BULLETIN

INTERNATIONAL CENTER FOR MATHEMATICS

June 2002

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Contents

Coming Events

Thematic Term on Mathematics and Biology

The programme of events is the following:

17-21 June: Advanced School and Workshop on Mathematical and Computational Modelling of Biological Systems

Organizers: João A. C. Martins and E. B. Pires (IST, Lisbon, Portugal).

Description

The event has two components: advanced course and research workshop.

The aim of the event is to provide an updated overview of some typical models and tools used in mathematical and computational studies of biological tissues, organs and systems.

The advanced course will include lectures on the:

- mechanics of soft tissues
- thermo-chemo-electro-mechanics of porous media
- skeletal muscles and neuromuscular control
- control and mechanics of human movement systems
- physiological fluid mechanics
- mechano-electrical function of the heart.

The course is directed to PhD students, applied mathematicians, physicists, biologists, medical doctors, engineers and other researchers working in related areas who wish a better understanding of biological phenomena and want to develop reliable models for them.

In the workshop, research papers submitted by the participants will describe new developments and discuss future research directions. It will also provide an opportunity for the establishment or development of interdisciplinary collaborations between researchers from different areas.

These events will be held at IST, Lisbon.

Advanced Lectures:

- *Mechanics of soft tissues, Finite Element Models*, Gerhard A. Holzapfel, Graz University of Technology, Institute for Structural Analysis - Computational Biomechanics, Austria.
- *Electro-mechanics of the heart, Finite Element Models*, Peter J. Hunter, Engineering Science Department, University of Auckland, New Zealand.
- *Thermo-chemo-electro-mechanics of saturated porous media*, Jacques Huyghe, Department of Biomedical Engineering, Technical University of Eindhoven, The Netherlands.
- *Dynamics of skeletal muscles, Neuromuscular control*, J. L. van Leeuwen, Wageningen University, Experimental Zoology Group, Department of Animal Sciences & Wageningen Institute of Animal Sciences, The Netherlands.
- *Control and mechanics of human movement systems*, Clyde F. Martin, Department of Mathematics and Statistics, Texas Technical University, Lubbock, Texas, U. S. A.
- *Physiological fluid mechanics*, Oliver E. Jensen, Division of Theoretical Mechanics, School of Mathematical Sciences, University of Nottingham, United Kingdom.

For more information on this event, please visit the site

http://www.civil.ist.utl.pt/bio.systems/

24-28 June: Advanced School and Workshop on Bone Mechanics - Mathematical and Mechanical Models for Analysis and Synthesis

Organizers: Helder C. Rodrigues and José M. Guedes (IST, Lisbon, Portugal).

Description

The event has two components: Advanced course and research workshop.

The course component will address the most significant problems in bone mechanics and describe the respective mechanical and mathematical modelling. Lectures will also be taught on specific topics of applied mathematics (e.g. homogenization, generalized shape design, convex analysis, optimization) which are important and may have a key role in overcoming the limitations observed in the more traditional models used in biomechanics. The course part is geared to an audience of postgraduate students and researchers (in applied mathematics, mechanics and biomechanics) who want to have an introduction to bone mechanics and the respective mechanical-mathematical modelling.

In the workshop component research papers, submitted by the participants, will describe new developments and discuss future research directions. The workshop is aimed at a mixed audience of postgraduate students and experienced researchers in mathematics, mechanics and medicine. It is the perfect forum to identify new areas of research within mathematics and biomechanics, to extend methodologies developed within the context of material design and optimization to the modelling of bone mechanics problems and to promote collaboration between researchers from the different areas.

These events will be held at IST, Lisbon.

Advanced Lectures:

- *Material models in topology optimization of structures*, Martin P. Bendsoe, Technical University of Denmark- Mathematical Institute, Lyngby, Denmark.

- *Optimization and biological designs*, Andrej Cherkaev, Department of Mathematics, University of Utah, Salt Lake City, USA.

- *Mechanosensation system in bone, Adaptive elasticity*, Stephen C. Cowin, Department of Mechanical Engineering at City College, City University of New York, New York, USA.

- *Bone prostheses and implants*, Manuel Doblaré, University of Zaragoza, Centro Politecnico Superior, Zaragoza, Spain.

- *Computational assessment of bone mechanical quality, Biological versus topological optimization models of bone*, Rik Huiskes, Department of Biomedical Engineering, Eindhoven University of Technology, Eindhoven, The Netherlands.

- *Bone remodelling: Analytical and computational models*, Harrie Weinans, Erasmus University Rotterdam, Erasmus Orthopaedic Research Lab, Rotterdam, The Netherlands.

- *Homogenization models for cellular materials*, José M. Guedes, Department of Mechanical Engineering, Instituto Superior Técnico, Lisbon, Portugal.

- *Material optimization models applied to bone remodelling simulation*, Helder Rodrigues, Department of Mechanical Engineering, Instituto Superior Técnico, Lisbon, Portugal.

For more information on this event, please visit the site

http://www.dem.ist.utl.pt/∼bonemec/

27-29 June: Workshop on Molecular Geometry Optimization

ORGANIZER: Fernando Nogueira (Univ. Coimbra, Portugal).

AIMS

The workshop is intended to bring together mathematicians, chemists and physicists who work in molecular geometry optimization. Its main goal is, therefore, to allow the interchange of ideas between scientists with very different backgrounds and to provide a basepoint for the development of joint research projects. The use of high-performance computing software and hardware for performing realistic calculations of molecular structure will also be highlighted.

This event will be held at the Physics Department, University of Coimbra.

INVITED TALKS:

- *How to avoid optimising molecular geometry*, Hugh Cartwright, Department of Chemistry, Oxford University, UK.

- *Structure prediction in protein folding*, Christodoulos A. Floudas, Department of Chemical Engineering, Princeton University, USA.

- *Geometry optimization and molecular dynamics in internal coordinates*, Peter Pulay, Department of Chemistry and Biochemistry, University of Arkansas, USA.

- *Enhanced sampling and global optimization techniques for complex systems*, John E. Straub, Chemistry Department, Boston University, USA.

- *Energy landscapes of clusters, biomolecules and solids*, David J. Wales, Department of Chemistry, Cambridge University, UK.

- *Genetic algorithms for molecular geometry optimisation*, Ron Wehrens, Laboratory of Analytical Chemistry Catholic University of Nijmegen, The Netherlands.

For more information on this event, please visit the site

http://cfc.fis.uc.pt/events/MGO2002/

15-19 July: Summer School on Mathematical Biology

ORGANIZERS: Alessandro Margheri (Univ. Lisbon, Portugal), Carlota Rebelo (Univ. Lisbon, Portugal) and Fabio Zanolin (Univ. Udine, Italy).

AIMS

The aim of this school is to present instances of interaction between two major disciplines, Biology and Mathematics, featuring recent issues from epidemiology and dynamics of populations. In this way, we expect to motivate the participants, biologists and mathematicians, to develop some future collaborations.

We intend to address a fairly wide audience, composed by mathematicians who work in differential equations and are interested in examples of applications of mathematics to real-life problems, and by biologists who intend to learn or deepen their knowledge of differential equations methods currently used in modelling. As little background as possible (both in mathematics and in biology) will be assumed throughout the lectures, so that advanced undergraduate, Master and PhD students both in Mathematics and in Biology will find most of the topics accessible.

The school will consist of several courses and a few seminars.

This event will be held at the Complexo Interdisciplinar (Univ. Lisbon).

Short Courses:

- *On Problems Related to Persistence and Extinction of Species*, Shair Ahmad, Division of Mathematics and Statistics, University of Texas at San Antonio, Texas, USA.
- *The Use of Mathematical Models in Epidemiology with Applications to Communicable and Sexually-Transmitted Diseases*, Carlos Castillo-Chavez , Biometrics Unit, Cornell University, USA.
- *Adaptive Dynamics*, Odo Diekmann, University of Utrecht, The Netherlands.
- *Population Dynamics of Multi-Strain Pathogens*, M.Gabriela M. Gomes, Ecology and Epidemiology Group, Department of Biological Sciences, University of Warwick, England.
- *From Simple Models of Transmission Dynamics to Understanding Infections Disease Epidemiology*, G. Medley, Ecology and Epidemiology Group, Department of Biological Sciences, University of Warwick, England.

For more information on this event, please visit the site

http://cmaf.lmc.fc.ul.pt/events/2002/ssmb/

Meeting on Bounded Systems and Complexity Classes

Organizer

Fernando Ferreira (Univ. Lisbon, Portugal).

Date

28-29 June.

Aims

To draw together people interested in bounded formal systems related to computational complexity classes in order to discuss current work and assess directions of research. If sufficient interest arises, international proceedings may be published.

This event will be held at the Complexo Interdisciplinar (Univ. Lisbon).

Invited speakers

- Jeremy Avigad - Department of Philosophy, Carnegie-Mellon University, USA.
- Martin Hofmann - Department of Computer Science, The University of Edinburgh, United Kingdom.
- Ulrich Kohlenbach - Department of Computer Science, University of Aarhus, Denmark.
- Jan Krajícek - Mathematical Institute of the Academy of Sciences of the Czech Republic in Prague, Czech Republic.
- Thomas Strahm - Forschungsgruppe für theoretische Informatik und Logik, Institute für Informatik und angewandte Mathematik, Universität Bern, Switzerland.

For more information on this event, please visit the site

http://alf1.cii.fc.ul.pt/∼ferferr/bacc2002/bacc.html

CIM News

CIM Events for 2003

The CIM Scientific Committee, in a meeting held in Coimbra on February 9, approved the CIM scientific program for 2003.

The **Thematic Term** for 2003 will be dedicated to Mathematics and Engineering. The application of mathematics to engineering is crucial to knowledge and the development of science. The main objective of the thematic term for 2003 is to improve and emphasize the interdependence between the most recent and important research fields in mathematics and the most important fields of contemporary engineering: informatics engineering, chemical engineering, mechanical engineering, civil engineering and electronics engineering.

The thematic term 2003 consists of four events. The first event is devoted to mathematics and informatics engineering and focuses on soft computing and complex systems. The second event deals with modelling and simulation in chemical engineering. The third event is related to modelling and numerical simulation in continuum mechanics. The fourth event is concerned with mathematics and telecommunications.

Each one of these events is an Advanced School and Workshop, where short courses, lectures and invited talks will be given by well-known invited scientists. So it is expected that the thematic term 2003 will attract a large number of postgraduate students, mathematicians and engineers, interested in contributing to the development of mathematics and its applications to engineering.

The Thematic Term 2003 Organizer-Coordinator is Isabel Maria Narra de Figueiredo (University of Coimbra)

The list of events is the following:

Workshop on Soft Computing and Complex Systems

23-27 June 2003

Organizers:

António Dourado Correia, Univ. Coimbra

Ernesto Jorge Costa, Univ. Coimbra

José Félix Costa, I. Superior Técnico - Lisbon

Pedro Quaresma, Univ. Coimbra

Workshop on Modelling and Simulation in Chemical Engineering

30 June - 4 July 2003

Organizers:

Alírio Egídio Rodrigues, Univ. Porto

Paula Oliveira, Univ. Coimbra

José Almiro Meneses e Castro†, Univ. Coimbra

José Augusto Mendes Ferreira, Univ. Coimbra

Maria do Carmo Coimbra, Univ. Porto

Advanced School and Workshop on Modelling and Numerical Simulation in Continuum Mechanics

14-18 July 2003

Organizers:

Luís Filipe Menezes, Univ. Coimbra

Isabel Maria Narra de Figueiredo, Univ. Coimbra

Juha Videman, I. Superior Técnico - Lisbon

Mathematical Techniques and Problems in Telecommunications

8-12 September 2003

Organizers:

Carlos Salema, I. Superior Técnico - Lisbon

Joaquim Júdice, Univ. Coimbra

Carlos Fernandes, I. Superior Técnico - Lisbon

Mário Figueiredo, I. Superior Técnico - Lisbon

Luís Merca Fernandes, I. P. Tomar

Furthermore, the 2003 program will contain the following event:

Third Debate on Mathematical Research in Portugal

Porto, June 2003

Organizers:

José Ferreira Alves, Univ. Porto

José Miguel Urbano, Univ. Coimbra

CIM Associates

The CIM General Assembly, in a meeting held in Coimbra on March 16, approved the admission of three new CIM Associates.

The current CIM Associate institutions are:

- Sociedade Portuguesa de Matemática
- Universidade de Coimbra
- Universidade do Porto
- Faculdade de Ciências da Universidade de Lisboa
- Universidade do Minho
- Universidade Nova de Lisboa
- Universidade de Aveiro
- Universidade dos Açores
- Universidade da Beira Interior
- Universidade de Évora
- Universidade de Trás-os-Montes e Alto Douro
- Cooperativa de Ensino Universidade Lusíada
- Universidade da Madeira
- Universidade do Algarve
- Centro de Matemática Aplicada do IST
- Centro de Investigação em Matemática e Aplicações da Universidade de Évora
- Centro de Álgebra da Universidade de Lisboa
- Centro de Matemática da Universidade de Coimbra
- Universidade de Macau
- Centro de Matemática da Universidade do Porto
- Centro de Estruturas Lineares e Combinatórias
- Instituto Superior de Economia e Gestão
- Centro de Física Computacional da Universidade de Coimbra
- Instituto de Sistemas e Robótica
- Centro de Lógica e Computação do IST

Research in Pairs at CIM

CIM has facilities for research work in pairs and welcomes applications for their use for limited periods.

These facilities are located at Complexo do Observatório Astronómico in Coimbra and include:

- office space, computing facilities, and some secretarial support;
- access to the library of the Department of Mathematics of the University of Coimbra (30 minutes away by bus);
- lodging: a two room flat.

At least one of the researchers should be affiliated with an associate of CIM, or a participant in a CIM event.

Applicants should fill in the electronic application form

http://www.cim.pt/cim.www/cim_app/application.htm

CIM on the WWW

Complete information about CIM and its activities can be found at the site

http://www.cim.pt

This is mirrored at

http://at.yorku.ca/cim.www/

Regularity for Partial Differential Equations: from De Giorgi-Nash-Moser Theory to Intrinsic Scaling

by José Miguel Urbano

Departamento de Matemática - CMUC
Universidade de Coimbra

1 A beautiful problem

In the academic year 1956-1957, John Nash had a visiting position at the Institute for Advanced Study (IAS) in Princeton, on a sabbatical leave from MIT, but he actually lived in New York City. The IAS at the time "was known to be about the dullest place you could find"[1] and Nash used to hang around the Courant Institute which was close to home and full of activity. That's how he came across a problem that mathematicians had been trying to solve for quite a while. The story goes that Louis Nirenberg, at the time a young professor at Courant, was the person responsible for the unveiling: "...it was a problem that I was interested in and tried to solve. I knew lots of people interested in this problem, so I might have suggested it to him, but I'm not absolutely sure", said Nirenberg recently in an interview to the Notices of the AMS (cf. [19]).

As so many other great questions of 20th century mathematics, it all started with one of Hilbert's problems presented on the occasion of the 1900 International Congress of Mathematicians in Paris, namely the 19th problem: *Are the solutions of regular problems in the calculus of variations always necessarily analytic?* A simple example of such a problem is, in modern terminology, the problem of minimizing a functional

$$\min_{w\in\mathcal{A}} \int_\Omega L(\nabla w(x))\,\mathrm{d}x$$

where $\Omega\subset\mathbf{R}^n$ is a bounded and smooth domain, the Lagrangian $L(\xi)$ is a smooth (possibly nonlinear) scalar function defined on $\mathbf{R}^n$ and $\mathcal{A}$ is a set of admissible functions (typically the elements of a certain function space satisfying a boundary condition like $w=g$ on $\partial\Omega$, for a given g). The question is to prove that, given the smoothness of L, the minimizer (assuming it exists) is also smooth.

Problems of this type are related to elliptic equations in that a minimizer u is a weak solution of the associated Euler-Lagrange equation

$$\sum_{i=1}^n \frac{\partial}{\partial x_i} L_{\xi_i}(\nabla u(x)) = 0 \quad \text{in} \quad \Omega\ .$$

This equation can be differentiated with respect to x_k, to give that, for any $k=1,2,\ldots,n$, the partial derivative $\frac{\partial u}{\partial x_k} := v_k$ satisfies a linear PDE of the form

$$\sum_{i,j=1}^n \frac{\partial}{\partial x_i}\Big(a_{ij}(x)\,\frac{\partial v_k}{\partial x_j}\Big) = 0\ , \tag{1}$$

with coefficients $a_{ij}(x) := L_{\xi_i\xi_j}(\nabla u(x))$. The PDE is elliptic provided L is assumed to be convex.

In the 1950's, regularity theory for elliptic equations was essentially based on Schauder's estimates which, roughly speaking, guarantee that if $a_{ij}\in C^{k,\alpha}$ then the solutions of (1) are of class $C^{k+1,\alpha}$, for $k=0,1,\ldots$ So if it could be shown that $u\in C^{1,\alpha}$ then $a_{ij}(x) := L_{\xi_i\xi_j}(\nabla u(x))$ would belong to $C^{0,\alpha}$, v to $C^{1,\alpha}$ and u to $C^{2,\alpha}$; a bootstrap argument would then solve Hilbert's 19th problem.

Meanwhile, the existence theory had been developed through the use of direct methods: the minimization problem has a unique solution provided L, apart from satisfying natural growth conditions like

$$|L(\xi)|\le C\,|\xi|^p\ ,$$

[1] Cathleen Morawetz, quoted in [15].

is also coercive and uniformly convex. The notion of solution had to be conveniently extended and the admissible set $\mathcal{A}$ taken to be the set of functions that, together with their first weak derivatives, belong to L^p, i.e., that belong to the Sobolev space $W^{1,p}$.

So the existence theory gave a minimizer $u \in W^{1,p}$ and the missing step for the regularity problem to be solved was

$$u \in W^{1,p} \Longrightarrow u \in C^{1,\alpha}$$

i.e., from first derivatives in L^p to Hölder continuous first derivatives. In terms of the elliptic PDE (1), regularity theory worked if the leading coefficients were already somewhat regular (at least continuous) since it was based on perturbation arguments and comparison of the solutions with harmonic functions. Assuming only the measurability and the boundedness of the coefficients (together with the essential structural assumption of ellipticity) was insufficient, and nothing was known about the regularity of the solutions in this case.

The problem was solved by C.B. Morrey in 1938 for the special case $n = 2$ but the techniques he employed were typically two dimensional, involving complex analysis and quasi-conformal mappings. The n-dimensional problem remained open until the late 50's and that's exactly what Nirenberg told Nash about.

2 De Giorgi's breakthrough

The problem wouldn't resist the genius of John Nash and Ennio De Giorgi. The two men worked totally unaware of each other's progress and solved the problem using entirely different methods.

It was De Giorgi who did it first (actually for $p = 2$; the result would later be extended to any $p \in (1, \infty)$) and it is his proof that will now be analyzed. To really understand in full depth De Giorgi's ideas there is no way around the technicalities. In what follows I did my best to explain things in a clear way but the reader should not expect everything to be trivial or immediately understandable; so please grab a pencil and a piece of paper and be prepared to struggle a bit with inequalities and iterations.

Consider the equation

$$\sum_{i,j=1}^{n} \frac{\partial}{\partial x_i}\Big(a_{ij}(x)\,\frac{\partial u}{\partial x_j}\Big) = 0 \quad \text{in} \quad \Omega \tag{2}$$

where $\Omega \subset \mathbf{R}^n$ is a smooth bounded domain and the coefficients a_{ij} are only assumed to be measurable and bounded, with

$$\|a_{ij}\|_{L^\infty} \leq \Lambda \ ,$$

and to satisfy the uniform ellipticity condition (for $\lambda > 0$)

$$\sum_{i,j=1}^{n} a_{ij}\xi_i\xi_j \geq \lambda|\xi|^2 \ , \quad \forall x \in \Omega, \ \forall \xi \in \mathbf{R}^n \ .$$

A weak solution of equation (2) is a function $u \in W^{1,2}(\Omega)$ which satisfies the integral identity

$$\sum_{i,j=1}^{n} \int_\Omega a_{ij}\frac{\partial u}{\partial x_i}\frac{\partial \varphi}{\partial x_j} = 0 \tag{3}$$

for all test functions $\varphi \in W_0^{1,2}(\Omega)$ (the elements of $W^{1,2}$ which vanish on the boundary $\partial\Omega$ in a suitable weak sense).

To simplify the writing we assume from now on that

$$\Omega = B_1 := \Big\{ \, x \in \mathbf{R}^n \ : \ |x| < 1 \, \Big\} \ .$$

Theorem 1 *Every weak solution of* (2) *is locally bounded.*

Proof. Let $k \geq 0$ and η be a smooth function with compact support in B_1. Put $v = (u-k)^+$ and take $\varphi = v\eta^2$ as test function in (3). The use of the assumptions and Young's inequality give

$$\int_{B_1} |\nabla v|^2\eta^2 \leq \frac{4\Lambda^2}{\lambda^2}\int_{B_1} |\nabla\eta|^2 v^2 \ . \tag{4}$$

These Cacciopoli inequalities on level sets of u will be the building blocks of the whole theory and once they are obtained the PDE can be forgotten: the problem becomes purely analytic.

Next, by Hölder and Sobolev's inequalities (with $2^* = 2n/(n-2)$ being the Sobolev exponent),

$$\begin{aligned}\int_{B_1}(v\eta)^2 \quad &\leq \quad \left(\int_{B_1}(v\eta)^{2^*}\right)^{\frac{2}{2^*}} |\{v\eta \neq 0\}|^{1-\frac{2}{2^*}} \\ &\leq \quad c(n)\ |\{v\eta \neq 0\}|^{\frac{2}{n}} \int_{B_1} |\nabla(v\eta)|^2\end{aligned}$$

and since, due to (4),

$$\int_{B_1} |\nabla(v\eta)|^2 \leq \left(\frac{4\Lambda^2}{\lambda^2} + 1\right)\int_{B_1} |\nabla\eta|^2 v^2$$

we arrive at

$$\int_{B_1}(v\eta)^2 \leq c(n,\lambda,\Lambda)\ |\{v\eta \neq 0\}|^{\frac{2}{n}} \int_{B_1} |\nabla\eta|^2 v^2 \ .$$

Now for fixed $0 < r < R < 1$, choose the cut-off function $\eta \in C_0^\infty(B_R)$ such that $0 \leq \eta \leq 1$, $\eta \equiv 1$ in B_r and $|\nabla\eta| \leq \frac{2}{R-r}$. Putting, for $\rho > 0$,

$$A(k,\rho) = \Big\{ \, x \in B_\rho \ : \ u(x) > k \, \Big\} \ ,$$

we obtain (with $C \equiv c(n,\lambda,\Lambda)$)

$$\int_{A(k,r)} (u-k)^2 \le \frac{C}{(R-r)^2}\ |A(k,R)|^{\frac{2}{n}} \int_{A(k,R)} (u-k)^2.$$

For $h > k$ and $0 < \rho < 1$,

$$\int_{A(h,\rho)} (u-h)^2 \le \int_{A(k,\rho)} (u-k)^2$$

and

$$(h-k)^2 |A(h,\rho)| \le \int_{A(k,\rho)} (u-k)^2$$

so we have

$$\begin{aligned} &\int_{A(h,r)} (u-h)^2 \\ \le\ & \frac{C}{(R-r)^2}\ |A(h,R)|^{\frac{2}{n}} \int_{A(h,R)} (u-h)^2 \\ \le\ & \frac{C}{(R-r)^2}\ \frac{1}{(h-k)^{\frac{4}{n}}} \left(\int_{A(k,R)} (u-k)^2 \right)^{1+\frac{2}{n}} \end{aligned}$$

or, equivalently, with $\psi(s,\rho) = \|(u-s)^+\|_{L^2(B_\rho)}$

$$\psi(h,r) \le \frac{C}{R-r}\ \frac{1}{(h-k)^{\frac{2}{n}}}\ \psi(k,R)^{1+\frac{2}{n}}\ , \qquad (5)$$

for any $h > k > 0$ and $0 < r < R < 1$.

We are now ready to use the brilliant iteration scheme devised by De Giorgi. Define, for $m = 0, 1, 2, \ldots$

$$\begin{aligned} k_m &= k\,(1 - \frac{1}{2^m}) \\ r_m &= \frac{1}{2}\,(1 + \frac{1}{2^m}) \end{aligned}$$

where k is to be determined later. Due to (5), we then have, for $m = 0, 1, 2, \ldots,$

$$\psi(k_m, r_m) \le C\ \frac{2^{m+1+\frac{2m}{n}}}{k^{\frac{2}{n}}}\ \psi(k_{m-1}, r_{m-1})^{1+\frac{2}{n}} \qquad (6)$$

and can prove, by induction, that, for some $\gamma > 1$,

$$\psi(k_m, r_m) \le \frac{\psi(k_0, r_0)}{\gamma^m}\ ,\ \forall\, m = 0, 1, 2, \ldots \qquad (7)$$

if k is chosen sufficiently large. In fact, it is trivial that it holds for $m = 0$; now suppose it holds for $m-1$ and write

$$\begin{aligned} \psi(k_{m-1}, r_{m-1})^{1+\frac{2}{n}} &\le \left\{ \frac{\psi(k_0, r_0)}{\gamma^{m-1}} \right\}^{1+\frac{2}{n}} \\ &= \frac{\psi(k_0, r_0)^{\frac{2}{n}}}{\gamma^{\frac{2m}{n} - (1+\frac{2}{n})}}\ \frac{\psi(k_0, r_0)}{\gamma^m}\ . \end{aligned}$$

From (6) we obtain

$$\psi(k_m, r_m) \le 2C\gamma^{1+\frac{2}{n}}\ \frac{\psi(k_0, r_0)^{\frac{2}{n}}}{k^{\frac{2}{n}}}\ \frac{2^{m(1+\frac{2}{n})}}{\gamma^{\frac{2m}{n}}}\ \frac{\psi(k_0, r_0)}{\gamma^m}$$

and choose first $\gamma > 1$ such that $\gamma^{\frac{2}{n}} = 2^{1+\frac{2}{n}}$ and then k large enough so that

$$2C\gamma^{1+\frac{2}{n}}\ \frac{\psi(k_0, r_0)^{\frac{2}{n}}}{k^{\frac{2}{n}}} \le 1\ \Leftarrow\ k = C^*\ \psi(k_0, r_0)$$

where $C^* \equiv C^*(n,\lambda,\Lambda)$.

Finally let $m \to \infty$ in (7) to get $\psi(k, \frac{1}{2}) \le 0$, i.e.,

$$\|(u-k)^+\|_{L^2(B_{\frac{1}{2}})} = 0\ .$$

Hence

$$\sup_{B_{\frac{1}{2}}} u^+ \le C^*\ \|u^+\|_{L^2(B_1)}\ .$$

Using a dilation argument, this estimate can be refined; indeed, for any $\theta \in (0,1)$ and $p > 1$, it holds

$$\sup_{B_\theta} u^+ \le \frac{C(n,\lambda,\Lambda)}{(1-\theta)^{n/p}}\ \|u^+\|_{L^p(B_1)}\ .$$

The same type of reasoning gives similar conclusions concerning u^- and the result follows. □

The basic general idea to obtain results concerning the continuity of a solution of a PDE at a point consists in estimating its oscillation in a nested sequence of concentric balls (cylinders in the parabolic case), centered at the point, and showing that it converges to zero as the balls shrink to the point. If this can be measured quantitatively it gives a modulus of continuity.

Denote the oscillation of a function u in B_r by $\mathrm{osc}(u,r)$. A further analysis, which uses the previous theorem, leads to

Theorem 2 *Let $u \in H^1(B_2)$ be a weak solution of* (2) *in B_2. There exists a constant $\gamma = \gamma(n,\lambda,\Lambda) \in (0,1)$ such that*

$$\mathrm{osc}\,(u, 1/2) \le \gamma\,\mathrm{osc}\,(u,1)\ .$$

Thus (see below), there exists some constant $\alpha \in (0,1)$ such that, for $0 < r < R < 1$,

$$\mathrm{osc}\,(u,r) \le C\ \left(\frac{r}{R}\right)^\alpha \mathrm{osc}\,(u,R)\ ,$$

which gives a Hölder modulus of continuity and

Theorem 3 (De Giorgi - Nash) *Every weak solution of* (2) *is Hölder continuous.*

3 Three papers... and a correction

In a series of three fundamental papers (and a correction) published in *Communications on Pure and Applied Mathematics*, the journal of the Courant Institute, Jürgen Moser made significant contributions to the theory. He first gave in [11] a new proof of De Giorgi's theorem, using the simple general principle that the estimates (4) hold for any convex function $f(u)$ of a solution u; the results were obtained by applying such estimates to powers $f(u) = |u|^p$, $p \geq 1$, and to the logarithmic function $\log^+ u^{-1}$. Then he proved Harnack's inequality for elliptic equations (cf. [12]):

Theorem 4 (Moser's Harnack inequality) *If u is a positive weak solution of* (2) *and K is a compact subset of Ω, then*

$$\max_K u \leq C \min_K u \ ,$$

where $C \equiv C(\Omega, K, \lambda, \Lambda)$.

The proof of the Harnack inequality made no use of the Hölder continuity of the solutions, which in turn is a simple consequence of that fact, as Moser showed in the paper. In fact, assume again that $\Omega = B_1$. Let, for $0 < r < 1$,

$$M(r) = \max_{\overline{B_r}} u \ , \qquad m(r) = \min_{\overline{B_r}} u \ ,$$

and apply Harnack's inequality to the domains B_r and $\overline{B_{\frac{r}{2}}}$ to get

$$\begin{aligned} \max_{\overline{B_{\frac{r}{2}}}} (M(r) - u) &= M(r) - m(r/2) \\ &\leq C \Big(M(r) - M(r/2) \Big) \\ &= C \min_{\overline{B_{\frac{r}{2}}}} (M(r) - u) \end{aligned}$$

and

$$M(r/2) - m(r) \leq C \Big(m(r/2) - m(r) \Big)$$

since $M(r) - u$ and $u - m(r)$ are positive solutions in B_r. Adding these two inequalities, we obtain

$$M(r/2) - m(r/2) \leq \frac{C-1}{C+1} \Big(M(r) - m(r) \Big)$$

or, with $\alpha = \frac{C-1}{C+1} < 1$,

$$\mathrm{osc}\,(u, r/2) \leq \alpha \, \mathrm{osc}\,(u, r) \ .$$

By induction,

$$\mathrm{osc}\,(u, 2^{-k} r) \leq \alpha^k \, \mathrm{osc}\,(u, r) \ ; \qquad k = 1, 2, \ldots$$

Now, for $\rho < r$, we can take k such that $2^{-k-1} r < \rho \leq 2^{-k} r$ to obtain

$$\mathrm{osc}\,(u, \rho) \leq C \left(\frac{\rho}{r} \right)^{\beta} \mathrm{osc}\,(u, r)$$

with $\beta = -\frac{\log \alpha}{\log 2} > 0$, and as a consequence the Hölder continuity of the function u.

Moser extended his results to parabolic equations, obtaining a Harnack inequality for nonnegative solutions of the parabolic analogue of (2), assuming only the boundedness of the coefficients and the condition corresponding to ellipticity (cf. [13] and [14]). Again his approach was essential *nonlinear* and contrasted dramatically with the approach via fundamental solutions that had been used by Hadamard and Pini to obtain Harnack estimates for solutions of the heat equation.

4 When Stanley met Livingstone

In 1958 the ICM would take place in Edinburgh and the deliberations on the Fields medalists were concluded early that year (the two medals were eventually awarded to Thom and Roth). Solving the regularity problem would probably be worth a medal. Nash in his own words [18]: "It seems conceivable that if either De Giorgi or Nash had failed on this problem (...) then the lone climber reaching the peak would have been recognized with mathematics' Fields medal."Nash solved the problem in the spring of 1957 using a *nonlinear* approach to attack *linear* equations. The main results would be announced in a note to the *Proceedings of the National Academy of Sciences* ([17]), submitted by Marston Morse of the IAS on October 6, 1957. By then, Nash had already found out, in late spring, about De Giorgi's proof: "(...) although I did succeed in solving the problem, I ran into some bad luck since, without my being sufficiently informed on what other people were doing in the area, it happened that I was working in parallel with Ennio De Giorgi of Pisa, Italy. And De Giorgi was first actually to achieve the ascent of the summit (...)."In fact, the seminal paper of De Giorgi was presented by Mauro Picone on April 24, 1957 to the Academy of Sciences of Torino and the results had been announced at the Congress of the Unione Matematica Italiana, which took place in Pavia in October, 1955. Some say that Nash was devastated when he learned about De Giorgi. That summer De Giorgi visited the Courant Institute and Peter Lax would say later about the meeting of the two men: "It was like Stanley meeting Livingstone."

The approach of Nash to the problem was totally different from De Giorgi's and some people think that his ideas were never fully understood (cf. [4]). He treated parabolic equations directly and obtained the results for elliptic equations as corollaries. The essence of his reasoning consisted of obtaining control of the properties of fundamental solutions of linear parabolic equations with variable coefficients. The crucial estimate is the moment bound

$$k_1 \sqrt{t} \leq \int |x| \, T(x,t) \, \mathrm{d}x \leq k_2 \sqrt{t} \ ,$$

which controls the moment of a fundamental solution T. About this result Nash wrote in [16]: "(...) it opens the door to the other results. We had to work hard to get (the bound), then the rest followed quickly."

Although the problem was "morally" solved, writing the paper proved to be technically very hard and Nash continued to work on it when he went back to MIT in the summer of 1957. A few steps in the proof were not clear and only a joint effort with such people as Lennart Carleson (who was visiting MIT on leave from Uppsala) and Elias Stein, both explicitly credited in the paper for some of the proofs, eventually led to his famous paper published in 1958 (Nash writes as an acknowledgement: "We are indebted to several persons" and then names eleven colleagues). There's a *petite histoire* about the publishing of the paper (cf. [15]): Nash first submitted it to *Acta Mathematica* through Carleson, who was an editor there, and made him know that he wanted the paper to be refereed quickly. Carleson gave it to Lars Hörmander (later a Fields medalist, together with John Milnor, in the Stockholm ICM in 1962) who did the job in two months and recommended the paper for publication. But Nash withdrew the paper, which would appear later in the *American Journal of Mathematics*. The reason for this might have been that, after "loosing" the Fields, Nash wanted the paper to be eligible for the AMS Bôcher Prize (awarded for a notable research memoir in analysis published during the previous five years in a recognized *North American* journal). The 1959 prize would be awarded to Louis Nirenberg for his work on partial differential equations.

Whether Nash's ideas were ever understood in full depth by anyone except himself remains unclear. The fact is that his work, although profusely cited, didn't give rise to much subsequent research. It was the more understandable approach of De Giorgi and Moser that the PDE community adopted and developed to full extent.

5 Intrinsic scaling

The work of De Giorgi, Moser and Nash concerned linear PDE's but the approach was essentially *nonlinear* since the linearity had no bearing in the proofs: it all stems out of the structure assumption on the differential operator.

In the elliptic case, this fact allowed for the extension to quasilinear equations of the type

$$\nabla \cdot \mathbf{a}(x,u,\nabla u) = b(x,u,\nabla u) \quad \text{in} \quad \Omega \ ,$$

where the principal part $\mathbf{a}$ satisfies the growth assumption

$$|\mathbf{a}(x,u,\nabla u)| \leq \Lambda \, |\nabla u|^{p-1} + \varphi(x)$$

and the ellipticity condition

$$\mathbf{a}(x,u,\nabla u) \cdot \nabla u \geq \lambda \, |\nabla u|^p - \varphi(x) \ ,$$

for constants $0 < \lambda \leq \Lambda$ and a bounded $\varphi \geq 0$; the prototype is the p-Laplacian equation

$$\nabla \cdot (|\nabla u|^{p-2} \nabla u) = 0 \ .$$

Notice the nonlinear dependence on the partial derivatives and the nonlinear growth with respect to the gradient. The equation is degenerate if $p > 2$ and singular if $1 < p < 2$, since its modulus of ellipticity $|\nabla u|^{p-2}$ vanishes or blows up, respectively, at points where $|\nabla u| = 0$. Ladyzhenskaya and Ural'tzeva established the Hölder continuity of weak solutions (cf. [9], the bible of elliptic equations), extending De Giorgi's results, and Serrin [20] and Trudinger [21] obtained the Harnack inequality for nonnegative solutions following Moser's ideas.

Surprisingly enough, the theory didn't succeed so well in the parabolic case

$$u_t - \nabla \cdot \mathbf{a}(x,u,\nabla u) = b(x,u,\nabla u) \quad \text{in} \quad \Omega \times [0,T)$$

and Moser's proof could only be extended (by Aronson, Serrin and Trudinger) for the case $p = 2$, which corresponds to principal parts with a linear growth on $|\nabla u|$. The same happened with the methods of De Giorgi, which the Russian school extended to the parabolic case (always for $p = 2$), thus rediscovering Nash's results concerning the Hölder continuity (of solutions of parabolic equations) by entirely different methods. So, unlike the elliptic case, degenerate or singular equations like

$$u_t - \nabla \cdot (|\nabla u|^{p-2} \nabla u) = 0 \ ,$$

for which the principal part of the equation grows nonlinearly with $|\nabla u|$, seemed to behave differently, and the questions of regularity remained open until the 1980's.

To understand the difficulty, consider a parabolic cylinder

$$Q(\tau, R) := B_R \times (0, \tau) \ .$$

The use of Cacciopoli inequalities on level sets leads in this case to expressions of the form (compare with (4))

$$\sup_{0<t<\tau} \int_{B_R \times \{t\}} v^2 \eta^p + \int_0^\tau \int_{B_R} |\nabla v|^p \, \eta^p \leq C \int_0^\tau \int_{B_R} |\nabla \eta|^p \, v^p \ .$$

The iterative argument of De Giorgi, as adapted by the Russian school to the parabolic case, required the equation to be nondegenerate ($p = 2$) so that the integral norms appearing in these estimates were homogeneous. This is not the case in the inequality above: the presence of the power p jeopardizes the homogeneity in the estimates and the recursive process itself. The key idea to overcome the difficulty presented by the inhomogeneity was introduced by DiDenedetto (cf. [3] for an account of the theory and an extensive list of references) and consists essentially of looking at the equation in its own geometry, i.e., in a geometry dictated by its degenerate structure. This amounts to re-scaling the standard parabolic cylinders by a factor depending on the oscillation of the solution. This procedure of *intrinsic scaling*, which somehow is an accommodation of the degeneracy, allows the recovering of the homogeneity in the energy estimates, written over these re-scaled cylinders, and the proof then follows more or less easily. One can say heuristically that *the equation behaves in its own geometry like the heat equation.*

Let's briefly describe the procedure for the degenerate case $p > 2$. Consider $R > 0$ such that $Q(R^{p-1}, 2R) \subset \Omega \times [0, T)$, define

$$\omega := \text{osc}\,(u, Q(R^{p-1}, 2R))$$

and construct the cylinder

$$Q(a_0 R^p, R)\ , \qquad \text{with} \quad a_0 = \left(\frac{\omega}{A}\right)^{2-p}$$

where A depends only on the data. Note that for $p = 2$, i.e., in the nondegenerate case, we have $a_0 = 1$ and these are the standard parabolic cylinders that reflect the natural homogeneity of the space and time variables. Assume, without loss of generality, that $\omega < 1$ and also that

$$\frac{1}{a_0} = \left(\frac{\omega}{A}\right)^{p-2} > R$$

which implies that $Q(a_0 R^p, R) \subset Q(R^{p-1}, 2R)$ and the relation

$$\text{osc}\,(u, Q(a_0 R^p, R)) \leq \omega\ . \tag{8}$$

This is in general not true for a given cylinder since its dimensions would have to be intrinsically defined in terms of the oscillation of the function within it; it is the starting point of the iteration process, in which the difficulties coming from the degenerate structure of the problem are overcome through the use of the re-scaled cylinders. The details are extremely technical and the interested reader can consult [3].

These ideas have been explored to obtain regularity results for other partial differential equations, like the porous medium equation or doubly nonlinear parabolic equations. I'll comment briefly on two extensions for which I am partly responsible.

The inclusion

$$\gamma(u)_t - \nabla \cdot (|\nabla u|^{p-2} \nabla u) \ \ni\ 0\ , \qquad p > 2,$$

where γ is a maximal monotone graph with a singularity at the origin, occurs as a model for the well-known two-phase Stefan problem when a nonlinear law of diffusion is considered, u being in that case the temperature and $\gamma(u)$ the enthalpy. As before, the equation is degenerate in the space part, but now it is also singular in the time part since "$\gamma'(0) = \infty$". In this case a further power appears in the energy estimates (the power one, which is due to estimating the singular term) and no re-scaling permits the compatibility of the three powers involved. The proof of the regularity in [22] uses the geometry of the nonsingular case to deal with the degeneracy but the price of a dependence on the oscillation in the various constants that are determined along the proof has to be paid. Owing to this fact, it is no longer possible to exhibit a modulus of continuity for the solution of the problem but only to define it implicitly. This is enough to obtain the continuity but the Hölder continuity, which holds in the nonsingular case, is lost.

Another example concerns the parabolic equation with two degeneracies

$$\partial_t u - \nabla \cdot (\alpha(u) \nabla u) = 0\ ,$$

where $u \in [0, 1]$ and $\alpha(u)$ degenerates for $u = 0$ and $u = 1$. An equation of this type is physically relevant since it shows up in a model describing the flow of two immiscible fluids through a porous medium and also in polymer chemistry and combustion. In [23] it is shown that u is locally Hölder continuous if α decays like a power at *both* degeneracies. A fine analysis of what happens near the two degeneracies leads to the construction of the cylinders used in the iterative process, with the appropriate geometry being once again dictated by the structure of the PDE.

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Great Moments in XXth Century Mathematics

by Marcelo Viana

In this issue we present the answer of Prof. Marcelo Viana to the question "If you had to mention one or two great moments in XXth century mathematics which one(s) would you pick?".

The first that comes to my mind are Gödel's theorems on the incompleteness of Arithmetics. These theorems changed our vision of human thought, Mathematics included, in much the same way as the great discoveries of Physics in the XXth century revolutionized our vision of the universe.

Mathematics had gone a long way towards establishing itself on a rigorous basis, from the times of Newton and Leibniz to those of Weierstrass and Hilbert. By the late XIXth century the axiomatic point of view seemed to allow every hope[2]*. To solve the crisis raised by Cantor's treatment of infinite sets, and the host of paradoxes unleashed by it, Hilbert proposed to establish the whole mathematical edifice on an axiomatic basis. A small number of postulates should be found, from which all other statements would be deduced through formal rules. Most important, one should prove that the postulates were consistent, that is, they would never lead to contradictory statements. Russell and Whitehead, Bourbaki, and others, set themselves to carry the gigantic task.*

Then, in 1931, Gödel proved that in any axiomatic system that is consistent and rich enough to contain Arithmetics, there are true statements that can not be proved nor disproved from the axioms. *In particular,* consistency of the axioms can not be proved within the system. *A fatal blow to Hilbert's program.*

The proof itself is a jewel of ingenuity. In a first step, Gödel shows how every formal statement, an admissible finite sequence of symbols, may be assigned a code, an integer number, in a constructive one-to-one fashion. Thence, assertions about formal statements may be interpreted in terms of integers, and properties like "provable from the axioms"may be expressed in the axiomatic system. The second step is to write down a special statement $\mathcal{S}$ *whose interpretation is "*N *can not be proved from the axioms", where* N *is precisely the number of* $\mathcal{S}$*. If the axiomatic system is consistent,* $\mathcal{S}$ *can not be proved nor disproved, which implies that it is true!*

Gödel's theorems have been used as an argument in favor of human over artificial intelligence: arguably, they show that humans are able to identify true statements that automatic machines, conditioned by formal rules, could never find. More certainly, these results prove that there are limitations to the axiomatic formulation of Mathematics, just as the uncertainty principle of quantum physics set fundamental limits to how much of reality can be aprehended experimentally.

Marcelo Viana is Professor of Mathematics and Chair for Scientific Activities at IMPA, Rio de Janeiro. He got his BSc from the University of Porto in 1984, and the PhD degree from IMPA in 1990. His research interests deal with Dynamical Systems and Ergodic Theory, especially the geometric and statistical properties of so-called chaotic systems. He was a plenary speaker at the ICM98-Berlin and at the International Congress of Mathematical Physics ICMP94-Paris, and an invited speaker at the ICM94-Zurich. In 1998 he received the Third World Academy of Sciences Award in Mathematics. He is an editor or editorial board member for several journals, including Portugaliae Mathematica. He enjoys reading and listening to music, and is a mild supporter of football teams Sporting (Lisbon) and Botafogo (Rio de Janeiro).

[2] Even the axiomatization of Physics, Hilbert's 6th problem!

Flies, weeds and statistical mechanics

The flies are two species of fruit flies of the genus *Drosophila*; the weeds are two species of mustards of the genus *Arabidopsis*; the statistical-mechanical techniques are applied by a six-person Harvard-Cornell-Washington University-North Carolina State team (Bustamante, Nielsen, Sawyer, Olsen, Purugganan, Hartl) and the results are reported in the April 4 2002 *Nature*. The goal is to tease out the pressure of natural selection on individual genes; they use a sophisticated "analytical method that borrows information from all the genes to make inferences about the magnitude of selection for any individual gene." The method, a "hierarchical bayesian analysis", leads to analytically intractable calculations. The authors handle them with Monte Carlo Markov Chain computation scheme borrowed from thermodynamics. The title of the work is "The cost of inbreeding in *Arabidopsis*".

The Erdös Prizes

In the April 5 2002 *Science* Charles Seife has a News Focus piece entitled "Erdös's Hard-to-win Prizes Still Draw Bounty Hunters." Paul Erdös died in 1996, but his personal, quirky influence lives on through the prizes he offered for solutions to problems he found intriguing. The prize would be proportional to the difficulty of the problem. There are \$10 problems, \$25 problems, and a couple worth over \$1000. Since his death the prizes have been administered by his long-time friend and associate Ronald Graham (U. C. San Diego), who will send a winner an Erdös-signed check ("suitable for framing") and another of his own, suitable for cashing. Graham "estimates that the outstanding bounties on unsolved problems total about \$25,000 " but does not seem to be worried about a run on the bank. A special case of a \$1000 problem was worth a Fields Medal for Klaus Roth (University College, London) in 1958.

DNA Computer solves a hard problem

Here's the problem: assign values 0 (False) or 1 (True) to the 20 variables A, B, C, D, E, F, G, H, I, J, K, L, M, N, O, P, Q, R, S, T so that the following 24-fold product gives the value 1:

$$(c+p+R)(E+L+i)(m+b+T)(L+h+e)(S+d+F)(I+L+e)(a+D+k)(M+b+s)(E+Q+I)(O+I+q)(e+i+l)(F+K+D)(o+q+G)(f+S+M)(l+i+E)(L+A+N)(T+C+B)(J+g+h)(e+I+l)(R+t+C)(j+r+p)(A+k+n)(H+g+o)(H+P+j)=1,$$

where $a=1-A, b=1-B$ etc., and $+$ stands for the logical "or".

The (unique) solution ($A=0$, $B=1$, $C=0$, $D=0$, $E=0$, $F=0$, $G=1$, $H=1$, $I=0$, $J=1$, $K=1$, $L=1$, $M=0$, $N=0$, $O=1$, $P=1$, $Q=1$, $R=0$, $S=0$, $T=0$) was found by a DNA computer in Pasadena, programmed by a Cal Tech - USC team (R. S. Braich, N. Chelyapov, C. Johnson, P. W. K. Rothemund, L. Adelman). "The DNA computation ... exhaustively searched all 2^{20} $(1,048,576)$ possible truth assignments in the process of finding the unique satisfying assignment." The work, described in a Research Article in the April 19 2002 Science, was picked up in the March 19 2002 New York Times, in a piece by George Johnson: "In Classic Math Riddle, DNA Gives a Satisfying Answer" available online. After joking about Mick Jagger and "Can't get no satisfaction," Johnson gives an apt real-world interpretation, corresponding to $(a+C+b)(c+E+F)(e+a+B)...$:

"Suppose Alice will attend a party only if Caroline does and Bobby doesn't, while Caroline insists that Eric and Francesca be there. Eric, though, refuses to be in the same room with Alice unless Bobby is there to distract her attention. Try to accommodate 20 such prima donnas and there are more than a million (2 to the 20th power) possible combinations to consider."

He notes that "The computation, which took four days of lab work to carry out, would have gone much faster with a regular old computer." In fact the team's report ends by saying "Despite our successes, and those of others, in the absence of technical breakthroughs, optimism regarding the creation of a molecular computer capable of competing with electronic computers on classical computational problems is not warranted." The team goes on to suggest specialized contexts in which molecular computation, as we know it today, might nevertheless be valuable. Johnson's take on the experiment: "What was remarkable was that a swarm of DNA molecules could be coaxed into solving a problem that would flummox an unaided human brain."

Poincaré conjecture on NPR, and in the Times

On Tuesday morning April 16 2002, listeners to National Public Radio's Morning Edition would have heard Bob Edwards say: "A British mathematician says he's found a way to solve a 100-year-old math mystery. Martin Dunwoody at Southampton University has been working on something called 'the Poincaré conjecture;' it suggests a kind of universal quality of multidimensional space. For example, mathematicians inspired by the conjecture already have proven that in two dimensions the surface of objects like a sphere and a tabletop are similar, but no one has proved the conjecture true in 3-dimensional space. If Dunwoody has solved it, he'll win a million dollars, but not until people such as Arthur Jaffe say it's correct. Jaffe is Professor of Math at Harvard and President of the Clay Mathematics Institute. The Institute will award the million-dollar prize for solving of one of seven math mysteries." Their minds befogged with images of tabletops and spheres, they would have heard Arthur Jaffe explain that the Poincaré conjecture "is regarded as one of the major outstanding problems of the field." Edwards questions him on the status of Dunwoody's claim. Jaffe: "There's a little skepticism." Edwards asks about the other six mysteries, and then "Shouldn't these great minds be working on cancer, or something?" Jaffe answers: "We feel that mathematics is really at the basis of all of science. Cancer of course is important. But these fundamental questions in mathematics have a way of coming up in every field of life." And he ends with: "We think it's very important that the brightest young people in the country, some of them, think about these questions which don't get quite as much publicity as cancer or other medical research at the moment."

Cellular automata at the seashore

A "letter to *Nature*," appearing in the October 25 2001 issue (and picked up in the March 29 2002 email journal *ScienceWeek*) explains how "an empirically derived cellular automaton model of a rocky intertidal mussel bed based on local interactions correctly predicts large-scale spatial patterns observed in nature." The thick-and-thin pattern of mussel colonisation on a typical mussel bed has a fractal-like aspect. J. Timothy Wooton (Chicago) analysed the factors affecting the spread of a mussel colony, including competition from other organisms, the impact of waves, and the tendency of mussels to attach themselves to other mussels. He gathered data for six years at 1400 reference points in a mussel bed on Tatoosh Island, Washington, used the data to specify transition probabilities for a cellular automaton model of the bed, and ran the model for 500 (simulated) years. At the end, the patterns exhibited by the model were found to be in excellent agreement with those occurring in on the site, showing that in this case "processes such as species interactions that occur at a local scale can generate large-scale patterns seen in nature" (the quote from *ScienceWeek).*

The Differential Equations of Pathogen Virulence

An imperfect vaccine can lead to increased virulence in a pathogen to the point where "overall mortality rates are unaffected, or even increase, with the level of vaccination coverage." This in a letter to *Nature* (*Imperfect vaccines and the evolution of pathogen virulence,* December 13 2001) from an Edinburgh team led by Sylvain Gandon and Margaret Mackinnon. Gandon, Mackinnon and their collaborators drew their conclusions from the long-term behavior of a system of differential equations, which were set up to analyze the long-term effect of vaccines designed to reduce pathogen growth rate and/or toxicity (as opposed to "infection-blocking" vaccines). The equations are nonlinear but simple in form. The population has two classes of hosts: those that are fully susceptible to the pathogen (density of uninfected x and infected y) and those that are partially immune (density of uninfected x' and infected y'). The system is a set of four differential equations in these unknowns.

Dynamic Catastrophe Theory

The September 14 2001 *Science* has an article by David J. Wales (Universal Chemical Laboratories, Cambridge UK) on a new application of catastrophe theory to the study of the kind of potential energy "landscapes" that occur in complicated energy-minimization problems like protein folding. His principal result in this context is "a quantitative connection between the potential energy barrier, the path length, and the lowest vibrational frequencies for a steepest-descent path linking a minimum and a transition state. This result may appear counterintuitive, for one might suppose that these quantities are independent." In a commentary piece ("Flirting with Catastrophe") in the same issue of Science, Robert Leary (San Diego Supercomputing Center) explains Wales' result in these terms: "He shows that neighboring stable states and the reaction paths that connect them can often be described by universal functional forms dictated by catastrophe theory." He mentions that "The results are validated with large databases of paths for various potentials, with excellent agreement where the minimum lies in close vicinity of the transition point" and concludes "Wales' application of catastrophe theory, an analytical tool not widely familiar to the scientific community, to energy landscapes is an exciting new development."

Tiny computer factors 15

The December 20/27 2001 *Nature* ran a "letter to Nature" from an IBM Almaden/Stanford University team describing their implementation of Peter Shor's quantum factoring algorithm using a molecule as quantum computer. (Ancillary details appear in an IBM Research News item: *IBM's Test-tube Quantum Computer Makes History.*) It takes 7 "qubits"to factor 15; Isaac Chuang and his team-mates custom synthesized a special molecule to accomodate and process them. In this molecule, a perfluorobutadienyl iron complex, the computing is done by the five Fluorine atoms and the two Carbon-13 atoms in the center. Those seven nuclear spins carry the qubits of a quantum computation. They can be programmed by radio pulses, they can interact, and they can be read out by nuclear magnetic resonance instruments.

The experiment depends crucially on properties of this special molecule, e.g.: "All seven spins in this molecule are remarkably well separated in frequency." But "the demands of Shor's algorithm clearly push the limits of the current molecule, despite its exceptional properties." And in fact the IBM News release concedes that "... it will be very difficult to develop and synthesize molecules with many more than seven qubits." This is still significant as the first physical realization of Shor's algorithm. The answer, 3 times 5, was obtained in about 720ms.

Waves of measles

An article in the December 13, 2001 *Nature* applies wavelets to the study of measles epidemics. In "Travelling waves and spatial hierarchies in measles epidemics," Bryan Grenfell (Cambridge), Ottar Bjornstad (Cambridge, Penn State) and Jens Kappey (Penn State) "use wavelet phase analysis" to "demonstrate recurrent epidemic travelling waves in an exhaustive spatio-temporal data set for England and Wales." One of their observations is that the increase in the vaccinated population from 1968 to the late 1980s generates a progressive increase in the period of this wave phenomenon.

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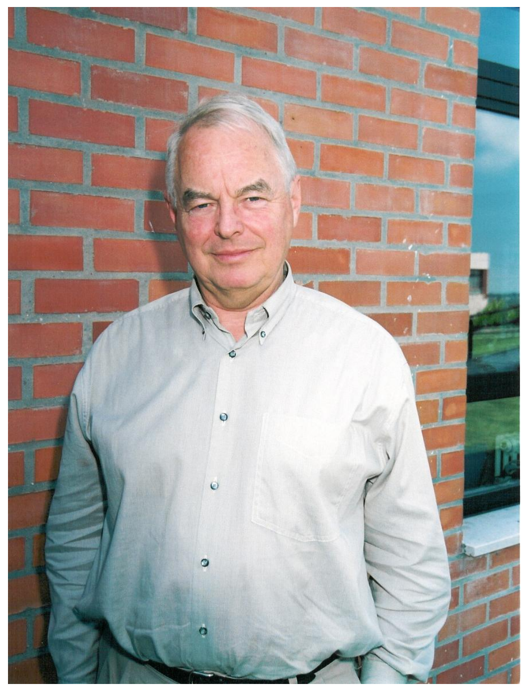
An Interview with with R. Tyrrell Rockafellar

There are obvious reasons for concern about the current excessive scientific specialization and about the uncontrolled breadth of research publication. Do you see a need for increasing coordination of events and publications in the mathematical community (in particular in the optimization community) as a way to improve quality?

There are too many meetings nowadays, even too many in some specialized areas of optimization. This is regrettable, but perhaps self-limiting because of constraints on the time and budgets of participants. In many ways, the huge increase in the number of meetings is a direct consequence of globalization—with more possibilities for travel and communication (e.g. e-mail) than before, and this is somehow good. The real problem, I think, is how to preserve quality under these circumstances. Meetings shouldn't just be touristic opportunities, and generally they aren't, but in some cases this has indeed become the case. I see no hope, however, for a coordinating body to control the situation.

An aspect of meetings that I believe can definitely have a bad effect on the quality of publications is the proliferation of "conference volumes" of collected papers. This isn't a new thing, but has gotten worse. In principle such volumes could be good, but we all know that it's not a good idea to submit a "real" paper to such a volume. In fact I often did that in the past, but it's clear now that such papers are essentially lost to the literature after a few years and unavailable. Of course, the organizers of a conference often feel obliged to produce such a book in order to justify getting the money to support the conference. But for the authors, the need to produce papers for that purpose is definitely a big distraction from their more serious work. Therefore it can have a bad effect on activities that are mathematically more important.

There are also too many journals. This is a difficult matter, but it may also be self-limiting. Many libraries now aren't subscribing to all the available journals. At my own university, for example, we have decided to omit many mathematical journals that we regard as costing much more than they are worth, and this even includes some older journals that are quite well known (I won't name names). And hardly a month goes by without the introduction of yet another journal. Besides the problem of paying for all the journals (isn't this often really a kind of business trick of publishers in which ambitious professors cooperate?), there is the quality problem that there aren't enough researchers to referee the papers that get submitted. Furthermore, one sees that certain fields of research that are perhaps questionable in value and content, start separate journals of their own and thereby escape their critics on the outside. The governments paying for all of it may some day become disillusioned, and that would hurt us all.



R. Tyrrell Rockafellar

Before I ask you questions about yourself and your work, let me pose you another question about research policy. How do you see the importance and impact of research in the professor's teaching activity? Do you consider research as a necessary condition for better university teaching?

Personally, I believe that an active acquaintance with research is important to teaching mathematics on many levels. The nature of the subject being taught, and the kind of research being done, can make a big difference in this, however. Ideally, mathematics should be seen as a thought process, rather than just as a mass of facts to be learned and remembered, which is so often the common view. The thought process uses logic but also abstraction and needs to operate with a clear appreciation of goals, whether coming directly out of applications or for the sake of more complete insights into a central issue.

Even with standard subjects such as calculus, I think it's valuable to communicate the excitement of the ideas and their history, how hard they were to develop and understand properly—which so often reflects difficulties that students have themselves. I don't see how a teacher can do that well without some direct experience in how mathematics continues to grow and affect the world.

On the higher levels, no teacher who does not engage in research can even grasp the expanding knowledge and prepare the next generation to carry it forward. And, practically speaking, without direct contact with top-rate researchers, a young mathematician, no matter how brilliant, is doomed to a scientifically dull life far behind the frontiers.

You started your career in the sixties working intensively in convex analysis. Your book "Convex Analysis", Princeton University Press, 1970, became a landmark in the field. How exciting was that time and how do you see now the impact that the book had in the applied mathematical field?

C. Carathéodory, W. Fenchel, V. L. Klee, J.-J. Moreau, F. A. Valentine,... Who do you really think that set the ground for convex analysis? Werner Fenchel?

Was it A. W. Tucker himself who suggested the name "Convex Analysis"? What are your recollections of Professor Tucker and his influential activity?

Some of the history of "convex analysis" is recounted in the notes at the ends of the first two chapters of my book Variational Analysis, written with Roger Wets. Before the early 1960's, there was plenty of convexity, but almost entirely in geometric form with little that could be called "analysis". The geometry of convex sets had been studied by many excellent mathematicians, e.g. Minkowski, and had become important in functional analysis, specifically in Banach space theory and the study of norms. Convex functions other than norms began to attract much more attention once optimization started up in the early 1950's, and through the economic models that became popular in the same era, involving games, utility functions, and the like. Still, convex functions weren't handled in a way that was significantly different from that of other functions. That only came to be true later.

As a graduate student at Harvard, I got interested in convexity because I was amazed by linear programming duality and wanted to invent a "nonlinear programming duality". That was around 1961. The excitement then came from all the work going on in optimization, as represented in particular by the early volumes of collected papers being put together by Tucker and others at Princeton, and from the beginnings of what later become the sequence of Mathematical Programming Symposia. It didn't come from anything in convexity itself. At that time, I knew of no one else who was really much interested in trying to do "new" things with convexity. Indeed, nobody else at Harvard had much awareness of convexity, not to speak of optimization.

It was while I was writing up my dissertation—focused then on dual problems stated in terms of polar cones—that I came across Fenchel's conjugate convex functions, as described in Karlin's book on game theory. They turned out to be a wonderful vehicle expressing for "nonlinear programming duality", and I adopted them wholeheartedly. Around the time the thesis was nearly finished, I also found out about Moreau's efforts to apply convexity ideas, including duality, to problems in mechanics.

Moreau and I independently in those days at first, but soon in close exchanges with each other, made the crucial changes in outlook which, I believe, created "convex analysis" out of "convexity". For instance, he and I passed from the basic objects in Fenchel's work, which were pairs consisting of a convex set and a finite convex function on that set, to extended-real-valued functions implicitly having "effective domains", for which we moreover introduced set-valued subgradient mappings. Nevertheless, the idea that convex functions ought to be treated geometrically in terms of their epigraphs instead of their graphs was essentially something we had gotten from Fenchel.

Less than a year after completing my thesis, I went to Copenhagen to spend six months at the institute where Fenchel was working. He was no longer engaged then in convexity, so I had no scientific interaction with him in that respect, except that he arranged for Moreau to visit, so that we could talk.

Another year later, I went to Princeton for a whole academic year through an invitation from Tucker. I had kept contact with him as a student, even though I was at Harvard, not Princeton, and had never actually met him. (He had helped to convince my advisor that my research was promising.) He had me teach a course on convex functions, for which I wrote the lecture notes, and he then suggested that those notes be expanded to a book. And yes, it was he who suggested the title, Convex Analysis, thereby inventing the name for the new subject.

So, Tucker had a great effect on me, as he had had on others, such as his students Gale and Kuhn. He himself was not a very serious researcher, but he believed in the importance of the new theories growing out of optimization. With his personal contacts and influence, backed by Princeton's prestige, he acted as a major promoter of such developments, for example by arranging for "Convex Analysis" to be published by Princeton University Press. I wonder how the subject would have turned out if he hadn't moved me and my career in this way.

I think of Klee (a long-time colleague of mine in Seattle, who helped me get a job there), and Valentine (whom I once met but only briefly), as well as Caratheodory, as involved with "convexity" rather than "convex analysis". Their contributions can be seen as primarily geometric.

Since the mid seventies you have been working on stochastic optimization, mainly with Roger Wets. It seems that it took a long while to see stochastic optimization receiving proper attention from the optimization community. Do you agree?

I owe my involvement in stochastic programming to Roger Wets. This was his subject when we first became friends around 1965. He has always been motivated by its many applications, whereas for me the theoretical implications, in particular the ones revolving around, or making use of duality, provided the most intriguing aspects. We have been good partners from that perspective, and the partnership has lasted for a long time.

Stochastic programming has been slow to gain ground among practitioners for several reasons, despite its obvious relevance to numerous problems. For many years, the lack of adequate computing power was a handicap. An equal obstacle, however, has been the extra mental machinery required in treating problems in this area and even in formulating them properly. I have seen that over and over, not just in the optimization community but also in working with engineers and trying to teach the subject to students. A different way of thinking is often needed, and people tend to resist that, or to feel lost and retreat to ground they regard as safer. I'm confident, though, that stochastic programming will increasingly be accepted as an indispensable tool for many purposes.

Your recent book "Variational Analysis", Springer-Verlag, 1998, with Roger Wets, emerges as an overwhelming life-time project. You say in the first paragraph of the Preface: "In this book we aim to present, in a unified framework, a broad spectrum of mathematical theory that has grown in connection with the study of problems of optimization, equilibrium, control, and stability of linear and nonlinear systems. The title Variational Analysis reflects this breadth." How do you feel about the book a few years after its publication? Has the purpose of forming a "coherent branch of analysis" been well digested by the book audience?

That book took over 10 years to write—if one includes the fact that at least twice we decided to start the job from the beginning again, totally reorganizing what we had. In that period I had the feeling of an enormous responsibility, but a joyful burden one even if involved with pain, somewhat like a woman carrying a baby within her and finally giving birth. I am very happy with the book (although it would be nice to have an opportunity to make a few little corrections), and Wets and I have heard many heart-warming comments about it. Also, it has won a prize[1].

Still, I have to confess that I have gone through a bit of "*post partum* depression" since it was finished. It's clear—and we knew it always —that such a massive amount of theory can't be digested very quickly, even by those who could benefit from it the most. Another feature of the situation, equally predictable, is that some of the colleagues who could most readily understand what we have tried to do often have their own philosophies and paradigms to sell. It's discouraging to run into circumstances where developments we were especially proud of, and which we regarded as very helpful and definitive, appear simply to be ignored.

But in all this I have a very long view. We now take for granted that "convex analysis" is a good subject with worthwhile ideas, yet it was not always that way. There was actually a lot of resistance to it in the early days, from individuals who preferred a geometric presentation to one targeting concepts of analysis. Even on the practical plane, it's fair to say that little respect was paid to convex analysis in numerical optimization until around 1990, say. Having seen how ideas that are vital, and sound, can slowly win new converts over many years, I can well dream that the same will happen with variational analysis.

Of course, in the meantime there are many projects to work on, whether directly based on variational analysis or aimed in a different direction, and such matters are keeping me thoroughly busy.

Nonlinear optimization has been also part of your research interests, in particular duality and Lagrange multiplier methods. Nonlinear optimization has been recently enriching its classical methodology with new techniques especially tailored to simulation models that are expensive, ill-posed or that require high performance computing. Would you like to elaborate your thoughts on this new trend?

The growth of numerical methodology based on duality

[1] *Frederick W. Manchester Prize (INFORMS, 1997).*

and new ways of working with, or conceiving of, Lagrange multipliers has been thrilling. Semi-definite programming fits that description, but so too do the many decomposition schemes in large-scale optimization, including optimal control and stochastic programming. Also in this mix, at least as close cousins, are schemes for solving variational inequality problems.

I've been active myself in some of this, but on a more basic level of theory a bigger goal has been to establish a better understanding of how solutions to optimization problems, both of convex and nonconvex types, depend on data parameters. That's essential not only to numerical efficacy and simulation, but also to the stability of mathematical models. I find it to be a tough but fascinating area of research with broad connections to other things. It requires us to look at problems in different ways than in the past, and that's always valuable. Otherwise it won't be possible to bring optimization to the difficult tasks for which it is greatly needed in economics and technology.

Let me now increase my level of curiosity and ask you more personal questions. The George B. Dantzig Prize (SIAM and Mathematical Programming Society, 1982), the The John von Neumann Lecture (SIAM, 1992), and the John von Neumann Theory Prize (INFORMS, 1999) are impressive recognitions. However, it is clear that it is neither recognition nor any other oriented-career goal that keeps you moving on. What makes you so active at your age? Are you addicted to mathematics?

It's the excitement of discovering new properties and relationships—ones having the intellectual beauty that only mathematics seems able to bring—that keeps me going. I never get tired of it. This process builds its own momentum. New flashes of insight stimulate curiosity more and more.

Of course, a mathematician has to be in tune with some of the basics of a mathematical way of life, such as pleasure in spending hours in quiet contemplation, and in dedication to writing projects. But we all know that this somewhat solitary side of mathematical life also brings with it a kind of social life that few people outside of our professional world can even imagine. The frequent travel that's not just tied to a few laboratories, the network of friends and research collaborators in different cities and even different countries, the extended family of former students, and the interactions with current students—what fun, and what an opportunity to explore music, art, nature, and our many other interests. All these features keep me going too.

Recently, at the end of a live radio interview by telephone that was being broadcast nationally in Australia, I was asked whether I really liked mountain hiking and backpacking. The interviewer had seen that about me on a web site and appeared to be incredulous that someone with such outdoor activities could fit her mental picture of a mathematician. So little did she know about the lives we lead!

Have you ever felt that a result of yours was unfairly neglected? Which? Why?

Yes, I have often felt that certain results I had worked very hard to obtain, and which I regarded as deep and important, were neglected. That was the case in the early days and still goes on now. For instance, the duality theorems I developed in the 1960's, connecting duality with perturbations, were ignored for a long time while most people in optimization thought only about "Lagrangian duality". And in the last couple of years, I and several of my students have worked very hard at bringing variational analysis to bear on Hamilton-Jacobi theory, but despite strong theorems can't seem to get attention from the PDE people who work in that subject.

In most cases the trouble has come from the fact that new ideas have been involved which other people didn't have the time or energy to appreciate. That can be an unhappy state of affairs, but time can change it. I've never been seriously bothered by it and have simply operated on the principle that good ideas will come through eventually. This has in fact been my experience.

Anyway, there are always so many other exciting projects to work on that one can't be very distracted by such disappointments, which may after all only be temporary.

What would you like to prove or see proven that is still open?

Oh, this is a hard kind of question for me. I belong to the class of mathematicians who are theory-builders more than problem-solvers. I get my satisfaction from being able to put a subject into a robust new framework which yields many new insights, rather than from cracking a hard nut like Fermat's last theorem. Of course, I spend a lot of time proving a lot of things, but for me the main challenge ultimately is trying to get others to look at something in a different and better way. Of course, that can be frustrating! But, to tie it in with an earlier question, a key part is getting students to follow the desired thought patterns. That's good for them and also for the theoretical progress. Without having been so deeply engaged with teaching for many years, I don't think I could have gone as far with my research.

So, if I would state my own idea of an open challenge, it would be, for instance, on the grand scale of enhancing the appreciation and use of "variational analysis" (by which I don't just mean my book!). I do nonetheless have specific results that I would like to be able to prove in several areas, but they would take much more space to describe.

What was the most gratifying paper you ever wrote? Why?

Oh, again very hard to say. There are so many papers, and so many years have gone by. And I've worked on so many different topics, often in different directions. Anyway, for "gratification" it's hard to beat books. The two books that I'm most proud of are obviously Convex Analysis and Variational Analysis. Both have greatly gratified me both "externally" (recognition) and "internally" (personal feeling of accomplishment). So far, Convex Analysis has been the winner externally, but Variational Analysis is the winner internally.

Interview by Luís Nunes Vicente (University of Coimbra)

R. Tyrrell Rockafellar completed his undergraduate studies at Harvard University in 1957, and his PhD in 1963 at Harvard as well. He has been in the faculty of the Department of Mathematics of the University of Washington since 1966.

His research and teaching interests focus on convex and variational analysis, optimization, and control. He is well known in the field and his contributions can be found in several books and in more than one hundred papers.

Professor Rockafellar gave a plenary lecture in the conference Optimization 2001, held in Aveiro, Portugal, July 23-25, 2001.

Outline of a biography of Professor João Guerreiro

We start with a foreword, to put into perspective the background from which this outline of a biography of Professor Guerreiro takes shape. Although this text is based on objective and historical data, it is also a deeply personal and subjective testimony. It is intended to go beyond an account of Professor Guerreiro's mathematical and teaching career: we want to give an accurate account of the extraordinary impact this mathematician and professor had on successive generations of people who had the privilege of talking and working with him. At a time where, more than ever, the value and influence of scientists and teachers is gauged by the number of their research papers and citations, Professor Guerreiro is one of the clearest and most unquestionable counter-examples of the global validity of this rule, evidence that any search for objectivity in these matters must always be put properly into perspective and contextualized, otherwise there is an obvious danger of endorsing the most absurd mechanical schemes, perverting the aim which a search for such criteria presupposes.

This outline is not a true history of mathematics text; for this a different approach would be needed, expanded and with a more neutral approach to the subject, including extended research on primary sources. What we want now is something more immediate, but no less just or less urgent: we intend to highlight this central personality of the academic life in the Faculty of Sciences of Lisbon University in the last 40 years who, among other important matters, helped successive generations of students to discover mathematics; in many cases it was his contribution that made students change their perception of mathematics, seeing it in a new way, passionate and, why not, creative.

I

Professor Guerreiro had that quality, so rare and precious, which was to point out what was essential in a simple and elegant way.

In the second half of the 20th century Professor João Cosme Santos Guerreiro (Funchal, 27/9/1923 - Lisbon, 5/11/1987) stands out as one of the most important figures in the Faculty of Sciences of Lisbon University. He graduated in Mathematical Sciences from this Faculty in 1954, and had a grant from *Instituto para a Alta Cultura*[1] to start research in *Centro de Estudos Matemáticos de Lisboa*[2], under the supervision of José Sebastião e Silva.

João Guerreiro
(Photo kindly loaned by Professor Campos Ferreira)

He was an assistant teacher in *Instituto Superior de Agronomia* from March 1957 to October 1958[3], where he worked with his supervisor, who was also teaching at the same institution. Then he became a full-time scholarship holder of *Centro de Estudos Nucleares*[4]. He entered the *Faculdade de Ciências de Lisboa* in 1959 as

[1] Institute for Higher Culture
[2] Centre for Mathematical Studies of Lisbon
[3] Here he had a special contract as a substitute for Professor Renato Coelho, who was away in Italy.
[4] Centre of Nuclear Studies

second lecturer. Here he worked successively with two major figures in Portuguese mathematics, José Vicente Gonçalves and José Sebastião e Silva. He gained his Ph.D in 1962, with a thesis entitled *Teoria directa das distribuições sobre uma variedade*[5] where he generalizes to an arbitrary differential manifold results that Sebastião e Silva established for R^N. He became *Professor extraordinário* in 1968, and Full Professor in 1973, the decision being made unanimously on both occasions.

In the Faculty of Sciences of Lisbon University it was Professor Guerreiro and his group of teaching assistants who extended the inheritance of Sebastião e Silva and of the so-called 40s generation of Portuguese mathematicians; it was they who continued the tradition of the *Centro de Estudos Matemáticos de Lisboa*, which was later to be continued by *Centro de Matemática e Aplicações Fundamentais*[6]. So the fact that Guerreiro was the first head of the Board of this Centre (then under the supervision of *Instituto para a Alta Cultura*) is deeply significant. After the revolution of April 25, 1974, the Portuguese Society of Mathematics was rebuilt, and Guerreiro became its first General Secretary. At the same time *Portugaliae Mathematica* was restructured in order to improve its standards and its world ranking (for this the work of Professors João Paulo Carvalho Dias and Alfredo Pereira Gomes was crucial, supported by SPM), and the *Boletim* of the Society was also restarted[7].

He was the supervisor of Ph.D. theses by Maria Higina Rendeiro Marques (*Secções-distribuições vectoriais e teorema dos núcleos em espaços fibrados*[8], 1972) and of Carlos Sarrico (*Produtos distribucionais multiplicativos*[9], 1988), the latter being examined a couple of months after Professor Guerreiro's death. He made a crucial contribution to the first Master's course on Applied Mathematics which Instituto Superior Técnico organized at the beginning of the 80s, and where he taught *Functional Analysis*. This course was essential for the creation, in the late 80s, in the same Institute, of its first graduate course in an area of mathematics, Applied Mathematics and Computation. In his later years he collaborated with other universities, including Évora University and Madeira University. During this time he also lectured on the *History of Mathematics*. He was one of the main organizers and the Chairman of the Organizing Committee of the International Meeting *Anastácio da Cunha, o Matemático e o Poeta*, which took place in 1987, in Lisbon, at Forum Picoas. This meeting was the touchstone for a reformulation and a restart in both the research and the popularization of the history of mathematics in Portugal. He was the translator into Portuguese of Dirk Struik's book *A Concise History of Mathematics*[10], but he died before completing it[11]. A second edition of this book was published in 1992, with an appendix on the history of Portuguese mathematics written by Professors José Joaquim Dionísio and Augusto Franco de Oliveira, something that Guerreiro had in mind to do when he started his translation. At the time of his death he was the Chairman of the Board of the General Assembly of the Portuguese Society of Mathematics.

II

João Cosme Santos Guerreiro was part of a whole generation of teachers who profoundly marked the life of the Mathematics Department of the Lisbon Faculty of Sciences in the sixties and seventies. This was due not only to the mathematics culture they were able to transmit, but also to their pedagogic and human qualities. He had this rare gift (which can only exist in those who live mathematics from within, in those who breathe it) of being able to transmit the essence of each subject, underlining the elegance of mathematical reasoning. His classes were not only a didactic but also an aesthetic model of teaching; he knew how to show students the simplicity and the beauty of what is profound. It is true that many small mathematical points were left open, that the proofs of results were often only outlined. When it came to choosing between what was essential and what was not, Guerreiro did not hesitate: the student could complete by himself what was left unfinished in the class, this work was part of the learning process; what in fact only the deeply thought experience of mathematics could give, this Guerreiro would present to the student, in such a way that he could understand its essence, so that he could infer the complex connections which resulted from it.

In this way he contributed decisively to the human and scientific education of successive generations of FCUL students. It is no wonder then that many see in the subjects he taught (from first year Analysis courses, to the final year Higher Analysis subjects), the turning point in their learning of mathematics, in their way of thinking and feeling about it. Those who discovered *Topology* with him, when this subject was the introductory part of his course on functions of complex variables, a yearly subject taught in the early 70s in the third year of the Mathematics course, learned how it was

[5] Direct theory of distributions on a manifold

[6] Centre of Mathematics and Fundamental Applications

[7] There was also an unsuccessful attempt to restart publication of *Gazeta de Matemática*.

[8] Vector sections-distributions and kernel theorem in fibre spaces

[9] Multiplicative distributional products

[10] Published in Portugal in 1989 by Gradiva, volume 33 of the series *Ciência Aberta*, with the title *Uma História Concisa das Matemáticas*.

[11] The translation was completed by Professor Paulo Almeida.

possible to teach an introductory theory of this central area of mathematics, rigorously obtaining results of extreme complexity, and at the same time without losing touch of a geometrical and intuitive vision of the subject, which to me seems essential, not only in the learning of mathematics but also in all the work that follows, including that of scientific research.

But it was not only as a lecturer that Guerreiro was outstanding. He was also an innovator in his way of being a teacher; there was something extraordinarily warm and sincere in his deeply human dimension, in the way in which he related to his students and colleagues. This is the same feeling as in the testimony of his student during the sixties, and later his assistant and colleague Augusto Franco de Oliveira: "Santos Guerreiro, more than anyone else (and initiating a new style), was approachable; he honoured and ennobled us with his human warmth and friendship. For more than 20 years it is he that we see as an example of the human condition of the Teacher, of the ability to communicate, as a paradigm of the modernity and elegance of mathematics and of its teaching (*Functional Analysis*, *Topology*, *Complex Variables*, *Differentiable Manifolds*, *Differential Geometry*). It was essentially through him, and by him, before, during, and after the political change of the 25th April 1974, with selfless sacrifice of his working conditions as a scientist, that the conditions and hopes of the scientific and academic life of the young *guerreiros* were established, today almost all full professors, and of the numerous others who had the good fortune to be his students or assistants."

Guerreiro was part of a brilliant group of teachers of the Faculty of Sciences of Lisbon who in the sixties and seventies defined a golden epoch in the teaching of mathematics at university level. Lecturers as José Vicente Gonçalves, José Sebastião e Silva, Fernando Veiga de Oliveira, José Joaquim Dionísio, António Simões Neto, Fernando Dias Agudo, Maria Luisa Galvão, Margarita Ramalho, António St. Aubyn, and others - this list is by no means exhaustive-, marked an unforgettable epoch for those who lived through it, and whose history is still to be written. Those were different times, with far fewer students (in the mathematics courses[12]), with the courses structured, in content as well as in form, according to a very different philosophy from today's, and that, before April 25, 1974, worked in a social and political framework that had little in common with present times. Therefore, any comparison must be made carefully, bearing these differences in mind. But it is poignant to know that students then, in contrast to the common feeling nowadays, generally saw in the majority of their lecturers cultured people, with knowledge that often went far beyond the boundaries of mathematics, and this was something that would overflow from their classes; mathematics was only one of the cultural sides of these teachers, the one about which they talked passionately during their classes[13].

Taking a general overview of higher education institutions in Portugal, it is teachers like Professor Guerreiro who make the Faculty of Sciences of Lisbon something to be remembered by its students as irreplaceable and invaluable, as a real singularity, as a school, in the noblest sense of the word, that makes his students proud of having been in that particular place at that particular time.

III
Published works by Professor Santos Guerreiro

1. Research publications

1. Les changements de variable en théorie des distributions, *Portugal. Math.*, 16, pp. 57-81, 1957.

2. La multiplication des distributions comme application linéaire continue, *Portugal. Math.*, 18, pp. 55-67, 1959.

3. Teoria directa das distribuições numa variedade, Ph. D. thesis, *Portugal. Math.*, 22, pp.1-92, 1963.

4. Secções-distribuições em espaços fibrados, *Revista da Faculdade de Ciências de Lisboa*, 2nd series, A-Mathematical Sciences, 11, pp. 223-246, 1965/66.

5. Cohomologia das correntes numa variedade com bordo, *Proceedings of the First Luso-Spanish Mathematical Meeting*, pp. 99-100, Lisboa, 1972.

6. Sobre as distribuições quase-periódicas, *Proceedings of the First Luso-Spanish Mathematical Meeting*, pp. 110-112, Lisboa,1972.

7. Sobre as distribuições quase-periódicas vectoriais. Uma aplicação à equação das ondas, *Revista de la Universidad de Santander*, Número 2, Parte I, pp. 237-241, 1979.

2. Monographs, courses, and other works

1. *Elementos de Análise Funcional*, additional notes to the course on Higher Analysis, published by Associação de Estudantes da FCUL, 1959/60.

2. *Uma construção axiomática do Integral de Lebesgue* (Lecture notes by Professor Guerreiro's students), AEFCUL, 1964/65.

[12] Up to 1986/87 the Faculty of Sciences of Lisbon also ran classes for the first years of the Engineering Courses.

[13] The pressure for publication of original research, which in those days did not exist, may explain something of this change, but it in no way seems sufficient; other parameters must be analysed.

3. *Curso de Geometria Superior. II. Variedades diferenciáveis*, Instituto para a Alta Cultura, Publications of Centro de Estudos Matemáticos de Lisboa, FCUL, 1964/65.

4. *Matemáticas Gerais* (Engineering and Geology Courses) - FCUL, 1966/67.

5. *Curso de Matemáticas Gerais*[14]

   Volume I. Conjuntos. Noções de Álgebra. 1st edition 1967. 2nd edition 1972. Livraria Escolar Editora, Lisboa.

   Volume II. Números reais. Séries. Funções contínuas. 1st edition 1967. 2nd edition 1973. Livraria Escolar Editora, Lisboa.

   Volume III. Derivadas e integrais das funções de variável real. 1st edition 1968. Livraria Escolar Editora, Lisboa.

   Volume IV. Noções de Álgebra Linear. 1970. Livraria Escolar Editora, Lisboa.

6. *Anastácio da Cunha e as Matemáticas em Portugal*, Catalogue # 23, Biblioteca Nacional, Exhibition *José Anastácio Da Cunha (1744-1787), o Matemático e o Poeta*, pp. 39-42, Lisboa, 1987 (This paper was posthumously included in the Proceedings of the International Conference *Anastácio Da Cunha (1744-1787), o Matemático e o Poeta*, pp. 27-30, Biblioteca Nacional-Casa da Moeda, 1990)

7. *Espaços Vectoriais Topológicos*, Colecção Textos e Notas 45, CMAF, 1990 (posthumous publication, organized by J. Campos Ferreira and J. Silva Oliveira)

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Luis Saraiva

CMAF / Faculdade de Ciências da Universidade de Lisboa

[14] Livraria Escolar Editora republished the first three volumes in a single tome, in 1989, with the title *Curso de Análise Matemática*.

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