e., under the assumption that the channel is *memoryless*. In fact, optimal rates of most information-processing tasks (or protocols), including transmission and compression of information, and manipulation of entanglement, were originally evaluated in the so-called *asymptotic, memoryless scenario*. This is the scenario in which one assumes that there is no correlation in successive uses of resources (e.g. information sources, channels and entanglement resources) employed in the protocols, and one requires the protocols to be achieved perfectly in the limit of asymptotically many uses of the resources. These asymptotic rates, e.g., the various capacities discussed above, are seen to be given in terms of entropic functions which can all be derived from a single parent quantity, namely, the quantum relative entropy.

In reality, however, this assumption of resources being uncorrelated, and the consideration of an asymptotic scenario, is not necessarily justified. This is particularly problematic in cryptography, where one of the main challenges is dealing with an adversary who might pursue an *arbitrary* (and unknown) strategy. In particular, the adversary might manipulate resources (e.g. a communications channel) and introduce undesired correlations. A more general theory of quantum information-processing protocols is instead obtained in the so-called *one-shot scenario* in which resources are considered to be finite and possibly correlated. Moreover, the information-processing tasks are required to be achieved only up to a finite accuracy, i.e., one allows for a non-zero but small error tolerance. This also corresponds to the scenario in which experiments are performed since channels, sources and entanglement resources available for practical uses are typically finite and correlated, and transformations can only be achieved approximately.

The last few years have witnessed a surge of research leading to the development of *one-shot quantum information theory*. The birth of this field can be attributed to Renner et al. (see [15] and references therein) who introduced a mathematical framework, called the *smooth entropy framework,* which facilitated the analysis of information-processing tasks in the one-shot scenario. They introduced new entropy measures of states, called *smooth min- and max- entropies*, which depend on a parameter (say, ε), called the smoothing parameter. The smooth entropies $H^{\varepsilon}_{\min}(\rho)$ and $H^{\varepsilon}_{\max}(\rho)$ of a state ρ can be defined as optimizations of the relevant non-smooth quantities, the (non-smooth) min- and max-entropies, over a set $B^{\varepsilon}(\rho)$ of neighbouring states, which are at a distance of at most ε from ρ, measured in an appropriate metric. For a bipartite state ρ_{AB}, they also defined conditional min- and max- entropies.

In the last three years, it has been proved (see e.g. [16]) that these conditional and unconditional smooth min- and max- entropies characterize the optimal rates of various information-processing tasks in the one-shot scenario, with the smoothing parameter corresponding to the allowed error tolerance. For example, the one-shot ε-error quantum capacity of a channel, which is the maximum amount of quantum information which can be transmitted over a single use of a quantum channel with an error tolerance of ε, has been proved to be given in terms of a smooth conditional max-entropy [21, 17]. Note that a single use of a channel can itself correspond to a finite number of uses of a channel with arbitrarily correlated noise. Hence the one-shot analysis indeed includes the consideration of finite, correlated resources. Further, one-shot rates of all the different information-processing tasks studied thus far, readily yield the corresponding known rates in the asymptotic limit, in the case of uncorrelated resources. Moreover, they also yield asymptotic rates of protocols involving correlated resources via the so-called Quantum Information Spectrum method (see e.g. [18] and references therein). Hence, one-shot quantum information theory can be viewed as the fundamental building block of quantum information theory, and its development has opened up various new avenues of research.

In [20] we defined a generalized relative entropy called the *max-relative entropy*, from which the min- and max- entropies can be readily obtained, just as the ordinary quantum (i.e., von Neumann) entropies are obtained from the quantum relative entropy. Hence the max-relative entropy plays the role of a parent quantity for optimal rates of various information-processing tasks in the one-shot scenario, analogous to that of the quantum relative entropy in the asymptotic, memoryless scenario. Moreover, it has an interesting operational interpretation, being related to the optimal Bayesian error probability, in determining which one, of a finite number of known states, a given quantum system is prepared in. The max-relative entropy also leads naturally to the definition of an entanglement monotone which is seen to have an interesting operational interpretation in the context of entanglement manipulation [19]. The different information-processing tasks in the one-shot scenario were initially studied separately. However, recently, we proved [21] that a host of these tasks can be related to each other and conveniently arranged in a family tree, thus yielding a unifying mathematical framework for analysing them.

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Report on the ESF PESC Strategic Workshop on "Signatures of Quantumness in Complex Systems"

- Nottingham (United Kingdom), 29 June – 3 July 2011
- Convened by Gerardo Adesso, School of Mathematical Sciences, University of Nottingham, U.K.
- Sponsored by ESF, IAMP, AQUTE, Q-ESSENCE, IOP, Stone, The University of Nottingham
- http://www.maths.nottingham.ac.uk/personal/ga/SQCS/

The workshop, mainly funded under the European Science Foundation Exploratory Workshop framework, aimed at establishing an all-European platform to explore the interplay between quantumness and complexity, and more broadly the border between the classical and the quantum world. By complex systems we classified biological, many-body, hybrid, disordered, and meso/macroscopic systems. By manifestations of quantumness we instead identified coherence, entanglement, non-classical correlations (quantum discord), non-locality, and information processing applications. The selection of speakers was made in such a way that each participant's recent research was identifiable with one or more crossroads among the above topics. A number of fundamental questions with technological impact were raised: What is the most essential signature of quantumness? Is quantum theory the only one able to explain experiments at the microscopic scale? To what extent quantum effects are manifest and play central roles in complex, many-body and macroscopic systems? What still unexplored possibilities are allowed by quantum laws and general types of quantum correlations for empowered quantum information processing and communication? The goal of the workshop was to gather a top-notch community of theorists and experimentalists to define the state of the art and future directions in this emerging area.

A group picture from the workshop, taken at Newstead Abbey

The meeting took place at the *Westminster Hotel* (*Best Western*) in Nottingham from Wednesday June 29, 2011 (arrival & registration day) to Sunday July 03, 2011 (departure day). It featured 30 participants (29 invited speakers + 1 convener) from 11 countries, plus the ESF rapporteur and a local organising team of 4 postgraduate staff members of the University of Nottingham. All the participants were lodged in the same hotel, where we had our meals and coffee breaks as well. The meeting room (Cromwell meeting room) was equipped with a projector and several round tables arranged "cabaret-style". Each speaker was allowed 30+5 minutes for the talk+questions. The convener gave a short introductive presentation at the beginning of the workshop, and the PESC rapporteur Prof. Aizenman introduced ESF and its activities later on the same day. The interaction among the participants was fervent, and many lively discussions arose during the talks and continued over coffee breaks and post-dinner discussions in the hotel or at a nearby pub, as well as during the social activities (trip to Newstead Abbey and conference dinner). The workshop was concluded by an interactive strategic session where summary and follow-ups were discussed. Most of the participants found the idea of an exploratory workshop, and this one in particular, a more valid experience compared to some conventional large-scale conferences where the interaction is somehow limited. We then agreed that a community has been created at the workshop, and we plan to sustain it with further activities. Some of the main scientific conclusions emerging from the workshop are summarised as follows.

- Fundamental quantum effects are now detectable and testable in macroscopic setups that involve up to living complex systems as detectors and processors. On the other hand, unambiguous mathematical criteria for the verification and quantification of general quantum correlations are still to be developed.

- The applications of such general correlations for quantum computation and quantum information processing are being explored beyond the first generation of protocols. There is a clear potential for novel primitives to be developed that could trigger a second quantum revolution, as general quantum correlations beyond entanglement are more robust to mixedness and may survive in realistically noisy environments.

- On parallel grounds, the role of quantum coherence in presence of noise has proven crucial for the efficiency and the survival of light-harvesting bacteria, and for more complex biological mechanisms such as the avian compass. The study of these systems is sparking progress in the description of open quantum systems, and new insight on locating the boundary between the classical and the quantum world.

Combining the above observations, we reached the conclusion that natural systems and tailored hybrid devices might be a source of robust quantum correlations apt to enable disruptive quantum information processing machines. Exploring the role of noise and developing new quantum theory tools for quantum technology, applicable beyond the traditional microscopic world, is the new frontier for the merged areas of quantum information and complexity. The workshop allowed us to form a core coordinated European team to pioneer these developments in the near future.

Beyond the scientific value, one main aim of the workshop was that of establishing strategic connections at the European level. The venue was particularly suitable for this purpose and the first steps in such a direction have been already undertaken. Having gathered experts in otherwise distant areas, the workshop inspired a strong interest, if not an actual necessity, to reach beyond the traditional discipline boundaries and develop a common vocabulary to understand the subject from a broader perspective. This triggered new collaborations formed during the workshop, and a reinforcement of existing ones. Specifically, we have already applied to organise an ESF Research Conference on Quantumness and Complexity in 2013, featuring tutorials, invited talks and contributed presentations, as well as extende discussion time. At a later stage, after the ESF restructuring is complete and calls are available again, we plan to apply for a dedicated Research Networking Programme.

Gerardo Adesso

Call for prize nominations for prizes to be awarded at ICMP 2012

The Henri Poincaré Prize

The Henri Poincaré Prize, sponsored by the Daniel Iagolnitzer Foundation, was created in 1997 to recognize outstanding contributions in mathematical physics, and contributions which lay the groundwork for novel developments in this broad field. The Prize is also created to recognize and support young people of exceptional promise who have already made outstanding contributions to the field of mathematical physics. The prize is awarded every three years at the International Congress of Mathematical Physics (ICMP), and in each case is awarded usually to three individuals. A list of previous winners can be found here: http://www.iamp.org/page.php?page=page_prize_poincare. Nominations for the 2012 Prize, which is to be awarded at the ICMP in Aalborg, can be made to the President (president@iamp.org) or Secretary (secretary@iamp.org). There is no prescribed format for a nomination but the following information will help the Prize Committee with its task:

- listing or description of the scientific work in support of the nomination
- a recent c.v. of the nominee
- a proposed citation should the nominee be selected for an award

Please use email for your nomination; you will receive an acknowledgment that your nomination has been received.

To ensure full consideration please submit your nominations by November 30, 2011.

The IAMP early career award

The IAMP Early Career Award was instituted in 2008 and was awarded for the first time at the ICMP in Prague in 2009. It will next be awarded to at the ICMP in Aalborg in August 2012. The prize rules:

- The prize will be awarded in recognition of a single achievement in mathematical physics. The total prize value amounts to 3000 Euro and is reserved for scientists whose age in years since birth on July 31 of the year of the Congress is less than 35
- A nomination for the prize should include the name of the candidate accompanied by a brief characterization of the work singled out for the prize. Members of the IAMP should send their nomination or nominations to the President (president@iamp.org) and to the Secretary (secretary@iamp.org) by e-mail, and a fax or a hard copy should follow the e-mail

- The recipient of the prize will be selected by a committee of 5 members determined by votes within the Executive Committee of the IAMP, which shall decide the membership of the committee within one month of the expiration of the deadline for nominations

- The Committee may consider candidates that have not been nominated by the members. The names of committee members will remain confidential within the EC until the prize ceremony. The IAMP will make every reasonable effort to make attendance at the ceremony possible for the selected candidate

Nominations should be made not later than on February 6, 2012.

Pavel Exner
Jan Philip Solovej

News from the IAMP Executive Committee

New individual members

IAMP welcomes the following new members

1. Pierluigi Falco, Department of Mathematics, California State University, Northridge, CA. USA
2. Pedram Hekmati, School of Mathematical Sciences, The University of Adelaide, Adelaide, Australia
3. Manuel Larenas, Department of Mathematics Rutgers University, Piscataway, NJ, USA
4. Jacob Schach Møller, Department of Mathematics, Aarhus University, Aarhus, Denmark
5. Miguel Arturo Ballesteros Montero, Institut für Analysis und Algebra, Technische Universität Braunschweig, Braunschweig, Germany
6. Chifu Ndikilar, Department of Physics, Gombe State University, Nigeria
7. Pradyumn Kumar Sahoo, Department of Mathematics, Birla Institute of Technology & Science, Hyderabad, India
8. Matěj Tušek, Facultad de Fisica, Pontificia Universidad Catolica de Chile, Santiago, Chile

Recent conference announcements

With support from IAMP:

- Jan 23–27, 2012, Classical and Quantum Integrable Systems, Bogoliubov Laboratory of Theoretical Physics, JINR, Dubna, Russia
  LINK: http://theor.jinr.ru/~cqis/2012/index.html

- April 10–13, 2012, Spectral days, University of Munich
  LINK: http://www.math.lmu.de/~specdays/

- Jul 30 – Aug 1, 2012, Mathematical Aspects of Quantum Field Theory and Quantum Statistical Mechanics, DESY, Hamburg, Germany

- Sep 2 – 9, 2012, Stochastic and Analytic Methods in Mathematical Physics, Yerevan, Armenia

Other conferences:

- Nov 7–10, 2011 International Workshop on Theoretical Aspects of the Discrete Time Quantum Walk, Instituto de Fisica Corpuscular, Valencia
  LINK: http://ific.uv.es/~perez/DTQW_Valencia.html

- Mar 12–16, 2012, The Arizona School of Analysis and Mathematical Physics, University of Arizona, Arizona, USA
  LINK: http://math.arizona.edu/~mathphys/school_2012

- Jul 12–14, 2012, The conference MATHEMATICS OF MANY-PARTICLE SYSTEMS will take place at TU Berlin in honor of Elliott Lieb on the occasion of his 80th birthday.

  To understand interacting particle systems has been, and still is, one of the great challenges of quantum and classical physics. Many developments in theoretical physics, mathematical physics, and mathematics originate from studying a specific many-particle system. During the past decades, Elliott Lieb obtained deep insights even into the most complicated such interacting systems. His work on stability of nonrelativistic and relativistic matter, on atoms and molecules in presence or absence of a magnetic field, on the (Fermi) Hubbard model and on Bose condensates has been ground-breaking for further studies or even open new scientific fields, his contributions to the understanding of models of statistical mechanics, like e.g. the ice model mark the beginning of one of the most important developments in physics and mathematics in the second half of the last century. The four volumes of his selecta are on every mathematical physicist's book shelf.

  These topics also define the focus of the conference herewith announced and are reflected in the speakers that have confirmed their participation:

  Rafael Benguria (UC Santiago de Chile),
  Eric Carlen (Rutgers U),
  Rupert Frank (Princeton U),
  Sabine Jansen (WIAS Berlin),
  Matthieu Lewin (U Cergy-Pontoise [Paris]),
  Robert Seiringer (McGill U Montreal),
  Heinz Siedentop (LMU Munich),
  Yakov Sinai (Princeton U),
  Jan Philip Solovej (Copenhagen U),
  Lawrence Thomas (U of Virginia),
  Kenji Yajima (Gakushuin U, Tokyo),
  Jakob Yngvason (U Vienna).

  These distiguished speakers, whose presentations will cover a broad spectrum of mathematics and physics, will hopefully attract many other participants to the conference with whom we then achieve the goal of the conference, namely, to enhance the interdisciplinary exchange of mathematics and physics.

Further information can be found at
`www3.math.tu-berlin.de/mathematics-of-many-particle-systems`
starting from 15-OCT-2011. Also, feel free to contact us by e-mail at v.bach@tu-bs.de.

Post-Doctoral position

Duration: 1 or 2 years, starting in Fall 2012

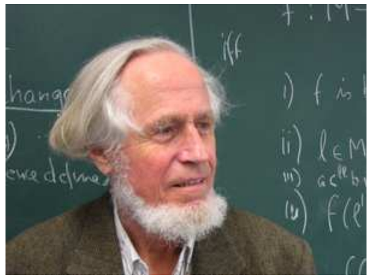
Location: Mathematics department of the University Paris-Dauphine, France

The candidate should have a strong background in the study of quantum systems, with tools from spectral theory, nonlinear analysis and PDEs, or numerical analysis. She/he is expected to work in the team of the ANR project NONAP (Nonlinear problems in Nuclear and Atomic Physics) and to interact with the other members of the project (both in University Paris-Dauphine and in University of Cergy-Pontoise).

To apply, please send a CV and a short description of your research to Maria J. Esteban: esteban@ceremade.dauphine.fr

Jan Philip Solovej (IAMP Secretary)

Hans-Jürgen Borchers



We have to convey the sad news that Hans-Jürgen Borchers died on September 10, 2011 at the age of 85.

Hans-Jürgen Borchers was a mathematical physicist, very well-known for his pioneering contributions to the axiomatic approach to quantum field theory.

He was born January 24, 1926 in Hamburg. Having obtained a first professional formation as a machine fitter and working as an engineer, after the end of the war he took evening lessons to catch up on the "abitur" (admission to the university). He then decided to study Physics in Hamburg. Borchers' mentor was Wilhelm Lenz, who also supervised his PhD thesis "Investigations on field equations of different spin and their energy eigenvalues in the Coulomb field" in 1956. He then became assistant to Harry Lehmann, working in the rapidly developing area of Quantum Field Theory. While the treatment of this theory was at the time dominated by pragmatic recipes, he quickly recognized the importance and the power of the general principles underlying this theory.

One of Borchers' first, and widely acknowledged achievements in these early years was the discovery that the very notion of "field" in quantum field theory has some arbitrariness: large equivalence classes of quantum fields (known as Borchers classes) can describe the same scattering processes. The notion of equivalence was linked to the principle of locality, or Einstein causality: Two fields are equivalent if they are relatively local (mutually commute with the other at spacelike distance), and relative locality is a transitive property. This insight put an end to the idea of a strict correspondence between particles and fields, and was another support for the Haag-Kastler reformulation of the quantum field theory, which is not based on individual fields but on the local algebras they generate.

Borchers was also interested in the status of the Hamiltonian, i.e., the energy operator as the generator of time evolution, in a quantum field theory. He could show on very general grounds that it can always be approximated by local field operators in a representation independent and gauge invariant way. In a first courageous attempt to understand the structure of superselection sectors, he emphasized the importance of the positivity of the Hamilton operator for the representation theory of quantum fields.

Borchers' proposal to reformulate the Gårding-Wightman axioms of quantum field theory as properties of a state on the associative tensor $*$-algebra of test functions (known as the Borchers-Uhlmann algebra; Uhlmann in Leipzig independently had the same idea around the same time) placed the problem of construction and analysis of quantum field theories into the context of noncommutative moment problems. He would later pursue this point of view together with his student and life-long collaborator Jakob Yngvason.

These early achievements earned him research positions in Princeton and in Paris, and finally in 1966 the prestigious professorship in Theoretical Physics in Göttingen as successor to Friedrich Hund. He was honoured by the membership of the Göttingen Academy of Sciences in 1970. Among his students, Jakob Yngvason and Erwin Brüning became prominent and internationally renowned scientists in Mathematical Physics.

Borchers continued his research on the interplay between locality and the spectrum of the Hamiltonian. He could show (with Detlev Buchholz) that Lorentz covariance is not required to conclude that the joint spectrum of the translations is always Lorentz invariant: this is due to locality alone. This is a very gratifying result in view of theories like Quantum Electrodynamics, where Lorentz invariance is spontaneously broken in charged sectors.

In recent years, his research became focussed on the relevance for Physics of the modular theory by Tomita and Takesaki. He contributed to this theory an important theorem that emphasizes the natural position of the theory in relativistic quantum field theory: the commutation relations between Lorentz boosts and translations naturally emerge in algebraic situations called "half-sided modular inclusions". Again, the positivity of the energy plays a crucial role. (A converse of the theorem by Wiesbrock and Araki-Zsido also allows one to derive the translations together with positivity of the spectrum from an inclusion of von Neumann algebras in a suitable relative modular position.) The idea of basing the entire construction of local quantum field theories on a small set of modular data (known as Borchers triples) relies on this theorem.

Borchers' review on the "revolutionary" influence of modular theory on quantum field theory became a standard reference in the field. Borchers wrote also two books: one on locality and spectrum, the other one (coauthored by Rathin Sen) on the geometric prerequisites of spacetime that would allow the emergence of their amazing interplay in Local Quantum Physics.

Mathematics was for Borchers the decisive instrument in the quest for understanding Quantum Physics. He had a mastership in combining (and advancing) different areas of Mathematics, e.g., operator algebras and complex calculus in several variables, in order to obtain new insights.

In 1994, Borchers was awarded the Max Planck medal, the highest honour of the German Physical Society (DPG), for his seminal contributions to Quantum Field Theory.

His fascination with Mathematical Physics never left him. He dedicated his life to research until his last days, and his research in turn gave him the strength to endure his last years, marked by ill health. The legacy of Hans-Jürgen Borchers' insights and achievements will continue to live in the scientific community.

Detlev Buchholz
Helmut Reeh
Karl-Henning Rehren

New Springer books

- Bryce DeWitts Lectures on Gravitation, edited by Steven M. Christensen, Lecture Notes in Physics 826
  http://www.springer.com/physics/theoretical%2C+mathematical+%26+computational+physics/book/978-3-540-36909-7
- The Pursuit of Quantum Gravity Memoirs of Bryce DeWitt from 1946 to 2004
  http://www.springer.com/physics/theoretical%2C+mathematical+%26+computational+physics/book/978-3-642-14269-7

Gabriele Hakuba (Springer Editorial Assistant, Physics)