BULLETIN

INTERNATIONAL CENTER FOR MATHEMATICS

DECEMBER 2007

23

CONTENTS

COMING EVENTS

February 19-22, 2008: International Conference on Mathematics and Continuum Mechanics

ORGANIZERS

António J. Mendes Ferreira (Univ. of Porto), Isabel Maria Narra de Figueiredo (Univ. of Coimbra) and Juha Videman (IST, Lisbon).

AIMS

The event focuses on a selected range of interdisciplinary topics handled from both a mathematical and an engineering applications point of view. Despite the apparent heterogeneity of the topics, they are certain to prompt interesting dialogue among the conveners.

The target audience is, besides engineers, physicists and mathematicians, graduate and Ph.D. students interested in doing research on problems related to Mathematics, Solid and Fluid Mechanics and Geophysics.

The conference will feature six thematic mini-symposia:

Computational Methods for Advanced Composites
Organizer: Pedro Camanho (FEUP, Porto)

Contact Mechanics
Organizer: Marius Cocou (CNRS, France)

Mathematics and the Atmospheric Sciences
Organizer: João Teixeira (NATO-URC, Italy)

Modelling of Industrial Processes
Organizer: Luisa Silva (ENSMP, France)

Numerical Analysis of Thin Structures
Organizer: Lourenço Beirão da Veiga (U. Milano, Italy)

Ocean Dynamics
Organizer: Aires dos Santos (IST, Lisbon).

Each symposium consists of one forty-five minute plenary session and two to four invited half-hour addresses. Some contributed papers will be selected for fifteen to twenty minute presentations and others to be on display in a poster session.

The event will take place at the University of Porto and the Proceedings will be published by CIM.

Invited and Plenary Speakers

Francisco Javier Llorca Martínez (Polytechnic Univ. of Madrid, Spain)
Moving surfaces and interfaces calculation in material forming

Silvestre Pinho (Imperial College, UK)
On the prediction of failure in laminated composites

Joris Remmers (University of Delft, The Netherlands)
to be announced

João Martins (IST, Lisbon)
Stability of quasi-static paths of finite-dimensional systems with Coulomb friction and persistent contact

Gianpietro Del Piero (Univ. di Ferrara, Italy)
Recent progresses in the modelling of material behavior

Lars-Erik Andersson (Linköping Univ., Sweden)
Existence and uniqueness for quasistatic contact problems with friction

Kevin Judd (Univ. of Western Australia, Australia)
Weather forecasting: It's about dynamics, it is not about statistics

Luca Bonaventura (Politecnico di Milano, Italy)
Flux form, conservative semi-Lagrangian schemes

A. Pier Siebesma (Royal Netherlands Meteorological Inst., The Netherlands)
Cloud structures and organisation

Thierry Coupez (CEMEF, École des Mines de Paris, France)
Moving surfaces and interfaces calculation in material forming

José César de Sá (FEUP, Porto)
Non-local models for localization in large deformations

Ramon Codina (CIMNE, Univ. of Catalunha, Spain)
to be announced

Dominique Chapelle (INRIA, France)
Fundamental and applicative challenges in the modelling and computations of shells

Carlo Lovadina (Univ. of Pavia, Italy)
A-posteriori error estimates for the Reissner-Mindlin plate problem

Harri Hakula (Helsinki Univ. of Technology, Finland)
Numerical shell eigenproblem benchmarks

Rui Ponte (Atmospheric and Environmental Research, Inc., USA)
Global ocean state estimation

Emanuel Ferreira Coelho (Univ. of Southern Mississippi, USA)
Operational oceanography: zoom-in modelling for local applications

Paulo Chambel (Hidromod, Modelação em Engenharia)
Towards an hydrodynamic and biogeochemical operational model of the Portuguese coast

Maria Valdivieso da Costa (ACTIMAR, France)
Mode water variability diagnosed from an eddy-permitting reanalysis of the North Atlantic

Henrique Coelho (Hidromod, Modelação em Engenharia)
Processes over submarine canyons

For more information about this event, see

paginas.fe.up.pt/~cim2008

June 12-14, 2008: CIM/CRM Workshop on Financial Time Series

Organizers

Paulo Teles (Porto School of Economics) and Pilar Muñoz (Technical University of Catalonia, Barcelona, Spain).

Aims

This workshop brings together leading experts and several other researchers on financial time series, enabling them to present and discuss the latest research and case studies on this important issue and consequently providing knowledge exchange.

Financial time series is an increasingly important topic nowadays. In fact, modelling financial data recorded over time has achieved such high standards that it is now possible to deal with most data encountered in practice. Nevertheless, several important issues remain to be improved requiring new research and different approaches. Presenting and discussing them is extremely important and useful. To this purpose, some of the best researchers will present their latest developments and will be open for discussion with other participants. Exchanging own experiences, problems, views and approaches will undoubtedly bring an important contribution and will stimulate new research in the field.

This Workshop takes place at the premises of CIM - Centro Internacional de Matemática.

Invited speakers

Daniel Peña (Univ. Carlos III de Madrid, Spain)

Esther Ruiz (Univ. Carlos III de Madrid, Spain)

Feridun Turkman (Lisbon Univ.)

Frank Diebold (Univ. of Pennsylvania, USA)

João Nicolau (ISEG, Lisbon Technical Univ.)

Nuno Crato (ISEG, Lisbon Technical Univ.)

Philip Hans Franses (Erasmus Univ. Rotterdam, The Netherlands)

For more information about the event, see

www.cim.pt/wfts2008

June 16-21, 2008: GAP VI - Workshop on Geometry and Physics

Organizers

Carlos Currás-Bosch (Univ. de Barcelona, Spain)

Rui Loja Fernandes (IST, Lisbon)

David Iglesias (CSIC, Madrid, Spain)

Eva Miranda (Univ. Autònoma de Barcelona, Spain)

San Vu Ngoc (Univ. de Rennes, France)

Ping Xu (Penn State Univ., USA)

Aims

GAP VI is the sixth edition of a series of "Séminaires Itinerants" that have taken place in different locations. This year the topic is Integrable Systems. There will be four mini-courses and several talks by participants.

The aim is to bring together young researchers from the two areas of Geometry and Physics and to fill in the GAP dividing this two deeply interconnected research fields.

The event is a joint CRM-CIM Workshop and will take place at CRM - Centre de Recerca Matemàtica, Barcelona (Bellaterra), Spain.

Mini-courses

Yves Colin de Verdière (Inst. Fourier, France)
Semi-classical Analysis of Integrable systems

Johannes Duistermaat (Univ. Utrecht, The Netherlands)
to be announced

Hakan Eliasson (Inst. Math. de Jussieu, France)
KAM for the non-linear Schrödinger equation

Rahul Pandharipande (Princeton Univ., USA)
Integrable systems and algebraic curves

For more information about the event, see

www.crm.cat/GAPVI

June 26-28, 2008: Workshop on Nonparametric Inference - WNI2008

Organizers

Carla Henriques (CMUC & Escola Superior de Tecnologia de Viseu)

Carlos Tenreiro (CMUC & Univ. of Coimbra)

Paulo Eduardo Oliveira (CMUC & Univ. of Coimbra)

Scientific committee

Antonio Cuevas (Univ. Autónoma de Madrid, Spain)

Emmanuel Candès (California Inst. of Techn., USA)

Enno Mammen (Univ. of Mannheim, Germany)

Irène Gijbels (Katholieke Univ. Leuven, Belgium)

Lászlo Györfi (Budapest Univ. of Technology and Economics, Hungary)

Paulo Eduardo Oliveira (CMUC & Univ. of Coimbra)

Phillippe Vieu (Univ. Paul Sabatier, Toulouse, France)

Aims

The goals of this workshop are:

- to illustrate active trends in a number of subjects in nonparametric statistics, including curve estimation, model checking, functional data, survival analysis, adaptive bandwidth choice and bootstrap;
- to give an opportunity for research students to develop their competence in nonparametric methods;
- to provide a meeting point for researchers in nonparametric inference, intending to contribute for the establishment of new links;
- additionally, it also hopes to contribute to incentive national research in non-parametric statistical topics.

The event will take place at the Department of Mathematics of the University of Coimbra.

INVITED SPEAKERS

Antonio Cuevas (Univ. Autónoma de Madrid, Spain)
On nonparametric estimation of boundary measures

Emmanuel Candès (California Inst. of Techn., USA)
to be announced

Enno Mammen (Univ. of Mannheim, Germany)
to be announced

Lászlo Györfi (Budapest Univ. of Technology and Economics, Hungary)
Nonparametric prediction of time series

Phillippe Vieu (Univ. de Paul Sabatier, Toulouse, France)
On Nonparametric functional data analysis

For more information about the event, see

www.mat.uc.pt/~wni2008

July 7-9, 2008: WEAA - Workshop on Estimating Animal Abundance

ORGANIZERS

Russell Alpizar-Jara (Dep. of Mathematics and CIMA, Univ. of Évora)

Anabela Afonso (Dep. of Mathematics and CIMA, Univ. of Évora)

João Filipe Monteiro (Dep. of Mathematics and CIMA, Univ. of Évora)

AIMS

This is an interdisciplinary workshop that intends to narrow the gap between statistical estimation theory for animal populations, and wildlife and fisheries applications of this methodology. The workshop will be an introductory overview of capture-recapture and distance sampling models and will include estimation of population size, survival rates and birth numbers. An emphasis will be placed on real examples and the importance of validation of model assumptions. Recent developments of capture-recapture applications to Epidemiological estimation problems could also be addressed.

Three days in a computer lab so participants will try out the programs: MARK, M-SURGE and U-CARE, and DISTANCE. Participants are encouraged to bring their own laptop and data sets for analyses. Additional selected data sets from fieldwork will also be available.

Topics will include:

- Closed and open capture-recapture models,
- The robust design,
- Designing capture-recapture studies,
- Multi-state capture-recapture models,
- Distance sampling methods.

The workshop will be held at the University of Évora.

INSTRUCTORS

Kenneth Pollock (North Carolina State Univ., USA)

Jean-Dominique Lebreton (CNRS, CEFE, France)

Theodore R. Simons (Cooperative Fish and Wildlife Research Unit, North Carolina State Univ., USA)

For more information about the event, see

www.eventos.uevora.pt/~weaa

July 21-26, 2008: CIM/UC Summer School on Dynamical Systems

This event will be held at the University of Coimbra.

September, 2008: International Meeting on Calculus of Variations and Applications

This event will be held at the New University of Lisbon.

Other CIM events in 2008:

Working Afternoons SPM/CIM

Hotel Quinta das Lágrimas, Coimbra

A joint initiative of the Portuguese Mathematical Society and CIM.

January 12, 2008 - Lie Algebras and Applications

Organizer: Helena Albuquerque (Univ. of Coimbra)

For more information, see

www.cim.pt/?q=spm_cim_lie_algebras_2008

March 1, 2008 - Graph Theory and Combinatorics

Organizer: Jorge Orestes Cerdeira (Instituto Superior de Agronomia, UTL)

For more information, see

www.cim.pt/?q=spm_cim_graphs_combin_2008

CIM short courses

Hotel Quinta das Lágrimas, Coimbra

February 8, 2008: Numerical Optimization: Theory and Practice

Lecturer: José Mario Martinez (Univ. Campinas, Brazil)

Schedule:

- 9:30 - 11:00 : Session 1
- 11:00 - 11:30 : Coffee-Break
- 11:30 - 13:00 : Session 2
- 13:00 - 15:00 : Lunch
- 15:00 - 17:00 : Session 3

For the abstract (in Portuguese) and registration, see

www.cim.pt/?q=numericaloptimization2008

For updated information on these events, see

www.cim.pt/?q=events

CIM News

Annual Scientific Council Meeting 2008

February 9, 2008, Coimbra

The CIM Scientific Council will meet in Coimbra, at the Hotel Quinta das Lágrimas, on February 9, to discuss the CIM scientific programme for 2009.

There will also be an opportunity to attend the Seminar of the Scientific Council Meeting, where the following talks will be delivered:

17:00 Rui Loja Fernandes (IST, Lisbon)
Stability of Leaves.

18:30 Irene Fonseca (Carnegie Mellon Univ., USA)
Variational Methods in Materials and Imaging.

For the detailed programme and registration, see

`www.cim.pt/?q=cscam08`

Annual Meeting of the ERCOM

March 7-8, 2008, Coimbra

The forthcoming ERCOM meeting will take place at the Hotel Quinta das Lágrimas, Coimbra, Portugal, on March 7 and 8, 2008.

Meeting of the General Assembly of CIM

April 5, 2008, Coimbra

The General Assembly of CIM will meet on April 5, 2008 in the CIM premises at the Astronomical Observatory of the University of Coimbra.

Research in Pairs at CIM

The programme is suspended until January 2008.

CIM on the Web

For updated information about CIM and its activities, see

`www.cim.pt`

Feature Article

Towards categorical behaviour of groups

Maria Manuel Clementino

CMUC/Departamento de Matemática, Universidade de Coimbra
3001-454 Coimbra, PORTUGAL

mmc@mat.up.pt

Abstract

We present a brief introduction to the study of homological categories, which encompass many algebraically-like categories, namely the category of groups, and also categories of topological algebras.

1. Introduction

Category Theory started more than 60 years ago, when Eilenberg and Mac Lane wrote a paper on natural transformations [11], having as role model a well-known example of natural equivalence:

For any vector space V over a field K, consider its dual V^* (i.e. the vector space of linear functionals from V to K). If V is finite-dimensional, then V^* has the same dimension as V, and so we can conclude that they are isomorphic, although there is no *natural* way of defining such isomorphism. This contrasts with the case of V^{**}, the dual of V^*. There is a (injective) linear transformation $\phi_V : V \to V^{**}$, which assigns to each $x \in V$ the linear transformation $\hat{x} : V^* \to K$, $f \mapsto \hat{x}(f) := f(x)$. Moreover, if V is finite-dimensional, ϕ_V turns out to be an isomorphism, defining this way a *natural equivalence*. That is, ϕ_V is not a mere equivalence between V and V^{**} but it is part of a collection $\phi = (\phi_V)_V$ of equivalences. To make this idea precise, in [11] Eilenberg and Mac Lane defined *categories*, *functors* (between categories) and then *natural transformations* (between functors).

Shortly after, the use of Category Theory proved to be useful in several areas of Mathematics. The notion of *abelian category* – encompassing abelian groups and, more generally, modules – became prominent. Quoting Mac Lane [20, Notes on Abelian Categories, page 209]:

"Shortly after the discovery of categories, Eilenberg and Steenrod [12] showed how the language of categories and functors could be used to give an axiomatic description of the homology and cohomology of a topological space. This, in turn, suggested the problem of describing those categories in which the values of such a homology theory could lie. After discussions with Eilenberg, this was done by Mac Lane [18, 19]. His notion of an "abelian bicategory" was clumsy, and the subject languished until Buchsbaum's axiomatic study [10] and the discovery by Grothendieck [15] that categories of sheaves (of abelian groups) over a topological space were abelian categories but not categories of modules, and that homological algebra in these categories was needed for a complete treatment of sheaf cohomology (Godement [14]). With this impetus, abelian categories joined the establishment."

Moreover, quoting now Borceux [2]:

An elementary introduction to the theory of abelian categories culminates generally with the proof of the basic diagram lemmas of homological algebra: the five lemma, the nine lemma, the snake lemma, and so on. This gives evidence of the power of the theory, but leaves the reader with the misleading impression that abelian categories constitute the most natural and general context where these results hold. This is indeed misleading, since all those lemmas are valid as well – for example – in the category of all groups, which is highly non-abelian.

However, in contrast with the smooth genesis of abelian categories, besides several attempts to identify relevant categorical features of the category of groups (cf. [16] for an account on the subject), it took a few decades until the right ingredient was identified. This was due to Bourn [6], who defined *protomodular category* and showed that a simple categorical condition (see condition (2) of Theorem 1) could become a key tool to handle with short exact sequences. In 1990, he presented his notion at the International Category Theory Meeting, CT90, but it took almost a decade until the mathematical community understood the potential of protomodularity. Indeed, the proposal, at CT99 (in Coimbra), of studying *semi-abelian categories* – which are in particular protomodular categories –, due to Janelidze, Márki and Tholen [16], was the main step for the recognition of the role of protomodularity in Categorical Algebra. The XXI century began with an explosion of results on protomodular and semi-abelian categories, being Bourn the main contributor. The monograph by Borceux and Bourn [3] contains the main achievements on the subject. While writing this monograph, the work of Borceux (together with the author of this article) on topological semi-abelian algebras [4, 5] led him to a new proposal for capturing the essential properties of group-like categories, which became known under the name *homological category*, basically because it turned out to be the right setting to develop Homological Algebra.

Throughout we will present a brief survey on these contributions.

2. Protomodularity

One of the key tools for Homological Algebra is the Short Five Lemma, which holds in abelian categories:

Short Five Lemma. *Given a commutative diagram*

$$\begin{array}{ccccccccc} 0 & \longrightarrow & K & \xrightarrow{u} & X & \xrightarrow{p} & Y & \longrightarrow & 0 \\ & & \downarrow{\scriptstyle a} & & \downarrow{\scriptstyle b} & & \downarrow{\scriptstyle c} & & \\ 0 & \longrightarrow & K' & \xrightarrow{v} & X' & \xrightarrow{q} & Y' & \longrightarrow & 0 \end{array}$$

with exact rows (i.e. p, q are regular epimorphisms and $u = \ker p$, $v = \ker q$), if a and c are isomorphisms, b is an isomorphism as well.

This result is still valid in the category $\mathscr{G}rp$ of groups and homomorphisms, hence it is not exclusive of abelian categories. The notion of protomodular category is based on a weaker form of this result, the Split Short Five Lemma, stated below. This statement makes sense only in *pointed categories*, that is categories with a zero object.

Definition. In a pointed category $\mathscr{C}$, the *Split Short Five Lemma* holds if, for any given commutative diagram

$$\begin{array}{ccccccccc} 0 & \longrightarrow & K & \xrightarrow{u} & X & \underset{p}{\overset{s}{\leftrightarrows}} & Y & \longrightarrow & 0 \\ & & \downarrow{\scriptstyle a} & & \downarrow{\scriptstyle b} & & \downarrow{\scriptstyle c} & & \\ 0 & \longrightarrow & K' & \xrightarrow{v} & X' & \underset{q}{\overset{t}{\leftrightarrows}} & Y' & \longrightarrow & 0 \end{array}$$

in the sense that $b \cdot u = v \cdot a$, $c \cdot p = q \cdot b$ and $b \cdot s = t \cdot c$, with p, q split epimorphisms, $p \cdot s = 1_Y$ and $q \cdot t = 1_{Y'}$, and $u = \ker p$, $v = \ker q$, if a and c are isomorphisms, b is an isomorphism as well.

Bourn observed that the Split Short Five Lemma holds in a pointed category $\mathscr{C}$ with pullbacks of split epimorphisms if and only if the *kernel functor*

$$\begin{array}{rcl} K : Pt\mathscr{C} & \longrightarrow & \mathscr{C}/0 \times \mathscr{C} \\ (\ X \underset{s}{\overset{f}{\rightleftarrows}} Y\ ) & \longmapsto & (\mathrm{Ker}\, f \to 0,\ X) \end{array}$$

is *conservative*, i.e. reflects isomorphisms. Here $Pt\mathscr{C}$ is the category of split epimorphisms – or *pointed objects* – of $\mathscr{C}$, i.e. pairs (f, s) with $f \cdot s = 1$; a morphism $(f, s) \to (f', s')$ in $Pt\mathscr{C}$ is a pair of morphisms of $\mathscr{C}$ (h, k) making the following diagram

$$\begin{array}{ccc} X & \xrightarrow{h} & X' \\ {\scriptstyle f}\downarrow\uparrow{\scriptstyle s} & & {\scriptstyle f'}\downarrow\uparrow{\scriptstyle s'} \\ Y & \xrightarrow[k]{} & Y' \end{array}$$

commute (that is $k \cdot f = f' \cdot h$ and $s' \cdot k = h \cdot s$).

To avoid the assumption of $\mathscr{C}$ being pointed, one can focus on the second component of this functor, that is, on the functor which assigns to each object (f, s) of $Pt\mathscr{C}$ the codomain of f (=domain of s):

$$\begin{array}{rcl} p : Pt\mathscr{C} & \longrightarrow & \mathscr{C} \\ (f, s) & \longmapsto & \mathrm{cod}\, f, \end{array}$$

which is a fibration, the so-called *fibration of pointed objects* of $\mathscr{C}$.

If $\mathscr{C}$ has split pullbacks, every morphism $v : X \to Y$ in $\mathscr{C}$ induces, via pullback, the change-of-base functor

$$v^* : Pt_Y\mathscr{C} \longrightarrow Pt_X\mathscr{C}.$$

Proposition 1. [6] *Let $\mathscr{C}$ be a category with split pullbacks. If $\mathscr{C}$ has split pushouts (i.e. admits pushouts of split monomorphisms), then the change-of-base functors of the fibration p have left adjoints (i.e. p is also a cofibration). Conversely, if p is a cofibration and $\mathscr{C}$ admits finite products, then $\mathscr{C}$ has split pushouts.*

The remarkable novelty of Bourn's protomodularity is the recognition of the role played by these functors:

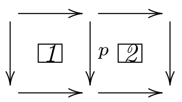
Definition. [6] A category $\mathscr{C}$ with split pullbacks is *protomodular* if the change-of-base functors of the fibration $p : Pt\mathscr{C} \to \mathscr{C}$ are conservative.

Protomodularity can be stated alternatively as a very simple condition on pullbacks.

Theorem 1. [6] *A category $\mathscr{C}$ is protomodular if and only if:*

(1) *$\mathscr{C}$ has split pullbacks;*

(2) *If in the commutative diagram*



the down-arrows are split epimorphisms and $\boxed{1}$ *and* $\boxed{1}\boxed{2}$ *are pullbacks, then* $\boxed{2}$ *is also a pullback.*

Moreover, in (2) one can assume only that p is a split epimorphism, provided that $\mathscr{C}$ has pullbacks.

Theorem 2. *Let* $\mathscr{C}$ *be a pointed category with split pullbacks. The following conditions are equivalent:*

(i) $\mathscr{C}$ *is protomodular;*

(ii) *The Split Short Five Lemma holds in* $\mathscr{C}$.

Examples. ([3])

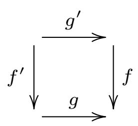
1. Every additive category with finite limits is protomodular, hence every abelian category is protomodular.
2. $\mathscr{G}rp$ is protomodular.
3. The dual category of an elementary topos is protomodular; in particular, $\mathscr{S}et^{\mathrm{op}}$ is protomodular.
4. If $\mathscr{C}$ is protomodular and $X \in \mathscr{C}$, then the slice category $\mathscr{C}/X$ and the coslice category $X\backslash\mathscr{C}$ are protomodular.
5. If $\mathscr{C}$ is protomodular, all the fibres $Pt_Y(\mathscr{C}) = p^{-1}(Y)$ of the fibration of points of $\mathscr{C}$ are protomodular.
6. If $\mathscr{C}$ is protomodular and finitely complete, then its category $\mathscr{G}rd(\mathscr{C})$ of internal groupoids is protomodular as well.

In Section 4 we will present a characterization of the protomodular varieties, i.e. of the varieties of universal algebras which are protomodular, as categories.

As for $\mathscr{G}rp$, monomorphisms in pointed protomodular categories do not need to be kernels. They behave however quite nicely. We select here some of the properties of monomorphisms and regular epimorphisms in protomodular categories.

Proposition 2. *Let* $\mathscr{C}$ *be a finitely complete protomodular category.*

(1) *Pulling back reflects monomorphisms, i.e. given a pullback*



f *is a monomorphism provided that* f' *is.*

(2) *If* $\mathscr{C}$ *is pointed, then:*

(a) f *is a monomorphism* $\Leftrightarrow$ $\mathrm{Ker}\, f = 0$*;*

(b) f *is a regular epi* $\Leftrightarrow$ $f = \mathrm{coker}(\ker f)$.

There is an interesting approach to normal subobjects in general protomodular categories that we will not describe here (cf. [3, 7]).

3. Semi-abelian categories

An *abelian category* is an additive category, with kernels and cokernels, and such that every monomorphism is a kernel and every epimorphism is a cokernel. We recall that an *additive category* is a *pointed* category *with biproducts* (i.e. finite products are biproducts, hence also coproducts) and with an additive abelian group structure in each hom-set so that composition of arrows is bilinear with respect to this addition (cf. [20, 13]).

Alternatively, an abelian category can be defined by the following two axioms:

(1) $\mathscr{C}$ has finite products, and a zero object,

(2) $\mathscr{C}$ has (normal epi, normal mono)-factorizations, i.e. every morphism factors into a cokernel followed by a kernel.

Observing that these two conditions imply that $\mathscr{C}$ is finitely complete and finitely cocomplete, so that condition (1) could be stated self-dually, and that condition (2) is obviously self-dual, it is clear that the notion of abelian category is self-dual, that is:

$$\mathscr{C} \text{ is abelian } \Leftrightarrow \mathscr{C}^{\mathrm{op}} \text{ is abelian.}$$

Roughly speaking, to define semi-abelianess Janelidze, Márki and Tholen replaced additivity by protomodularity, in a convenient way. The bridge they used was *Barr-exactness.*

We recall that a category $\mathscr{C}$ is *Barr-exact* [1] if

(1) $\mathscr{C}$ has finite products,

(2) $\mathscr{C}$ has pullback-stable (regular epi, mono)-factorizations,

(3) Every equivalence relation is effective (i.e. the kernel pair of some morphism).

If $\mathscr{C}$ satisfies (1) and (2) it is called *regular*.

A category is abelian if, and only if, it is additive and Barr-exact. Barr-exactness by itself is not restrictive enough to capture essential properties of group-like categories. It includes, for instance, pointed sets and monoids.

The authors of [16] observed that categories which are both Barr-exact and protomodular – including of course $\mathscr{G}rp$ – are very well-behaved. Namely, for a Barr-exact category with split pushouts, protomodularity is equivalent to the existence of semi-direct products (cf. [8]). They proposed the following:

Definition. A category $\mathscr{C}$ is *semi-abelian* if it is pointed, protomodular and Barr-exact.

It is interesting to notice that, although to be semi-abelian is not self-dual,

$$\mathscr{C} \text{ is abelian } \Leftrightarrow \mathscr{C} \text{ and } \mathscr{C}^{\mathrm{op}} \text{ are semi-abelian.}$$

This result follows essentially from the following

Proposition 3. *If $\mathscr{C}$ is pointed and both $\mathscr{C}$ and $\mathscr{C}^{\mathrm{op}}$ are protomodular, then $\mathscr{C}$ has biproducts.*

In semi-abelian categories many algebraic results are valid, specially results involving the behaviour of exact sequences (see Section 5). We refer to [16] for a very interesting incursion into this subject, and to [3, 2] for a thorough study of the properties of semi-abelian categories.

4. Semi-abelian varieties and topological algebras

The varieties of groups, of loops (or more generally semi-loops), of cartesian closed (distributive) lattices, of locally boolean distributive lattices, are varieties of universal algebras which are semi-abelian categories. In fact, for a variety, to be pointed protomodular is equivalent to be semi-abelian, since it fulfils always the exactness condition.

In 2003 Bourn and Janelidze [9] characterized semi-abelian (in fact protomodular) varieties as those having a finite family of generalized "subtractions" and a generalized "addition", as stated below.

Theorem 3. *A variety $\mathscr{V}$ of universal algebras is protomodular if and only if, for a given $n \in \mathbb{N}$, it has:*

(1) *n 0-ary terms $e_1, \cdots, e_n$;*

(2) *n binary terms $\alpha_1, \cdots, \alpha_n$ with $\alpha_i(x,x) = e_i$ for all $i = 1, \cdots, n$;*

(3) *one $(n+1)$-ary term θ satisfying*

$$\theta(\alpha_1(x,y), \cdots, \alpha_n(x,y), y) = x.$$

A variety $\mathscr{V}$ is semi-abelian if, and only if, it fulfils conditions (1)-(3) with $e_1 = \cdots = e_n = 0$.

In case $\mathscr{V}$ is the variety of groups, in the Theorem we put $n = 1$, $\alpha(x,y) = x - y$ and $\theta(x,y) = x + y$. It is clear that any variety which contains a unique constant and a group operation is semi-abelian. This is in particular the case of groups, abelian groups, Ω-groups, modules on a ring, rings or algebras without units, Lie algebras, Jordan algebras. Any semi-abelian variety has a Mal'cev operation p, defined as

$$p(x,y,z) = \theta(x, \alpha_1(y,z), \cdots, \alpha_n(y,z)).$$

(For more examples, see [4].) It is particularly interesting to study the corresponding topological algebras.

Let $\mathscr{C}$ be the category of topological algebras for a given semi-abelian variety $\mathscr{V}$. That is, objects of $\mathscr{C}$ are elements of $\mathscr{V}$ equipped with a topology making the operations continuous, and morphisms of $\mathscr{C}$ are continuous homomorphisms. Our basic example is of course the category $\mathscr{T}op\mathscr{G}rp$ of topological groups and continuous group homomorphisms. The main ingredient in the study of classical properties of topological groups is the existence of the homeomorphisms

$$G \xrightarrow{(-)+x} G$$

($x \in G$) which, although not living in $\mathscr{T}op\mathscr{G}rp$ (they are not homomorphisms), show that – topologically – G is homogeneous, i.e. its local properties do not depend on the point x considered. For topological semi-abelian algebras one replaces this set of homeomorphisms by a set of sections and retractions, as follows.

Let $A \in \mathscr{C}$. Condition (3) of the Theorem asserts that, for each $a \in A$, the continuous maps

$$\begin{array}{rcl} \iota_a : A & \longrightarrow & A^n \\ x & \longmapsto & (\alpha_1(x,a), \cdots, \alpha_n(x,a)) \end{array}$$

and

$$\begin{array}{rcl} \theta_A : A^n & \longrightarrow & A \\ (x_1, \cdots, x_n) & \longmapsto & \theta(x_1, \cdots, x_n, a) \end{array}$$

satisfy $\theta_a \cdot \iota_a = 1_A$, hence present A as a topological retract of A^n. Condition (2) says that $\iota_a(a) = (0, \cdots, 0)$, which allows the comparison between local properties at a and at 0. Indeed, from these properties one may conclude that, *for any $a \in A$, each of the sets*

$$\{\iota_a^{-1}(U \times \cdots \times U) \mid U \text{ open neighbourhood of } 0\}$$

and

$$\{\theta_a(U \times \cdots \times U) \mid U \text{ neighbourhood of } 0\}$$

is a fundamental system of neighbourhoods of a, the former one consisting of open neighbourhoods.

A convenient use of these properties guides us straightforward to the establishment of most of the classical topological properties known for topological groups. (For details see [4, 5].) Here we would like to mention one important property, which in fact follows directly from the existence of a Mal'cev operation:

In $\mathscr{C}$ a morphism is a regular epimorphism if and only if it is an open surjection.

We remark that a well-known important property of topological groups, namely that the profinite topological groups – i.e. projective limits of finite discrete topological groups – are exactly the compact and totally disconnected groups, in general is not true for semi-abelian topological varieties. For instance, the result fails for topological Ω-groups. It remains an open problem to characterize those varieties for which this equality holds. (Cf. Johnstone [17, Chapter 6] for more results on the subject.)

Analyzing now the categorical behaviour of such categories of topological semi-abelian algebras it is easy to check that *protomodularity is inherited from protomodularity of $\mathscr{V}$, but not exactness, hence they are not semi-abelian.* Indeed, the kernel pair of a continuous homomorphism $f : G \to H$ between, say, topological groups, is constructed like in $\mathscr{G}rp$ and it inherits the subspace topology of the product topology on $G \times G$. Hence, any equivalence relation on G provided with a topology which is strictly finer than the subspace topology of $G \times G$ is not a kernel pair, hence equivalence relations are not effective.

However, regularity is guaranteed, since the (regular epi, mono)-factorization of a morphism $f : A \to B$ is obtained via the (regular epi, mono)-factorization in $\mathscr{V}$

$$A \xrightarrow{f} B = A \xrightarrow{e} M \xrightarrow{m} B$$

equipping M with the quotient topology, which makes e necessarily an open map. Since open maps are pullback-stable, the factorization is pullback-stable as claimed.

At this stage one can raise the question: Are pointed regular protomodular categories interesting? The answer is definitely yes. These are the *homological categories* we will consider in the next section.

5. Homological categories

A category $\mathscr{C}$ is said to be *homological* if it is pointed, regular and protomodular.

Every semi-abelian category is homological, but there are interesting homological categories which are not semi-abelian, like $\mathscr{T}op\mathscr{G}rp$, and, more generally, any category of topological semi-abelian algebras.

As in any pointed category, in a homological category a sequence of morphisms

$$0 \longrightarrow K \xrightarrow{k} X \xrightarrow{f} Y \longrightarrow 0$$

is a *short exact sequence* if $k = \ker f$ and $f = \operatorname{coker} k$. Since in a pointed protomodular category every regular epimorphism is the cokernel of its kernel, in a homological category $0 \longrightarrow K \xrightarrow{k} X \xrightarrow{f} Y \longrightarrow 0$ *is a short exact sequence if, and only if, $k = \ker f$ and f is a regular epimorphism.*

Furthermore, in a homological category the (regular epi, mono)-factorization of a morphism is obtained like in abelian categories, i.e. *if $f = m \cdot e$ is the (regular epi, mono)-factorization of f, then $e = \operatorname{coker}(\ker f)$.* Hence *every kernel has a cokernel* and, moreover, *every kernel is the kernel of its cokernel.*

Using (regular epi, mono)-factorizations, one can define exact sequences as follows.

Definitions. (1) In a homological category a *sequence of morphisms*

$$X \xrightarrow{f} Y \xrightarrow{g} Z$$

is *exact* if, in the (regular epi, mono)-factorizations of f and g, $m = \ker e'$:

$$X \xrightarrow{f} Y \xrightarrow{g} Z,\qquad f = m \cdot e,\ X \xrightarrow{e} M \xrightarrow{m} Y,\qquad g = m' \cdot e',\ Y \xrightarrow{e'} M' \xrightarrow{m'} Z$$

(2) A *long exact sequence* of composable morphisms is *exact* if each pair of consecutive morphisms forms an exact sequence.

In a homological category a morphism $f : X \to Y$ can be part of an exact sequence $X \xrightarrow{f} Y \xrightarrow{g} Z$ only if, in its (regular epi, mono)-factorization $f = m \cdot e$, m is a kernel. (Such morphisms are called *proper*.) Still, in a homological category exact sequences identify monomorphisms and regular epimorphisms as follows.

Proposition 4. [3] *If $\mathscr{C}$ is a homological category and $f : X \to Y$ is a morphism in $\mathscr{C}$, then:*

(1) *f is monic if and only if the sequence* $0 \longrightarrow X \xrightarrow{f} Y$ *is exact;*

(2) *$k = \ker f$ if and only if the sequence* $0 \longrightarrow K \xrightarrow{k} X \xrightarrow{f} Y$ *is exact;*

(3) *f is a regular epimorphism if and only if the sequence* $X \xrightarrow{f} Y \longrightarrow 0$ *is exact;*

(4) *for a proper morphism f, $q = \operatorname{coker} f$ if and only if* $X \xrightarrow{f} Y \xrightarrow{q} Q \longrightarrow 0$ *is exact.*

Finally we would like to stress that the key results on short exact sequences are valid in this setting (cf. [3]):

Theorem 4. (Short Five Lemma) *For a pointed regular category $\mathscr{C}$, the following conditions are equivalent:*

(i) *$\mathscr{C}$ is homological.*

(ii) *The Short Five Lemma holds, that is, given a commutative diagram*

$$\begin{array}{ccccccccc}
0 & \longrightarrow & K & \xrightarrow{u} & X & \xrightarrow{p} & Y & \longrightarrow & 0 \\
 & & \downarrow{\scriptstyle a} & & \downarrow{\scriptstyle b} & & \downarrow{\scriptstyle c} & & \\
0 & \longrightarrow & K' & \xrightarrow{v} & X' & \xrightarrow{q} & Y' & \longrightarrow & 0
\end{array}$$

with exact rows, if a and c are isomorphisms, b is also an isomorphism.

Theorem 5. (3×3 Lemma) *Let $\mathscr{C}$ be a homological category. Consider the commutative diagram*

$$\begin{array}{ccccccccc}
0 & \longrightarrow & K'' & \xrightarrow{k''} & X'' & \xrightarrow{f''} & Y'' & \longrightarrow & 0 \\
 & & {\scriptstyle u'}\downarrow & & {\scriptstyle v'}\downarrow & & \downarrow{\scriptstyle w'} & & \\
0 & \longrightarrow & K' & \xrightarrow{k'} & X' & \xrightarrow{f'} & Y' & \longrightarrow & 0 \\
 & & {\scriptstyle u}\downarrow & & {\scriptstyle v}\downarrow & & \downarrow{\scriptstyle w} & & \\
0 & \longrightarrow & K & \xrightarrow{k} & X & \xrightarrow{f} & Y & \longrightarrow & 0
\end{array}$$

where the horizontal lines are short exact sequences and $v \cdot v' = 0$. Then, if two of the columns are short exact sequences, the third one is also a short exact sequence.

The *Noether Isomorphisms Theorems* are still valid in homological categories, as well as the *Snake Lemma* (for exact formulations of these results see [3]). Furthermore, one can associate to each short exact sequence of chain complexes the *long exact homology sequence*, provided that the chain complexes are proper.

References

[1] M. Barr, Exact categories, in: *Springer Lect. Notes in Math.* **236**, pp. 1–120, 1971.

[2] F. Borceux, A survey of semi-abelian categories, in: *Fields Inst. Commun.* **43**, pp. 27–60, Amer. Math. Soc., Providence, RI, 2004.

[3] F. Borceux and D. Bourn, *Mal'cev, Protomodular, Homological and Semi-Abelian Categories*, Kluwer, 2004.

[4] F. Borceux and M.M. Clementino, Topological semi-abelian algebras, *Adv. Math.* **190** (2005), 425–453.

[5] F. Borceux and M.M. Clementino, Topological protomodular algebras, *Topology Appl.* **153** (2006), 3085–3100.

[6] D. Bourn, Normalization equivalence, kernel equivalence and affine categories, in: *Springer Lect. Notes in Math.* **1488**, pp. 43–62, 1971.

[7] D. Bourn, Normal subobjects of topological groups and of topological semi-abelian algebras, *Topology Appl.* **153** (2006), 1341–1364.

[8] D. Bourn and G. Janelidze, Protomodularity, descent and semi-direct products, *Theory Appl. Categ.* **4** (1998), 37–46.

[9] D. Bourn and G. Janelidze, Characterization of protomodular varieties of universal algebra, *Theory Appl. Categ.* **11** (2002), 143–147.

[10] David A. Buchsbaum, Exact categories and duality, *Trans. Amer. Math. Soc.* **80** (1955), 1–34.

[11] S. Eilenberg and S. Mac Lane, General theory of natural equivalences, *Trans. Amer. Math. Soc.* **58** (1945), 231–294.

[12] S. Eilenberg and N.E. Steenrod, *Foundations of Algebraic Topology*, Princeton Univ. Press 1952.

[13] P. Freyd, *Abelian Categories: An Introduction to the Theory of Functors*, Harper & Row, New York, 1964. Reprints in Theory and Applications of Categories, n° **3** (2003), pp. 23-164.

[14] R. Godement, *Théorie des Faisceaux*, Paris, Hermann 1958.

[15] A. Grothendieck, Sur quelques points d'algèbre homologique, *Tôhoku Math. J.* **9** (1957), 119–221.

[16] G. Janelidze, L. Márki and W. Tholen, Semi-abelian categories, *J. Pure Appl. Algebra* **168** (2002), 367–386.

[17] P.T. Johnstone, *Stone Spaces*, Cambridge Studies in Advanced Mathematics, 3. Cambridge University Press, Cambridge, 1986.

[18] S. Mac Lane, Groups, categories, and duality, *Proc. Nat. Acad. Sci. U.S.A.* **34** (1948), 263–267.

[19] S. Mac Lane, Duality for groups, *Bull. Amer. Math. Soc.* **56** (1950), 485–516.

[20] S. Mac Lane, *Categories for the Working Mathematician*, 2nd Ed., Springer, New York, 1998.

An Interview with Hans Triebel

Hans, you are immediately recognized in the mathematical analysis community by your expertise within the theory of function spaces, your books on the subject serving as unavoidable reference. Can you tell us a little about how this interest started?

Thank you for the compliments, António, you are very kind. Indeed, you may be surprised that this interest developed somehow by chance.
My Ph.D. project, supervised by Professor Maier in Jena, was concerned with Lamé's differential equation, that is, complex function theory. Later an elder colleague recommended me Sobolev's book from 1950, which I studied with great interest since I was always fascinated both by mathematics and physics.
In the past it was also quite usual in an academic career in the former GDR (East Germany) to go for one year abroad, but the difference was that there was not a big choice. Certainly one could apply for one or the other university in the East, mainly within the Soviet Union, but the decision was made somewhere else by the authorities. In my case it finally turned out to be a rather lucky circumstance to send me to Leningrad (now St. Petersburg) though this has not been my first choice. So before leaving to Leningrad I polished up my Russian learned at school – but even then it was not so easy at the beginning. Later my pronunciation improved such that people did not immediately recognise me as a foreigner, but at the beginning . . . you may recall that in 1963/64 when I came to Leningrad, less than 20 years had passed since the end of World War II. In other words, there still lived many people who had suffered from the Germans, especially in this town. So I was a bit afraid when I arrived, but my experience was that I was met with a kind reception. What concerns Leningrad university, I had no direct personal contact there, I mainly worked on my own and read a lot of books. But I enjoyed the very active and inspiring atmosphere due to many great mathematicians working there. In particular, I had the great pleasure to attend lectures by Birman on functional analysis, spectral theory, quantum mechanics – he really was an impressive lecturer. His main concern was at that time applications to the spectral theory of partial differential equations, using methods from functional analysis. So to study function spaces was a natural task in this direction. Other people working there included, of course, Solomyak, but also Uraltseva and Ladyzhenskaya who's seminar I attended. Later, back in Jena, I read Nikol'skiĭ's book from 1969 . . . but at that time I had already started working on function spaces myself.

Hans Triebel (photo by Alexandre Almeida, used with permission).

Occasionally, when talking about function spaces with other people, I have heard them wondering about the reason the letter F is used for the so-called Triebel-Lizorkin (or Lizorkin-Triebel) spaces ...

Honestly, there is no mystery at all about this letter (in contrast to other spaces and their letters which caused longstanding stories and discussions afterwards). As I occasionally explained, it was the first 'suitable', i.e., free letter when I needed one for the new spaces and invented this symbol around 1970. I even had some concern that it may cause confusion with Fréchet spaces, but it turned out later that this was not the case. As far as I remember, the symbol made its first official appearance in two of my papers in 1973 (it always took very long to get all the necessary permissions to publish some paper abroad).

Before choosing function spaces, you had to choose mathematics as a subject to study. Was it already your childhood dream to become a (famous) mathematician?

Not at all! I really liked all the science subjects in school, mainly mathematics, physics and chemistry. So when it came to choose a subject to study I hesitated what to take. But then someone suggested that I should try physical chemistry since this was expected to have a bright future soon — and shared the advantage to combine at least two of my favourite subjects. As recommended, I applied for physical chemistry in Jena. As a second choice only I had named mathematics. Unfortunately my application was not approved probably because of a rather bad evaluation of my insufficiently developed socialistic personality. Nevertheless the university in Jena invited me to follow some other career: they offered me a place to study mathematics and physics to become a teacher. Though I really never wanted to teach at schools, I accepted this offer since I was told that at the beginning many lectures are the same for diploma students and teacher students in mathematics and physics and changing after a while would be much easier. This was in fact the case and I followed both, mathematics and physics, almost up to the end in a parallel way. Apart from very few tasks at the end I could have also completed physics with a diploma like mathematics, but for some reason I only did it in mathematics.

I was looking in the Mathematics Genealogy Project and found out that you are a mathematical descendant of Gauss and Weierstrass. What does it mean to you?

Nothing particular, I would say. I was amused when I discovered it first time – and I enjoy to point it out to my Ph.D. students and their Ph.D. students that they now enter the famous descendant line of Gauss and Weierstrass in $n+1$st, $n+2$nd generation with my humble person in between.

You obtained your Ph.D. from the University of Jena in 1962 and I have always known of your name in connection with this same university. Apart from the one year abroad which you have already mentioned, have you been there all this time, and if yes, for what reason?

An academic career in GDR times was in some sense very much different from what you would expect nowadays – and what all my younger Ph.D. students experience now. Almost everything was more restricted, not only publication in 'Western' journals as already mentioned, and – of course – going abroad for research stays or only to take part in conferences was very difficult. But also life was more steady and more regulated at that time, so for many reasons it was not easy and also not usual to move too often. Apart from the year in Leningrad I worked a year outside of the university in a company after I had received my diploma. Otherwise I followed the academic career in Jena. Only at the beginning of the 1970s I really thought of leaving Jena for many different reasons, including professional ones. Finally I decided otherwise and stayed there until my retirement some years ago. But as you know, António, we have so many fine, well-equipped spaces with sufficiently many dimensions, what influence should a three- or four-dimensional world in a medium-sized town like Jena have then in the end?

With S.M. Nikolskii in May 2005 (Moscow), at the Conference celebrating his 100th birthday (photo by Alexandre Almeida, used with permission).

Can you tell us about mathematicians that have influenced you most? Also some that you interacted with in some crucial moments in the development of the theory of function spaces.

Apart from Birman and Solomyak who I met first during my time in Leningrad, I would name here S.G. Krejn. I think it was at the mathematical congress in Moscow 1966 where I first get to know him. Another colleague that influenced and motivated my studies essentially at some time is certainly Jaak Peetre from Lund. As far as I remember we first talked in Berlin in 1969, where I really understood some advantage of Besov spaces (defined by differences) showing up as interpolation spaces from Sobolev spaces. Later in Lund he directed my interest to the book of Stein from 1970, which also had consequences on my further studies in function spaces. Indeed, in 1975 when I stayed for some time in the Banach Center in Warsaw I really considered to change subjects and turn to the theory of relativity where I already lectured about in Jena. This fascinated me very much – and, in addition, I thought that I am finished at some level with function spaces. I had completed my habilitation thesis about function spaces and nonlinear analysis, had already become a professor at the age of 34. So I thought it a good opportunity to concentrate on something else. But just in Warsaw I read some papers from Fefferman and Stein about the Fourier analytical approach to function spaces ... and this convinced me that research in function spaces is not outdated. As you know yourselves, António, there is still a lot to do and, even worse or better, there are so many new surprising connections to other areas, not only of analysis, and further ideas, open questions that serve as source of Ph.D. projects, admit to write papers and books, collaborate with colleagues …

You are always full of new ideas in your research. Is there some recipe we can learn about? How do your new ideas usually come?

Unfortunately I do not have a special method or secret that I could share with you. Ideas come from time to time, I rather have the belief of a sea of potential thoughts and ideas, that only partly and occasionally become more detailed and visible. Certainly essential from my point of view is to read a lot of specific literature, I always consumed various monographs and papers, but also textbooks. Moreover, I meanwhile get many questions, whether in seminars or at conferences, that initiate further conclusions or interesting questions. Nowadays I even receive many emails with more or less tricky problems. But I would not say that I systematically search for new ideas, they rather come to me sporadically.

Giving a lecture during the OTFUSA Conference held in Aveiro in July 2005.

I think one of the first impressions people have when meeting you is that you are a very happy person, always willing to play with words or with unusual (or even common) situations. Together with your easiness in getting a good laugh and the expressive way you put in teaching, maybe this is one of the explanations for the huge number of Ph.D.'s that you have supervised: the number 36 is impressive, and still growing. Do you have a secret recipe for this?

Sorry, but I have to disappoint you again: there is no special trick at all. Even worse, I never really propagated fascinating Ph.D. topics in order to attract especially good students, they rather came by themselves and asked for something to concentrate on. Of course, when I was very much involved in teaching duties I knew many students – and they knew me. So it was easier to come in contact and to promote some of them. Later, in particular when we had the graduate school in Jena, there sometimes appeared the phenomenon that young students were directly sent to me from their supervisors abroad, in order to do a Ph.D. in Jena under my supervision, sometimes already with some special interest and well-prepared mathematical knowledge.

Let us still talk about this graduate school in mathematics, which you have had in Jena, already for some years. Can you tell us how this works? … This is a topic of special interest nowadays for Portuguese universities, because there has been a trend to set such doctoral programs, though not always backed up with the funds necessary to support students!

We hosted two graduate schools during the last 15 years: the first one with the title 'Analytic and stochastic structures and systems' lasted for the maximal number of years from 1992 until 2002. At that time usually around 10 professors of a faculty (or different faculties) submitted an application and described some topic of joint interest which was wide and promising enough to admit sufficiently many Ph.D. projects and further research, but should also be concentrated enough to have a substantial kernel of collaboration within the different research groups. In the lucky case it is then approved for 3 years and this procedure can be repeated twice at most. The final year is then given to complete the last projects. In our case we had grants for 12 Ph.D. students and 2 Postdoc positions (per three year period), that is, the Ph.D. grants were given usually for 2+1 year, the Postdoc position for one or two years. Students had to apply and were chosen by this small group of professors forming the graduate school in view of their submitted documents and a talk before the audience. In addition to the personal grants for the graduate students (around 1000 Euros at that time, as far as I remember) we received extra money to invite guests, to finance a small separate special library, to support research and conference stays of the students in a modest way, and to organise two workshops or conferences per year. For a long time I was the speaker of this graduate school which was the first mathematical one within the former GDR territory and the first at all that was installed in Thuringia, the federal state Jena belongs to. Apart from the convenient situation to have Ph.D. positions at all and to have some money to spend for conferences, guests and books, the main advantage was in my opinion the uncomplicated and direct administration with short connections between all the people involved, Ph.D. students as well as professors. Our graduate school really worked successfully, almost all Ph.D. theses could be completed.
There existed a second graduate school from 2002 un-

til 2006 in our faculty, this time in combination with applied mathematics and computer science, called 'Approximation and algorithms'. It followed essentially the same scheme.

Would you like to comment about other avenues that your research has taken, besides the concern with the function spaces? I'm thinking, in particular, that for quite some time the underlying domains which you were considering were smooth ones, and afterwards, maybe during the 1990s, you started to systematically consider irregular, even fractal, sets.

The close connection to fractal geometry turned out within the Ph.D. project of Heike Winkelvoss (who, by the way, also had a grant from the graduate school we talked about before). At that time the atomic decomposition in function spaces was already available and sufficiently developed to serve as building blocks also for spaces on fractals. This localised description fits pretty well to the nature of fractals, or, more precisely, d-sets and generalisations like h-sets, which were investigated by Michele Bricchi, another of my Ph.D. students living on a grant from the graduate school. Similarly other areas like wavelet theory entered function spaces scene whenever needed and appropriate. Of course, these extensions to the theory of function spaces are very much welcome.

And what about outside mathematics? Is there anything – certainly less interesting than function spaces – that you enjoy doing when not concerned with mathematics?

Well, it is not very exciting, I confess: it is again reading what I like. In particular, I am more and more interested in historical topics, especially linked to mathematics or physics. I am fascinated by the way in which scientists and science developed in the past. You may imagine me sitting in my garden, reading and reading – and the only witnesses for this picturesque scene are brave birds, shy deer and old trees ...

Which mathematicians do you admire particularly? Do you have a favourite mathematician from before the 20th century? And from the 20th century?

Certainly Archimedes, Pythagoras and Riemann. Concentrating on the last century, then I would first mention Einstein, especially how he came from special to general relativity. Secondly, there is, of course, David Hilbert who can be seen in some sense as successor of Pythagoras in his approach of assumptions and proofs. One of his great credits may be the idea to mathematise physics by models. Finally, related to my field of analysis, let me refer to Sobolev and Laurent Schwartz.

If you had to mention one or two great moments in 20th century mathematics which ones would you pick?

Probably one should allude to the proof of Fermat's Last Theorem by Andrew Wiles here, and to the contribution to the continuum hypothesis by Paul Cohen. But related to my field of research, this is doubtless the discovery of distributions by Laurent Schwartz. One can read in his memories that in the beginning the mathematical society behaved rather hostile against this new ideas, or better to say, the general opinion was split: a smaller part of his colleagues regarded this approach to be ingenious, whereas the majority thought it too simple to be useful and far-reaching. But they were wrong obviously. Nowadays, this theory well-equipped with the tools of Fourier analysis, essentially included and further developed within the concept of function spaces, becomes more and more the language of numerical analysis, too. It took some time until Laurent Schwartz became famous for his discovery.

One of your former students once told me that you know exactly where things should lead to in your area of research and that you have a program to get there. I myself can testify that you have strong feelings about the truth or falsity of some conjectures. Would you like to share with us some clues about important results in your areas of interest that should be possible to prove in the near (or not so near) future?

You are very kind, thank you. But thinking about it, yes, I guess you are right, there are very rare occasions when I was mistaken in my assumptions. The reason might be, that I have a certain feeling for the topography of the territory of function spaces. So I rather have the idea to inspect hidden caves, whether they are promising or boring. There is some inner voice which usually prevents me from falling into a trap, that is, I better circumvent dangerous parts of this area. Sometimes I find something what I have not looked for, this may lead to a Ph.D. topic or a paper afterwards, but not always. In such cases I collect these pieces of new ideas in some small booklet. I see myself strolling around on my own, sometimes listening to music by Bach during these walks ... But to avoid misunderstanding, I do not systematically dig and find new plants in this function space territory, I rather feel like promenading in a fog of thoughts and ideas which only by chance get caught by me. In other words, I cannot predict what I will find next – or what you asked me about future developments. Probably we should meet in some years again and then I will review and honestly tell you what important results could be proved in the past.

Interview by António Caetano (University of Aveiro) and Dorothee Haroske (University of Jena)

Hans Triebel (born February 7, 1936 in Dessau) has retired from Friedrich-Schiller-University Jena (Germany) in 2001, where he was Professor (Chair) in Analysis for more than 30 years, after earning there his Ph.D. (1962) and Habilitation (1966). He also served as Dean of its Faculty of Mathematics and Computer Science for the period 1990-93 and as Speaker of the Graduate College "Analytic and Stochastic Structures and Systems" between 1993 and 2002.

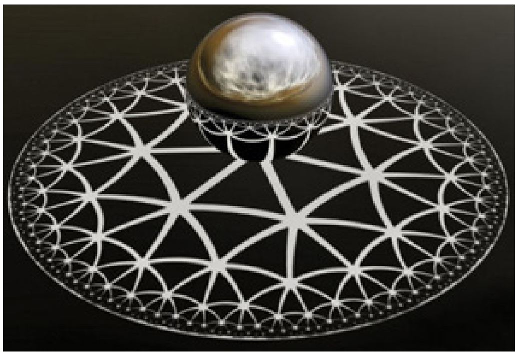
Professor Triebel has written more than 170 papers in the areas of Function spaces, Functional Analysis, Interpolation Theory, Partial Differential Equations and Fractal Analysis and has 13 titles in the list of written textbooks and monographs (one further addition to this collection being in preparation at this moment). Perhaps he is best known by the series of books he has written on the Theory of Function Spaces and its relations with other parts of Mathematical Analysis. He also served as editor of 7 volumes of Proceedings, belongs to the editorial boards of 7 international journals in mathematics and, as yet, supervised 36 Ph.D. theses.

He was a corresponding member of the Academy of Sciences of the former GDR between 1978 and 1987 and a full member of the same Academy between 1987 and 1992. Since 1993 he is a regular (full) Member of the Academy of Sciences of Berlin-Brandenburg. He was awarded, in 1983, the National Prize (of third order) of the former GDR for Science and Technology and, in 1990, a D.Sc.h.c. by the University of Sussex at Brighton (UK).

Math in the Media

Originally published by the American Mathematical Society in MATH in the MEDIA, a section of the AMS Website, www.ams.org/mathmedia, edited by Tony Phillips. Reprinted with permission.

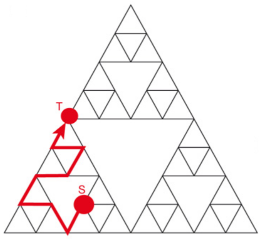
Cosmic geometries.



The cover art from *Science News*, November 17, 2007. Design by Anders Sandberg (Future of Humanity Institute, Oxford), used with permission.

This elegant image means to illustrate "the link between laws of physics as they are perceived in universes with different geometries, even different numbers of dimensions" (from the caption in *Science News Online*, at www.sciencenews.org/articles/20071117/bob9.asp). The accompanying article, by David Castelvecchi, sketches some recent developments related to Juan Maldacena's 1997 ideas about string--particle duality: "Just as a hologram creates the illusion of the third dimension by scattering light off a 2-D surface, gravity and the however many dimensions of space could be a higher-dimensional projection of a drama playing out in a flatter world." Castelvecchi quotes Maldacena to the effect that recently "very strong evidence" has been found that the conjecture is true. But then we read: "Unfortunately, the equations ... seem a good match only for the mathematics of strings living in a contracting universe." So what about this universe here? A semi-theological argument has it that "It would be too much of a coincidence ... if such a seemingly miraculous mathematical duality were to apply to a particular kind of abstract universe but not to our own." On the other hand Abhay Ashketar (Penn State) reminds us, as Castelvecchi puts it, that "In the 1860s, Kelvin pointed out that many of the known properties of chemical elements could arise naturally if atoms were knotted vortices in the fabric of the ether. The uncanny coincidence went away once physicists demonstrated that the ether probably didn't exist."

First encounters in strange places.



Candamin *et al.* give the Sierpinski gasket as an example of the kind of fractal for which they can compute the mean first-passage time from one point S to another T. A typical random path is shown. Image reprinted by permission from McMillan Publishers Ltd: *Nature* (Vol. 450, 1 November 2007, p. 77), copyright (2008).

"First-passage times in complex scale-invariant media" by a team (S. Candamin, O. Bénichou, V. Tejedor, R. Voituriez, J. Klafter) at Paris-VI and Tel-Aviv University appears in the November 1, 2007 *Nature*. It leads off with the definition of first-passage time (FPT): "How long does it take a random walker to reach a given target point?" and continues: "Our analytical approach provides a universal scaling dependence of the mean FPT on both the volume of the confining domain and the source-target distance." In all cases the mean FPT $\langle T\rangle$ from point S to point T scales linearly with the volume N of the medium; and scales with a power of the distance r from S to T, according to the relative size of the "walk dimension" d_w and the fractal dimension d_f. The walk dimension is defined so that the first time a random walk reaches a point at distance r from its start scales as r^{d_w}; the fractal dimension so that the number of sites within a sphere of radius r scales as r^{d_f}. For the Sierpinski gasket illustrated above, $d_f = \ln 3/\ln 2$ and $d_w = \ln 5/\ln 2$ so we are in the $d_f < d_w$ regime for which their general result gives $\langle T\rangle$ scaling as $r^{d_w - d_f}$; a prediction the authors buttress with numerical simulations. In an interview with *Nature*, Bénichou explains how his team worked around the problem of boundary conditions: "... we use a mathematical trick to isolate and replace the confinement effect. Then, we relate the mean FPT in confined conditions to properties of random walks in infinite space, which are easier to estimate." Nature also published an appraisal of this work, by M. Shlesinger, in their "News and Views" section.

Euclid in China, in 1607.

400 years ago, the first six volumes of Euclid's *Elements* were published in China, in Chinese. Last October the Partner Institute for Computational Biology (Shanghai) marked the anniversary with a meeting, reported on by Richard Stone under the title "Scientists Fete China's Supreme Polymath" (*Science*, November 2, 2007). Stone is referring to Xu Guangqi, a prominent Ming-dynasty scholar/administrator, who along with the Jesuit missionary Matteo Ricci carried out the translation.

Matteo Ricci and Xu Guangqi, from Kircher's *China Illustrata* (1667). Athanasius Kircher was a Jesuit colleague of Ricci's; the image evokes Xu's conversion to Catholicism.

Xu's long career spanned agriculture ("His experiments in Shanghai with yams, then a new import from South America, led to the widespread adoption of the high-energy crop."), weaponry ("Xu also trained imperial soldiers to use a newfangled device from Europe, the cannon.") and calendar reform. His most lasting contribution may have been the vocabulary he and Ricci developed for their translation. They chose the characters *ji he* for "geometry," as well as the Chinese terms for "point," "line," "parallel," etc. which remain in use today.

How complex is mathematics?

Richard Foote (University of Vermont) has a review article, "Mathematics and Complex Systems," in the October 19, 2007 *Science*. His goal is to analyze mathematics itself as a complex system. (There is in fact no exact and generally accepted definition of "complex systems," but they are usually characterized as a) made up of many interconnected elements and b) expressing emergent behaviors that require analysis at a higher level that that appropriate for the component elements. The standard example is the brain, with neurons as its component elements, and consciousness as emergent behavior.) Foote proposes "that areas of mathematics, even ones based on simple axiomatic foundations, have discernible layers, entirely unexpected 'macroscopic' outcomes, and both mathematical and physical ramifications profoundly beyond their historical beginnings."

The area he chooses to examine in detail is Finite Group Theory: he gives the axioms, defines a *simple group*, and studies the history of the classification problem for finite simple groups as one might study the evolution of a life-form, emphasizing the points where the theory underwent a transformation comparable to an emergent behavior. He distinguishes three epochs:

• From Galois to the early 1960s. It was understood how any finite group could be (essentially uniquely) decomposed into simple groups; the classification of simple groups was underway. There were 18 (infinite) families of finite simple groups and in addition 5 "sporadic" finite simple groups belonging to no family.

• The Feit-Thompson Odd Order Theorem (1962; *the only odd-order simple groups are the cyclic groups of order* > 2) was, according to Foote, "a breakthrough to the next level of complexity." Their huge paper "spawned the first 'quantum jump' in technical virtuosity that practitioners would need in order to surmount problems in this arena." The road to classification was not smooth: a sixth sporadic group was discovered in 1965, 20 more surfaced during the next few years, but by 1980 the enormous project was done.

• "The Monster and Moonshine." The Monster (the king of the sporadics, with some 10^{54} elements) is "the nexus of a new level of complexity." Starting in 1978, "striking coincidences," mysterious enough to merit the appellation Moonshine, were discovered between the structure of the Monster and the classical theory of modular functions. Finding a basis for this correspondence led to a Fields Medal for Richard Borcherds in 1998; the new level of complexity comes from the string theory methods used in Borcherds' work. These directly connect Moonshine to current research, often mathematically problematical, in theoretical physics.

Foote concludes by remarking: "... the work of scientists is inherently incremental and precise. On the other hand, it is incumbent on us all to work toward enhancing the understanding of 'big picture' issues within our own disciplines and beyond."

Hardy and Ramanujan - the novel.

Last September saw the publication of *The Indian Clerk*, David Leavitt's novelistic imagining of the Hardy-Ramanujan story. Nell Freudenberg's very positive review of *The Indian Clerk* took the front page of the *New York Times Book Review* for September 16, 2007. As she explains it, the genre here is "a novel about people who really existed, recreated by an author who plays with the facts, and especially the intriguing lacunae, of their lives." Leavitt is a special-ist in gay-themed intellectual history, and this book seems to be no exception. "Hardy was a member of the Cambridge Apostles, an illustrious secret society that counted Bertrand Russell, G. E. Moore, John Maynard Keynes and Lytton Strachey among its members. Many of the Apostles were homosexuals," as, we are given to understand, was Hardy himself. "Leavitt has been praised and condemned for the explicit sex in his fiction," Freudenberg tells us. But rest assured, readers: whatever bodice-ripping (or the equivalent) takes place in the novel, it will not involve our two protagonists. As Freudenberg puts it: "... what he makes of their relationship is much more subtle than a love affair. Initially frustrated by the young genius's tendency to pursue several ideas in an associative fashion, Hardy eventually realizes he has come in contact with a mind that expands his notion of their discipline."

Math: Gift from God or Work of Man?

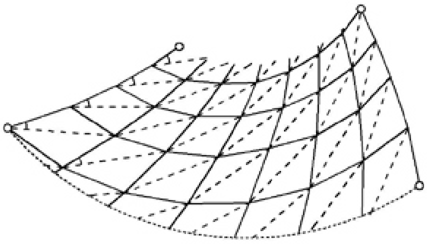
This is John Allen Paulos' column, posted September 2, 2007 on the ABC news website abcnews.go.com/Technology/WhosCounting/story?id=3543453&page=1; the subtitle: "Mathematics, Religion and Evolution in School Curricula." The insertion of religion into science courses (under the guise of "intelligent design," etc.) has now begun to spread to mathematics. So far, it does not seem too worrisome. Most of the examples Paulos shows us are merely peculiar: a standard mathematics curriculum with clumsily interpolated references to a higher being. "The study of the basics of geometry through making and testing conjectures regarding mathematical and real-world patterns will allow the students to understand the absolute consistency of God as seen in the geometric principles he created." (Many of us have done worse in trying to justify pure mathematical research to federal funding agencies). The staff at Maharishi University are more creative: "Infinity: From the Empty Set to the Boundless Universe of All Sets – Exploring the Full Range of Mathematics and Seeing its Source in Your Self." Still OK, as long as that Boundless Universe is not itself a set.

Next we take on the transcendentalists in our midst; like Eugene Wigner who believes, Paulos tells us, that the "ability of mathematics to describe and predict the physical world is no accident, but rather is evidence of a deep and mysterious harmony." For these people Paulos has a nice statement of the natural history of mathematics:

"The universe acts on us, we adapt to it, and the notions that we develop as a result, including the mathematical ones, are in a sense taught us by the universe. ... evolution has selected those of our ancestors (both human and not) whose behavior and thought are consistent with the workings of the universe. The usefulness of mathematics is thus not so unreasonable."

Origami pinecones.

Nature, on July 26, 2007, ran a "News and Views" piece (www.nature.com/nature/journal/v448/n7152/edsumm/e070726-05.html) by Ian Stewart about a new breed of mathematically inspired origami. Stewart begins by reminding us of the mathematical complexity hidden in this ancient Japanese art. "The basic problem of origami is the flat-folding problem: given a diagram of fold lines on a flat sheet of paper, can the paper be folded into a flat shape without introducing any further creases? ... [T]his question is ... an example of an NP-hard problem." Taketoshi Nojima (Department of Aeronautics and Astronautics, Kyoto) has recently published a series of papers where, among other things, he shows how to crease a sheet of paper so that it folds flat, but can also be uncompressed into a conical structure presenting equiangular spirals analogous to those produced by phyllotaxis. For example, the following fold diagram, with the dotted lines interpreted as "ridges" and the solid lines as "valleys," gives a flat object which, after stretching to bring the opposite vertical edges into coincidence, produces a cone:



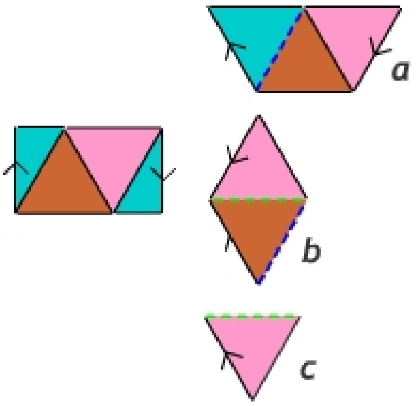
Folding diagram for origami pinecone. The angles and lengths are carefully calculated so as to satisfy the local flat folding criterion (around each vertex, the sum of every other angle must be π), to ensure that the edges and the diagonals form piecewise equiangular spirals (with respect to an origin at the center of the circle implied by the lower dotted edge), and finally to ensure that the free vertical edges match up properly. Image by Taketoshi Nojima (Origami Modelling of Functional Structures based on Organic Patterns, impact.kuaero.kyoto-u.ac.jp/pdf/Origami.pdf), used with permission.

This cone was assembled from the diagram above, enlarged by a factor of 3. Image by Taketoshi Nojima, used with permission.

Note that unlike the cones produced by phyllotaxis, this one has all three sets of equiangular spirals turning in the same direction. More "natural" configurations are also possible (see "Origami-Modellings of Foldable Conical Shells Consisting of Spiral Fold Lines," by Nojima and Takeuki Kamei, *Trans. JSME* 68 (2002) 297-302, in Japanese).

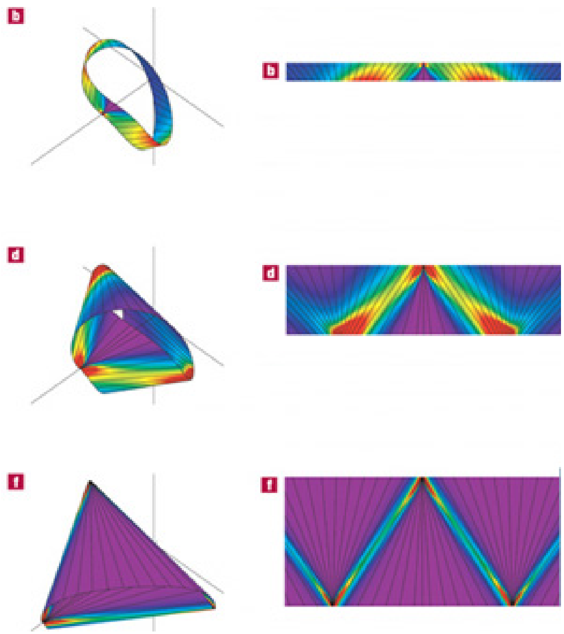
"A Twist on the Möbius Band".

That's the title that Julie J. Rehnmayer used for her *Science News Online* report (www.sciencenews.org/articles/20070728/mathtrek.asp) on recent answers to the question: when an inelastic rectangle (for example, a strip of paper) is twisted into a Möbius band in 3-dimensional space, what exactly is the resulting shape?



Any rectangle with side ratio $\sqrt{3}$ to 1 can be folded into a Möbius Band by reassembling it as a trapezoid (a), folding along the blue dotted line (b), and then folding along the green. The last fold (c) brings into congruence, with proper orientation, the sides that are to be identified. This configuration is the limiting case of the embeddings studied by Starostin and Van der Heijden.

When the side ratio is $\sqrt{3}$ to 1, the strip can be folded into a configuration that respects the edge identification. For narrower strips the band assumes a "characteristic shape" minimizing the total bending energy; the exact determination of this shape has been an outstanding problem at least since 1930. Evgueni Starostin and Gert van der Heijden (University College London) recently nailed down the solution using "the invariant variational bicomplex formalism" and numerical methods. (The variational bicomplex is, according to Ian Anderson — see www.math.usu.edu/~fg_mp/Publications/VB/vb.pdf—, a double complex of differential forms defined on the infinite jet bundle of any fibered manifold $n : E \to M$.) They report: "Solutions for increasing width show the formation of creases bounding nearly flat triangular regions ...".



Three of six characteristic shapes for length $= 2\pi$ and various widths shown in Srarostin and Van der Heijden's article: width 0.2 (b), 0.8 (d) and 1.5 (f). "The colouring changes according to the local bending energy density, from violet for regions of low bending to red for regions of high bending." Image reprinted by permission from MacMillan Publishers Ltd: *Nature Materials* (Vol. 6, 15 July 2007, p. 563), copyright (2008).

The Myth. The Math. The Sex.

"Everyone knows men are more promiscuous by nature." That's how Gina Kolata starts her piece "The Myth. The Math. The Sex." in the New York Times for August 12, 2007. We even have darwinian explanations for the phenomenon, with woman being "genetically programmed to want just one man who will stick with her and help raise their children." Surveys bear this out: Kolata mentions a British study which "stated that men averaged 12.7 heterosexual partners in their lifetimes and women, 6.5." Whoa! It turns out this is mathematically impossible. Kolata refers to David Gale, who sanitizes the context and gives us the

High School Prom Theorem: *We suppose that on the day after the prom, each girl is asked to give the number of boys she danced with. These numbers are then added up, giving a number G. The same information is then obtained from the boys, giving a number B.*

Theorem: $G = B$.

Proof: Both G and B are equal to C, the number of couples who danced together at the prom. Q.E.D.

If the numbers of men and women in the active heterosexual population are the same, as they approximately seem to be, the HSP Theorem does indeed imply that the average number of partners must be the same for both sexes. This should settle the matter. But Kolata makes the error of mentioning one study which reported an (almost identical) difference in the *medians* of the two distributions; this earns her a rebuke from Jordan Ellenberg, *Slate*'s math guru: "Mean Girls: The *New York Times* slips up on sexual math" (August 13, 2007, slate.com/id/2172186). "It's not every day I get to read a mathematical theorem in the *New York Times*, so I hate to complain. But Kolata isn't quite right here." Ellenberg goes on to give obvious examples of different medians with the same mean. Towards the end of the piece he acknowledges that some of Kolata's examples did in fact involve *means*; he changes tack and quotes serious studies of the problem of inaccurate self-reporting (unreliable memory plays a part). Kolata's essay is available online, thanks to the *Dallas Morning News*.

Geometry and the Imagination.

The 5-day conference with this title, held at Princeton on June 7-11 in honor of Bill Thurston's 60th birthday, was surveyed by Barry Cipra in the July 6, 2007 *Science*.

Bill Thurston (Princeton, March 1990).

Cipra's 2-page spread covers four of the presentations:

• **The smallest hyperbolic manifold.** In hyperbolic geometry, similar triangles must have the same area, and each hyperbolic manifold has its own specific volume. In the 1970s, Cipra tells us, "Thurston ... proved a surprising property of hyperbolic manifolds. Given any infinite collection of such manifolds, one member of the collection will be of smallest volume." In particular, one hyperbolic 3-manifold must have the smallest volume of all. A candidate was discovered shortly thereafter, by Jeff Weeks. The "Weeks manifold" remained for a long time the smallest hyperbolic 3-manifold known; only this year did David Gabai, Robert Meyerhoff and Peter Milley prove that there can be no smaller. Their work was posted on arXiv May 30, 2007 (www.arxiv.org/abs/0705.4325).

• **Infinite trajectories in outer billiards.** Outer billiards was devised as a simple analogue of planetary motion. "An object starting at point x_0 outside some convex figure zips along a straight line just touching the figure to a new point x_1 at the same distance from the point of contact. It then repeats this over and over, thereby orbiting the figure in, say, a clockwise fashion." (Cipra). Are all such orbits bounded, or for some figure and some x_0 could the x_i wind up arbitrarily far away? The question had been open since the 1950s, but a set of unbounded examples was recently discovered by Richard Schwartz. The convex body he uses is the *kite* from Penrose tilings, and he exhibits "larger and larger clouds of smaller and smaller regions" converging to "a set of points from which the trajectories are unbounded." Details at Rich's website (www.math.brown.edu/~res).

• **Crossing number of the sum of two knots.** It is known that knots can't cancel. But how about partial simplification? "... if two knots are strung together to form one larger, more complicated knot, can the new knot be redrawn with fewer crossings than the original two knots combined?" Cipra quotes Colin Adams: "This problem has been out there forever." Some recent progress towards proving that the minimal crossing number $c(K_1\#K_2)$ of the knot sum is the sum $c(K_1) + c(K_2)$ of those of the addends was reported by Mark Lakenby, who showed that $c(K_1\#K_2)$ is at least $(1/281)[c(K_1) + c(K_2)]$. Cipra: "The basic idea is to think of each knot as enclosed in a spherical bubble and then carefully analyze what must happen to the bubbles if the knot sum is twisted into a new shape with fewer crossings." He remarks, "To prove the full conjecture, mathematicians will need to whittle this number [281] all the way down to one."

• **Update on the Poincaré conjecture.** "Pricey Proof Keeps Gaining Support" is Cipra's heading for his report on John Morgan's overview of Perelman's proof. "After poring over Perelman's papers for 4 years, topologists are confident of the result. ... Much of the confidence derives from alternative proofs researchers have devised in the wake of Perelman's work." Cipra quotes Thurston at the conference banquet: "I never doubted it would be proved. It's really wonderful to see the community ownership of this mathematics."

An Interview with F. William Lawvere - Part One

This is the first part of a conversation with F. W. Lawvere, that took place in Braga on the 28th of March 2007, during the Workshop "Applied and Computational Category Theory", a satellite event of the ETAPS 2007 Conference, and continued in June, in Carvoeiro (Algarve), during the Category Theory 2007 Conference — that celebrated the 70th birthday of F. W. Lawvere. The second part of this interview, conducted by Maria Manuel Clementino and Jorge Picado (University of Coimbra), will appear in the next issue of the Bulletin.

You have written a paper, published for the first time in 1986, entitled "Taking categories seriously"[1]*. Why should we take categories seriously?*

In all those areas where category theory is actively used the categorical concept of adjoint functor has come to play a key role. Such a universal instrument for guiding the learning, development, and use of advanced mathematics does not fail to have its indications also in areas of school and college mathematics, in the most basic relationships of space and quantity and the calculations based on those relationships. By saying "take categories seriously", I meant that one should seek, cultivate, and teach helpful examples of an elementary nature.

The relation between teaching and research is partly embodied in simple general concepts that can guide the elaboration of examples in both. Notions and constructions, such as the spectral analysis of dynamical systems, have important aspects that can be understood and pursued without the complications of limiting the models to specific classical categories.

The application of some simple general concepts from category theory can lead from a clarification of basic constructions on dynamical systems to a construction of the real number system with its structure as a closed category; applied to that particular closed category, the general enriched category theory leads inexorably to embedding theorems and to notions of Cauchy completeness, rotation, convex hull, radius, and geodesic distance for arbitrary metric spaces. In fact, the latter notions present themselves in such a form that the calculations in elementary analysis and geometry can be explicitly guided by the experience that is concentrated in adjointness. It seems certain that this approach, combined with a sober application of the historical origin of all notions, will apply to many more examples, thus unifying our efforts in the teaching, research, and application of mathematics.

I also believe that we should take seriously the historical precursors of category theory, such as Grassman, whose works contain much clarity, contrary to his reputation for obscurity.

Other than Grassman, and Emmy Noether and Heinz Hopf, whom Mac Lane used to mention often, could you name other historical precursors of category theory?

The axiomatic method involves concentrating key features of ongoing applications. For example, Cantor concentrated the concept of isomorphism, which he had extracted from the work of Jakob Steiner on algebraic geometry. The connection of Cantor with Steiner is not mentioned in most books; there is an unfortunate tendency for standard works on the history of science to perpetuate standard myths, rather than to discover and clarify conceptual analyses. The indispensable "universe of discourse" principle was refined into the idea of structure carried by an abstract set, thus making long chains of reasoning more reliable by approaching the ideal that "there is nothing in the conclusion that is not in the premise". That vision was developed by Dedekind, Hausdorff, Fréchet, and others into the 20th century mathematics.

F. William Lawvere (Braga, March 2007).

[1] *Revista Colombiana de Matematicas* 20 (1986) 147-178. Reprinted in *Repr. Theory Appl. Categ.* 8 (2005) 1-24 (electronic).

Besides the portraits of the inventors of category theory, Eilenberg and Mac Lane, the front cover of our book "Sets for Mathematics", written in collaboration with Robert Rosebrugh, contains the portraits of Cantor and Dedekind.

The core of mathematical theories is in the variation of quantity in space and in the emergence of quality within that. The fundamental branches such as differential geometry and geometric measure theory gave rise to the two great auxiliary disciplines of algebraic topology and functional analysis. A great impetus to their crystallization was the electromagnetic theory of Maxwell-Hertz-Heaviside and the materials science of Maxwell-Boltzmann. Both of these disciplines and both of these applications were early made explicit in the work of Volterra. As pointed out by de Rham to Narasimhan, it was Volterra who in the 1880's not only proved that the exterior derivative operator satisfies $d^2 = 0$, but proved also the local existence theorem which is usually inexactly referred to as the Poincaré lemma; these results remain the core of algebraic topology as expressed in de Rham's theorem and in the cohomology of sheaves.

Commonly, the codomain category for a quantitative functor on $\mathfrak{X}$ is a category $Mod(\mathfrak{X})$ of linear structures in $\mathfrak{X}$ itself; thus it is most basically the nature of the categories $\mathfrak{X}$ of spaces that such systems of quantities have as domain which needs to be clarified. Concentrating the contributions of Volterra, Hadamard, Fox, Hurewicz and other pioneers, we arrive at the important general idea that such categories should be Cartesian closed. For example, the power-set axiom for sets is one manifestation of this idea – note that it is not "justified" by the 20th century set-theoretic paraphernalia of ordinal iteration, formulas, etc., since it, together with the axiom of infinity, must be in addition assumed outright. Hurewicz was, like Eilenberg, a Polish topologist, and his work on homotopy groups, presented in a Moscow conference, was also pioneer; too little known is his 1949 lecture on k-spaces, the first major effort, still used by algebraic topologists and analysts, to replace the "default" category of topological spaces by a more useful Cartesian closed one.

Speaking of Volterra, it reminds us that you have praised somewhere[2] the work of the Portuguese mathematician J. Sebastião e Silva. Could you tell us something about it ?

Silva was one of the first to recognize the importance of bornological spaces as a framework for functional analysis. He thus anticipated the work of Waelbroeck on smooth functional analysis and prepared the way for the work of Douady and Houzel on Grauert's finiteness theorem for proper maps of analytic spaces. Moreover, in spite of my scant Portuguese, I discern in Silva a dedication to the close relation between research and teaching in a spirit that I share.

Where did category theory originate ?

The need for unification and simplification to render coherent some of the many mathematical advances of the 1930's led Eilenberg and Mac Lane to devise the theory of categories, functors and natural transformations in the early 1940's. The theory of categories originated in their GTNE article[3], with the need to guide complicated calculations involving passage to the limit in the study of the qualitative leap from spaces to homotopical/homological objects. Since then it is still actively used for those problems but also in algebraic geometry, logic and set theory, model theory, functional analysis, continuum physics, combinatorics, etc.

G. M. Kelly, S. Mac Lane and F. W. Lawvere
(CT99 conference, held in Coimbra on the occasion of the 90th birthday of Saunders Mac Lane; photo by J. Koslowski, used with permission).

Mac Lane entered algebraic topology through his friend Samuel Eilenberg. Together they constructed the famous Eilenberg-Mac Lane spaces, which "represent cohomology". That seemingly technical result of geometry and algebra required, in fact, several striking methodological advances: (a) cohomology is a "functor", a specific kind of dependence on change of domain space; (b) the category where these functors are defined has as maps not the ordinary continuous ones, but rather equivalence classes of such maps, where arbitrary continuous deformations of maps serve to establish the equivalences; and (c) although in any category any fixed object K determines a special "representable" functor that assigns, to any X, the set $[X, K]$ of maps from X to K, most functors are not of that form and thus it is remarkable that the particular cohomological functors of interest turned out to be isomorphic to

[2] F. W. Lawvere, Volterra's functionals and covariant cohesion of space, *Suppl. Rend. Circ. Mat. Palermo*, serie II, 64 (2000) 201-214.
[3] S. Eilenberg and S. Mac Lane, General Theory of Natural Equivalences, *Trans. Amer. Math. Soc.* 58 (1945) 231-294.

$H^*(X) = [X, K]$ but only for the Hurewicz category (b) and only for the spaces K of the kind constructed for H^* by Eilenberg and Mac Lane. All those advances depended on the concepts of category and functor, invented likewise in 1942 by the collaborators! Even as the notion of category itself was being made explicit, this result made apparent that "concrete" categories, in which maps are determined by their values on points, do not suffice.

Already in GTNE it was pointed out that a preordered set is just a category with at most one morphism between any given pair of objects, and that functors between two such categories are just order-preserving maps; at the opposite extreme, a monoid is just a category with exactly one object, and functors between two such categories are just homomorphisms of monoids. But category theory does not rest content with mere classification in the spirit of Wolffian metaphysics (although a few of its practitioners may do so); rather it is the mutability of mathematically precise structures (by morphisms) which is the essential content of category theory. If the structures are themselves categories, this mutability is expressed by functors, while if the structures are functors, the mutability is expressed by natural transformations.

The New York Times, in its 1998 obituary of Eilenberg, omitted completely Eilenberg's role in the development of category theory.

Yes, and the injustice was only slightly less on the later occasion of Mac Lane's obituary, when the *Times* gave only a vague account.

In a letter to the NYT in February 1998, written jointly with Peter Freyd, you complained about that notable omission. In it you stress that the Eilenberg-Mac Lane "discovery in 1945 of the theory of transformations between mathematical categories provided the tools without which Sammy's important collaborations with Steenrod and Cartan would not have been possible. That joint work laid also the basis for Sammy's pioneering work in theoretical computer science and for a great many continuing developments in geometry, algebra, and the foundations of mathematics. In particular, the Eilenberg-Mac Lane theory of categories was indispensable to the 1960 development, by the French mathematician Alexander Grothendieck, of the powerful form of algebraic geometry which was an ingredient in several recent advances in number theory, including Wiles' work on the Fermat theorem". Could you give us a broad justification of why category theory may be so useful?

Everyday human activities such as building a house on a hill by a stream, laying a network of telephone conduits, navigating the solar system, require plans that can work. Planning any such undertaking requires the development of thinking about space. Each development involves many steps of thought and many related geometrical constructions on spaces. Because of the necessary multistep nature of thinking about space, uniquely mathematical measures must be taken to make it reliable. Only explicit principles of thinking (logic) and explicit principles of space (geometry) can guarantee reliability. The great advance made by the theory invented 60 years ago by Eilenberg and Mac Lane permitted making the principles of logic and geometry explicit; this was accomplished by discovering the common form of logic and geometry so that the principles of the relation between the two are also explicit. They solved a problem opened 2300 years earlier by Aristotle with his initial inroads into making explicit the Categories of Concepts. In the 21st century, their solution is applicable not only to plane geometry and to medieval syllogisms, but also to infinite-dimensional spaces of transformations, to "spaces" of data, and to other conceptual tools that are applied thousands of times a day. The form of the principles of both logic and geometry was discovered by categorists to rest on "naturality" of the transformations between spaces and the transformations within thought.

What are your recollections of Grothendieck? When did you first meet him?

I had my first encounter with him at the ICM (Nice, 1970) where we were both invited lecturers. I publicly disagreed with some points he made in a separate lecture on his "Survival" movement, so that he later referred to me (affectionately, I hope) as the "main contradictor". In 1973 we were both briefly visiting Buffalo, where I vividly remember his tutoring me on basic insights of algebraic geometry, such as "points have automorphisms". In 1981 I visited him in his stone hut, in the middle of a lavender field in the south of France, in order to ask his opinion of a project to derive the Grauert theorem from the Cartan-Serre theorem, by proving the latter for a compact analytic space in a general topos, then specializing to the topos of sheaves on a parameter space. Some needed ingredients were known, for example that a compact space in the internal sense would correspond to a proper map to the parameter space externally. But the proof of these results classically depends on functional analysis, so that the theory of bornological spaces would have to be done internally in order to succeed. He recognized right away that such a development would depend on the use of the subobject classifier which, as he said, is one of the few ingredients of topos theory that he had not foreseen. Later in his work on homotopy he kindly referred to that object as the "Lawvere element". My last meeting with him was at the same place in 1989 (Aurelio Carboni drove me there from Milano): he was clearly glad to see me but would not speak, the result of a religious vow; he wrote on paper that he was also forbidden to discuss mathematics, though quickly his mathematical soul triumphed, leaving me with some precious mathematical notes.

F. W. Lawvere, A. Heller, R. Lavendhomme (in the back) and A. Carboni (CT99, Coimbra).

But the drastic reduction of scientific work by such a great mathematician, due to the encounter with a powerful designer religion, is cause for renewed vigilance.

You were born in Indiana. Did you grow up there?

Yes. I have been sometimes called "the farmboy from Indiana".

Did your parents have any mathematical interest?

No. My father was a farmer.

You obtained your BA degree from Indiana University in 1960. Please tell us a little bit about your education there. How did you learn about categories? We know that you started out as a student of Clifford Truesdell, a well-known expert on classical mechanics.[4]

I had been a student at Indiana University from 1955 to January 1960. I liked experimental physics but did not appreciate the imprecise reasoning in some theoretical courses. So I decided to study mathematics first. Truesdell was at the Mathematics Department but he had a great knowledge in Engineering Physics. He took charge of my education there.

Eilenberg had briefly been at Indiana, but had left in 1947 when I was just 10 years old. Thus it was not from Eilenberg that I learned first categories, nor was it from Truesdell who had taken up his position in Indiana in 1950 and who in 1955 (and subsequently) had advised me on pursuing the study of continuum mechanics and kinetic theory. It was a fellow student at Indiana who pointed out to me the importance of the galactic method mentioned in J. L. Kelley's topology book; it seemed too abstract at first, but I learned that "galactic" referred to the use of categories and functors and we discussed their potential for unifying and clarifying mathematics of all sorts. In Summer 1958 I studied Topological Dynamics with George Whaples, with the agenda of understanding as much as possible in categorical terms. When Truesdell asked me to lecture for several weeks in his 1958-1959 Functional Analysis course, it quickly became apparent that very effective explanations of such topics as Rings of Continuous Functions and the Fourier transform in Abstract Harmonic Analysis could be achieved by making explicit their functoriality and naturality in a precise Eilenberg-Mac Lane sense. While continuing to study statistical mechanics and kinetic theory, at some point I discovered Godement's book on sheaf theory in the library and studied it extensively. Throughout 1959 I was developing categorical thinking on my own and I formulated research programs on "improvement" (which I later learned had been worked out much more fully by Kan under the name of adjoint functors) and on "galactic clusters" (which I later learned had been worked out and applied by Grothendieck under the name of fibered categories). Categories would clearly be important for simplifying the foundations of continuum physics. I concluded that I would make category theory a central line of my study. The literature often mentioned some mysterious difficulty in basing category theory on the traditional set theory: having had a course on Kleene's book (also with Whaples) and having enjoyed many discussions with Max Zorn, whose office was adjacent to mine, I had some initial understanding of mathematical logic, and concluded that the solution to the foundational problem would be to develop an axiomatic theory of the Category of Categories.

Why did you choose Columbia University to pursue your graduate studies?

The decision to change graduate school (even before I was officially a graduate student) required some investigation. Who were the experts on category theory and where were they giving courses on it? I noted that Samuel Eilenberg appeared very frequently in the relevant literature, both as author and as co-author with Mac Lane, Steenrod, Cartan, Zilber. Therefore Columbia University was the logical destination. Consulting Clifford Truesdell about the proposed move, I was pleased to learn that he was a personal friend of Samuel Eilenberg; recognizing my resolve he personally contacted Sammy to facilitate my entrance into Columbia, and I sent documents briefly outlining my research programs to Eilenberg.

[4] C. Truesdell was the founder of the journals *Archive for Rational Mechanics and Analysis* and *Archive for the History of Exact Sciences*.

The NSF graduate fellowship which had supported my last period at Indiana turned out to be portable to Columbia. The Mathematics Department at Columbia had an arrangement whereby NSF fellows would also serve as teaching assistants. Thus I became a teaching assistant for Hyman Bass' course on calculus, i.e. linear algebra, until January 1961.

When I arrived in New York in February 1960, my first act was to go to the French bookstore and buy my own copy of Godement. In my first meeting with Eilenberg, I outlined my idea about the category of categories. Even though I only took one course, Homological Algebra, with Eilenberg, and although Eilenberg was very occupied that year with his duties as departmental chairman, I was able to learn a great deal about categories from Dold, Freyd, Mitchell, Gray; with Eilenberg I had only one serious mathematical discussion. Perhaps he had not had time to read my documents; at any rate it was a fellow student, Saul Lubkin, who after I had been at Columbia for several months remarked that what I had written about had already been worked out in detail under the name of adjoint functors, and upon asking Eilenberg about that, he gave me a copy of Kan's paper.

In 1960 Eilenberg had managed to attract at least ten of the later major contributors to category theory to Columbia as students or instructors. These courses and discussions naturally helped to make more precise my conception of the category of categories, as did my later study of mathematical logic at Berkeley; however the necessity for axiomatizing the category of categories was already evident to me while studying Godement in Indiana.

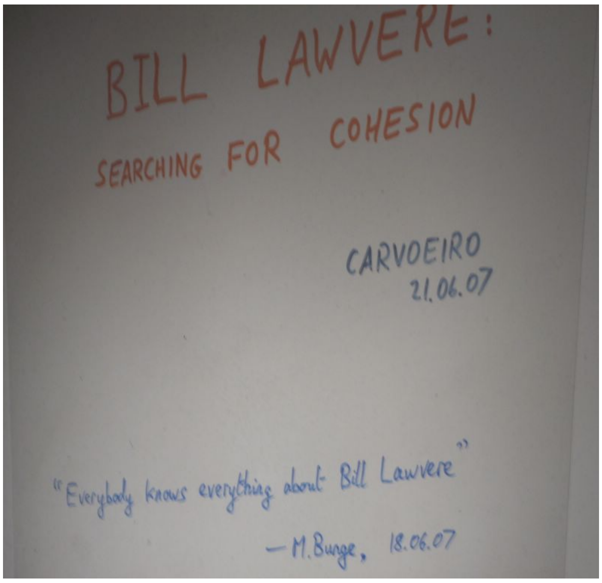
A few months later when Mac Lane was visiting New York City, Sammy introduced me to Saunders, jokingly describing my program as the mystifying “Sets without elements”.

In his autobiography[5]*, Mac Lane writes that “One day, Sammy told me he had a young student who claimed that he could do set theory without elements. It was hard to understand the idea, and he wondered if I could talk with the student. (...) I listened hard, for over an hour. At the end, I said sadly, ‘Bill, this just won't work. You can't do sets without elements, sorry,’ and reported this result to Eilenberg. Lawvere's graduate fellowship at Columbia was not renewed, and he and his wife left for California.” ...*

... I never proposed “Sets without elements” but the slogan caused many misunderstandings during the next 40 years because, for some reason, Saunders liked to repeat it. Of course, what my program discarded was instead the idea of elementhood as a primitive, the mathematically relevant ideas of both membership and inclusion being special cases of unique divisibility with respect to categorical composition. I argue that set theory should not be based on membership, as in Zermelo-Frankel set theory, but rather on isomorphism-invariant structure.

About Mac Lane's autobiography, note that when Mac Lane wrote it he was already at an advanced age, and according to his wife and daughter, he had already had several strokes. Unfortunately, the publisher rushed into print on the occasion of his death without letting his wife and his daughter correct it, as they had been promised. As a consequence, many small details are mistaken, for example the family name of Mac Lane's only grandson William, and Coimbra became Columbia[6], etc. Of course, nobody's memory is so good that he can remember another's history precisely, thus the main points concerning my contributions and my history often contain speculations that should have been checked by the editors and publisher.

With respect to that episode, it is treated briefly in the book, but in a rather compressed fashion, leading to some inaccuracies. The preliminary acceptance of my thesis by Eilenberg was encouraged by Mac Lane who acted as outside reader and I defended it before Eilenberg, Kadison, Morgenbesser and others in Hamilton Hall in May 1963.



First slide of Peter Johnstone's talk, about the work of F. W. Lawvere, at CT2007 (Carvoeiro, Algarve).

... to be continued in the next issue.

[5] Saunders Mac Lane, *A Mathematical Autobiography*, A K Peters, 2005.
[6] Idem, ibidem, p. 351.

Editors: António Caetano (acaetano@ua.pt)
Jorge Picado (picado@mat.uc.pt).

Address: Departamento de Matemática, Universidade de Coimbra, 3001-454 Coimbra, Portugal.

The CIM Bulletin is published twice a year. Material intended for publication should be sent to one of the editors.
The bulletin is available at www.cim.pt.

The CIM acknowledges the financial support of FCT - Fundação para a Ciência e a Tecnologia.