An Interview with James Yorke

This is the mathematical part of a conversation with James Yorke, that took place at the University of Aveiro on the 21st of July 2006, the first day of the Conference "Views on ODEs" — in honor of Arrigo Cellina and James A. Yorke. It ranged from his recent research interests to opinions on how to teach mathematics and how to write a paper. It was fun playing Watson to his Sherlock — I hope you enjoy the conversation as much as I did![1]

I'll start where Cellina was calling you "Maestro". You seem to like to work with other people: I've stopped counting at 140 collaborators. Nowadays it is more common for mathematicians to do collaborative work than when you started.

One time, I found — around 1970 — I was writing a paper that I really liked. But for months it was sitting around and all I had to do was finish off some references, and I couldn't force myself to do it. So I find that it is simply not productive to try and write something by myself. I think it is the interaction that drives it. People add their ideas and I add my ideas and it becomes something better. So I have stopped trying to collaborate with myself.

Well, other people give you pressure and feedback.

That's right, but you get it all, you get different directions, different viewpoints. I got into chaos by switching areas.

You started with Lyapunov functions and control, and ordinary differential equations and things like that.

Differential delay equations... I did a lot of switching even in graduate school. And I feel that one should continue to switch, putting different ideas together.

My most cited paper is with Ed Ott and Celso Grebogi, on controlling chaos. The ideas we describe in the paper are quite simple, but it had a big impact on physicists. There is one reason, namely, that physics ought to be about observing, not disturbing, and control theory is not part of the literature of physics, even elementary linear control theory. So, we got physicists interested in control theory, and they found they liked to control things and there has been way over a thousand papers referring to our paper on controlling chaos.

Well, it also had chaos as an ingredient, right?

Yes, but people had known about chaos for quite a while. One of the things we've tended to do is to try to describe situations by inventing concepts that are appropriate for the physicists. Very often the mathematicians will say there is nothing interesting here, because they do not see what interests the physicists, because they are not physicists. Sometimes the mathematicians think my work is stupid, although they're more happy with the work that is more aimed at mathematicians.

James Yorke

The next question was on the interplay between theory and application which is also something you seem to have worked a lot on.

I'm having problems with the concept — what is an application?

Mathematics and other things?

One of the most applied topics I work on is what I call the billion dollar logic puzzle. This is about genomes: how do you figure out what the DNA is in a chimpanzee or a rat or whatever? I got interested in this 10 years ago just reading the newspaper. And I got some of my collaborators interested, sometimes I can't get them interested and then nothing happens. The idea was, could we find better methods than people already had? It is a question of taking little bits of DNA, and you figure out what the little fragments are, and then you want to figure out which ones overlap which ones, and you have no idea where in the genome it came from. You have these millions of fragments several hundred or a thousand letters long and you try to put them together. And there are errors, and all kinds of problems, but these are not biological problems, you don't have to know anything about the biology.

[1] Isabel S. Labouriau (University of Porto).

That is where the mathematician starts, look at the problem and take out the context.

But this is the way they do it.

I call it a billion dollar problem because that is how much the National Institute of Health (NIH) is spending over a period of about ten years on making these little fragments for lots of different species. I'm not including the human genome, that's a separate project, another billion dollars.

And what sort of maths is involved?

Just trying to be smarter than the next person.

(laughs) Well, that's maths in general.

Well, yes... Little bits of probability theory, and just trying to understand the problem. I don't usually think about the kind of mathematics when I get into a problem, I just try to get into the problem.

I'm not asking what you think beforehand, I'm asking what you have been doing.

OK, I have to say this defensively, you see.

First of all, a lot of the people who have been involved in this problem are extremely smart people. So the problem of trying to outthink these very smart people, is somewhat difficult. That's why I call it a logic puzzle, rather than a math puzzle, you want to find algorithms which work. And there is a little bit of probability theory, and just trying to understand the problem. It is very hard to think about it in simple terms. Sometimes you come up with a simple idea, after working for months.

So it is a problem you have to work with almost no tools.

Right. A lot of computer programming to implement these things, but the ideas are pretty simple.

That sounds very hard.

Yes, it is very hard. Another aspect about this is that there are several centers that create these fragments for the NIH and they are basically paid to create the fragments and by the way put them together into a genome. Nobody checks how good a job they do. If someone else puts it together in a different way, the original center gets to pick the best answer, which by chance, almost always seems to be their own. You see, other people don't want to generate answers, because the answer will be ignored. The data is on the web.

Now, some of these guys are very much afraid of using other people's ideas. This is not true of everybody in the field. One group has a particularly weak set of tools, and so (this is very non scientific) they feel that if they use other people's tools it will make their old stuff look bad.

We are trying to talk to all of the labs, and with some we are having considerable progress. We have more success with the better ones.

Not surprising is it? Maybe that's why they're better.

Right. They don't have to fear looking bad. But this is one project that we've been working on and it's a very big effort.

So that is really switching areas, because that's - no tools.

No tools, right.

Which is really hard to face. A lot harder than, say, a slightly different form of dynamical systems.

One thing that you'll find is that mathematicians tend to emphasize how difficult the problem is, not how big the impact is. And this is counterproductive, it allows people to get deeper and deeper into little problems, which remain difficult but it is hard for them to have much impact.

For me the concept of compactness, for example, at least today it is a very simple idea, and I would love to create an idea like compactness, you see? But for a Fields medals what you need to do is have extremely difficult stuff with very little impact.

Maybe that is a way maths is different from physics.

I disagree. It is the way mathematicians are different from physicists.

And it is not all mathematicians or all physicists. It is not healthy to not worry about impact. Mathematicians will say, "well, maybe it will have impact and maybe it won't, but how can I predict?" Well, life is about predicting the results of your efforts.

Like, I can get food faster.

Or I try to pick up a girl or whatever, you see? One cannot abdicate the responsibility of worrying about the impact of one's efforts. One talks about archival journals, which are journals where you write results up and then they get stuck on shelves, so they'll be there forevermore. This is a terrible concept.

(laughs) On the other hand it is nice to have 2000 year old results that we know about.

Absolutely. Now that's a way mathematics differs from physics, the results that we prove are, hopefully, true forever. I lecture my class in advanced calculus and the Riemann integral about Archimedes, and how he calculated the value of π by upper and lower bounds and moving the lower bounds up and the upper bounds down, and it's converging down to a number, and this is exactly what you do when you define an integral. It's wonderful stuff...

...done more than two thousand years ago.

twenty five hundred...

Let's go back to collaborators. Any one you would like to talk specially about?

I gave a thank you speech for the Japan prize where I listed collaborators who I worked a lot with. These people are absolutely crucial to the whole operation, they contributed a lot of the ideas, I contributed some of the ideas and it all hangs together. The person I have written the most papers with is Edward Ott. He is a physicist and electrical engineer. My view is that if I wanted to talk about chaos, I should talk to non-mathematicians. The reason being that very few mathematicians really have to know about dynamical systems. Everybody else has to understand how things change in time, so there is a huge possible audience out there.

However, I have found that it is very difficult to communicate with an audience who speaks a little bit differently from what you do. You don't realize how different the language is. With Ed Ott who was a very creative guy and a physicist, we're able to come up with ideas that could be expressed in a way that physicists would understand. I collaborated also with Celso Grebogi, but he was more the junior partner in many cases, but he is a very bright guy too.

We were the first people to talk about fractal basin boundaries, aside from some classical cases of complex variables which are very non-physical in nature. We're the first people to do so, but the problem was, how do you write a paper about this, that many mathematicians understand. If you could take someone like John Guckenheimer and asked him with five minutes notice to give a twenty minute talk on this, he could do it, but in a language which has no impact, sometimes. And John Guckenheimer is an excellent expositor and a very bright guy, and he's from the Smale school.

The point is, you have to reformulate these ideas in such a way that you can communicate them. Give them something that they could measure. The mathematician will not understand the right importance of measuring anything, but that's what physics is about.

So that became the problem, how do you communicate these ideas?

Having a partner that's a physicist and electrical engineer solves the language problem at the home level.

Right. And he has done tremendous things by himself. He came to the University of Maryland from Cornell, where he was a full professor and by chance his office was down the hall from my office. We started talking, and we found we had interests in common. That's the way things sometimes work.

Another example of the same thing: the University of Maryland hired a new head of meteorology. A woman named Eugenia Kalnay. There was a reception for her at the president's house and we started talking. It turned out that a lot of the ideas that she was using in meteorology were like the ideas we were using in dynamical systems, so this seemed like an excellent opportunity to collaborate. I suggested that we apply for a Keck foundation grant. The Keck foundation does not require that the person have an excellent track record in that particular question. Otherwise we would never have been funded. We started a group and brought in our other collaborators and they all contributed a lot of ideas on weather prediction.

The basic question becomes how do you determine the initial conditions. All prediction is extrapolation from the present. So they have methods for determining what the weather is today at noon all over the earth, but we've found that we could come up with different methods, and we did, and they are being tested and that's an ongoing project. The other guys were contributing all of the ideas, so I dropped out, but nonetheless, this is how it has got started. Eugenia Kalnay, by the way, was for several years head of the National Weather Service's group that comes up with new methods, she wasn't at that time, she was then a faculty member of the University of Maryland. She was a member of the National Academy of Engineering, she is a well established and well known person. We have all had a great time. It turned out that other people had similar ideas, so it wasn't totally new, but it was new enough that it was quite worth while.

I would add that, when you get theorems that are extremely difficult, it is very hard to apply those results. It is much easier to apply simple ideas. So I try to tell people, if they write a paper, which is a complicated paper, figure out what is the key, the simple kernel, and display this. Give it a name, and I don't mean "the Yorke method".

(laughs)

Name it after what it does, so people can focus on it and use it. Sometimes these wonderful ideas get buried, they're more proud of the complicated structure, and this is the wrong idea, you should find this kernel, this idea, their compactness idea, you see...

The hardest part of mathematics is throwing away all the technicality that was so much hard work to do.

Speaking of throwing things away, you see, I have dealt with students who deal with complicated questions and discover complicated mathematical algorithms. We have been discussing fluid flow in a pipe, using other people's equations, and so you do a tremendous amount of mathematical analysis to describe what is happening. I try to tell them that what people are really interested in are the results, not how you got them. And all you want to talk about, if you're a student, is how you got them.

That is what you work hard for.

You want to throw away 90%. If people like your results then they might be interested in how you got them, but not before. I find this is a recurrent problem with the students, that they are unwilling to get rid of all the hard work and say "here is why you should pay attention".

Students — that's another interesting issue. You've had a lot.

A bunch, over thirty. We have created an approach at Maryland, of group advising. A person will work with different people, sometimes on different but related topics. The emphasis tends to be on writing a dissertation. As many applied mathematicians do, we prefer to emphasize writing papers.

Because the coin of the realm is writing papers, and someone who writes a dissertation is learning how to write, generally speaking, unpublishable material. So we say, the student should write something like three papers, and should staple these together with as few changes as possible, and call it dissertation. At Maryland we have finally set up a rule that says that when a student graduates, he or she should have at least one paper submitted. Now that's pretty easy, since you can submit total trash. Nonetheless it is a goal, you see, that the student should know about.

The rule is there to focus the attention, I imagine.

Right, and it is a nonenforceable thing, in the sense that you can just submit the phonebook.

The difficult part is getting it accepted.

But it is a goal. People do not get tenure for writing a good unpublishable exposition.

Writing a dissertation, a student often is not focused. A student is usually unable to write more than ten or twenty decent pages, so the idea of a hundred and fifty page dissertation for someone who cannot write decent text is a somewhat contradictory concept. I feel strongly about this, that a big failing of many advisors is this hundred and fifty page dissertation. The student is released upon the world, with nobody to teach him, or her, how to write papers. And so maybe they get a post-doc, well, the people giving you a post-doc don't want to hire someone so they can write up their dissertation, or if they are willing to do that it is very questionable.

What is the point... since they already have done it.

I'll give you some more opinions. In terms of education, when I went to graduate school I had quite a few good professors, excellent educators who were also researchers and so on.

But what I remember most and learned best, was material I read on my own. That's the way I remember it. Stuff that I read on my own, particularly over summers, on homotopy theory and cohomology theory. If I took courses on these things, I wouldn't understand them. I took one course, on one theorem. At the end of the course, I couldn't tell you what the theorem said. I still can't. The Riemann-Roch theorem. The professor wanted me to publish a paper with a new proof I had come up with. So I was doing something, but, somehow I've just never clicked as to what this theorem said, you see?

(laughs) That's a different level of not understanding.

I was betting that there would be no final exam, and I won the bet. Nonetheless, I did not say I had no understanding but nonetheless I didn't know what the theorem said, I mean, I couldn't state it.

Now it may be harder to learn things from books but the idea is, you go at your own pace, if you don't understand a paragraph you read it a couple of times. In class, it gets read once, and the professor goes on, often without a good book to follow. There are countries, I've been told for example that in Germany in physics, people are not supposed to follow a book because following a book would not show they have the expertise to jump from book to book. Thereby leaving the student stranded, I believe. So, whenever possible, courses should be taught with good books, that are readable. And in high school, I have been told, they try not to teach you material, but try to teach how to learn. Do we tell our students we're teaching them how to learn?

No, we don't, because if you wanted to learn something about a fast Fourier transform, the model would be to find a course that meets three times a week, and is lecturing on this material, which is of course impossible. Well, you're not gonna find this. What you need to do is to go and find a book and sit down and read the book.

And, yes, some students do read a fair amount, but this is much less the education than it should be. They should be told they are going to learn things by sitting down and reading. And reading a book in mathematics is a separate skill from reading books about history, or novels. So I try to force them to learn how to read math books, and with some I'm successful, with some I fail. But we should be teaching them how to learn. As far as I can tell, it means they should be able to go out and find materials on their own and read them and understand them.

I tell them that we run a factory at the University of Maryland and at other universities, and the basic unit of work is professors taking courses and students taking courses, but this is not the natural way to learn things. I particularly dislike professors lecturing without questions, writing stuff on the board and students copying it down, because in this double translation from the book there are many errors, and the professor is speaking and writing and the student can only write down what the professor wrote down, not what the professor said, and so you have a very low quality version of the material. Twice translated, unintelligible, I think... And that's what they are left with. Now when someone writes a book, they spend many hours on the equivalent of one lecture. And when I lecture, I cannot spend many hours for each lecture.

So, the book is better. If I write a book and I give a lecture, my book is much better than my lecture.

The lecture is for a different purpose... the problem is trying to use the lecture for the purpose of the book.

I tell the students to call me coach. I sign my e-mails to them "coach Yorke". What is the difference between a professor and a coach? I say, actually I don't know the answer. I know a coach is supposed to help you excel. I don't know what a professor does. He gets up there and writes down what is in a book that the students haven't found, and the students copy it down. Well, perhaps the professor puts in more material, or something. He puts in what the book left out, the book should have put that material in. The books should have the motivation. And sometimes they do, and sometimes they don't. So I keep telling them to call me coach. Some of them do, some will write "Professor Yorke".

I tell them to bring the books to class, I point out materials in the book, so they're familiar with the books, I try to encourage them to read it, I emphasize the high points in the books, the easy points they can read... often failing at getting them to read the books.

I tend to teach more advanced undergraduates. This is not the problem of the professor who is teaching Calculus. I'm talking of advanced maths students, math majors, rigorous courses, or semi-rigorous courses. I teach from my chaos book, I say, I wrote the book, I'm not gonna read it to you. Of course I wrote it with my collaborators.

I am a person with many opinions, you see...

When I was in the seventh grade, they've split the class into two parts those who could do mathematics and those who couldn't, half and half. So they felt that since I wasn't very good at long division because I would make errors, that I wasn't very good at mathematics. And so I got the subjects with people who took Latin, and I certainly was not good at Latin.

Along the way I had to catch up with the other students, who took Calculus but, over the summer before going to college I had to read, in order to get into an advanced course, I had to read Halmos' "*Finite dimensional vector spaces*", where I learned how to read maths books. Based on this reading before I went to college, I never took an actual linear algebra course, because I thought I had learned this, eventually I learned it better and better.

James Yorke presenting his talk ("Views on ODEs", July 2006)

It doesn't sound like a bad starting point, Halmos' book.

No. But I think it is all about reading books, or papers or whatever. But to get basic education you read books, not papers. And there is a lot of basic education.

I talk to our seniors, who are taking Riemann integration, rigorous proofs, and I ask them, how many know about the theory of Fourier series? Anything, what a Fourier series is? And perhaps one in thirty will know something about this. There are so many topics in mathematics that they have no concepts of. Simply because the day is limited, and they have to learn all kinds of materials.

Our graduate students at the University of Maryland, don't have to take any Analysis or Probability theory and they can get a PhD and while they probably need to know some probability to get into graduate school, they will know less when they graduate than when they came in.

There are two ways of thinking about mathematics, one is the axiomatics, and so the traditional way courses are Algebra, Topology and Analysis. There's another way to look at it, which are the applications of mathematics and these are by and large, Differential Equations, Numerical Methods, Probability and Statistics. This is how mathematics interfaces with the world.

Then you have to do the interface, which is hard work. Really hard work.

But you get to talk to people. About their problems, if you're gonna interface and use these ideas to interface.

I do tell students about wrong ideas in lectures, on purpose. A really fantastic book would mention the wrong answer, but nobody can spend too much space and time in a book for that. So that's something that can be done in a lecture that a book doesn't do.

But I think they can. I think whatever you can say in a class they can put in a book.

I acted as a, shall we say, editor, for a colleague who was revising his advanced calculus book. He felt there were many things that professors should do and therefore should be left out of the book. But my greatest ally was the person he had take advanced calculus from, who did a totally terrible job. And I say, think of students in a class, what do you want them to know. I go into class on the first day and I say, let's talk about teacher evaluation, what have people said in the past about my chaos course. First thing they say is "what about organisation?", and the students say "there is none". And the reason there is none is I go into class and I ask students what questions they have about the book. Because the book is prominent. How do I force them to read the book? So we have a discussion about what they're finding difficult in the book. And the easy parts, they can all read. This is the approach, you see. When they ask a narrow question, you respond with a broad discussion which covers a certain amount of material, in the book.

So we ended up talking about teaching. Well, it is a big part of our life.

It is a big part of our life, and there are not many ways in which you can talk about teaching in the mathematics media.

As Cellina was saying, passing it on to the next generation.

In America there's *American Mathematical Monthly*, and they talk about topics which supposedly can be understood more or less by undergraduates, advanced undergraduates. But they tend not to talk about actually teaching. In Portugal the number of students taking classes in math or math majors is decreasing, what can you find that is written about peoples' opinions on this topic? Nothing, probably it is like the United States.

Here it has a very specific reason.

What does it mean?

Here the maths major is the same course as the secondary school teacher training course. So we had a peak some ten years ago because there were lots of positions, now there are no positions, we have a drop. So it is a sort of job related issue.

If you ask your students who have just graduated from studying math, why someone should take mathematics, what is it good for? Clearly one answer is to teach high school. What other answers would they give? This is a very important question. And I think they basically can't give any good answer. Because they have not got into good material. Ask them who has learned any mathematics done in the twentieth century, specially the last half.

In the teacher training course, the only twentieth century part is probability.

I don't know what probability theory is in the twentieth century. You've got Lebesgue integration. Aside from Lebesgue integration.

Well, Kolmogorov axiomatics, that's definitely twentieth century.

Ok. But that's an example which they feel can be used.

Probability in itself, not really, in a student vision. Statistics is used a lot.

We use probability theory all over the place, genomics... Is there one course that discusses a lot of ideas that have come in twentieth century mathematics? No. At least not in the United States.

Everybody went downstairs, I think we should switch this off and leave you to have dinner.

Interview conducted by Isabel S. Labouriau (University of Porto)

James A. Yorke (born August 3, 1941) is a Distinguished University Professor of Mathematics and Physics at the University of Maryland, College Park, and a recipient of the 2003 Japan Prize for his work in chaotic systems.

Born in Plainfield, New Jersey (USA), Professor Yorke earned his bachelor degree from Columbia University in 1963, and came to the University of Maryland for graduate studies, in part because of interdisciplinary opportunities offered by the faculty of IPST (an Institute established in 1950 and committed to interdisciplinary research in the sciences). After receiving his doctoral degree in 1966 in Mathematics, Yorke stayed at the University as a member of IPST. He is perhaps best known to the general public for coining the mathematical term "chaos" with T.Y. Li in a 1975 paper entitled "*Period Three Implies Chaos*".

Professor Yorke has coauthored three books on chaos and a monograph on gonorrhea epidemiology, has supervised 40 Ph.D. dissertations in the Departments of Mathematics and Physics, and has published more than 300 papers. Professor Yorke's current research projects range from chaos theory, weather prediction and genome research to the population dynamics of the HIV/AIDS epidemic.
