of $\mathcal{R}_{n(g-1)}(\pi_1\Sigma, \mathrm{Sp}(2n,\mathbb{R}))$ in Theorems 7.1 and 7.2. For more information, in particular on the rather delicate issue of the exact count of the connected components, we refer to [15,10].

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An Interview with Robert MacKay

by **João Lopes Dias** [CEMAPRE and ISEG, Universidade Técnica de Lisboa]

Robert MacKay is currently Professor of Mathematics, Director of Mathematical Interdisciplinary Research and Director of the Centre for Complexity Science at the University of Warwick, United Kingdom. After completing his undergraduate studies at the University of Cambridge, he did his Ph.D. at the University of Princeton, United States. Since then he has held positions at several prestigious universities and research centres, including a professorship at the University of Cambridge. His research interests include Dynamical Systems and its applications to Physics, Engineering, Chemistry, Biology and Economics. He is a Fellow of the Royal Society of London, of the Institute of Physics and of the Institute of Mathematics and its Applications.

Professor MacKay visited Lisbon in June and participated at the Jornadas LxDS-CIM-SPM, at the Colóquio de Matemática DM-ISEG, and at the Doctoral Program in Complexity Sciences ISCTE-IUL/FCUL.

When did you realize your interest for mathematics?

My mother remembers worrying that I was late home from primary school one day aged 4 or 5 and she found me in the garden counting crocuses! I used to do a lot of calculating.

You did Part III Maths at Cambridge, the oldest and most famous mathematics examination in the world. What did you think about the Cambridge experience, in particular your mathematical education?

I received a good education in Cambridge. Mathematics in Cambridge includes a lot of physics, which suited me well, as I'd originally intended to study Physics. So I learnt a lot about mechanics, waves, electromagnetism, fluid dynamics, and quantum mechanics, as well as an initiation into analysis, linear algebra, groups, probability, ODEs, PDEs, numerical analysis and so on.

Was there anyone who you recall as being a major influence on your future choices and views?

I was particularly influenced by James Lighthill and Michael McIntyre, notably on the theory of waves, and wrote essays on solitons and on waves in stratified atmospheres. My director of studies John Hinch pointed me in good directions, like to read Hirsch and Smale on dynamical systems. During my final year Nigel Weiss and Mike Proctor welcomed me into their Astrophysics research seminars, which I appreciated as an opportunity to see what research is like.

After Cambridge you went to Princeton for a PhD. Why did you choose the Plasma Physics Lab?

I wanted to work on a problem of potential social value that was nevertheless mathematically challenging. So I chose plasma physics, with a view to realising controlled nuclear fusion energy. I wanted also to see something different from Cambridge: wonderful as it had been I was sure the world had other good things to offer. Nigel Weiss and Mike Proctor recommended I should go to Princeton Plasma Physics Laboratory.

Who influenced you most at Princeton?

The main influences on me at Princeton were my PhD supervisor John Greene who gave me good problems to work on; John Mather whose course I attended for two and a half years non-stop; and fellow students like Rafael de la Llave with whom we met regularly to go through papers and books and to bring talks and conferences to each other's notice.

How did you develop an interest for dynamical systems?

My father copied Robert May's 1976 Nature paper on chaos in population dynamics for me while I was an undergraduate, my director of studies recommended Hirsch and Smale's book as summer reading, and Alistair Mees offered a Pt III project on "Period three implies chaos". All these struck me as fun but not serious enough mathematics, so I did a Pt III project on wave propagation in inhomogeneous atmospheres instead. But in Princeton the plasma physics programme included an introduction to dynamical systems theory and I got together with a bunch of students mainly from the Physics department to read papers and books and educate each other on the topic. We started going to John Mather's course, who treated various topics in dynamical systems theory, culminating in what is now called Aubry-Mather theory. We made day trips to a conference on Nonlinear Dynamics in New York in 1979 and I think that is when I decided nonlinear dynamics was what I wanted to do. When John Greene gave a seminar three months later about his 1979 J Math Phys paper I asked if he could suggest anything similar to do and he put me on to numerical investigation of period doubling in area-preserving maps and I was hooked.

You are very much interested in the interactions between dynamical systems theory and concrete problems arising in several different areas of knowledge. How do you manage to talk to people outside mathematics?

It takes a lot of time to understand differences in use of language, the unstated assumptions and world-view, and the often huge literature, and then to formulate worthwhile mathematical versions of their problems. I do not feel particularly good at it.

Is it too hard for a mathematician to read their literature?

For some topics there are good reviews or collections of papers setting out the subject. That is the easiest way in. There are also some good books, but they tend to be too much one author's view or to miss the state of the art.

Do you share the view that there is not a clear distinction between pure and applied mathematics, just good or bad mathematics?

The usage of the terminology is unhelpful. What is called "applied mathematics" is often not applied to anything, and some "pure mathematics" is applied to many areas. The distinction is sometimes more between attention to rigour which for the purposes of applications can limit one's analysis so much that the result is irrelevant for the original problem versus making approximations and plausible assumptions in order to get at least some form of relevant answer. Both approaches have their place and indeed a good analysis of a problem may involve moving between the two extremes in an iterative process that builds an answer that is both rigorous and relevant. The important thing is to be clear about what one is claiming. The other distinction is one of motivation: is your mathematical work driven by scientific problems or pure mathematical curiosity? Again there is a place for both.

What different cultures do you find within mathematics?

Apart from the pure v. applied culture difference, there is the algebra v. geometry difference. Some prefer symbol manipulation, others pictures. I'm more on the geometry side but I like explicit formulae when they are available.

Some of your research has been strongly motivated by scientific problems from physics, biology and social sciences. Do you find any fundamental relation between problems in those areas?

I tend to think laterally, which can be fruitful though I recognise that it is also limited, as it won't provide major paradigm shifts. Thus, for example, I hit on the idea in 1994 that the way the cochlea frequency-analyses sound may be mode-conversion rather than critical layer absorption. Mode conversion seemed to be unknown to the physiologists and the fluid mechanics working in this area, though in retrospect it is what Andrew Huxley was proposing in 1969; but I knew about it from my training in plasma physics. I think it is the right explanation, though have not persuaded a suitable journal to publish my paper yet. I'm currently in the process, with colleague Nick Chater in the Business School, of trying to formulate a thermodynamics of economics, aided by the abstract framework of Lieb and Yngvason, but there is a long history of such attempts and it may be a mistake to force economics into a physics mould.

Could you describe your work trajectory, from renormalisation of area-preserving maps to complexity science and emergence phenomena? How did your choice of problems evolve from the previous ones?

I went to Princeton to do research in plasma physics, but found that basic problems like the magnetic field line flow in a tokamak were not understood, except in the axisymmetric case, for which there is a foliation by invariant tori. To study the question of invariant tori for non-symmetric perturbations, I considered area-preserving maps. Following numerical observations of Kadanoff and Shenker I formulated a renormalisation theory for the breakup of invariant circles and verified it numerically. It was subsequently proved with computer-assisted estimates by Koch, following a direction that I proposed in 1994 going back to the continuous-time problem. Anyway, that led me into understanding the transport through the gaps of broken tori, where I interpreted Mather's action difference as a flux across a cantorus. I also developed a sufficient condition for non-existence of invariant tori that is easy to implement and with enough work is exhaustive. At IHES, Charles Tresser invited me to join a project on the boundary of chaos for circle maps, in which we proved that the boundary of complicated dynamics is itself complicated. At Warwick I pursued a number of further themes in Hamiltonian and non-Hamiltonian dynamics. One was stimulated by numerics presented at a conference by Philip Saffman on stability of periodic water waves, where using Hamiltonian theory I was able to explain his results; I followed that theme to also explain the diagram for instability of Karman's vortex street. Another came as a by-product from a visit of Philip Boyland in which he introduced me to the topological behaviour of dynamics on surfaces: this led me to some nice results for example about the rotation set and toroidal chaos for homeomorphisms of the torus. Another was stimulated by breakfast with Serge Aubry at a workshop in Minnesota, when he explained his anti-integrable limit to me and I realised I could use the idea to prove all sorts of results about area-preserving maps, and also improve his results for some quantum-mechanical models of solid-state physics. Also at this time I realised I could extend my renormalisation theory for area-preserving maps to the statistical mechanics of some classical models of solid-state physics. Perhaps my first attempts to tackle complex systems came when I took over from David Rand management of a grant with Dave Broomhead on Dynamics of large-scale networks. We didn't really achieve much on the subject, but it laid the seeds. Instead we developed an approach to extract topological information from time series; this was a precursor of what is now the very popular domain of computational topology. An important event was on a visit to Aubry he returned from a conference very excited about discrete breathers: spatially localised time-periodic solutions of networks of oscillators that physicists saw in numerics. He asked if we could prove their existence using the anti-integrable limit and I said yes and did. This initiated a series of results on their stability and interaction. In parallel I pursued a number of ideas in topological dynamics, the best of which was prompted by happening to read Thurston and Weeks' Scientific American article on Three-manifolds. I was struck by the example they gave of a two-manifold, namely the configuration space of a triple linkage, which they showed has genus 3. I asked myself whether the free dynamics of the linkage might be Anosov and following numerics by a PhD student Tim Hunt in Cambridge, managed to prove this in a certain parameter regime. In Cambridge I was also invited to help steer a project on spatially extended dynamics. I tried out an idea I had for responding to Sinai and Bunimovich's challenge to make a coupled map lattice with non-unique phase on the group. One of the postdocs Guy Gielis explained to me some interesting stochastic systems that showed similar effects and I realised we could simulate them using coupled map lattices. This was probably my real entry into complex systems. Using the understanding gained, I proposed a mathematical formulation of the trendy concept of "emergence". Actually I did this first in response to a new PhD student David Sanders in 2000 when I'd just returned to Warwick, who wanted a project on emergence. More recently I went through Dobrushin's proof of ergodicity for weakly dependent probabilistic cellular automata and realised it could be expressed more nicely in terms of a metric on spaces of multivariate probabilities, which I have found useful in talking about the amount of emergence and the dependence on parameters. This is just a sample of things I've worked on and how I got into them. It is mostly serendipitous: just happening to pick up something where I could see I could do something, putting together things I'd already understood, interacting with interesting people.

Is stochastic dynamics closer to real systems than deterministic dynamics? Do you think that that is a fruitful direction for future work?

My view of stochastic dynamics is that the random terms represent aspects of the system that we choose not to attempt to model more accurately. In the absence of further knowledge or analytical ability this can be a sensible approach. Nevertheless, there are examples where the effect of some deterministic dynamics is rigorously equivalent to some noise process, the randomness being with respect to initial conditions, and then it makes sense to use the stochastic model. For example, a Langevin equation is widely used for the dynamics of slow degrees of freedom in a Hamiltonian system whose fast degrees of freedom are mixing. Anosov an I have scketched a derivation of this.

In the last few decades the number of active researchers and the quality of the mathematical work produced in Portugal has grown considerably. In your professional life have you ever had this perception?

It has been my privilege to interact with the dynamical systems group from Porto for at least 20 years and to supervise three PhD students from Portugal. And I've just taken on another one.

In contrast to older times, today mathematics is very much a collaborative effort. Do you have any preference between working alone or in teams? Is the challenge different?

There is still plenty of room in mathematics for single author research. But there are advantages to collaborations: broader perspective, shared work, wider dissemination.

Who is your favourite mathematician? Why?

I have many heroes, for example Moser, Arnol'd, Anosov and Sinai. I like what they have written and I like them as people (though unfortunately Arnol'd and Moser are no longer alive). Moser made many important advances in Hamiltonian dynamics; he was particularly nice to me, accepting me early in my career even though my approach was very non-standard mathematically and suggesting fruitful lines of research. Arnol'd was brilliant in a wide range of directions; he could be famously caustic but he was always nice to me and willing to answer my questions in considerable detail. Anosov I feel is a greatly under-rated mathematician: the insights he had in the 1960s about the Holder continuity of the foliations of hyperbolic dynamical systems and its implications for their measure theory are profound; I enjoyed making his acquaintance and showing him my mechanical Anosov system. Sinai I feel is the main architect of the theory of how deterministic dynamical systems can behave stochastically: he showed that the Markov partitions that had been constructed for special systems are a general feature of hyperbolic dynamical systems and that they give a correspondence of the dynamics to a generalisation of Markov processes called Gibbsian processes (which allow infinite-range but decaying memory). He has a very warm character and has been very supportive of my work. Going further back in time, I'd say Poincaré is my biggest hero: he developed so much interesting mathematics and presented it in such a readable way. And before him there was Newton, who was so creative, but apparently an awful character.

Detailed mathematical models in neurobiology—Storing information in membrane conductances dynamics

by **Eduardo Conde-Sousa*** and **Paulo Aguiar****

1. Introduction

Neurons are Nature's solution to the problem of information processing and information storage. Nervous systems have been engineered by evolution to sense information from the environment, process this information and store experiences for the purpose of improving future decisions. Ubiquitous in all these stages is the necessity of information buffers. In the case of mammals, there are different mechanisms providing storage in a wide range of time scales: from the ephemeral facilitation of a synapse to the life-long memories of childhood. As expected, neuronal dynamics are an extremely rich subject from a mathematically point of view. In this paper we focus on a model for a short-term memory mechanism called *working memory*. Regions of the mammal brain engaged in providing this functional resource are capable of retaining neuronal spatial patterns of activity for the duration of a few seconds. Basically, working memory provides a temporary buffer where information is held for short-time, while it is being actively used in cognitive tasks; this information can then be passed on to longer-term storage mechanisms or be simply discarded and forgotten. We humans use our working memory system when we temporarily retain a phone number or a name, when we mentally perform an arithmetic calculation, or when our wives tell us by phone the grocery list.

Our goal in this article is to give a glimpse into some of the methodologies used in theoretical neuroscience targeting a particular problem: to describe a mathematical model, closely fitted into the biophysical constrains of the nervous system, that helps understanding how working memory can be produced in a network of neurons. Our approach is different from other working memory models [1] in the sense that it does not rely on synaptic plasticity{1} nor connectivity structure to store information. In our model we store information in the dynamical states of the neuron's membrane conductances. An important feature in working memory systems is that it is possible to retain complex activity patterns after a single exposure to the stimuli. This constrain is better supported by the time scales found in conductances dynamics, than by synaptic plasticity temporal properties, even if we take into account short-term plasticity mechanisms.

In a population of N interconnected neurons engaged in working memory, we define as information content the particular subset of neurons that are co-activated

{1} Synapses are the structures that mediate most of the communication and transfer of signals between neurons. A strong synapse produces a large signal in the target neuron while a weak synapse will produce a small response. By modulating the synaptic strengths it is possible to both store information and to change the computational/functional capabilities of populations of neurons. The present dogma in neuroscience is that information is stored in the efficacy, or strength, of synapses.

* Faculdade de Ciências da Universidade do Porto
** Centro de Matemática da Universidade do Porto