The Poetry of Analysis

Conference in honour of
ANTONIO CÓRDOBA
on the occasion
of his 60th birthday

Madrid, June 22nd-26th, 2009

Index

Antonio's life

Antonio’s webpage:
http://www.uam.es/antonio.cordoba

Antonio Córdoba was born in Murcia, on January the 12th, 1949. In 1966, he moved to Madrid to study Mathematics at the Universidad Complutense. Later, in 1971, Antonio joined the Doctorate Program at the University of Chicago, where he obtained his PhD in 1974 under the supervision of Charles Fefferman with the thesis: “The Kakeya maximal function and the spherical summation multipliers”.

Antonio held academic positions as assistant professor at Princeton University (1974-79); senior researcher at the Consejo Superior de Investigaciones Científicas CSIC, Madrid (1977-79); visiting professor at the University of Chicago (1983-84); member of the Institute for Advanced Study, Princeton (1988-89); and visiting professor at Princeton University (1994-95). Since December 1979, Antonio has been full professor at the Universidad Autónoma de Madrid.

He was a long term visitor at the Mittag-Leffler Institute (spring of 1979, after an invitation from L. Carleson to participate in a workshop in Harmonic Analysis and Analytic Number Theory); at the Universitè de París, Orsay (winter of 1978, invited by Y. Meyer to take part in the Harmonic Analysis Seminar); at the ETH Zurich (semester in Fourier Analysis, organized by J. Garnett); at the Institute for Advanced Study (spring of 1992) and at the University of Texas, Austin (spring of 1996, to participate in workshops in nonlinear PDE’s, organized by L. Caffarelli).

He has been plenary speaker at many international meetings, among which we may mention: Williamstown Harmonic Analysis Conference (Williamstown, 1979); the conference in honor of Antoni Zygmund’s 80th birthday (Chicago, 1981); Harmonic Analysis Conference (Pisa, 1983); the conference to celebrate the 65th birthday of A. Calderón (Chicago, 1985); Oberwolfach meeting on Harmonic Analysis (1986); El Escorial Conference on Harmonic Analysis (1979, 1983 and 1996); Joint Meeting AMS-RSME (Seville, 2003); Harmonic Analysis Meeting in honor of I. Meyer and R. Coifman (Univ. de Paris Orsay, 2003); Workshop on Harmonic Analysis (E. Schrödinger Institute, Vienna, 2003); Barcelona Analysis Conference (Universidad de Barcelona, 2006); Jornadas de Teoría de Números (Universidad Autónoma de Madrid, 2007); Workshop on Fluid Mechanics (Pisa, 2008); Workshop on Euler and Navier-Stokes equations (AIM, Palo Alto, 2009). Antonio has also been a Colloquium speaker at many universities: Chicago, Princeton, Rutgers, Maryland, MIT, Madison (Wisconsin), UCLA, Austin, Orsay (Paris), Milano (Inst. Federigo Enriques), Barcelona (UAB, UB, UPC), Zaragoza, Murcia, Instituto Argentino de Matemáticas (Buenos Aires), Universidad de Santa Fe, Cádiz, Granada, Valencia, Bilbao, Oviedo, Vigo, UIMP, etc.

He has been part of the Scientific Committees of international meetings such as the Joint AMS-RSME Meeting (Seville, 2003), the European Math Society

Conference (Granada, 2000), or the Conference in honor of Prof. C. Fefferman (Princeton, 2009). He was President of the Scientific Committee of the Real Sociedad Matemática Española Meetings held at Madrid (2000) and Tenerife (2002), and leading researcher in several projects supported by the National Science Foundation, the USA-Spain Cooperation Committee; NATO, CYCIT (Spanish Research Agency) and Ministerio de Educación y Ciencia (Spain).

Antonio's PhD Dissertation was about the Kakeya maximal function and the spherical summation multipliers, and contains sharp estimates of their norms, in terms of the eccentricity of the involved rectangles, in the two-dimensional case. The concept and its name, Kakeya maximal function, which were introduced in his thesis, have become a standard topic in recent Fourier analysis nowadays, his results leading to continuation in the work of mathematicians like J. Bourgain or T. Tao, among others.

During the early stages of his mathematical career, Antonio investigated maximal functions associated with different kind of geometries, obtaining some new covering lemmas. In a collaboration with R. Fefferman, they proved the "exponential type" covering property satisfied by parallelepipeds in $\mathbf{R}^n$. Later (1978), he gave a positive answer to a conjecture formulated by A. Zygmund (around 1930) and which was considered an "object of desire" by the harmonic analysis school of Chicago: The basis of parallelepipeds in $\mathbf{R}^3$, whose sides are parallel to the coordinate axes with one of their dimensions depending monotonically on the others, differentiates the integrals of a function f so long as f belong locally to the space $L(\log^+ L)$. He pursued in that line of research for several years, with publications on Fourier multipliers, restriction lemmas, weighted inequalities for singular integrals, Hilbert transforms along curves and their associated maximal functions, and developing a plan to treat the difficult higher dimensional Kakeya questions, which nevertheless are still open.

In collaboration with C. Fefferman, they introduced the theory of "wave packets transform" in order to have a more flexible pseudo-differential calculus where "canonical transformations" were just rearrangements of wave packet coefficients, a theory has been used and extended by different authors. He was also interested in several inverse problems, especially in those relevant to crystallographers when the properties of quasi-crystals were uncovered around 1985. For instance, Antonio gave the mathematical proof for the basic fact that if one gets perfect Bragg's peaks in the X-ray diffraction spectrum, then we have a combination of a finite number of periodic structures, *i.e.* a crystal.

Questions about the Bochner-Riesz multipliers and their connection with restriction lemmas and Kakeya maximal functions led him to get involved in different problems in Number Theory, especially those related with the so-called "lattice-point problems", the distribution of lattice points in small arcs, and the properties of Fourier series whose frequencies are kth-powers (L^p boundedness, differentiability properties, fractal dimensions of their graphs). With J. Cilleruelo and F. Chamizo (his two PhD students in Number Theory), Antonio published several papers which could now be considered as belonging to the emerging field of Additive Combinatorics.

Antonio has published other works related to the summation of Fourier series taking into account the sice of the coefficients and the uncertainty principle, which is a problem at the interface of Crystallography and Harmonic Analysis; the L^p convergence of series in eigenfunctions of the Laplacian in balls of $\mathbf{R}^n$ and the theory of band-limited functions; the question of atomic energies of large atoms, to which he contributed with several papers in collaboration with C. Fefferman and L. Seco; his work with L. Caffarelli to develop a theory of phase transitions around the Ginzburg-Landau mean field theory, minimal surfaces and their connections with Ennio de Giorgi´s conjecture, and finally, his recent publications on Fluid Mechanics, searching for singularities in different models related with the quasi-geostrophic equation as well as studying the evolution of fluid interfaces in the Muskat problem, the Hele-Shaw cells or the theory of water waves.

When Antonio returned to Spain after a long period of work, mainly at the universities of Princeton and Chicago, he became deeply involved in the task of improving the situation of Mathematics in Spain, including the "creation" of the Mathematics Department at the Universidad Autónoma de Madrid, as well as participating in many panels and commissions to suggest and develop appropriate policies with the governmental agencies in charge of research in Spain. He also took part in the "re-foundation" of the Royal Spanish Mathematical Society (RSME), acting as president of its Scientific Committee for several years. He organized a Mathematical Summer School at the Universidad Internacional Menéndez Pelayo in collaboration with L. Caffarelli, then at the IAS (with workshops on Number Theory, Fluid Mechanics and Partial Differential Equations), as well as the first two International Conferences on Harmonic Analysis (El Escorial, Madrid). In addition to all this work, we should also mention the creation of Revista Matemática Iberoamericana in 1985. Antonio is still the Director of this journal, together with José Luis Fernández.

Antonio has been the PhD advisor to Alberto Ruiz (inverse problems); Bernardo López-Melero (maximal functions, Bochner-Riesz summability); Bartolomé Barceló (restriction of the Fourier transform to a conical surface); Luis Vega (Schrödinger operators); Juan Antonio Barceló (band limited functions); Javier Cilleruelo and Fernando Chamizo (Number Theory); Pedro Balodis (Quantum Mechanics: stability of matter); and Pablo Fernández-Gallardo (Fourier Analysis and Crystallography). See his family tree on page 31.

He has published more than 80 papers in the most prestigious journals, such as Annals of Mathematics, Inventiones, PNAS, American Journal of Mathematics, Crelle, CPAM, Advances in Mathematics, etc. A detailed list of publications can be found on page 25. Antonio has a younger brother who is a professor of Geophysics, specializing in Seismology, at the Universidad Complutense. He also has two sons (Diego, who is also a mathematician, and Rubén, a PhD in Organic Chemistry working at a medical research laboratory), and three grandchildren (the twins Adrián and Miguel, and Ainhoa, a granddaugther still at a tender age). His first wife, Maricarmen, who passed away in 1993, was a low dimensional topologist who wrote her PhD dissertation under the supervision of William Thurston. Since 1995 Antonio has been married to Amelia, who also has a degree in Mathematics; she is a high school teacher and enjoys gardening, taking care of her many plants, flowers and a splendid cactus collection.

PABLO FERNÁNDEZ: To begin with, Antonio: Have there ever been any mathematicians in your family?

ANTONIO CÓRDOBA: No, not at all. As far as I know, I'm the first one in my family ever to have been to university. My mother was a schoolteacher, which was rather odd, because in Murcia in the late twenties it was unusual for a woman to do her Baccalauréat. My maternal grandparents were practically illiterate, but out of their three children it was my mother, who was the youngest, whom they encouraged to study. She told me once that she liked Literature and Poetry, and she was thinking about studying that at the University, but finally she decided it would be more sensible to do a Teacher Training course. That was during the Republic. After the Civil War, all those qualifications were declared invalid, so she took it upon herself to sit the exams in all the same subjects again. For some reasons she never spoke about, although I deduced they were political, she never passed the competitive state exams, so for many years she worked as a supply teacher in different towns and villages around Murcia.

PF: What do you remember about your first years of study?

AC: I did my Baccalauréat at the *Instituto Alfonso X el Sabio* in Murcia, and I have fond memories of those times. Some of my teachers were extraordinary, like Francisco Soto, my Mathematics teacher, who certainly had an influence on my choice of career. He had a way of teaching that was unusual for those times; he used to sit down among us, the better to stimulate discussion about the problems he set us, and only when we'd turned the question over together from different angles did he go to the blackboard and complete the process. I met him again many years later in Valencia, at the *Escuela de Magisterio*, where he was teaching at the time, and he told me he'd been awarded a prize for a study on how to teach mathematics to blind children. At that time he was working on an analogous method for the deaf, although it apparently turned out to be much more difficult.

PF: A special kind of person then?

AC: Yes, although he wasn't the only one. There were some teachers working at that Institute who had been teaching at Universities during the Republic, but who had lost their jobs after the Civil War. I felt very much at home at the Institute, both with the teachers and with my fellow students, with whom I was very popular, partly for the studies themselves and partly because of football. It was about then that some of Rey Pastor's books came into my hands; I was fascinated by them, and they got me involved in the world of differential and integral calculus. Thanks to my teachers at the Institute, I came into contact with some lecturers from the University of Murcia, and that's how I joined the team representing the province of Murcia in a kind of cultural contest on TV: "United We Stand" [*"La unión hace la fuerza"*]. It may sound incredible today, but those were the early years of television in Spain and the programme was enormously popular. So much so that for the year and a half that it lasted, the team from Murcia was very successful (we reached the semi-final) and I became very well known in the city, which just about closed down every time the team from Murcia was on — not to mention the success I had with the girls! You can imagine what it was like for a 15 year-old kid, representing the province alongside well-known local journalists, university professors, and even an army colonel. Nevertheless, partly because there was no Faculty of Mathematics in Murcia, and partly to escape too much public exposure, I decided to move to Madrid.

Murcia, 1965

PF: That was in 1966. Did you have any help or any contacts in Madrid?

AC: I'd been awarded a grant from the Mathematical Olympiad, and together with a friend I'd met in the Olympiad, Mikel Bilbao, we applied for places at the *Colegio Mayor Pío XII*, which was offering some very special and advantageous conditions. I was accepted, and they provided me with a grant to cover all my expenses. That college turned out to be a very interesting place; it was a centre of Christian-Democratic activities where people who played an important part in the Transition, such as Landelino Lavilla, Marcelino Oreja and Peces Barba had their headquarters. On the one hand, it was a highly stimulating atmosphere; those years were full of political effervescence and student mobilization. On the other, I felt very disappointed when I discovered the mediocrity of that University. Generally speaking, the teaching standards at the Faculty of Mathematics were very poor, and many of the lecturers were official members of the regime, although there were some honourable exceptions, such as Father Alberto Dou, a cultured man with a more open mind, and Ancochea as well.

PF: Not a very encouraging outlook. What memories do you have of that course?

AC: It was particularly involved in political struggle; there were people belonging to the PCE (Spanish Communist Party) and others of a more pro-Chinese persuasion. For example, I remember José María Buendía, who was later to become general secretary of the CNT (*Confederación Nacional de Trabajo*); he took political leadership of the assemblies and meetings in which students from other degree courses took part, and where I met other people such as Paco Bernis and Cayetano López, who at that time were studying Physics and with whom I met up again at the UAM (*Universidad Autónoma de Madrid*). Academically speaking, I think it was an exceptional course with some particularly brilliant people. I've always had the feeling that that University managed to "expel" some of its best students, who were forced to find different paths for their lives.

PF: From your description of that course and the university teachers, one might guess that you all carried out a kind of self-learning.

AC: Absolutely. We were lucky in so far as the atmosphere, given that the Franco regime was coming to an end, was a bit more relaxed. For example, books from abroad were beginning to become more available, through bookstores such as Aguilar or Díaz de Santos, which were importing them. So we were able to read books by Bourbaki, Halmos on measure theory, Rudin on real and complex analysis, Dieudonné on functional analysis, and so on. They were mainly French books. We spent hours and hours at night reading these books and working with them. I remember that we gave our lecturers some hard times, what with all the knowledge we'd acquired. Of course, we did it on purpose, as a way of showing up the system. I was a fourth-year student when I met Ancochea, an elegant man who remained a little aloof from the rest of Faculty, and I always had a special relationship with him. It was he who told me that Miguel de Guzmán had just come back from Chicago. Miguel brought with him problems that provided food for thought, and for me it was a shock to discover that the mathematical edifice was far from complete, that there were still questions that required investigation and to which one could make contributions. It was Miguel who invited Alberto Calderón to Madrid, where he stayed for a couple of months, giving a course on pseudo-differential operators, which I was able to attend. Then it was Calderón who, via Miguel, provided me with the chance of going to Chicago. That was a decisive step in my life because, given the way things were in Spanish universities at the time, I was already thinking seriously about seeking a professional future somewhere else.

PF: So then you joined the doctoral programme in Chicago. Did any other of your fellow students go to study in the United States?

AC: I went to Chicago alone in the autumn of 1971, although García Cuerva and Alberto de la Torre went to St. Louis, also thanks to Miguel's mediation. And of course, Mari Carmen came over a few months later. The first year of the doctoral course in Chicago consisted of three analysis courses, three of algebra and three of geometry. At the end of the first year, according to your results, you had the chance of continuing the course with a fellowship for three more years, which was what happened in my case. Maybe my background was a bit better than those of my companions, because I'd been introduced to more subjects when doing my degree, but even so those courses were very helpful for me. What's more, Calderón set me some problems to which I began to devote some thought. However, at the end of that year, Calderón accepted an offer from Argentina, which meant spending six months in his home country and six more in Boston at the MIT. Alberto invited me to Boston, but by then I'd already met and become friends with Charlie [Fefferman], who was just about the same age as me (two months younger, in fact), and I'd already decided to work with him. Calderón thought this was a good idea, except for the uncertainty about mine being the first doctoral thesis supervised by Charlie.

PF: But when did you decide to become an analyst?

AC: Well, that was really a late decision. When I was doing my degree I felt more drawn towards Algebraic Geometry. I'd read Grothendieck's and Atiyah-MacDonald's books and felt more at home in fields such as category theory. But later, when I met Miguel, I changed direction and moved over to Fourier series and singular integrals. And then there was Chicago: Calderón was there, and so was Zygmund, and Stein's book on singular integrals had just come out, and of course Charlie was also there. It was a very special time. There's an anecdote from then; one of the courses I was doing during the first year was given by Saunders Mac Lane: one half was devoted to Galois Theory, and the other to category theory. One day in class Mac Lane asked if we had noticed any inconsistencies in what he'd been explaining to us so far. It was really an almost rhetorical question, an elementary question (the objects of a category are not a set but a class) for someone who knew anything about the subject. And since I'd already taken a look at these things, I gave him the answer, which surprised him quite a lot, and from then on I became his favourite student. I guess he felt a bit disappointed later when he found out that I was going to be an analyst instead of an algebraist. So in the summer of 1972 I was working on my thesis with Charlie, who suggested I study the Bochner-Riesz and Kakeya problems, and so on. I was lucky, and by January I'd solved them, and so my thesis was completed. Charlie, of course, was delighted and said we had to celebrate it. So we went out to dinner, and over dinner, with misplaced optimism, Charlie and I made a toast that in a few months we'd have solved the problem for more dimensions... and well, today it's still unsolved. I read my thesis in the summer of 1974, and during all that time, among other things, I was struggling with the multi-dimensional problem, without the success I would have wanted, naturally.

PF: What about after you completed your thesis? When did you decide to return to Spain?

AC: Well, it was a really smooth transition. Just after completing my thesis in Chicago, I got an offer from Princeton. I suppose the fact that Charlie was there had a lot to do with it. At the time, I wasn't so aware of the importance or relevance of the place. In Princeton I broadened my horizons, and for example I solved one of the problems that was one of the objects of desire of the Chicago school, a conjecture by Zygmund which was my first paper in the Annals. After this work I was promoted to tenure track (which consisted of a 5-year contract, after which the decision rested with the University). It was a really great period during which I learned a lot, and as it came to an end I was weighing up the different job offers I had. There was one from UCLA that seemed particularly attractive: I was really tempted by the chance of living in California. In Chicago too they were keen on my going back, but although I liked the city a lot I have to admit that, for someone born in Murcia, the thought of going there to live was hard to take. I was also in contact with Yale, with Washington University in St. Louis Missouri... And it was only a few months before Princeton would make a decision about whether I'd continue there or not. But there was no need to stretch things out to the end. In 1978 I'd taken a sabbatical year, which I spent between Madrid, Paris and Stockholm, and while I was in Madrid Miguel de Guzmán took the trouble to put me down for competitive exams, so I sat one of them and got a post (*profesor agregado* [tenured lecturer], it was called at the time) at the *Universidad Complutense*. However, I have to say that it wasn't a very good experience. For some months I had to live side by side with those who'd been

Princeton, 1976

my teachers in the dark years, which was unpleasant to say the least. What's more, any attempt to change the structure, any effort at modernization, was unthinkable. It was very discouraging, so in 1979, with consent from the Complutense, I decided to go back to Princeton, where I spent the whole year. Then Maricarmen put her foot down and said: "Antonio, you've got to make your mind up. We can either live in the USA or in Madrid... but I'm not going from place to place lugging a suitcase." At that time, Rubén was 1 year old and Diego 4. So I decided to come back.

PF: To the *Universidad Autónoma*?

AC: Yes. There was a professor's post open at the UAM, one that had just been vacated by Fernández Viñas, who had gone to Murcia. In fact, I'd known Fernández Viñas during my time as a student at the Complutense, and I have to say that he'd created a good impression; a cultured man whose mathematical rigour (coming from the French school, in which he'd been trained) contrasted sharply with the tricks we'd seen others up to. That was a chance to start in a place from scratch; an adventure that naïvely perhaps seemed exciting to me. At that time there wasn't just one Maths Department as such at the UAM, but five, which was the general rule in that period: one for the theory of functions (the most numerous, and the one I was going to be in charge of); one for functional equations; one for algebra; one for geometry and topology, and the other for statistics and operations research. All these structures – for hardly 30 lecturers, only some of who had done their thesis. It was really only a kind of academy for imparting classes; the people who worked there (many of them combined their work with teaching jobs in high schools) just gave their classes and left, which was par for the course at that time. So I got all the members of the Department together and let them know that the situation had changed; those who worked in different places were to choose just one place. And, of course, all the lecturers in the Department would have to have their Doctorates. I even offered to propose subjects for their theses, but most of them ended up by leaving. We used these vacancies to hire people of prestige. For example, we brought Roberto Moriyón, who'd done his thesis at Princeton with Fefferman, and Rubio de Francia as well; we also tried to get Antonio Ros... I have to say that that policy soon began to bear fruit, so that by the beginning of the 1980s the Department had become a reference point on the Spanish scene.

The reader is referred to the text (in Spanish) "El Departamento de Matemáticas de la UAM", to be found at

http://www.uam.es/antonio.cordoba/publicaciones-ensayos.shtml

See also there further articles and essays by Antonio Córdoba.

PF: How did you manage to make all those changes so quickly?

AC: To begin with, the Departments didn't just disappear; we decided that, even though they continued to exist formally, they wouldn't carry on working as such. We set up the Mathematics Division instead, the assembly of all the lecturers, which was where all the decisions were made. One of the indispensable requirements, a demand I absolutely insisted on, was to set up a shared, rotational teaching system. That was too much for some; I remember that several Algebra professors thought the obligation of giving classes in differential calculus was intolerable, so they switched Universities. We were so short of staff in some areas that for several years, for example, I was the Department's "official" professor of Algebra. On occasion, some of the students I met up with again later were surprised to find that I was really an analyst. How were we able to do so much, to bring about so many changes? Maybe it just happened to be the right time.

PF: I suppose you mean the first Socialist Governments. You were involved with all kinds of commissions at that time, weren't you?

AC: Yes, you could feel the winds of change blowing in a lot of ways. As far as universities were concerned, Pedro Pascual was the factotum at the Ministry of Education (first with Maravall, then with Solana). He was the one who put together the reform programme in the areas of knowledge, Department structures and the first project assessments. One of the first measures, for example, was changing (cutting down!) the names of many of the university chairs, some of which were really spectacular. Mine, just to mention one, was called the "Chair of Mathematical Analysis II", with the subtitle "real and complex variable". Pedro always managed to get me involved in the commissions that were being set up at that time. Maybe I was on more commissions than was good for me, bearing in mind that I wasn't much older than 30, but I felt that I just had to take some steps in the right direction. The most interesting of all those programmes was without doubt project assessment. There was no tradition at all in Spain in that field, and we managed to draw up a complete assessment system, which included the participation of experts from abroad. That wasn't to the liking of some, because when the reports and assessments came in, it actually turned out that some people who were supposed to have a certain renown and prestige in mathematical circles practically didn't have any. That said, I never wanted to be on any teaching commissions! No sir; to get out of it I always set one condition; those who came to me to discuss study plans had to have taught in at least three universities, one of which had to be abroad. As you can imagine, the condition was a bit excessive, and even though I admit using it to get out of those chores, I also have to say that it seemed reasonable and natural to me. Only those who'd had the experience of other systems were really qualified to decide which was the best one for us.

PF: I don't suppose those affairs made you very popular. However, to change the subject: What was the process for setting up the *Revista Matemática Iberoamericana* (RMI)?

AC: The forerunner of the RMI was the *Revista Hispanoamericana de Matemáticas*, a journal that just before the Civil War, in Rey Pastor's time, was very well considered. I'm not saying it was top class, but at least it was decent. Then it went downhill, and at the time we're talking about the quality of the journal had fallen very low. In the late 1970s there were several attempts to revive it. In particular, Pedro Abellanas asked me to prepare a scheme for recycling it, which is what I did, and I put together a set of conditions; nothing very special, in fact: a prestigious editorial board, independent of other institutions and with complete freedom for editorial decisions, and so on. It's the least you can ask for a serious journal. I promised to use my contacts for getting renowned mathematicians onto the project. But it seemed that those conditions were too demanding and they weren't accepted. A few years later, in 1984, when I was at the UAM, Pedro Luis García and Abellanas got in touch with me again and told me that they were now ready to accept the proposal. Or better still, draw up a new one, more adapted to the times. Almost at the same time, I got a call from the new president of the CSIC, José Elguero, who apparently had my references, and he asked me to join the advisory council. One of the first tasks they entrusted to me was precisely to study the viability of the *Revista Hispanoamericana*. So I told him about my negotiations with other bodies (*Real Sociedad Matemática Española* [RSME] — the Spanish Royal Society of Mathematics) and that seemed all right to him. One of the conditions we made was for the *Hispanoamericana* to publish a final issue, in order to wind up any outstanding commitments and start again from scratch. We managed to get together an initial editorial board of some standing: Fefferman, Calderón, Yves Meyer, Caffarelli, along with some members suggested by the other participants. We already had the first issue ready for publication, an excellent one, in fact, with contributions from Cafarelli, Nieremberg, Fefferman, etc., with the galleys all ready… just when the president of the Council resigned.

Los Narejos, 1986

Ruiz Trillas, with whom I'd previously had some disagreements at the Ministry, took his place. As far as mathematics was concerned, his first decision was to bring the project for the setting up of the *Instituto de Matemáticas* at the CSIC to a halt, but that's another story. As regards the *Revista*, the president of the RSME started dropping hints about making some changes to the editorial board. That seemed particularly dangerous to me, not so much because of the names that came up — although that as well — but because it would set a precedent. However, I didn't want to lose that first issue after all the effort we'd made in getting it together. They were really difficult weeks. It so happened that just then Meyer and Fefferman were visiting me in Madrid. When I was called to a meeting of the Council, I suspected the members were planning to corner me. So we turned up there, Yves, Charlie, Miguel de Guzmán and me, with the galley-proofs of the first issue. Faced with that situation, all they could do was congratulate me and encourage me to go ahead.

PF: But that collaboration didn't last very long ...

AC: At the start, the *Revista* needed a subsidy, which was supposed to be provided partly by the CSIC and partly by the Ministry (although it would all be handled through the Council). To put it mildly, it didn't work out very well. In the initial proposal we pointed out that the *Revista* had to be self-supporting in at most 5 years: if by then it was still unable to fend for itself with subscriptions, it would be a sign that it hadn't aroused enough interest and the project wouldn't be worthwhile pursuing. I have to say that it only took 4 years for the journal to be self-supporting. But by then the CSIC had lost interest in the matter. I remember a meeting I attended at which they told me that the CSIC didn't want to go on with the *Revista*, among other reasons because nobody on the Council had anything published in it, and as far as they were concerned we could do what we liked with it. That came as a surprise to me, because it was a project that by then wasn't costing them any money and, of course, for me it was a really worthwhile undertaking. Anyway, that's how we finally became independent of the Council and the RSME. At that time I thought it better to remain "in limbo" for a while, without being attached to any institution... And really that's how we remained until just a short time ago.

PF: The *Revista* has just celebrated its 25th anniversary.

AC: Yes, and it's attained a very respectable status. Instead of rankings or citations, I'd rather illustrate the fact by pointing out that, at present, Charlie

Fefferman is channelling all his production solely through Annals and the *Revista*. It started out as an adventure, as part of the plan to put Spain on the international scientific map. It seemed to us that having a journal published here (even though it was international and most of the articles were written in English, of course) was a letter of introduction for Spanish Mathematics. There was no money on the line, but a lot of prestige. The first years were tough, but little by little, and with the help of Josechu Fernández, after the third or fourth year we managed to make a go of it.

PF: Let's go back to your research work; you've been involved in different fields: harmonic analysis, mathematical physics, number theory, differential equations... What's this process, this evolution been like? What milestones stand out on the way?

AC: You have to remember that when I was doing my degree I wanted to be an algebraic geometer, although later, when I went to Chicago, I went over to the other side. But both types of training, creating language and tackling tough problems (with a language already made), were good for me. I wanted to be a physicist, and in fact I started both degree courses, but I dropped Physics in the third year. I thought it was a waste of time, and I believed I was cut out to study Mathematics on my own. The Physics degree course, what with all the labs, was more difficult to manage by myself. For example, I remember a practical assignment where we spent several months calculating the adiabatic index of air — surreal! But I've always liked Physics. When I got to Chicago I rediscovered my enthusiasm for mathematics, and I felt very comfortable in that school of analyists, as if I was one more member of the club. But by then I had the feeling that the golden age of the singular integral school (with its pseudo-differential operators and their applications to equations) was over. I'm not saying that very good things haven't been done after the 1960s, but it seemed to me that the high point had passed. At that time I thought, pedantically, that I could learn everything. And I attended all the courses, like one by Chandrasekhar on relativity, or another by Narasimhan on analytic number theory. It was on this last one where I discovered that some problems in the field went beyond the Calderón-Zygmund theory. Today, it's true, many of them are still open: questions on Sidon sets, on Fourier series with spectra in special sets... From the point of view of harmonic analysis, the big difficulty is that the kernels that appear in these problems are much more singular than those belonging to the C-Z theory. It's a different world, but at least the connection existed, and I realized right away that I could apply my knowledge


Intitute for Advanced Study, 1989

to those questions. There were the equations as well, the applications to hyperbolic problems, Schrödinger's equation… maybe not following them up was a strategic mistake in my career; that is, if we want to measure it, quantitatively, by the number of published papers. At Princeton I continued going to all the seminars; at that time several complex variables were in fashion, although I have to say I was never very excited about that. I remained faithful to my Kakeya sets and my number theory problems. Among various other things, because Charlie and I, for example, were working on a generalization of pseudo-differential operators.

In reality, all these continuations were in a certain sense natural. Apart from being subjects that I liked or were close to my own field, the other driving force behind my professional career has always been friendship. I'm lucky to have two very special friends, Charlie Fefferman and Luis Caffarelli, and collaborating with them, apart from being a natural thing, has been a pleasure. Luis arrived in Minnesota in 1974; we happened to coincide at a meeting and soon become good friends — a friendship that has grown over the years. In 1986 I took a sabbatical and was in Chicago. Luis was there. At the start we got interested in compressible fluids, shockwave theory, but we didn't get very far. Luis had already developed his free boundaries theory, and I'd always been fascinated by minimal surfaces, so our collaboration came about quite naturally and led to the work we've done on Di Giorgi's theorem, phase transitions, and so on. Charlie is the same age as me. When I met him he was barely 20 years old, and he'd always been surrounded by colleagueses much older than himself. With me it was the first time he was able to talk and work with someone his own age. They're two very special friendships, in every sense. Because, for example, they serve to put you in your right place. Until then, sure, I'd met some brilliant people, but you could still say, well, I'm pretty good, too. And then all at once you come across someone like Charlie, someone of a different order of magnitude, and that, of course, shows you where you really stand in life.

Although behind all this there's also an unbroken thread; it's not all leaps in the dark. For instance, in the work we're doing together now, on fluid mechanics, we're using various analytic techniques. Although I'm not denying that Diego's involvement hasn't been a source of encouragement for me. To cut a long story short, when he was studying for his degree Diego never wanted to talk about mathematics with me, but when he came back from the United States, given the situation maybe, he found out that speaking about mathematics with his dad wasn't so bad after all. And of course I was delighted.

The work on quantum mechanics is more or less in the same line. Here it was Luis Seco, who as a student I'd sent to Princeton to do his thesis, who insisted on me being part of the team. So I was able to help in the calculations for these asymptotic developments that have to do with the periodic table, and in which Weyl sums appear. Once again, another adventure guided by friendship and harmonic analysis.

PF: To round off, Antonio, I'd like to ask you for a final reflection, a kind of look back at the things you've enjoyed doing, your favourite results, and so on.

AC: If one had to live one's life over again, there are some things you'd do differently, of course. At that time, there were some phrases by Che that were in fashion, about having to start the revolution with small groups, and I thought it was necessary to do something similar in Mathematics in Spain. It was a bit naïve, and maybe I wasted too much energy in the attempt. If we judge a professional career by the number of articles published, then mine would certainly have been more successful if I hadn't come back to Spain; if I hadn't had to spend so much time and effort on all the things I got involved in here. But anyway, at that time I felt I had to do it.

In the last 40 years I've seen the general attitude in Science change, especially in Mathematics. In Chicago, I remember people like Calderón or Zygmund — people I admired — as types of gentlemen, mathematicians who only published a paper when it was something exquisite, when they'd solved a really difficult problem, and not just to increase their number of publications. These days, however, what we're seeing is a rat race of publications, impact ratings and so on, which quite frankly I don't like at all. It's true that during the first project assessment committee I served on, nobody got published, but now we've gone to the other extreme. I think that the status of mathematicians has gone downhill, that the mathematician ought to be a kind of artist, interested only in really original things. If not, what's it all for?

I particularly like some of the results I've achieved: the proof of the theorem of maximal operators, my first serious result; I still like that one. I still think that the proof of Zygmund's conjecture, with the covering lemmas for parallelepipeds, is pretty nice. In number theory, of course, there's a big gap between the things I've tried to do and those I've finally managed to solve. But the result with Javi Cilleruelo, for example, on the number of points on small

Soto del Real, 1992

arcs, I particularly like, and curiously it's being increasingly cited by people. That was a question that came up when we were studying restriction problems for the Fourier transform, where it was necessary precisely for the points not to accumulate on small arcs. Or the results I got with Fernando Chamizo, where we used Feynman integrals (quantum electrodynamics always fascinated me). Generally speaking, I think I'd still stand by almost all my results. They interested me at the time, they didn't seem trivial to me, and that's why I devoted myself to them.

There were some problems I tackled that I couldn't solve. To take an obvious example, the Kakeya problem in higher dimensions. One of my favourites in number theory is still to be solved: deciding whether the series with frequencies in the squares belong to L^p for p less than 4. It's a tough problem, because the kernels that appear are extremely singular integrals. The lattice point problem also intrigued me at one time. When I read what Hardy and Littlewood had done I realized right away that I could improve on their methods. They worked on the basis of the Bessel function properties, and I'd learned in Bochner-Riesz that it wasn't really relevant, but rather how and where to cut. I thought that by breaking the radial symmetry, as I'd done in Bochner-Riesz, I could go a long way. But it didn't turn out like that; I only managed to find new proofs for the classical results. While it's true that the best Iwaniec estimate breaks the radial symmetry, it wasn't as easy as I'd thought at the beginning.

PF: Did you ever try to tackle any of the "famous" problems, the classical ones, let's say?

AC: Well, you know, the lattice point is a famous problem, and now Kakeya's problem is too. That said, I can't deny that I tried it with Riemann's hypothesis; a couple of times in recent years I even thought I was onto a new idea, but it wasn't to be. Now I'm working on Navier-Stokes, so you never know...

Pablo Fernández (PF in the interview) is one of Antonio Córdoba's (AC) scientific descendents. This interview was conducted in March, 2009.

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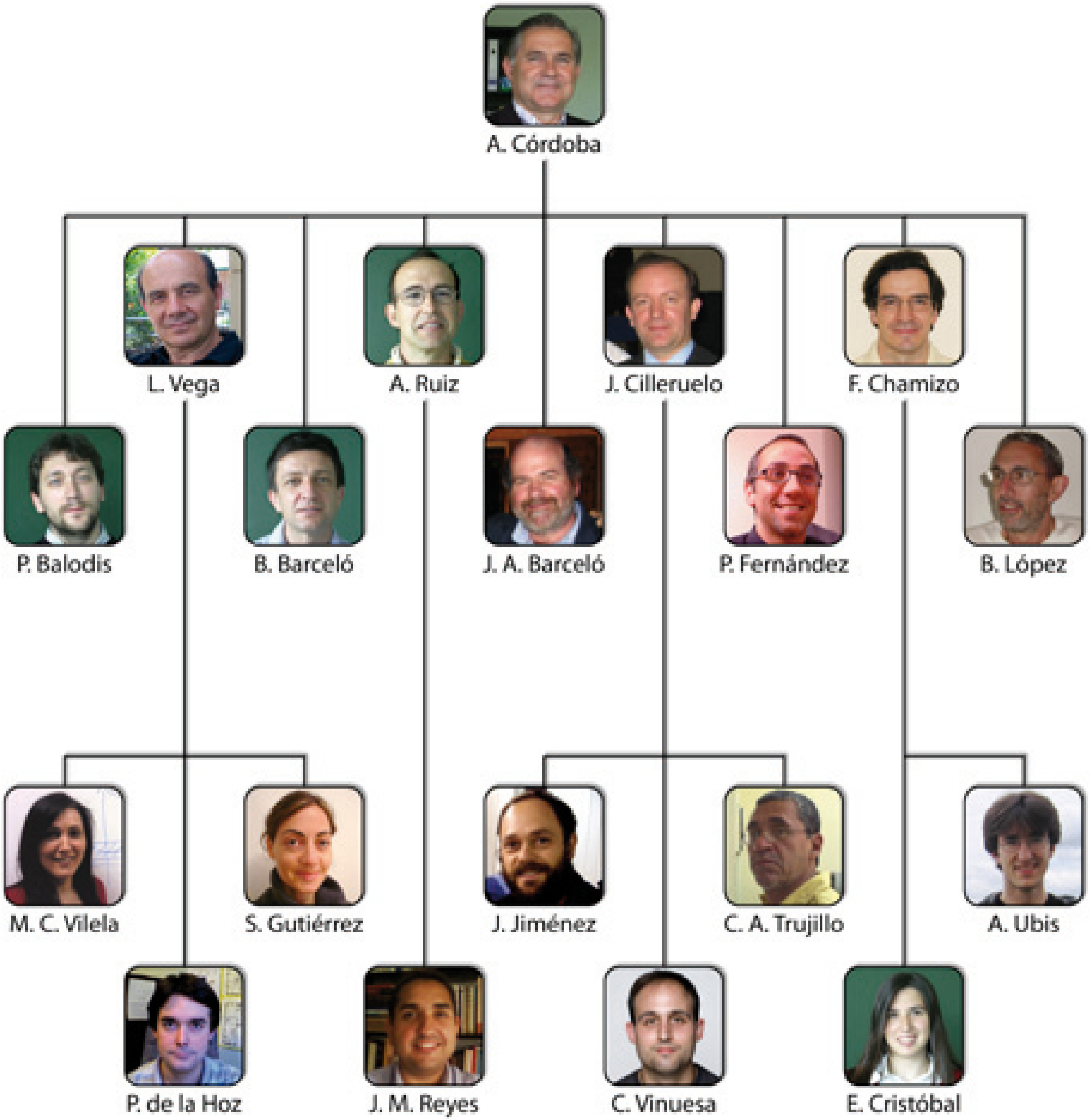
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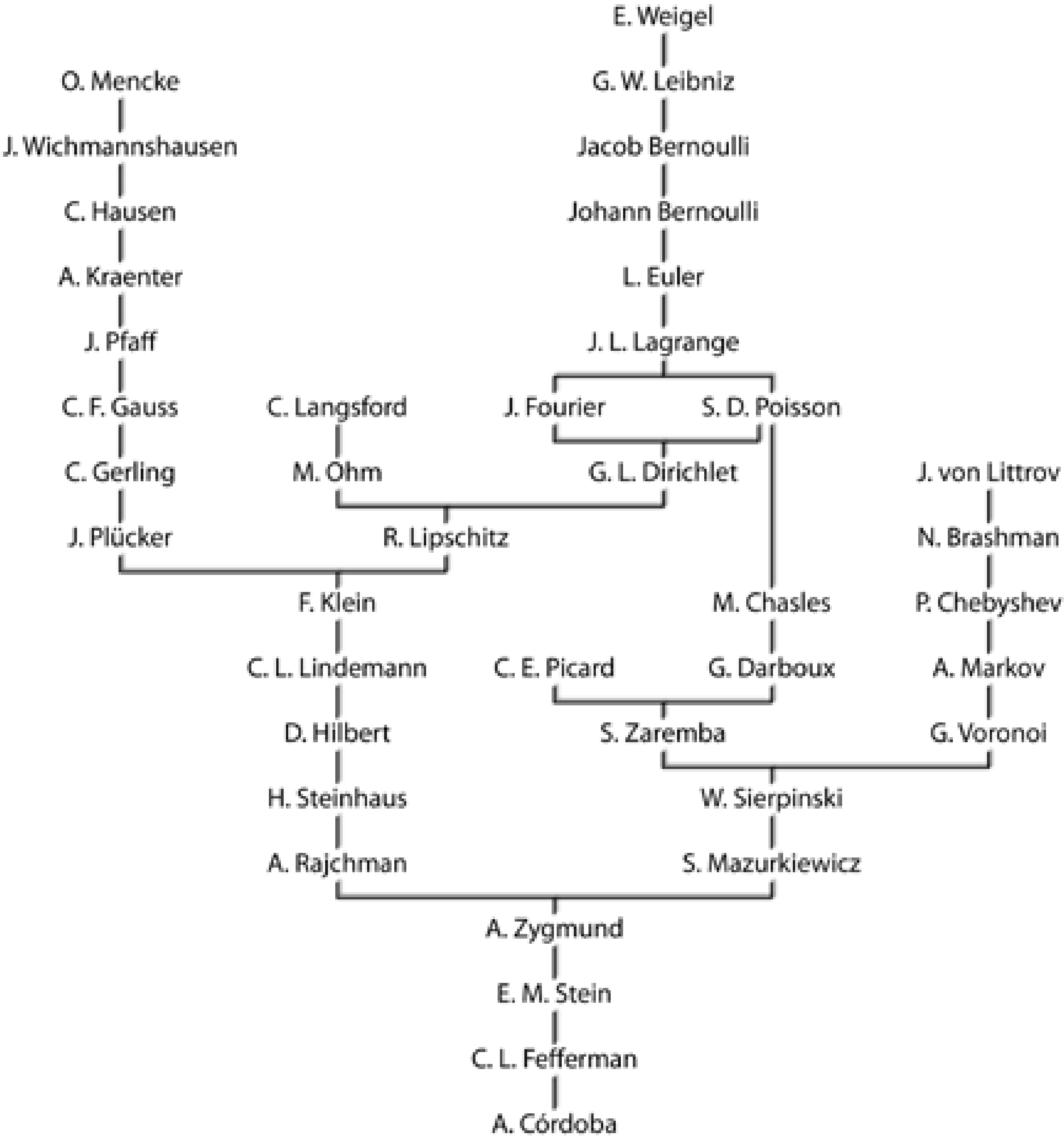
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Fotografía de Marcel.lí Bayer

Pilar Bayer graduated in mathematics at the University of Barcelona in 1968 and obtained her doctorate in mathematics at the same university in 1975. Previously, in 1967, she qualified as a piano teacher at the Municipal Conservatory of Music of Barcelona. She has been a lecturer at the University of Barcelona (1968-1975), the Autonomous University of Barcelona (1969-1977; 1981-1982), Regensburg Universität (Germany, 1977-1980) and University of Santander (1980-1981). Since 1982, she is full professor of Algebra at the University of Barcelona. Her research field is number theory. Her publications focus on zeta functions, automorphic forms, Galois theory, elliptic curves, modular curves and Shimura curves. She has supervised 10 PhD theses and numerous research projects. She has given lectures and seminars at universities and research centres in Germany, Austria, Spain, France, Greece, Poland, Russia and Tunisia. In 1998 she was awarded the *Narcís Monturiol Medal* for scientific and technological achievement by the Catalan government. In 2004 she was named *Emmy-Noether-Professorin* by the Georg-August-Universität Göttingen, Germany.

Fake elliptic curves and their moduli

Shimura curves associated to non-split quaternion algebras are coarse moduli spaces for *fake* elliptic curves. One of the first applications of Shimura curves was provided in the 1990s. By using fine properties of the Néron models of their jacobian varieties, K. Ribet was able to prove a very particular case of a deep modularity conjecture of Serre. Ribet's theorem was the starting point of Wiles' proof of Fermat's Last Theorem.

A convenient generalization of the classical complex multiplication theory led G. Shimura to his theory of the canonical models. Fake elliptic curves with complex multiplication play a key role in the theoretical construction of class fields by means of special values of arithmetic automorphic functions. As a result of joint work with A. Alsina (2004), J. Guàrdia (2005) and A. Travesa (2008), we shall present a method to compute these functions as well as to obtain their values at CM-points.

Pilar Bayer's talk will be held on Thursday, June 25th, at 16:45 pm.

Joaquim Bruna's talk will be held on Thursday, June 25th, at 15:30 pm.

He graduated in Mathematics at the University of Barcelona in 1975, and got his PhD in Mathematics in 1978 at the Universitat Autònoma de Barcelona, UAB, under the supervision of Joan Cerdá. Postdocs at Paris-Sud, Madison-Wisconsin and Albany. Since 1984, he is full professor at the UAB. Editor of *Publicacions Matematiques* for three years. Chairman of the Math Department from 1998 to 2001, he promoted the Servei de Consultoria and the Servei d'Estadistica of the UAB, the reform of the Grade in Mathematics and the plan for Joint Degrees. He is currently Director of the Centre de Recerca Matemàtica, CRM. He has supervised nine doctoral theses.

Joaquim used to be a very good football player, at least as good as Antonio. Now he enjoys climbing and long route cycling.

Basis of exponentials and related problems

The Fuglede conjecture stated that a domain U tiles $\mathbf{R}^n$ by translations if and only if $L^2(U)$ admits an orthornormal basis of exponentials (that is, sines and cosines)

$$E(\Lambda) = \{e^{i\lambda t}, \lambda \in \Lambda\}$$

corresponding to a discrete set of frequencies Λ. This conjecture has been shown to be false in general by Tao, but remains open in low dimensions, and is true under some further restrictions on U, for instance when U is convex in two dimensions.

In this scenario, the question of the existence of *Riesz basis* $E(\Lambda)$ of exponentials in $L^2(U)$, as good as orthonormal bases from the point of view of applications, appears as one of the most natural ones and yet unresolved problems in Fourier analysis. For instance, we do not know whether the unit ball B admits a Riesz basis of exponentials, and we do not know neither if an arbitrary finite union of intervals does (multiband problem).

Riesz basis are the meeting point of two other important related notions: frames and free systems. Very influential work of Beurling and Landau served to establish a critical density condition that Λ must satisfy in general, but only for intervals in one dimension a complete description is known of those Λ for which $E(\Lambda)$ is a Riesz basis.

All these questions can be restated using Fourier-Laplace transforms and complex analysis in the resulting Paley-Wiener spaces $PW(U)$ of signals with frequency content in U. Frames $E(\Lambda)$ correspond to *sampling sequences* Λ for $PW(U)$, while free systems correspond to *interpolating sequences*. The existence of sequences that are simultaneously sampling and interpolating can be considered for general spaces of analytic functions, and more generally for abstract Hilbert spaces with reproducing kernels. Some have, some do not, and some we do not know, the deep understanding of why remaining unclear.

Related to the above, the following conjecture is lately receiving much attention: every frame in an abstract Hilbert space is a finite union of Riesz sequences (Riesz basis for its linear span).

A closely related area of research going back at least to Wiener deals with the spanning capability of translates of a fixed function. Although very classical, very natural questions remain unsolved, for instance the description of the so called generator functions for $L^p(\mathbf{R})$.

In the talk we will survey known results on these topics and call the attention to several open problems. We will also consider natural generalizations of all the above to spherical harmonics and to Riemanian manifolds.

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Xavier Cabré's talk will be held on Wednesday, June 24th, at 16:45 pm.

Xavier Cabré was born in Barcelona, in 1966. He got his PhD in 1994, under the supervision of Louis Nirenberg, at the Courant Institute, New York University. He also got the *Habilitation à diriger des recherches* at the Université Pierre et Marie Curie-Paris VI in 1998. He has been awarded with the Kurt Friedrichs Prize (New York University, 1995). Member of the Institute for Advanced Study, Princeton, 1994-95, Harrington Faculty Fellow (University of Texas at Austin, 2001-02) and Tenure Associate Professor (University of Texas at Austin, 2002-03). He is now ICREA Research Professor at the Universitat Politècnica de Catalunya (since 2003) and Full Professor at the Universitat Politècnica de Catalunya (since 2008). His main field of research are Partial Differential Equations.

Front propagation and phase transitions for fractional diffusion equations

Long-range or "anomalous" diffusions, such as diffusions given by the fractional powers $(-\Delta)^\alpha$ of the Laplacian, attract lately great interest in Physics, Biology, and Finance. They appear in diffusions in plasma, dislocations in crystals, in finance (American options modelled with jump processes), in geophysical fluid dynamics (the quasi-geostrophic equation), in certain reaction fronts and flames, and in population dynamics.

The fractional powers of the Laplacian are the infinitesimal generators of the symmetric stable Lévy diffusion processes. These -also called Lévy flights- are diffusion processes that combine Brownian motion together with a jump process. From the mathematical point of view, nonlinear analysis for fractional diffusions has been mostly developed in the last years.

In this talk, I will mainly describe recent results concerning front propagation for the nonlinear fractional heat equation, as well as phase transitions for the fractional elliptic Allen-Cahn equation.

In [2] we study the propagation of fronts for the fractional KPP equation

$$\partial_t u + (-\Delta)^\alpha u = u(1-u) \quad \text{in } (0,\infty)\times R^n, \qquad 0 \le u \le 1,$$

with $\alpha \in (0,1)$. In [8,9], by heuristic considerations it was predicted that fronts should propagate at exponential speed -in contrast with the classical case $\alpha=1$ for which there is propagation at a constant KPP speed. In particular, no travelling wave should exist when $\alpha < 1$. In [2] we establish mathematically these results. For instance, given an initial condition with compact support in $\mathbf{R}^n$, we prove that every level set of u is located at time t, up to an error, near $\{ |x| = exp(\mu^* t) \}$, where $\mu^* = f'(0)/(n+2\alpha)$ and $f(u)$ is equal to $u(1-u)$ or to another concave monostable nonlinearity. Such exponential speed originates from the fact that the fundamental solution of the fractional heat equation has a power decaying tail at infinity -instead of the exponential tail of the Gaussian corresponding to $\alpha=1$.

In [1,3,4] we are concerned with the equation

$$(-\Delta)^{\alpha} u = f(u) \qquad \text{in } R^n$$

with $\alpha \in (0,1)$. The case $\alpha=1/2$ was studied in [5]. Crucial to our analysis for $\alpha \in (0,1)$ is a result of [7] which allows to realize this nonlocal equation as a degenerate elliptic equation posed in $\mathbf{R}_+^{n+1}$ together with a nonlinear Neumann boundary condition on $\mathbf{R}^n = \partial \mathbf{R}_+^{n+1}$. In [3,4] we characterize the nonlinearities f for which there exists a "layer" solution -meaning, essentially, a solution increasing in one direction. We establish several properties of these solutions, such as their uniqueness in $\mathbf{R}$, minimality, symmetry in certain dimensions, and decay at infinity. In [1] we find sharp energy estimates for these and other solutions (such as "saddle-shaped" solutions). These estimates allow to improve the 1D symmetry results of De Giorgi type for the nonlocal equation.

Finally, we will describe results from [6] on the problem

$$\begin{cases} (-\Delta_{Dir})^{1/2} u = u^p & \text{in } \Omega, \\ u = 0 & \text{on } \partial\Omega, \\ u > 0 & \text{in } \Omega, \end{cases}$$

where $(-\Delta_{Dir})^{1/2}$ stands for the unique positive square root of the Laplacian in a bounded domain $\Omega \subset \mathbf{R}^n$ with zero Dirichlet boundary conditions. Using also a local realization of the problem in the cylinder $\Omega \times [0,\infty)$, we establish existence and regularity results of positive solutions as well as a priori estimates of Gidas-Spruck type for subcritical powers, Liouville type theorems in a half space, and a symmetry result of Gidas-Ni-Nirenberg type.

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Luis A. Caffarelli was born December 8th, 1948, in Buenos Aires. He obtained his Master of Science (1968) and PhD (1972) at the University of Buenos Aires. Since 1996 he has held the Sid Richardson Chair in Mathematics at the University of Texas at Austin. He also has been a professor at the University of Minnesota, the University of Chicago, and the Courant Institute of Mathematical Sciences at New York University. From 1986 to 1996 he was a permanent member of the Institute for Advanced Study in Princeton. In 1991, he was elected to the National Academy of Sciences. He has been awarded Doctor Honoris Causa from l'Ecole Normale Superieure, Paris; Universidad Autónoma de Madrid, and Universidad de la Plata, Argentina. He has received the Bôcher Prize (1984), the Rolf Schock Prize in Mathematics (Royal Swedish Academy of Sciences, 2005) and the AMS Leroy P. Steele Prize for Lifetime Achievement (2009).

The focus of Professor Caffarelli's research has been in the area of elliptic nonlinear partial differential equations and their applications. His research has reached from theoretical questions about the regularity of solutions to fully nonlinear elliptic equations to applications like combustion. Some of his most significant contributions are the regularity of free boundary problems and solutions to nonlinear elliptic partial differential equations, optimal transportation theory and, more recently, results in the theory of homogenization and of non linear problems for non local diffusions. He continues his work with his collaborators in minimal surfaces and free boundaries in periodic media, equations of "non-divergence type", and fully nonlinear equations in periodic or random media. Another current direction of his work is the connection between optimal transportation theory and the Monge-Ampere equation.

Phase transition and minimal surfaces for non local operators

Movement by mean curvature, *i.e*, when a surface evolves with normal speed proportional to its mean curvature, appears in the modelling of phase transition phenomena, for instance as a limit of phase field models In the case of slow decay of long range interactions, the corresponding limiting transition surface moves proportionally to an "integral version" of mean curvature. We will describe these phenomena, and the geometric properties of the corresponding "integral minimal surfaces".

Fernando Chamizo's talk will be held on Friday, June 26th, at 12:30 pm.

Fernando Chamizo got his PhD degree in 1994 under the supervision of Antonio Córdoba and currently is professor at Universidad Autónoma de Madrid since 1997. His research area is Number Theory. He has worked specially in lattice point theory and non-holomorphic modular forms.

Antonio Córdoba and Number Theory

Antonio is a harmonic analyst fond of Arithmetic and his devotion materializes in quite a number of number theorist descendants: He has supervised two PhD theses in this subject, J. Cilleruelo (1990) and F. Chamizo (1994), who have generated 4 grandsons, J. Jiménez-Urroz (1995), C. Trujillo (1998), A. Ubis (2006) and E. Cristóbal (2009), and around half a dozen of new grandsons and great-grandsons are in project.

He also has co-authored (jointly with J. Cilleruelo) the book "The theory of numbers" [8] and previously he had published the notes of a graduate-level course "Lessons in Number Theory" [10] in the University of Extremadura which were very influential for me.

My purpose in this talk is to present some of the results produced by Antonio in collaboration with his big family combining Arithmetic and his harmonic analytic expertise touch.

Intertwining Analytic Number Theory and Harmonic Analysis. Antonio's opinion is that a noticeable difference between classical Harmonic Analysis and Analytic Number Theory is that the former considers kernels with simple singularities (for instance Calderón-Zygmund kernels) and the latter employs kernels with wild singularities (for instance associated to the natural boundary in the classical circle method). One should add that on the other hand Harmonic Analysis deals with very general function spaces (containing wild functions) while Analytic Number Theory focus mainly on specific simple examples.

Antonio firstly arrived to lattice point problems, a classical topic in Analytic Number Theory, through Bochner-Riesz means (a problem in which he did an outstanding contribution). The duality, the chopping of the kernel, the problems in the boundary, etc. keep a perfect analogy (see [4] §3.2). Restriction theorems for the planar Fourier transform led him to study lattice points in short arcs [8], [9] or equivalently an L^4, L^2 theorem for some short trigonometric polynomials [7]. The relation between Fourier Analysis and Additive Theory is very old (and nowadays has been drastically empowered), awakening actively his interest in this topic [6].

Strange Fourier series. From the harmonic analytical point of view lacunary Fourier series (we mean Fourier series with frequencies growing as geometric progressions) are quite good, but we do not know too much about the, so to speak, sublacunary case. Antonio has claimed sometimes that it is the missing chapter in Zygmund's volumes on trigonometric series. Some of the natural questions have a number theoretical flavour, for instance Rudin conjecture (that always has captivated Antonio). We mention here a couple of his contributions strongly linked to Number Theory: under some conditions polynomial frequencies produce a fractal behaviour [3] and some arithmetical sequences (squares, primes) produce Fourier series counterexamples for hypothetical rearrangement theorems [17].

Atomic Number Theory! The energy of the ground state of an atom is given by the bottom of the spectrum of a certain quantum Hamiltonian. It is too involved to expect any explicit solution (with the classical exception of the hydrogenian case) and some authors have faced the problem of giving an asymptotic formula for the energy when the atomic number Z goes to ∞. The variational problem coming from the quantum mechanic model leads to an exponential sum that is treated in [16] with common methods in Analytic Number Theory. The result is an unexpected "quasiperiodic" term in the asymptotics of the energy that avoids any new main term. Could one explain the periodic table with Arithmetic? This is a big challenge and hope for Antonio.

Irrational and rationals thoughts. The senior researchers surely recall the great stir caused by Apéry's proof of the irrationality of $\zeta(3)$. Later Beukers gave a simple and short proof that largely killed the attention for the new ideas. It is noticeable that Antonio got Beukers' proof independently. He has come back several times in expository papers to $\zeta(2)$ and $\zeta(3)$ and not so far he found [12] a new and easy proof of the closed formula for $\zeta(2)$.

Antonio's works touching Number Theory

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Alice Chang was born in Xi'an, China. She grew up in Taiwan and received her bachelor degree at the National Taiwan University in 1970. She moved to the United States, receiving her PhD degree in mathematics at the University of California, Berkeley in 1974. Since then, she has taught at various universities in the US, including UCLA (1981-1998), U.C. Berkeley (1989-1991). She became a Professor of Mathematics at Princeton University in 1999. She is a Sloan and Guggenheim Fellow, and was a plenary speaker at the International Congress of Mathematics in Beijing, 2002.

In her thesis and the early stages of her career, her research concentrated on problems in classical harmonic analysis. In recent years, her research interests are in geometric analysis and conformal geometry. She is involved in a project applying PDE methods to classify a class of manifolds of dimension four via conformal invariants.

Q-curvature in Conformal Geometry

In this talk, I will survey some analytic results concerned with the top order Q-curvature equation in conformal geometry. Q-curvature is the natural generalization of the Gauss curvature to even dimensional manifolds. Its close relation to the Pfaffian, the integrand in the Gauss-Bonnet formula, provides a direct relation between curvature and topology.

The notion of Q-curvature arises naturally in conformal geometry in the context of conformally covariant operators. In 1983, Paneitz gave the first construction of the fourth order conformally covariant Paneitz operator in the context of Lorentzian geometry in dimension four. The ambient metric construction, introduced by Fefferman and Graham, provides a systematic construction in general of conformally covariant operators. Each such operator gives rise to a semi-linear elliptic equation analogous to the Yamabe equations which we shall call the Q-curvature equation. These equations share a number of common features. Among these we mention the following: (i) the lack of compactness: the nonlinearity always occur at the critical exponent, for which the Sobolev imbedding is not compact; (ii) the lack of maximum principle: for example, it is not known whether the solution of the fourth order Q-curvature equation on manifolds of dimensions greater than four may touch zero. In spite of these difficulties, there has been significant progress on questions of existence, regularity and classification of entire solutions for these equations in the recent literature.

In this talk, I will give a brief survey of the subject with emphasize on applications to problems in conformal geometry.

Alice Chang's talk will be held on Thursday, June 25th, at 11:00 am.

Diego Córdoba was a student of Charles Fefferman and received his doctoral degree from Princeton University in 1998. After being a Member of the Institute of Advanced Studies at Princeton, Dickson Instructor at the University of Chicago and Assistant Professor at Princeton University, he returned to Spain in 2002 and since then he works at the Instituto de Ciencias Matemáticas of the Consejo Superior de Investigaciones Científicas in Spain. His research interest includes harmonic analysis, partial differential equations and fluid mechanics.

Interface dynamics: the Muskat problem and Euler equations

A main research topic in the mathematical analysis of fluid mechanics is focused on solving problems that involve the possible formation and propagation of singularities. In these scenarios it becomes crucial to understand the role played by the singularities in the formation of patterns. For this purpose I will describe in my talk two physical models that are of interest from this mathematical point of view as well as for their applications in physics.

I will discuss a family of contour dynamics equations given by two dimensional fluids which provides weak solutions to the incompressible porous media and Euler equations.

We regard these models as transport equations for the density, considered as an active scalar, with a divergence free velocity field given by Darcy's law (Hele-Shaw and Muskat) or Bernoulli's law (irrotational incompressible Euler equation). It follows that the vorticity is then a delta distribution at the interface multiplied by an amplitude. The dynamics of that interface is governed by the Birkhoff-Rott integral of the amplitude from which we may subtract any component in the tangential direction without modifying its evolution. We treat the case without surface tension which leads to equality of the pressure on the free boundary, and in both problems it is assumed that the initial interface does not self-intersect. We quantify that property by imposing that the arc-chord quotient be initially strictly positive. It is part of the evolution problem to check carefully that such positivity prevails for a short time, as does the Rayleigh-Taylor condition, although depending, in both cases, upon the initial data. This is a joint work with Antonio Córdoba (my dad) and Francisco Gancedo.

Diego Córdoba's talk will be held on Wednesday, June 24th, at 15:30 pm.

The classical Vortex Sheet equation provides outstanding kind of weak solutions of the two dimensional Euler equation

$$v_t + (v \cdot \nabla)v = -\nabla p, \quad \nabla \cdot v = 0,$$

where the evolution of an interface between two immiscible fluids of the same densities is modeled in such a way that the vorticity is concentrated on the free boundary $z(\alpha; t)$, and is given by a Dirac distribution as follows:

$$w(x,t) = \gamma(\alpha,t)\delta(x - z(\alpha,t)),$$

with $\gamma(\alpha; t)$ the vortex-sheet strength, i.e. ω is a measure defined by

$$< \omega, \eta >= \int \gamma(\alpha,t)\eta(z(\alpha,t))d\alpha,$$

with $\eta(x)$ a test function. We study solutions with finite energy which implies zero mean strength. In this context we choose a term in the tangential direction for the motion of the vortex sheet for which we prove well-posedness for analytic initial data. For the equation of the strength we show ill-posedness for non-analytic initial data. This is a joint work with Ángel Castro and Francisco Gancedo.

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Guy David was born June 1st, 1957, in St Omer, France. He got his PhD in 1986, under the supervision of Yves Meyer. He has been Attaché, then Chargé de recherche, CNRS (at Ecole Polytechnique, France, 1982-89), Gibbs Instructor at Yale University (1983-85), visiting associate professor at UCLA (Spring 89), visiting professor at the Mathematical Sciences Research Institute, Berkeley, CA (87-87 and 97-98) and junior member of the Institut Universitaire de France (1996-2001). He is currently Professor at the Université de Paris 11 (Orsay), France

He has been awarded the Prix Salem (1987), the Prix IBM-France (Mathematics, 1990), the Prix Institut Henri Poincaré Gauthier-Villars (Analyse non linéaire, with S. Semmes), the Ferrán Sunyer i Balaguer prize 2004 (for the book "Singular sets of minimizers for the Mumford-Shah functional"), the Grand prix Servant de l'Académie des Sciences (2004) and the Médaille d'argent du CNRS (2001). He was main speaker at the International Congress of Mathematicians (Berkeley, 1986) and is foreign honorary member of the American Academy of Arts and Sciences (since 1999).

His research interests include singular integral operators, the calculus of variations, geometric measure theory, and in particular minimal and almost minimal sets.

Guy David's talk will be held on Tuesday, June 23rd, at 12:30 pm.

About Reifenberg's topological disk theorem

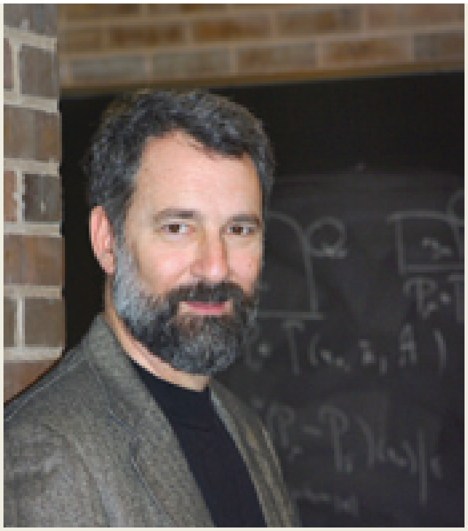
Reifenberg's topological disk theorem roughly says that if a set $E \subset \mathbf{R}^n$ is close enough (in normalized Hausdorff distance) to an affine d-plane in every ball centered on E, then it is locally equivalent to a d-plane through bihölder homeomorphisms of $\mathbf{R}^n$. In particular, it has fairly good parameterisations and is nicely embedded.

The proof, which we shall try to allude to in the lecture, is a very nice example of top-down geometric algorithm where we start from the rough description of E at the unit scale, and refine it scale by scale to get a very good description of E at the end of the argument.

We shall try to present work with T. Toro where a better control of this algorithm can also be used to give bilipschitz parameterizations of E, or parts of E, when E is Reifenberg-flat as above, and for instance we control the sum of the squares of the P. Jones numbers $\beta_q(x,r)$. Recall that these numbers measure the good approximation of E by d-planes; they were initially introduced, when $d=1$, to control the Cauchy integral operator on Lipschitz and Ahlfors-regular curves. They were also used by Jones and Okikiolu to characterize the compact sets in $\mathbf{R}^n$ that are contained in a rectifiable curve (with finite length).

Thus this work connects with results of C. Bishop, P. Jones, G. Lerman, S. Semmes, and many others where geometric properties of E (such as good average approximation by planes) are connected to more analytical properties (such as the existence of good parameterizations, or boundedness properties of singular integral operators on E). Our additional uniform flatness assumption makes the situation, and some proofs simpler.

Traditionally, Reifenberg's theorem has been used to control minimal surfaces and sets, and for instance something similar is used in J. Taylor's work on almost minimal sets, but we shall probably not have too much time to give many examples of this.



Charles Fefferman was born in Washington, D.C., in 1949. He received his B.S. at the University of Maryland in 1966 and his Ph.D. at Princeton in 1969 under the supervision of E. M. Stein. He taught at Princeton from 1969 to 1970, at the University of Chicago from 1970 to 1974, and again at Princeton since 1974. Fefferman has worked in classical Fourier analysis, partial differential equations, several complex variables, conformal geometry, quantum mechanics, fluid mechanics, and computational geometry.

His honors include the Salem Prize, the Waterman Award, the Fields Medal, the Bergman Prize, Bôcher prize, and several honorary doctorates (Doctor Honoris Causa from the Universidad Autonoma de Madrid). He has served as chairman of the Princeton mathematics department and currently chairs the board of trustees of the Mathematical Sciences Research Institute in Berkeley. He is a member of the American Academy of Arts and Sciences, the National Academy of Sciences, and the American Philosophical Society.

Selected Theorems of Antonio Córdoba

A tribute to Antonio's work.

Charles Fefferman's talk will be held on Tuesday, June 23rd, at 9:30 am.

Educated at Cambridge and Queens' University in Canada, he completed his PhD in 1987 under the direction of Paulo Ribenboim. Then he held postdoctoral positions with John Friedlander in Toronto and Enrico Bombieri in Princeton, before becoming a professor at the University of Georgia. Now Granville is a Canadian Research Chair at the Université de Montréal.

He works primarily in analytic number theory, though also with articles in algebraic number theory, arithmetic geometry, additive combinatorics, graph theory, enumerative combinatorics, theoretical computer science and links between these areas.

He has well-established links with Spanish number theory going back to the 1991 school in wonderful Santander organized by Antonio Córdoba. He has written papers with Javier Cilleruelo, Jorge Jiménez-Urroz and Adrián Ubis, and has worked with three Spanish students who came to North America for a few months during their doctoral studies.

Analysis in number theory: The circle method and arithmetic

In this talk we will present recent research by the author, with Soundararajan, as well as De la Breteche and Balog, in which we explore questions about the mean values of multiplicative functions in terms of analysis as well as arithmetic. We will see how certain classical results about these mean values can be interpreted in terms familiar from the circle method.

Andrew Granville's talk will be held on Friday, June 26th, at 11:00 am.

Miguel Ángel Herrero was born in Madrid (Spain) on 1951. He has been postdoctoral visitor in Paris VI and Oxford, and after that he has been Invited Lecturer in several institutions, in Spain and abroad. He is Professor of Applied Mathematics at Universidad Complutense in Madrid since 1988.

Miguel Ángel has published over 80 research papers on the subjects of his scientific interests, which include the theory of differential equations, reaction–diffusion systems and models in mathematical Biology. He has collaborated with several agencies, including the European Science Foundation and European Research Council (European Union), National Science Foundation (USA) and Agencia Nacional de Evaluación (ANEP) and ANECA in Spain. He has been a member of the Scientific Committee of the Real Sociedad Matemática Española, and currently serves at the Board of the European Society for Mathematical and Theoretical Biology (ESMTB). He has been also an Editor for SIAM Journal of Mathematical Analysis from 1997 to 2003, and he currently belongs to the Boards of European Journal of Applied Mathematics (since 1997) and Birkhäuser Series on Modeling and Simulation in Science, Engineering and Technology (since 2003). After having been Head of Departamento de Matemática Aplicada at Universidad Complutense de Madrid, UCM (2004-8), he is currently Director of Instituto de Matemática Interdisciplinar (IMI) at UCM.

Mathematical problems in blood coagulation

Blood coagulation is a robust security mechanism of human organisms, which prevents bleeding from minor injuries to occur. Any disruption in such system may have significant consequences. For instance, an impaired ability of blood to coagulate is cause of haemophilia, a serious hereditary disorder. On the other hand, an inordinate increase in the activation of the blood coagulation system may lead to abnormal thrombi formation, and consequently to a number of thrombotic pathologies.

The process of blood coagulation makes use of a complex array of interdependent, and finely tuned, biochemical reactions (the so-called biochemical cascade, BC), of which many details are known by now. In this lecture we shall derive a simplified mathematical model which allows us to gain insight into the early stages of thrombi formation. Such phenomena is characterized by the onset of

Miguel Ángel Herrero's talk will be held on Wednesday, June 24th, at 11:00 am.

a strong polymerization process, leading to the appearance of a microthrombi cloud (MC) that can be detected by means of ultrasound devices. We shall make use of our model to discuss on issues as the triggering of strong (thrombotic) coagulation in terms of biochemical parameters describing activation and lysis. In the case where coagulation is induced by external, pathological sources, a relation between the location of the MC and that of the activation source will be proposed. Finally, a number of problems and possible future directions on the area will be presented.

The work to be reported has been made in collaboration with G. Th. Guria and K. E. Zlobina, from the National Center for Hematology at Moscow, Russia.

Henryk Iwaniec graduated in 1971 from Warsaw University, got PhD next year and became professor at the Institute of Mathematics of the Polish Academy of Sciences before leaving for the USA in 1983. After taking several visiting positions in the USA (including a long term appointment at the Institute for Advanced Study) in January 1987 he was offered a chair of New Jersey State Professor at Rutgers, which position he enjoys to this day.

The main interest of Iwaniec is in analytic number theory and automorphic forms. Prime numbers are his passion. Iwaniec's accomplishments were acknowledged by numerous invitations to give talks at conferences and International Congresses of Mathematicians. Iwaniec is a member of the Polish Academy of Sciences, the American Academy of Arts and Sciences, the National Academy of Sciences and the "Polska Akademia Umiejetnosci".

Among several prizes which Iwaniec received are: Jurzykowski Foundation Award (New York), Sierpinski Medal (Warsaw), Ostrowski Prize (Switzerland), Cole Prize in Number Theory (USA).

Iwaniec teaches graduate students and collaborates with researchers from various countries. In 2005 he was honored by receiving the doctorate honoris causa of Bordeaux University. In 2006 the Town Council of his native city made Iwaniec an Honorary Citizen of Elblag, which is the closest to his heart life time recognition.

Henryk Iwaniec's talk will be held on Friday, June 26th, at 9:30 am.

Applications of Harmonic Analysis to Sieve Theory

(in collaboration with J.B. Friedlander)

1) Introduction: In number theory we like to count things, prime numbers, solutions to equations, and so on. Not only does this bring us pleasure but, if the count is positive, we may announce the existence of some beautiful objects (without even constructing any of them).

In counting things, perhaps the most basic ingredient is the ability to distinguish one from zero. Moreover, the greater the number of different ways we can find to express this distinction the better our chances of success. This is where analysis, especially harmonic analysis, rides to the rescue. One easy example of this is the exponential integral

$$\int_0^1 e(\alpha n)\, d\alpha = \begin{cases} 1 & \text{if } n = 0, \\ 0 & \text{otherwise.} \end{cases}$$

This allows the study of additive problems like that of Goldbach where the number of representations of $p_1 + p_2 = N$ is given by

$$\nu(N) = \int_0^1 e(-\alpha N)\, S_N^2(\alpha) d\alpha$$

with

$$S_N(\alpha) = \sum_{p \leqslant N} e(\alpha p) \ .$$

For multiplicative problems the generating functions are given by Dirichlet series rather than power series and a good way to scoop up the first few coefficients of these, say those up to x, is with the Cauchy integral

$$\frac{1}{2\pi i} \int_{\sigma - i\infty}^{\sigma + i\infty} \left(\frac{x}{n}\right)^s \frac{ds}{s} = \begin{cases} 1 & \text{if } n < x, \\ 0 & \text{if } n > x, \end{cases}$$

for $\sigma > 0$.

In sieve theory we rely on a different detector, the Möbius formula

$$\sum_{d|n} \mu(d) = \begin{cases} 1 & \text{if } n = 1, \\ 0 & \text{if } n > 1. \end{cases}$$

Recall that the Möbius function is defined as

$$\mu(d) = (-1)^r \text{ if } d = p_1 ... p_r,\ p_j \text{ distinct primes,}$$

and $\mu(d) = 0$ otherwise.

This detector is particularly convenient for studying the sum

$$S(\mathcal{A}, P) = \sum_{\substack{n \leqslant x \\ (n,P)=1}} a_n$$

for any number of sequences $A = (a_n)$ which are interesting for arithmetic reasons. We call $S(A, P)$ the sifting function because in our minds P is the product of all small primes so that $S(A, P)$ registers the support of A on those integers n free from small primes, hence almost-primes. We get

$$S(\mathcal{A},P)=\sum_{n\leqslant x}a_n\sum_{\substack{d|n\\d|P}}\mu(d)=\sum_{d|P}\mu(d)A_d(x)$$

where $A_d(x)$ is the congruence sum

$$A_d(x)=\sum_{\substack{n\leqslant x\\n\equiv 0\,(\mathrm{mod}\,d)}}a_n\ .$$

In essence we have reduced the study of counting almost-primes to counting multiples of d on average over d. In modern sieve theory the Möbius function is replaced by other functions which are similar but offer some technical advantages, nevertheless the problem is still reduced to precisely the same congruence sums. The latter are amenable to treatment by various tools from harmonic analysis. Here we shall exhibit a number of such instances.

2) Poisson Summation: In evaluating the congruence sums the main issue is to succeed with d as large as possible. Sometimes, thanks to harmonic analysis, we can even succeed when d is so large that the number of elements is less than one! Consider A to be the characteristic function of integers in the short interval

$$x-y<n\leqslant x$$

where y is small compared to x, say $y = x^{1/3}$. Now the congruence sum counts integers in the even shorter interval

$$\frac{x-y}{d}<n\leqslant\frac{x}{d}\ .$$

Clearly, if $d > y$ then the answer is usually zero or sometimes one. The sieve axioms however want us to think that it is y/d. Harmonic analysis, in the person of the Fourier expansion

$$t-[t]-\frac{1}{2}=-\sum_{h=1}^{\infty}\frac{\sin 2\pi ht}{\pi h}\ ,$$

allows us to do so to some extent on average over d.

In more delicate (for example higher-dimensional) cases it is more convenient to introduce smoothing devices and apply Poisson summation rather than Fourier expansion:

$$\sum_{n\in\mathbb{Z}^r} F(n) = \sum_{h\in\mathbb{Z}^r} \hat{F}(h)\ .$$

The expected main term comes from the frequency $h = 0$ which coincides with the sieve presumption. The fact that the next integer is far from zero is crucial for success. This spectral gap allows us to prove that the Fourier integrals from the non-zero frequencies are small, at least on average over d.

3) Weyl Sums and Quadratic Polynomials: Fascinating features appear when Poisson summation is combined with intrinsic characteristics of the sequence more subtle than simply summing over a lattice. This can already be seen for quadratic polynomials.

When evaluating the congruence sum

$$A_d(x) = \sum_{n^2+1\equiv 0\,(\mathrm{mod}\,d)} F\Big(\frac{n}{x}\Big)$$

we first split into classes $n \equiv \nu \pmod d$ with $\nu^2 + 1 \equiv 0 \pmod d$ and apply Poisson summation in each, getting

$$A_d(x) = \frac{x}{d}\sum_h W_h(d)\,\hat{F}\Big(\frac{hx}{d}\Big)$$

where

$$W_h(d) = \sum_{\nu^2+1\equiv 0\,(\mathrm{mod}\,d)} e\Big(\frac{h\nu}{d}\Big)$$

is the Weyl sum. Here one needs not only a good bound for the Fourier transform $\hat{F}$ but more importantly for Weyl sums, again on average over d.

In more sophisticated situations the Weyl sums become products of exponential sums on varieties over finite fields and the profound theory of Deligne brings powerful bounds. Here the roots of L-functions constitute the spectrum and the fact that there are only a few of them means that they can be treated as separate.

Understanding of the Weyl sums just for quadratic polynomials leads to objects more classical, yet still challenging. Recall that for our sieve problems we only need results which hold on average over d and that is a good thing because that is all we shall be able to get. The arithmetically most interesting moduli are primes (think finite field) and the general case can be reduced to these by combinatorial devices.

Due to the sign changes of the Weyl harmonics $e(h\nu/p)$ we expect, for each $h\neq 0$, a substantial cancellation when summing over p. Using modern technology we succeeded in proving the following:

Theorem 1. *(Duke-Friedlander-Iwaniec)*

$$\frac{1}{\pi(x)}\sum_{p\leqslant x} W_h(p) \to 0$$

as $x \to \infty$.

Note that this means, due to the Weyl equidistribution criterion, that the roots ν (mod p) are equidistributed.

4) Congruence Sums over Congruence Groups: To perform summations over primes along the lines of the Riemann-Hilbert-Pólya ideas by means of a spectral interpretation of the zeros of L-functions remains alas an incomplete dream. In its absence, the elementary combinatorial ideas (inclusion-exclusion procedure) of the sieve offer chances for success in handling some sums over primes. As with the initial sieve problems, the issue becomes one of evaluating a congruence sum, this time one of a different type:

$$S_h(x;q) = \sum_{\substack{c\leqslant x \\ c\equiv 0 \,(\mathrm{mod}\, q)}} W_h(c) \ .$$

Here, the summation over c of the highly arithmetic objects (Weyl sums) admits a spectral interpretation, not with respect to the Euclidean Laplacian, but rather the hyperbolic one due to the spectral theory of automorphic forms architected by Selberg. Because of the congruence $c \equiv 0$ (mod q) the action takes place on the Riemann surface $\Gamma_0(q) \setminus \mathrm{H}$.

By analogy it is no surprise that the smallest eigenvalue $\lambda_1(q)$ determines the size of $S_h(x;q)$. In general λ_1 could be close to zero which would be useless, but for congruence groups the arithmetic prevents this and Selberg showed that

$$\lambda_1 \geqslant \frac{3}{16} \ .$$

One cannot resist pointing out that this successful gap principle also depends on the Riemann Hypothesis, in this case for curves over a finite field, proved and applied to Kloosterman sums by André Weil. It is fascinating that we appeal to the individual Kloosterman sums to deduce bounds for the eigenvalues and then, through the spectral resolution, go backwards to deduce cancellation in sums of Kloosterman sums. It is almost as if we could use statements about primes to say something about the zeros of the zeta-function and then return by the Riemann explicit formula to say more about the primes.

5) Beyond the Riemann Hypothesis: Actually, there is a way we can use this automorphic information to say things about primes that go beyond what the Riemann Hypothesis tells us. Using the harmonic analysis of classical Dirichlet characters (large sieve inequality) one gets the Bombieri-Vinogradov bound for primes:

Theorem 2. Let $a \neq 0$ be an integer. Then

$$\sum_{\substack{q\leqslant Q\\(q,a)=1}} \left|\pi(x;q,a) - \frac{1}{\varphi(q)}\operatorname{li} x\right| \ll x(\log x)^{-2009}$$

with $Q = x^{\frac{1}{2}}(\log x)^{-9999}$.

Since one can take Q almost up to $\sqrt{x}$ this result has served in many applications as a substitute for the Riemann Hypothesis. Enhancing the abelian harmonic analysis of characters with the non-abelian harmonic analysis of eigenvalues we were able to surpass the square root barrier.

Theorem 3. *(Bombieri-Friedlander-Iwaniec)* Let $a \neq 0$ be an integer. Then

$$\sum_{\substack{q\leqslant Q\\(q,a)=1}} \left|\pi(x;q,a) - \frac{1}{\varphi(q)}\operatorname{li} x\right| \ll x\Big(\delta(x) + \frac{\log\log x}{\log x}\Big)^2$$

with $Q = x^{1/2+\delta(x)}$ for any $\delta(x) \geq 0$ the implied constant depending on a.

Here Q is only slightly larger than $\sqrt{x}$ but we have similar results, more complicated to state but far more useful, with Q as large as $x^{4/7}$.

Rafael de la Llave was born in Madrid. He became licenciado en Ciencias Físicas in Universidad Complutense in 1979 and immediately -thanks to the good efforts of A Córdoba, A. Casal, M. de Guzmán and others to whom he is quite grateful- he became a graduate student in Princeton. He received the PhD in Mathematics in 1983. He also spent 5 years as an assistant professor in Mathematics in Princeton. He has been affiliated as a postdoc in IMA, Univ. Minnesota, IHES. Since 1989, he has been at University of Texas.

He has supervised 10 PhD thesis (4 in Physics) and a number of postdocs. He has contributed to organize several conferences and special semesters. He has written papers with over 50 people. He has been visiting Universitat Politecnica de Catalunya for over 20 years. He is in the editorial board of several journals. He was among the founders of Mathematical Physics Electronic Journal and the mathematical Physics preprint archive -the first electronic preprint server and has been active in several committees related to publication.

His main area of research is dynamical systems (hyperbolic systems, KAM theory, variational methods, numerical methods) but he also works occasionally in partial differential equations and other areas of analysis.

Rafael de la Llave's talk will be held on Tuesday, June 23rd, at 11:00 am.

How to get far with little effort: skipping away

1. Introduction. One of the deepest aspirations of human kind is to get far using small effort. As anybody who has tried knows, this is not easy and it requires a careful planning.

A mathematical formulation of the problem is the study of Hamiltonian systems subject to small periodic forces which average out. One can ask the question of whether there are solutions for which the forces accumulate or whether they average out.

For linear systems, the answer is easy, We all know that if the forces have the same frequency as one of the natural frequencies of the system, then there is polynomial growth and if not, the solutions remain bounded. In nonlinear systems (with some appropriate non-degeneracy and regularity conditions), there are two obstructions. The KAM theorem asserts that most points are stable for all time. The Nekhorosev theorem states that all points are stable for long times.

Nevertheless, it was pointed out in [3, 17, 18, 16] that you cannot get stability for all points and all time. Some related phenomena were discovered in the celestial mechanics papers [1].

2. The problem of instability. In [2], one can find conjectures that instability should be very abundant in many systems. There are many precise versions of these conjectures[1]. Of course, besides the mathematical work, there has been extensive numerical work, some of which has yielded invaluable insights [5, 20].

Among the rigorous mathematical work we can distinguish several modalities:

A) Construct examples of the phenomenon. [3, 17, 18, 11, 8, 12, 14, 15].

B) Establish the phenomenon under some genericity (which topology?) conditions in one part of phase space (e.g. positive definite systems) [6, 7, 4].

C) Give explicit computations that show that a system experiences the phenomenon (often these conditions are satisfied for generic systems).

3. Some recent developments. In this lecture, we will cover some developments pertaining to approach C) following the work of Delshams, Gidea, Seara and the author.

The main idea is that, rather than considering just whiskered tori, it is more advantageous to consider normally hyperbolic manifolds with a rich dynamics.

The mechanisms of escape can be described as jumping away from the normally hyperbolic invariant manifold when the forcing is favorable, staying close to it when the motion is unfavorable.

The main tool to study these homoclinic excursions is the so-called scattering map. We will also discuss the method of correctly aligned windows.

By combining these tools we can exhibit large scale effects in much simpler ways than before. The new methods eliminate some assumptions from [9, 10, 13] and give estimates on the time for the phenomenon to happen. In contrast with previous estimates, we do not need to hang around a long time in the manifold, just touch and skip away so, there is no need to use KAM methods.

[1] In [19] there was a round table that included Arnol'd, Gallavotti, Herman, Moser, Neishtadt, Sinai and many others. The moderators posed the question of whether there should be a canonical mathematical definition of Arnol'd diffusion. There was strong consensus that it was better to let each paper make its own precise definition.

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Antonio Ros's talk will be held on Thursday, June 25th, at 12:30 pm.

Antonio Ros is a professor of the University of Granada and his research focuses in Geometric Analysis. This area arises from the interaction of Geometry with other parts of the mathematics as Partial Differential Equations, Topology or Complex Analysis.

Area minimizing surfaces

The study of surfaces of least area, or more generally surfaces with stationary area under a natural constraint, has influenced the development of mathematics and, in particular, of geometry. In our talk we will present some recent results in this field.

We first consider classical theory of minimal surfaces. As an example we have the Bernstein Theorem, which characterizes the plane as the unique entire minimal graph on $\mathbf{R}^2$, whose extensions and generalizations continue nowadays to be and active field of research.

One of these extensions consists of classifying properly embedded simply connected minimal surfaces in $\mathbf{R}^3$. The solution of this problem by Meeks and Rosenberg [5] is one of the mayor achievements of the theory in the last years. The proof depends on results by Colding and Minicozzi about curvature estimates for embedded minimal disks and about minimal laminations. More generally, we can consider the classification of properly embedded minimal surfaces of genus zero in $\mathbf{R}^3$. The case of nontrivial finite topology was solved as a combination of works by Collin [1] and López and Ros [2] and recently Meeks, Pérez and Ros [4] have completed this program by classifying the surfaces in this family with infinite topology. Joining these results we have the following

Theorem. *Let $S \subset \mathbf{R}^3$ be a properly embedded minimal surface with the topology of an open subset in the plane. Then S is one of the following classic surfaces:*

i) The plane.
ii) The Helicoid.
iii) The Catenoid.
iv) A Riemann minimal example.

The second main topic we want to consider is the Isoperimetric Problem, which consists of the study of area minimizing surfaces among those enclosing a prescribed volume. When the surface sits in $\mathbf{R}^3$, the solution is the round sphere; but the problem appears naturally in many other contexts: Riemannian manifolds, spaces with a distance and a measure and, even without leaving the Euclidean geometry, we can study the isoperimetric problem for surfaces enclosing a volume in a certain region (this problem connects to constant mean curvature surfaces with free boundary). We can also consider the Periodic Isoperimetric Problem, which consists of describing, among surfaces dividing the space in two G-invariant regions with prescribed volume fraction, those which have least area (per unit cell), where G is a symmetry group of $\mathbf{R}^3$, see for instance [7,9].

Many basic questions appearing in the above situations remain open. As an example we will discuss the following

Theorem. *Let $S \subset T^3$ be a closed surface of genus g in a flat 3-torus which is a local minimum of the isoperimetric problem. Then S is one of the following:*

- *i) The round sphere, $g = 0$.*
- *ii) The (quotient of a) circular cylinder, $g = 1$.*
- *iii) A (quotient of a) doubly periodic surface with $g = 2$.*
- *iv) A (quotient of a) triply periodic surface with $g = 3$.*

This result is sharp and combines contributions by Ritoré, Ros [8,9] and other authors. It extends to the periodic case the characterization of the sphere, obtained by Barbosa and do Carmo, as the unique local minimum of the isoperimetric problem in $\mathbf{R}^3$ and provides a theoretical support to explain the geometry of certain sophisticated interfaces appearing in mesoscale phase separation phenomena. An interesting example of a local minimum of the isoperimetric problem is the Gyroid of A. Schoen which is a triply periodic minimal surface of genus 3 in the *bcc* torus.

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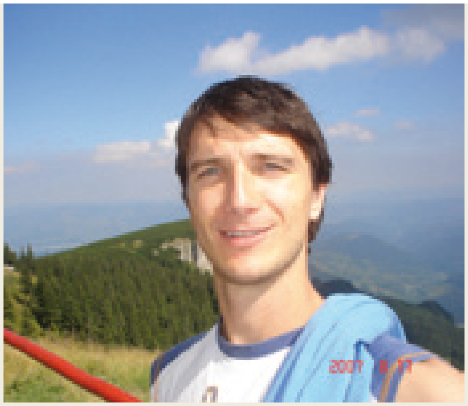

Ovidiu Savin's talk will be held on Wednesday, June 24th, at 12:30 pm.

Ovidiu Savin was born in Piatra Neamt (Romania), January 1st, 1977. He got his MS in Mathematics at the University of Pittsburgh, 1999, and his PhD at the University of Texas at Austin, 2003, under the supervision of Luis Caffarelli. He has been a Miller Postdoctoral Fellow at UC Berkeley (2003-2006) and from 2006 he is Associate Professor at Columbia University. His research interests include partial differential equations, with emphasis on the regularity of solutions to elliptic PDEs.

Minimizers of convex functionals arising in random surfaces

We consider the classical problem in the calculus of variations of minimizing

$$\int_\Omega F(\nabla u)\,dx$$

in two dimensions for certain classes of convex, possibly non-smooth functions F, and discuss the C^1 regularity of minimizers.

Our work is motivated by a series of recent papers in statistical mechanics and combinatorics on random surfaces and random tilings by Cohn, Kenyon, Okounkov, Sheffield and others. The functions F which appear in these papers are defined on a polygonal domain $\bar{N}$ (called the Newton polygon), they are smooth in the interior of N except at a finite number of points, and they are piecewise linear on ∂N. We investigate the regularity of minimizers for functionals with these properties, without any assumptions about the behavior of F on ∂N. In this case the set of degeneracy of D^2F can be thought as the union of a finite set with ∂N. This variational problem can be thought also as a degenerate obstacle problem.

Our main result says that minimizers are C^1 in Ω except on a number of segments which have an end point on $\partial\Omega$ and have directions perpendicular to the sides of N. On these segments the minimizer coincides with either the lower or the upper obstacle.

We also discuss a type of continuity result at the points of non-differentiability: if a sequence of points converges to such a point then their corresponding gradients must approach ∂N.

Our techniques also improve the classical regularity results of De Giorgi-Nash-Moser in two dimensions. We obtain C^1 regularity of Lipschitz minimizers for two large classes of non-uniformly elliptic functionals F defined in $\mathbf{R}^2$. One class consists of functionals for which there exist two open sets $O_\lambda \subset \{ D^2F > \lambda I \}$ and $V_\Lambda \subset \{ D^2F < \Lambda I \}$ that cover $\mathbf{R}^2$. The second class consists of those functionals that have bounded second derivatives only from below except at a finite number of points.

This is a joint work with D. de Silva.

Luis Seco is a Professor of Mathematics at the University of Toronto. He is also the President and CEO of Sigma Analysis & Management, a Canadian portfolio management firm, the director of the Mathematical Finance Program at the University of Toronto and the director of RiskLab-Toronto. He obtained his Bachelor's degree from Universidad Autonoma de Madrid in 1985, and his Ph.D. from Princeton University in 1989. He was a Bateman Instructor at the California Institute of Technology.

He has worked on Quantum Mechanics and for the last decade his research interests have been in the area of mathematical finance, mainly on financial risk management and asset management.

Managing Financial Risk with Laplace

It is remarkable that something as prosaic as the banking sector develops a poetic dimension when viewed through a Antonio's mathematics lens. This is what some ideas of Antonio did.

On October 19th, 1987, the stock market plunged 25%. This event had two important consequences. The first was bad: companies went bankrupt, people lost money and jobs. The second was good: banking regulators realized that the derivatives markets which had started to develop in the seventies had changed the financial landscape forever, from a financial sector that was linear to another one where convexity matters, a lot.

The linear world of finance was simple: banks had assets and liabilities, and changes in prices had linear impact on those: if markets go up, say 1%, assets go up in value more or less the same. Financial derivatives changed this: a seemingly small event somewhere could be amplified and have terrifying consequences. This is what happened in 1987, and then again in 1995 (after the earthquake in Kobe that ended Barings Bank), the Russian default crisis of 1998, and of course the burst of the sub-prime bubble of 2007. The seventies changed the world of finance from linear to non-linear, and this became obvious for the first time in 1987.

From a mathematical viewpoint, a linear financial world is fairly simple; most financial risk factors are described by normal -or gaussian- distributions. Linear combinations of gaussians are gaussians, because adding random variables means convolution of their distribution densities, or taking products on Fourier

Luis Seco's talk will be held on Tuesday, June 23rd, at 16:45 pm.

space; and products of gaussians are gaussian. When Banking regulators set to work in the early nineties, they realized -in non-mathematical terms- that loss of linearity will lead to mathematically sophisticated regulation. One component of this new piece of regulation was the RiskMetrics methodology, which had been voluntarily developed by the Bank J. P. Morgan in 1994 and adopted by the Basel Committee. Their new piece of regulation was based on a new concept: Value-at-Risk, or VaR, which in mathematical terms is nothing but a quantile of the profit-and-loss function, or P&L, of a bank. The problem is that the P&L is not a function given by a mathematical expression; banks had the ability to calculate with computers, more or less accurately but only after hours of computing time, their P&L as a function of market values, but generating a random variable out of this was a daunting task. Therefore simplifications were needed.

The best known simplification is called the delta-normal VaR which, in simple terms, is a first order Taylor expansion of the P&L as a function of the risk factors; of course, this turns the non-linear problem back into a linear one, which can be solved explicitly, but at the expense of large inaccuracies. The problem with something as simple as a quadratic correction is that, while sums of gaussians are gaussians, quadratic forms of gaussians are not easily tractable objects.

In 1997, Maite Quintanilla -then a graduate student at the University of Toronto- and myself, started to talk to Antonio about how to deal with this problem, and one idea came up: whereas the distribution of a quadratic form of gaussians is messy, the asymptotic expansion of its tail around infinity is something that could be done with a stationary phase expansion -one of Antonio's specialties- and would lead to a simple explicit expression. The mathematical statement of this became Maite's Master's thesis; upgrading this mathematical idea to address the original problem of VaR calculation is something that we did after expanding the collaborative team to include Raymond Brummelhuis. Eventually, Maite designed a methodology to minimize portfolio risks from this asymptotic perspective which became her PhD thesis at the University of Toronto.

The result of all this is a geometric interpretation of the VaR concept for non-linear portfolios, which expresses the main sources of market risk as a mathematical expression involving curvatures of certain manifolds: mathematical financial poetry.

Luis Vega is a professor at Universidad del País Vasco/EHU in Bilbao (Spain). He works on Harmonic Analysis and Partial Differential Equations, and more concretely in the analysis of dispersive phenomena in wave propagation. His recent work is related to Hardy's version of the uncertainty principle, the connection between non-linear Schrödinger equations and vortex dynamics, and to the study of the scattering operators of linear and non-linear wave propagators.

A new approach to Hardy's uncertainty principle and applications

Hardy's theorem states that if a function and its Fourier transform decay faster than a given Gaussian then they must vanish. In the talk I'll present some joint work with Escauriaza, Kenig and Ponce where we restate Hardy's result as a uniqueness result for solutions of the free Schrödinger equation that have a gaussian decay at two different times. Then we extend the result considering perturbations of zero order. Our approach is based on the use of Carleman estimates and log convexity properties.

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