An Interview with Jean-Éric Pin

Let us start with your initial academic path. Your first university degree is in Mathematics and then you did your third cycle thesis[1] *on automata, more precisely on Černý's Conjecture, a conjecture that is still unsolved...*

I did not start working on automata, it happened by chance. I started by attending a course on categories, which I did not like very much. Afterwards I followed a course on semigroups taught by Paul Dubreil. This was his last course and it was fairly elementary, but Dubreil suggested that the students attend the seminars given by Klaus Keimel, a German researcher living in France. I attended these conferences and once, Keimel and I met on the suburban train to Paris. At that time, I had done some work on semigroups by myself and I asked him for advice since I did not know whether it was worth publishing and how to do it. This work was published some years later under the horrible title *Holoïdes factoriels*. I also asked advice about my future studies and Klaus Keimel told me that Prof. Dubreil was about to retire, and suggested that I move to Schützenberger's school on automata. According to Keimel, Marcel-Paul Schützenberger was a genius, an opinion to which I fully subscribe. The following year there were two courses on the same day, one on the algebraic theory of automata, managed by Jean-François Perrot, and another one on the theory of context-free languages, managed by Maurice Nivat. I took the first course, together with no more than four other students. This course was taught by Jean-François Perrot, Dominique Perrin, Gérard Lallement, Jean Berstel and the tutorials were given by Jacques Sakarovitch and Jean-Michel Autebert, so we had the best lecturers of that time for just a few students. I learned a lot in this course and it was here that Perrot talked about Černý's Conjecture. The conjecture was easy to understand and quite fascinating, so I decided to work on it.

Were you so naive then as to think that you could solve the problem easily?

Not really. What happened was that, during my DEA[2], I solved a particular case and Perrot encouraged me to pursue this direction. In the second year of the third cycle thesis, I became interested in varieties of semigroups and varieties of languages and this topic ultimately became my main topic of interest.

Do you feel that during the time you were preparing your two Ph.D. theses you had a master?

Perrot, my supervisor, taught me a lot of things. I learned from him how to write a paper and organize a conference presentation. He took me to a conference in Italy at the end of the first year of my Ph.D. On this occasion, he introduced me to several people, notably Antonio Restivo and Aldo De Luca, that I met for the first time there. All of this turned to be very important for the future of my career. Aldo and Antonio and I remain very good friends. We met up just two weeks ago in Belgium.

Jean-Éric Pin

And Schützenberger?

I was not a direct student of Schützenberger, but, of course, I learnt a lot reading his articles. I have to say that Perrot helped me to start reading Schützenberger,

[1] Until 1984, the Ph.D. programme in France was composed of two theses: the first one was called *thèse de troisième cycle* and the second one *thèse d'état*.

[2] *Diplôme d'études approfondis*, a French degree corresponding to a master's degree.

whose style is rather peculiar. Perrot's claim was that Schützenberger was acting like a fox rubbing out its track with its tail. Therefore, reading Schützenberger was sometimes quite demanding for a young student. But when you read an article really in full detail, then at some point you become so familiar with its content that you have the feeling it is your article. There are actually very few papers that I read in such a depth in my life, but they include some of Schützenberger's and also an article by Wolfgang Thomas on the connections between automata and logic that I read a few years later. Schützenberger had a singular personality and this was a handicap for me to a closer relationship with him. Thus I was mostly influenced by Perrot, Perrin and Berstel. I only met Schützenberger from time to time, but I learned a lot from his papers. And he was also the president of the jury for the defense of my thesis.

But did you discuss his work with him?

There are a certain number of questions that I discussed with him, but these discussions were rather peculiar because he was on a much higher level than me and there were some sentences that I could not even understand at the time. But it became easier when I became more expert in automata theory. I remember that when I explained him my ideas about ordered semigroups, he got really interested and suggested that I read some related articles.

In your huge list of publications, there are some papers in which you approach finite automata in a purely combinatorial way and other papers where the main tool is logic, but most of them are about finite semigroups, the algebraic counterpart of finite automata. Why? Is that just a matter of taste?

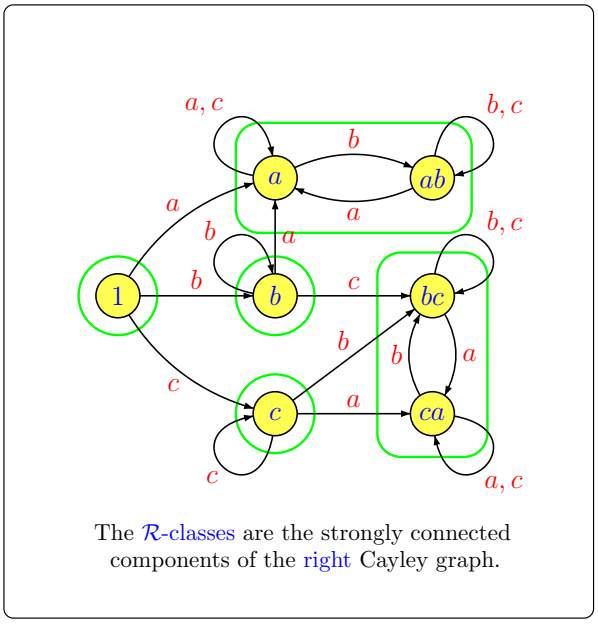
This is a matter of taste. Semigroups lured me even before I started my research. I always liked algebra very much. I read a first course on algebra by Roger Godement during the holidays immediately after the high school final exams and algebra became my favorite topic. Next I became interested in logic and more specifically, in model theory. Schützenberger, who worked with McNaughton, apparently never got interested in logic, I don't know why. Neither was it a conversation topic with Perrin and Berstel at that time. But it changed later. My interest in logic started when I studied the paper by Wolfgang Thomas I mentioned earlier. I really wanted to understand this article and I knew absolutely nothing about logic, even the basic definitions. I then asked one of my colleagues, Michel Parigot, a logician, for a reference book on logic and finite model theory. He recommended the first chapter of the Handbook of Mathematical Logic edited by Jon Barwise, which turned out to be an excellent advice. I read this chapter carefully and actually attempted to read other chapters as well. After that, I really got interested in the relationship between languages and logic. Even if my preference goes to algebra as I said before, I really enjoy the connections with logic. As any mathematician, I like the interaction between different areas of science that are apparently far apart and I have always looked for them.

In the late eighties you began using profinite tools, which allowed you to find remarkable results...

This is an interesting story. At that time Jorge Almeida was working on profinite monoids, but he preferred the approach by implicit operations, which hides a bit of the topological aspect. Independently, I read a paper by Christophe Reutenauer, called *Une topologie du monoïde libre*, which contained very interesting ideas that I decided to explore. In 1983, Stuart W. Margolis was an invited professor at the University Paris 6 and he spent these nine months at my place. We worked together in several directions and wrote several papers about inverse semigroups and semigroups with commuting idempotents. On this occasion, Stuart told me about the Rhodes Conjecture. This conjecture proposed an algorithm to compute the group radical of a finite monoid. I remember that I got the idea of my first crucial result on this subject in 1984, in the kitchen of Howard Straubing: I realized that computing the group radical of a finite monoid amounts to computing the topological closure of a regular language for the pro-group topology. This was the first of a long series of results and I wrote several papers about this subject. The first one was entitled *Topologies for the free monoid* and was published in the Journal of Algebra. This paper proposed a conjecture that one could compute the topological closure of a regular language by a simple algorithm, and discussed its consequences for the Rhodes Conjecture. It took over four years to be published and other papers about the same subject that were submitted later on were actually published much earlier. The next paper was entitled *A topological approach to a conjecture of Rhodes* and was published in the Bulletin of the Australian Mathematical Society. It gave a complete proof that the topological conjecture implies the strong form of the Rhodes Conjecture. Finally, Reutenauer and I reduced the topological conjecture to a conjecture on the free group: if $H_1, \ldots, H_n$ are finitely generated subgroups of the free group then the set $H_1H_2 \cdots H_n$ is closed in the profinite topology. This latter conjecture was proved by Ribes and Zalesskii in 1992. But Rhodes' Conjecture was also proved by Ash using different arguments, actually a few years earlier.

Later you got into the equational part of the varieties and you, along with Pascal Weil gave, for instance, an equational characterization of the Mal'cev products of two varieties of finite semigroups...

I have been working on the Mal'cev product since the time of my Ph.D. I was studying some variants of the concatenation product, such as the unambiguous product, and Pascal Weil brought the equational part to me. I remember the precise moment I discovered one of the key arguments of this paper. I have to confess it was during a talk by Denis Thérien at the NATO School at the University of York in 1993. I suddenly had the intuition that Imre Simon's Factorization Forest Theorem was the technical tool we needed, although I did not remember precisely its statement. Victoria Gould was kind enough to comply with my surprising urgent request to get a copy of Simon's paper and I could verify that my intuition was right. Simon's theorem is nowadays considered to be a major combinatorial tool in semigroup theory.



The $\mathcal{R}$-classes are the strongly connected components of the right Cayley graph.

Computation of an $\mathscr{R}$-class using the right Cayley graph of a semigroup.

Unlike Jorge Almeida, you never worked heavily on combinatorics of profinite words...

That is true. I am certainly less attracted by combinatorics on words than Schützenberger, Berstel, Perrin, etc. I always felt more comfortable with algebra. I use combinatorial results, such as the Factorization Forest Theorem, when I need them, but I generally prefer algebra to combinatorial arguments. It is just a matter of taste.

For a long time, the algebraic study of recognizable languages relied on Eilenberg's theory of varieties inspired by some results of the 60's characterizing classes of languages in terms of semigroups, such as those of Schützenberger and Simon. Now, some people working in this area, such as Straubing, Thérien and yourself, were also interested in classes of languages that are not necessarily varieties, although they can be studied algebraically. Do you think that the interesting remaining open problems in this area are only the very difficult ones, like the decidability of the group complexity or the decidability of the dot-depth hierarchy?

No, not at all, this is a flourishing area and there are plenty of interesting open problems, old and new. The recent paper *Duality and equational theory of regular languages*, by Mai Gehrke, Serge Grigorieff and myself, goes far beyond the classical context of the varieties. The classes of languages considered in this paper are more general than Eilenberg varieties and the theory developed in this paper also applies to infinite words, words over linear orders, tree languages, etc. By the way, exciting results on tree languages were recently obtained by Mikołaj Bojańczik, Zoltan Esik, Luc Segoufin, Howard Straubing, Igor Walukiewicz, Pascal Weil, etc. Concerning the main open questions in the area, some of them, such that the decidability of the dot-depth hierarchy, can be viewed not only from the perspective of the algebra, but also from the perspective of logic, and therefore they can be treated in the theory of finite models. The profinite approach also opens up fascinating perspectives on the classification of languages. I recently proposed to study the Wadge hierarchy associated with some profinite uniformly continuous functions and Pedro Silva and I just founded a non-commutative p-adic analysis. The connection with Fraïssé-Ehrenfeucht games is also on the way. There are also some very nice connections with duality theory, symbolic dynamics, combinatorial group theory or tropical geometry. There is now a continuum between topology, algebra, logic and automata theory. Top researchers of the new generation, like Bojańczik and Walukiewicz, who were both trained as logicians, are now convinced of the power of the algebraic approach. This makes me very optimistic for the future of this field.

When did you start being interested in infinite words?

My first paper on this topic dates back to 1984. A few years later, Dominique Perrin suggested me one day to write a book about infinite words with him, and without really thinking about it, I agreed. But it took us over fifteen years to complete this book!

You are now quite interested in the work that you, Mai Gehrke and Serge Grigorieff started together about languages and dualities, two subjects that seemed apparently far apart. How did that start?

At a conference in Nashville in 1996, I gave a talk about varieties and profinite topologies. Mai Gehrke, who was in the audience, mentioned she was interested in this

topic. We started discussing its connection with non-standard analysis but we didn't go very far at that time. We met again ten years later by pure chance. In November 2005, I was looking for a paper on the internet and I stumbled upon Mai's homepage, but the link to the paper I was looking for was broken. I wrote Mai an e-mail to warn her of the broken link and she answered she was curious to know why I was interested in her paper. In the course of the discussion that followed, I mentioned that I was looking for an expert in spectral topologies. I received an immediate and very enthusiastic answer from Mai, explaining this was one of her favorite topics. This is the way it started. In 2006, we were expecting an invited professor for a three month position at my research group but he had to decline, due to the late arrival of the official approval. I then asked Mai if she was interested in this position and she said yes. Thus in June 2006, Mai, Serge Grigorieff (a colleague of mine) and I started to work together. Mai had realized that the work I talked about in Nashville had a duality flavor, but she knew nothing about automata and Serge Grigorieff and I knew nothing about dualities. Thus our collaboration started by giving each other an introductory course on our favourite topics. But after a few weeks, we could understand each other and we started to make fast progress. It took us another year to publish our results in a short article that won a best paper award at ICALP 2008. We are now writing its complete version, and some other papers are on their way.

Are these results another way to see things?

Exactly that. The duality between regular languages and profinite words was known to Jorge Almeida for a long time. The novelty is the use of this duality to obtain an equational theory for any lattice of regular languages. This is particularly appealing for all the classes of regular languages defined by a fragment of logic closed under conjunctions and disjunctions, because this means that, in principle, an algebraic study is possible for these classes.

Among your many results do you have a favorite one?

My favourite result is probably the topological approach to the Rhodes Conjecture I mentioned earlier. But I also like the concept of ordered syntactic monoid that I introduced in 1995. It is a very simple idea, but it has far reaching consequences. I also like the result on dualities, but it is too early to measure its consequences.

Are you still interested in Černý's Conjecture?

Let me first recall this fascinating conjecture for the reader: *If an n-state automaton is synchronizing, there exists a synchronizing word of length* $\leq (n-1)^2$. I am still interested in the conjecture, but I am no longer working on it. However, I am still receiving requests to talk about this topic in seminars and workshops. The only thing I have done in recent years was an article with Stuart Margolis and Mikhail Volkov, published in 2004. In my opinion, there have been two major results in recent years. The first one, due to Jarkko Kari, gives a counter-example to the extension of the Černý Conjecture that I proposed in my thesis. This is an important result because it more or less kills any hope of interpreting the upper bound $(n-1)^2$ as the dimension of some vector space. The other major result is Volkov's result on automata preserving a chain of partial orders.

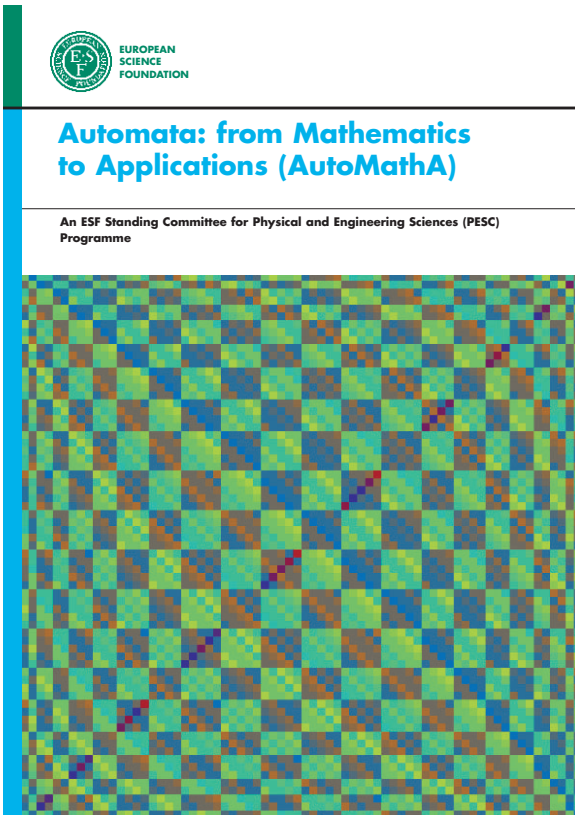
In your opinion, why is Cerný's Conjecture still a conjecture?

It is not unlikely that Cerný's Conjecture is false, but finding a counterexample might be difficult. One may have to work with large automata and testing whether a relatively small automaton is synchronizing may already exceed the capacity of a computer. The best known upper bound is still cubic and did not improve since 1982, although the conjecture has been proved in many particular cases. However, we are still very far from a solution and it might well be a very difficult combinatorial problem.

You worked at the Bull company for two years at the beginning of the 90's. How did it happen?

This story is similar to that of the book on infinite words that I wrote with Perrin. I was a member of the National Council of CNRS, and another member of this council was a engineer at the Bull Research Center. He was taking the opportunity of seeing many CVs of researchers, to hire a researcher from time to time for the Bull Research Center. One day, jokingly, I asked him when he would hire me, but to my surprise, I got a concrete job offer as an answer! This was the beginning of the story, because Bull was interested in someone with my profile and I got interested in the experience. At that time, the research department at Bull consisted mostly of young people, including Ph.D. students and numerous top-level researchers, but very few senior researchers. One part of my work was to play this role of scientific management. An important thing which I did was to initiate a seminar. At the beginning, people were skeptical and I was only allowed to set up a monthly seminar, but after three months, it had been sufficiently successful to become a weekly seminar. There was an external speaker every two weeks, and each other week we had a speaker from Bull. This turned out to be very important to promote the activities of the Bull Research Center to the academic world and to keep permanent contact with researchers from outside. One thing made me very happy once: a colleague of mine working at the Ministry told me after his visit that his opinion about the Bull Research Center had completely changed, from

a strongly negative to a very positive opinion. During my second year, Bull started to have serious financial problems and my colleagues found themselves in a delicate situation since the research center was shrinking day after day. But as far as I know, all of them managed to find a position elsewhere in companies or universities. And for some of them, their new connections with the academic world certainly helped. The most successful of them, Dominique Bolignano, founded his own company, Trusted Logic, with the initial support of Bull and INRIA. This company is now a world leader in embedded security solutions. Three months after I left Bull, I became the head of LITP[3], a joint research unit of the CNRS and the University, and my experience at Bull, which included some management courses, helped me a lot in this task.



Leaflet cover of the networking programme of the European Science Foundation AutoMathA chaired by J.-E. Pin (the picture is from Jorge Almeida and represents the action of Thue-Morse operator on the cyclic group of order 70 - see the feature article by J. Almeida in the CIM Bulletin n. 14 from June 2003).

Let us talk a little about your laboratory, LIAFA. What is the initial academic profile of the members of LIAFA?

Most of the French members come from the Grandes Écoles, mainly from the Écoles Normales Supérieures and a few from École Polytechnique or other engineer schools. Many members also come from foreign countries. Some have a degree in Mathematics, others, notably among the youngest members, have a degree in Computer Science, but also often a good background in Mathematics.

What is the reason for such a number of foreigners?

This is due to the scientific policy of the department, in particular mine when I was the head from 2003 to 2008. There are two kinds of positions. The selection process for the permanent research positions offered by the CNRS is a national competition. Our department is not involved in this process, except that the candidates have to express their wish to be appointed in LIAFA. High level research departments like LIAFA are very attractive to candidates so we have each year some good recruits. Foreigners are entitled to apply for a CNRS position and as a consequence, several of our full time researchers at LIAFA are non-natives. There are also university positions, such as Assistant Professor and Professor, and for these positions, especially for Professor, local people are usually discouraged. During the last twelve years, only one Assistant Professor from LIAFA was promoted Professor in the department. But many became professors elsewhere and thus there is a high turnover in the department.

You have been a member of several scientific evaluation or advisory committees in France, and also in other countries, such as Portugal. In your opinion, are there bad relationships between people from theoretical computer science and people from applied computer science?

I don't have this feeling, at least at the level of people. It is true however, that nowadays there is a strong tendency, in France and elsewhere, to favor short-term research. Killing research with no immediate application will have disastrous consequences in the long-term, and perhaps sooner. The other strong tendency is to focus on fashionable keywords. For instance, the forthcoming French-Spanish collaboration programme PICASSO selected the following topics: biomedicine, biotechnology, energy and climate change, nanotechnologies. For the French-Portuguese Pessoa programme, it is announced that "a thematization will be committed as from 2011 in order to reinforce the structuring character of the cooperation". "Structuring character" is one of these fancy keywords which, like "synergy", are mandatory in any cooperation application...

Do you have the same opinion about the European Commission?

The European Commission has kept some important support for fundamental research. For instance, the

[3]Laboratoire d'Informatique Théorique et Programmation, later called Laboratoire d'Informatique Algorithmique: Fondements et Applications (LIAFA).

Future and Emerging Technologies Open Scheme is a flexible tool for exploratory research, where one can submit proposals with a component of a fundamental nature. The European Research Council (ERC) offers advanced grants for both young and senior researchers. These grants are very selective, which is not a bad thing. The drawback is that writing applications is time-demanding.

Let us talk now about your connections with Portugal. When did they start?

I do not remember exactly when they started. It certainly begun with some early correspondence between Jorge Almeida and me, but I am not sure about the year. Later, I met Gracinda Gomes in a conference in Szeged, Hungary, in 1987. The cooperation between Jorge's and Gracinda's groups and my own group developed over the years. Three portuguese students did their Ph.D. in Paris and I participated in the juries of several other portuguese students. We collaborated in many scientific projects, either between France and Portugal or at the European level. I was also a member of the scientific committee of several semigroup conferences organized in Portugal and I was co-organizer, together with Gracinda Gomes and Pedro Silva, of the Thematic Term "Semigroups, Algorithms, Automata and Languages" held in Coimbra in May, June and July 2001. Finally, I am the chair of the programme "Automata: from Mathematics to Applications" (AutoMathA), a very successful research networking programme of the European Science Foundation gathering 15 European countries, including France and Portugal. One of the highlights of this programme has been a two week school organized in 2008 by Gracinda Gomes in Lisbon. The thematic term and the 2008 school were both completely successful and it is my opinion that training young students in high level research also has its "structuring character".

In France there are several research centers in theoretical computer science, some of them quite big, LIAFA being a very good example. In Portugal there are none at that level. In your opinion, does Portugal need one?

I would not say that. A few emblematic people, like Jorge Almeida and Pedro Silva in Porto for instance, or Gracinda Gomes in Lisbon, suffice to create a solid research group. Two or three dynamic people are enough to set up a research lab, as long as they get sufficient support to attract good researchers and Ph.D. students. Of course, money is needed to run such a center, since it is essential to send people to international conferences and to collaborate with other universities, both in Portugal and abroad, to invite researchers to give lectures in seminars, to hire researchers, to maintain a library and computer facilities, etc. Large research groups certainly have a broader international dimension, but the advantages over a strong small group are limited. There is now a tendency to create larger centers to improve the ranking of universities in various evaluations and comparing systems, such as the Academic Ranking of World Universities. But one should be very pragmatic about the policy of research centers. I know of some small, very active research groups. As long as their activity is excellent, why should one change their structure? Quarrels between different groups are a danger for large centers, increase of bureaucracy is another one. If these two hazards can be avoided, if each group has enough resources to avoid fighting, I have no objection to large centers, but the most important thing is research activity.

Jean-Éric Pin with Gracinda Gomes at Centro de Álgebra da Universidade de Lisboa in June 2009.

You are a very active person. You can manage a lot of things at the same time. For instance, you do research very actively, you give many talks abroad, you supervise Ph.D. students, you are a member of several committees and boards of examiners, you are editor of four scientific journals, you were the director of your laboratory from 1994 to 1997 and from 2003 to 2008, etc. How can you do all these?

I do not think I have been doing so many things at the same time and plenty of people I know are much busier than I am. I have been primarily a full time researcher, apart from a 14 year period when I taught part-time

at École Polytechnique. Concerning the management of research units, LITP first and LIAFA later, the key word is *delegate*. Each time you can entrust a task or a responsibility to another person, do it. For the main decisions, it is a good idea to listen to people and to look for their advice and support. This can be time consuming, but it is really worth for important decisions. This way, I was able to reach a common agreement most of the time. Further, several decisions were taken by specialized committees, notably for hiring people. But then again I tried to reach a common agreement by using only fair arguments. Once the decision was taken, I also used to explain it to the members of the unit. On a more personal level, I am a well organized person regarding computer files and e-mail. Further, unlike some colleagues of mine, I did not hesitate to decline some invitations (programme committees, cocktails, etc.).

Jean-Éric Pin is Directeur de Recherches at the CNRS in the Laboratoire d'Informatique Algorithmique: Fondements et Applications (LIAFA), of CNRS and University Paris 7, and a member of the Automata and Applications team. He works on theoretical computer science and he is well known for his wide contribution to the area, including many breakthroughs and original ideas. Born in 1953, Jean-Éric Pin got his university degree in Mathematics from École Normale Supérieure de Cachan and his Ph.D. degree in Mathematics from University Paris 6 in 1981. He has always worked on theoretical computer science, with a particular emphasis on its connections with algebra. He has been a researcher at CNRS since 1979. He has also been a professor at École Polytechnique from 1992 to 2005 and a research engineer at the Bull Corporate Research Center from 1991 to 1993. His research is mainly devoted to the algebraic theory of finite automata via the study of finite semigroups, in particular varieties of finite semigroups. He has always looked for relationships between different areas of Mathematics and his work also includes papers about logic, topology or combinatorics. Jean-Éric Pin is the author of over 130 scientific publications, including two undergraduate books, two research books and 14 book chapters. Twenty-two students have received their Ph.D. under his supervision. He earned the IBM France Scientific Prize in Computer Science in 1989 and a Best Paper Award at the conference ICALP 2008. He is a member of the editorial board of four scientific journals and he has a wide experience as a member of programme committees as well as management positions. To give some examples, he has been the head of the Laboratoire d'Informatique Théorique et de Programmation (LITP, from which LIAFA originated), of the Laboratoire d'Informatique Algorithmique: Fondements et Applications (LIAFA) and scientific director of the European Research Consortium in Informatics and Mathematics (ERCIM). He is chairman of the European Science Foundation (ESF) programme AutoMathA (2005-2010). This interview with Professor Pin is intended to give an overview of his life as a researcher with its many facets.

Interview by Mário Branco (University of Lisbon)

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1 - ERCOM is an European Mathematical Society (EMS) committee consisting of Scientific Directors of European Research Centres in the Mathematical Sciences, that aims to contribute to the unity of Mathematics, from fundamentals to applications. The 2009 annual meeting took place March 13 and 14, 2009 at the Institut Mittag-Leffler, Djursholm, Sweden, and the next one will take place March 12 and 13, 2010 at the International Centre for Mathematical Sciences, Edinburgh, UK. The full list of ERCOM centres can be found at http://www.ercom.org.

2 - The purposes of ERCOM are:

- to constitute a forum for communications and exchange of information between the centres themselves and EMS;
- to foster collaboration and coordination between the centres themselves and EMS;
- to foster advanced research training on a European level;
- to advise the Executive Committee of the EMS on matters relating to activities of the centres;
- to contribute to the visibility of the EMS;
- to cultivate contacts with similar research centres within and outside Europe.