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Inhalt

Dodekaederstern — Titelseite: Vor dem neuen Gebäude der Fakultät für Mathematik der Universität Wien am Oskar Morgenstern-Platz 1 (Roßauerlände) wurde am 28.11.2013 eine Skulptur einer algebraischen Fläche enthüllt, die dieselbe Symmetrie wie dasjenige reguläre Dodekaeder aufweist, in dessen Ecken die 20 Singularitäten der Fläche liegen. Die Gleichung „$f = 0$“ dieser Fläche wurde in der Forschungsgruppe von H. Hauser im Rahmen einer Diplomarbeit bestimmt. Mit der Symmetriegruppe $G \cong A_5 \times \mathbb{Z}_2$ des Dodekaeders ergibt sich das G-invariante Polynom f als eine geeignete Kombination von bekannten Erzeugern u, v, w von $\mathbb{R}[x,y,z]^G$:

$$u = x^2 + y^2 + z^2, \;\; v = (x^2 - \varphi^2 y^2)(y^2 - \varphi^2 z^2)(z^2 - \varphi^2 x^2),$$
$$f = 5c(2\varphi - 3)v - (1-u)^3 + \frac{5}{27} c u^3 \qquad (\varphi = \tfrac{1+\sqrt{5}}{2},\; c = 81).$$

Für mathematische Details siehe: Alexandra Fritz und Herwig Hauser, Platonic Stars. *Math. Intelligencer* 32 (2010), 22—36. Hintergrundinformation zur Skulptur an der Universität Wien finden sich auf *http://www.dodekaederstern.cc*.

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Geometric Multiscale Analysis: From Wavelets to Parabolic Molecules

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1 Introduction

The 21st century is typically referred to as the century of data. And indeed, today we face a deluge of data even already in daily life arising from, for instance, wireless communications or medical imaging procedures, which need to be acquired, analyzed, transmitted, and stored. These tasks pose very interesting challenges to mathematicians such as developing efficient methodologies for extracting key features from data or to derive optimality results concerning achievable compression rates.

The area of *applied harmonic analysis*, whose origin dates back to the 18th century and the introduction of the Fourier transform, promotes the following general approach. Given a class of data $\mathcal{C}$ in a Hilbert space, the data is decomposed according to

$$\mathcal{C} \ni x \longrightarrow (\langle x, \varphi_i \rangle)_{i \in I},$$

where $(\varphi_i)_{i \in I}$ is a carefully designed representation system. One key idea is that this *decomposition* now allows access to governing features of x. For instance, the location and direction of edges of an image x might be encoded in the set of indices $i \in I$ of those coefficients $\langle x, \varphi_i \rangle$ which are large in absolute value. In general, one might say that the associated coefficients $(\langle x, \varphi_i \rangle)_{i \in I}$ shall present the data in a form convenient for analysis and processing tasks.

A yet different set of applications such as, for instance, PDE solvers require *effi-*

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cient expansions of some $x \in \mathcal{C}$ in terms of a representation system $(\varphi_i)_{i\in I}$ by

$$x = \sum_{i\in I} c_i \varphi_i.$$

The representation system is ideally chosen such that the coefficient sequence $(c_i)_{i\in I}$ has fast decay in modulus, which is sometimes today coined a *sparse* representation. Certainly, if $(\varphi_i)_{i\in I}$ constitutes an orthonormal basis, the coefficients c_i have to be chosen as $(\langle x, \varphi_i\rangle)_{i\in I}$. In contrast to this, a redundant system allows for optimizing the sparsity of the sequence. One further key issue arising from numerical algorithms – which obviously require an approximation by finite sums – is the question which decay rate of the error of best N-term approximation is achievable. Already on an intuitive level, this shows the relation to the decomposition problem, since if the governing features are contained in the large coefficients, very few terms should already lead to high approximation rates.

Applied harmonic analysis poses certain desiderata to the choice of representation systems for decompositions and expansions. First, typically multiscale systems are chosen to allow different levels of resolution. Second, these representation systems are usually designed according to their partition of Fourier domain. And, third, for both the decomposition and the expansion fast algorithms should be available.

One prominent example are *wavelet systems* which are nowadays used in a variety of both theoretical and practical applications such as, for instance, in optimal schemes for solving elliptic PDEs [4] or in the compression standard JPEG2000 [16]. However, multivariate functions are typically governed by anisotropic – in the sense of directional – features such as singularities on lower dimensional embedded manifolds, which wavelets as isotropic systems cannot efficiently encode. Because of this reason, various novel anisotropic representation systems such as *curvelets* [3] and *shearlets* [13] have been suggested, which even has initiated the new research area of *geometric multiscale analysis*. For many of those systems, optimally sparse approximations have been proven for a particular function class in $L^2(\mathbb{R}^2)$, called cartoon-like functions, which serves as a model for functions governed by anisotropic features. Very recently, a general framework called *parabolic molecules* has been proposed in [9], which includes all those systems as special cases and, for the first time, provides a higher level viewpoint on and deep insight into representation systems providing optimally sparse approximations of most types of multivariate functions.

This article shall serve as an introduction to and a survey about geometric multiscale analysis and, in particular, the novel theory of parabolic molecules. For this, we will first give an introduction into wavelet systems (Section 2). After a discussion about the appearance of anisotropic features in multivariate versus univariate functions in Section 3, we will introduce shearlet systems (Section 4) followed by an introduction of curvelet systems (Section 5). Section 6 is then devoted to the theory of parabolic molecules. Finally, an outlook to a framework coined α-*molecules* [8], which covers to some extent even wavelets and ridgelets [2], and a framework called *universal shearlets* [7], which provides a significantly improved flexibility in scaling, is given.

2 Wavelets

We start our endeavour with introducing and discussing wavelets. A wavelet system consists of one or a few generating functions to which scaling and translation operators are applied. To introduce a wavelet system for $L^2(\mathbb{R}^2)$, let us first take a look at the one-dimensional situation.

Definition 2.1. Let $\phi, \psi \in L^2(\mathbb{R})$. Then the associated *wavelet system* for $L^2(\mathbb{R})$ is defined to be

$$\{\phi_m := \phi(\cdot - m) : m \in \mathbb{Z}\} \cup \{\psi_{j,m} := 2^{j/2}\psi(2^j \cdot - m) : j \geq 0, m \in \mathbb{Z}\}.$$

It should be noted that ϕ and ψ can be constructed so that the associated wavelet system forms an orthonormal basis for $L^2(\mathbb{R})$ [16], and one then refers to ϕ as the *scaling function* and ψ as *wavelet*. As it is typical in applied harmonic analysis, this system is designed to partition Fourier domain in a particular way. Figure 1 shows how usually the essential support of the elements in a wavelet system tile the Fourier domain into different frequency bands.

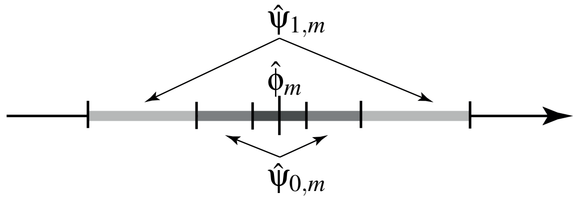

Figure 1: The partition of Fourier domain induced by a wavelet system for $L^2(\mathbb{R})$.

A wavelet system for $L^2(\mathbb{R}^2)$ can now be derived by a tensor product construction, leading to the following system.

Definition 2.2. Let $\phi, \psi \in L^2(\mathbb{R})$. Then the associated *wavelet system* for $L^2(\mathbb{R}^2)$ is defined by

$$\{\phi^{(1)}(x-m) : m \in \mathbb{Z}^2\} \cup \{2^j\psi^{(i)}(2^j x - m) : j \geq 0, m \in \mathbb{Z}^2, i = 1,2,3\},$$

where $\phi^{(1)}(x) = \phi(x_1)\phi(x_2)$, $\psi^{(1)}(x) = \phi(x_1)\psi(x_2)$, $\psi^{(2)}(x) = \psi(x_1)\phi(x_2)$, and $\psi^{(3)}(x) = \psi(x_1)\psi(x_2)$.

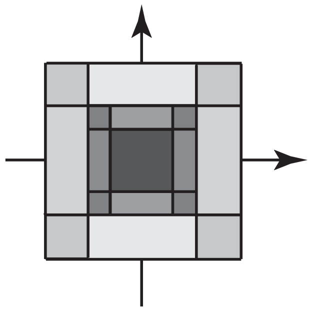

Figure 2: The partition of Fourier domain induced by wavelets for $L^2(\mathbb{R}^2)$.

The tiling of Fourier domain now takes the form as displayed in Figure 2, which the reader might want to compare with Figure 1.

One should point out that, on the application side, such 2D wavelet systems are used in the new compression standard JPEG2000. On the mathematical side, wavelet systems are proven to highly efficiently approximate L^2-functions which are smooth except for finitely many point singularities.

3 Anisotropy versus Isotropy

In contrast to univariate functions, multivariate functions cause the additional problem that they do not only exhibit point singularities, but also curvilinear singularities. And in fact, most multivariate functions appearing in applications are governed by such structures, which can be given either explicitly such as edges in images or implicitly such as shock fronts in transport equations.

A suitable model for such functions, called the model of cartoon-like functions, was introduced in [6] in 2001.

Definition 3.1. The set of *cartoon-like functions* $\mathcal{E}^2(\mathbb{R}^2)$ is defined by

$$\mathcal{E}^2(\mathbb{R}^2) = \{f \in L^2(\mathbb{R}^2) : f = f_0 + f_1 \cdot \chi_B\},$$

where $B \subset [0,1]^2$ with ∂B a closed C^2-curve with bounded curvature and $f_0, f_1 \in C_0^2([0,1]^2)$.

An illustration is shown in Figure 3.

Based on this model situation, the following result now provides a benchmark concerning the maximally achievable approximation rate.

Theorem 3.1 ([6])**.** *Let $(\psi_\lambda)_{\lambda\in\Lambda}$ be a frame for $L^2(\mathbb{R}^2)$. Then the optimal asymptotic approximation error of $f \in \mathcal{E}^2(\mathbb{R}^2)$ is*

$$\|f - f_N\|_2^2 \asymp N^{-2} \quad \text{as } N \to \infty, \quad \text{where } f_N = \sum_{\lambda\in\Lambda_N} c_\lambda \psi_\lambda$$

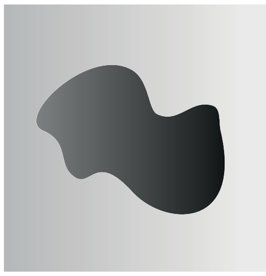

Figure 3: Illustration of a cartoon-like function.

is the (nonlinear) best N-term approximation.

As might be expected, since wavelets are isotropic objects due to the isotropic scaling matrix, they are only able to deliver a significantly suboptimal approximation rate of

$$\|f - f_N\|_2^2 \asymp N^{-1} \quad \text{as } N \to \infty.$$

The intuitive reason for this failure is illustrated in Figure 4.

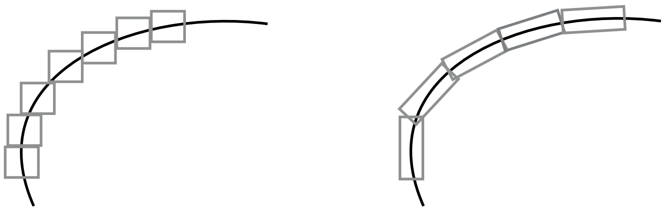

Figure 4: Approximation of a curve by isotropic and anisotropic objects.

This raises not only the question of whether there exist *anisotropic* representation systems $(\psi_\lambda)_{\lambda\in\Lambda}$ which meet this benchmark, but also whether they can be chosen to be 'conveniently' defined. More precisely, the new area of *geometric multiscale analysis* which arose from this question seeks to introduce representation systems which satisfy the following list of desiderata:

(D1) The system should be generated by one or few generating functions.
(D2) The benchmark from Theorem 3.1 should be met.
(D3) The system should allow for compactly supported analyzing elements.
(D4) The continuum and digital realm should be treated uniformly.
(D5) The associated transform should admit a fast implementation.

Item (D3) ensures high spatial localization, whereas item (D4) allows for faithful implementation of the continuum domain theory.

An abundance of approaches have been suggested with the probably most well-known ones being curvelets [3], contourlets [5], and shearlets [11]. We now continue by introducing shearlets, which are by now the only system actually satisfying the previously stated list of desiderata.

4 Shearlets

To accomodate (D1), shearlets are as wavelets based on very few generating functions to which scaling and translation operators are applied. Since these are however anisotropic systems, a third operation is required which changes their orientation.

As scaling, *parabolic scaling* is chosen – the reason being discussed in Section 6 – which is defined by

$$A_{2^j} = \begin{pmatrix} 2^j & 0 \\ 0 & 2^{j/2} \end{pmatrix} \quad \text{and} \quad \tilde{A}_{2^j} = \begin{pmatrix} 2^{j/2} & 0 \\ 0 & 2^j \end{pmatrix}.$$

To change the orientation via rotation would prevent (D4), since rotation does not leave the digital grid $\mathbb{Z}^2$ invariant. Hence *shearing*, given by

$$S_k = \begin{pmatrix} 1 & k \\ 0 & 1 \end{pmatrix}, \quad k \in \mathbb{Z},$$

is selected, and this selection indeed ensures (D4). By using the framework of *affine systems*, which consists of systems of the form

$$\{|\det M|^{1/2}\psi(M \cdot -m) : M \in G \subseteq GL_2,\, m \in \mathbb{Z}^2\}, \quad \psi \in L^2(\mathbb{R}^2),$$

the translation operation is already 'built-in'.

Since shearing does not provide a resolution of the directions as uniform as rotation, we require two separate systems to handle the more horizontally and the more

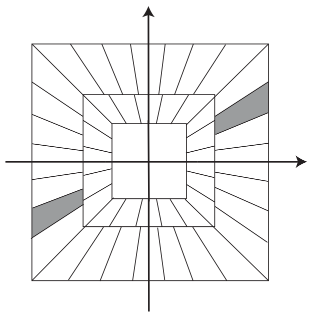

Figure 5: The partition of Fourier domain induced by a shearlet system.

vertically aligned directions. More precisely, we aim for a partition of Fourier domain as illustrated in Figure 5.

This leads to the following definition, in this form first stated in [14]. It should be noted before that the translation part is made more flexible by the introduction of the matrices M_c and $\tilde{M}_c$ in order to also enable finer sampling. We further remark that those systems are sometimes also referred to as *cone-adapted* shearlet systems.

Definition 4.1. For $\phi, \psi, \tilde{\psi} \in L^2(\mathbb{R}^2)$ and $c = (c_1, c_2) \in (\mathbb{R}_+)^2$, the *shearlet system* $SH(\phi, \psi, \tilde{\psi}; c)$ is defined by

$$SH(\phi, \psi, \tilde{\psi}; c) = \Phi(\phi; c_1) \cup \Psi(\psi; c) \cup \tilde{\Psi}(\tilde{\psi}; c),$$

where

$$\begin{aligned} \Phi(\phi; c_1) &:= \{\phi_m = \phi(\cdot - c_1 m) : m \in \mathbb{Z}^2\}, \\ \Psi(\psi; c) &:= \{\psi_{j,k,m} = 2^{\frac{3}{4}j}\psi(S_k A_{2^j} \cdot - M_c m) : j \geq 0, |k| \leq \lceil 2^{j/2} \rceil, m \in \mathbb{Z}^2\}, \\ \tilde{\Psi}(\tilde{\psi}; c) &:= \{\tilde{\psi}_{j,k,m} = 2^{\frac{3}{4}j}\tilde{\psi}(S_k^T \tilde{A}_{2^j} \cdot - \tilde{M}_c m) : j \geq 0, |k| \leq \lceil 2^{j/2} \rceil, m \in \mathbb{Z}^2\}, \end{aligned}$$

with

$$M_c = \begin{pmatrix} c_1 & 0 \\ 0 & c_2 \end{pmatrix} \quad \text{and} \quad \tilde{M}_c = \begin{pmatrix} c_2 & 0 \\ 0 & c_1 \end{pmatrix}.$$

A first large class of functions considered as generators was the following class of band-limited functions, i.e., functions whose Fourier transform is compactly supported.

Example 4.1. *Classical shearlets* are functions $\psi \in L^2(\mathbb{R}^2)$ of the form

$$\hat{\psi}(\xi) = \hat{\psi}(\xi_1, \xi_2) = \hat{\psi}_1(\xi_1)\, \hat{\psi}_2(\tfrac{\xi_2}{\xi_1}),$$

where $\psi_1 \in L^2(\mathbb{R})$ is a discrete wavelet in the sense that it satisfies the discrete Calderón condition, given by

$$\sum_{j \in \mathbb{Z}} |\hat{\psi}_1(2^{-j}\xi)|^2 = 1 \quad \text{for a.e. } \xi \in \mathbb{R},$$

with $\hat{\psi}_1 \in C^\infty(\mathbb{R})$ and $\operatorname{supp} \hat{\psi}_1 \subseteq [-\frac{1}{2}, -\frac{1}{16}] \cup [\frac{1}{16}, \frac{1}{2}]$, and $\psi_2 \in L^2(\mathbb{R})$ is a 'bump function' in the sense that

$$\sum_{k=-1}^{1} |\hat{\psi}_2(\xi + k)|^2 = 1 \quad \text{for a.e. } \xi \in [-1, 1], \tag{4.1}$$

satisfying $\hat{\psi}_2 \in C^\infty(\mathbb{R})$ and $\operatorname{supp} \hat{\psi}_2 \subseteq [-1, 1]$.

One might now ask whether there exist generators so that the associated shearlet system constitutes an orthonormal basis for $L^2(\mathbb{R})$. However, no constructions of shearlet orthonormal bases are known to date due to the redundancy coming from the shear component. But there exists a variety of shearlet systems which still have superior stability properties in the sense of frames. For those readers not familiar with this functional analytic concept, let us briefly recall the basics from frame theory.

Definition 4.2. A sequence $(g_i)_{i\in I}$ is a *frame for* $L^2(\mathbb{R}^2)$, if there exist constants $0 < A \leq B < \infty$ such that

$$A\|f\|_2^2 \leq \sum_{i\in I} |\langle f, g_i\rangle|^2 \leq B\|f\|_2^2 \quad \text{for all } f \in L^2(\mathbb{R}^2).$$

A and B are called the *lower* and *upper frame bound*, respectively. If $A = B$ is possible, $(g_i)_{i\in I}$ is called a *tight* frame. In case $A = B = 1$, it is referred to as a *Parseval frame*.

A frame $(g_i)_{i\in I}$ for $L^2(\mathbb{R}^2)$ allows the analysis of elements in $L^2(\mathbb{R}^2)$ through application of the *analysis operator* given by

$$T : L^2(\mathbb{R}^2) \to \ell_2(I), \quad T(f) = (\langle f, g_i\rangle)_{i\in I}.$$

The associated *frame operator* $Sf = T^*Tf = \sum_{i\in I}\langle f, g_i\rangle g_i$ in turn gives rise to the reconstruction formula

$$f = \sum_{i\in I}\langle f, g_i\rangle S^{-1}g_i \quad \text{for all } f \in L^2(\mathbb{R}^2).$$

Coming back to the situation of shearlets, it was then shown in [11], that $SH(\phi, \psi, \tilde{\psi}; (1,1))$ with suitable ϕ, with $\psi, \tilde{\psi}$ being classical shearlets ($\tilde{\psi}$ with interchanged variables), and with small modifications of the boundary elements forms a Parseval frame for $L^2(\mathbb{R}^2)$.

To however accommodate (D3), we require compactly supported generators. This forces us to give up on optimal stability, i.e., on a Parseval frame. But the following result shows that one still has a certain degree of stability by being able to control the frame bounds.

Theorem 4.1 ([12])**.** *For* $\alpha > \gamma > 3$, $q > q' > 0$ *and* $q > r > 0$, *let*

$$|\hat{\psi}(\xi_1, \xi_2)| \leq C \cdot \min\{1, |q\xi_1|^{\alpha}\} \cdot \min\{1, |q'\xi_1|^{-\gamma}\} \cdot \min\{1, |r\xi_2|^{-\gamma}\},$$

and

$$\sum_{j,k} |\hat{\psi}(S_{-k}^T A_{2^{-j}}\xi)|^2 \geq C' > 0,$$

and similar for $\tilde{\psi}$. Then there exists a sampling constant c_0 such that $SH(\phi, \psi, \tilde{\psi}; c)$ is a frame for $L^2(\mathbb{R}^2)$ for all $c \le c_0$ with

$$c^{-2}C_1(\alpha,\gamma,q,q',r,c) \le A \le B \le c^{-2}C_2(\alpha,\gamma,q,q',r,c),$$

where explicit formulas for $C_1(\alpha,\gamma,q,q',r,c)$ and $C_2(\alpha,\gamma,q,q',r,c)$ exist.

Our original goal was though to meet the benchmark from Theorem 3.1. This is the content of the next theorem, which shows that also (D2) is satisfied by shearlets. In fact, it meets it up to a log-factor, wherefore we included the '(almost)'. However, if one regards a log-factor as negligible, this is indeed the optimal achievable rate.

Theorem 4.2 ([14]). *Let $\psi \in L^2(\mathbb{R}^2)$ (similar for $\tilde{\psi}$) be compactly supported such that, for $\alpha > 5$, $\gamma \ge 4$, $h \in L^1(\mathbb{R})$,*

(i) $|\hat{\psi}(\xi)| \le C \cdot \min\{1,|\xi_1|^{\alpha}\} \cdot \min\{1,|\xi_1|^{-\gamma}\} \cdot \min\{1,|\xi_2|^{-\gamma}\}$,

(ii) $\left|\frac{\partial}{\partial \xi_2}\hat{\psi}(\xi)\right| \le |h(\xi_1)| \cdot \left(1+\frac{|\xi_2|}{|\xi_1|}\right)^{-\gamma}$.

Suppose $SH(\phi,\psi,\tilde{\psi};c)$ forms a frame for $L^2(\mathbb{R}^2)$. Then it provides (almost) optimally sparse approximations of cartoon-like functions $f \in \mathcal{E}^2(\mathbb{R}^2)$ in the sense that

$$\|f - f_N\|_2^2 \le C \cdot N^{-2} \cdot (\log N)^3 \quad \text{as } N \to \infty,$$

where f_N is the N-term approximation consisting of the N largest shearlet coefficients.

To provide some intuition, let us give a heuristic argument which shows why the rate N^{-2} can be achieved. Due to the form of f_N, we obtain

$$\|f - f_N\|_2^2 \le \frac{1}{A} \cdot \sum_{n>N} (|\langle f, \sigma_\eta \rangle|)^2_{(n)}, \tag{4.2}$$

where $(\sigma_\eta)_\eta$ is the shearlet frame with lower frame bound A and $(|\langle f,\sigma_\eta\rangle|)_{(n)}$ denotes the nth largest shearlet coefficient. To estimate these coefficients, we have to distinguish three cases which are illustrated in Figure 6. In the cases of Figure 6(a)+(b), the coefficients are negligible, mainly because of the assumed (directional) vanishing moment conditions. In the case of Figure 6(c), we can estimate

$$|\langle f, \sigma_\eta \rangle| \le \|f\|_\infty \|\sigma_\eta\|_1 \le C \cdot 2^{-\frac{3}{4}j}.$$

Thus, we know that there exist $2^{j/2}$ of such coefficients – recall that each shearlet has a length of $2^{-j/2}$ – and we have an estimate for each of those. This allows us to complete (4.2) by

$$\|f - f_N\|_2^2 \le \frac{1}{A} \cdot \sum_{n>N} (|\langle f, \sigma_\eta \rangle|)^2_{(n)} \le \frac{C}{A} \cdot \sum_{n>N} (n^{-\frac{3}{2}})^2 \le \frac{C}{A} \cdot N^{-2},$$

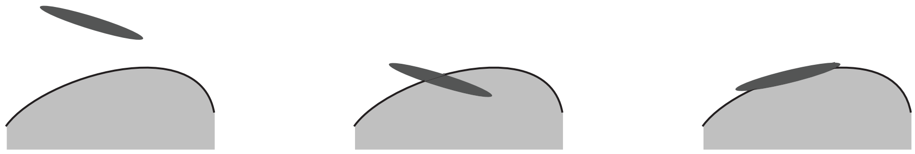

Figure 6: Positions of different shearlets with respect to a discontinuity curve of a cartoon-like function.

thereby finishing the argument.

Finally, also (D5) is satisfied by shearlets. In fact, *www.shearlab.org* provides an extensive software package for the 2D and 3D shearlet transform based on compactly supported shearlets with accompanying publication [15].

5 Curvelets

The system of curvelets predates shearlets, and was originally introduced based on ridgelets [2], which are systems of certain ridge functions to optimally sparsely approximate ridge-like singularities. The nowadays utilized curvelet system, also called second generation curvelets, was introduced in [3]. As typical for the area of applied harmonic analysis, they are designed to partition Fourier domain in a particular way, which in this case is illustrated in Figure 7. As can be seen, it provides a perfect resolution of the different directions in contrast to the approximate one provided by shearlets. This however comes with the disadvantage that there does not exist a faithful implementation of curvelets, which are because of this fact a purely continuum domain theory. It should though be emphasized that curvelets were the first system shown to deliver (almost) optimally sparse approximations of cartoon-like functions [3], which can justifiably be called a breakthrough.

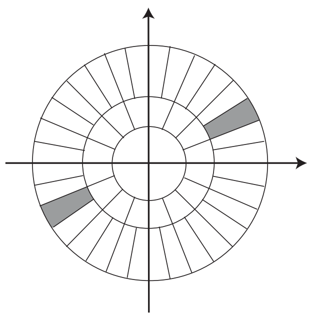

Figure 7: The partition of Fourier domain induced by a curvelet system.

The definition of curvelets is as follows.

Definition 5.1. Let $W \in C^\infty(\mathbb{R})$ be a wavelet with $\operatorname{supp}(W) \subseteq \left(\frac{1}{2}, 2\right)$, and $V \in C^\infty(\mathbb{R})$ be a 'bump function' (cf. (4.1)) with $\operatorname{supp}(V) \subseteq (-1,1)$. Then the *curvelet system* $\left(\gamma_{(j,\ell,k)}\right)_{(j,\ell,k)\in\Lambda^0}$, where

$$\Lambda^0 := \left\{ (j,\ell,k) \in \mathbb{Z}^4 \;:\; j \geq 0,\; \ell = -2^{\lfloor \frac{j}{2} \rfloor -1}, \cdots, 2^{\lfloor \frac{j}{2} \rfloor -1} \right\} \tag{5.1}$$

is defined in polar coordinates by

$$\hat{\gamma}_{(j,0,0)}(r,\omega) := 2^{-3j/4} W\left(2^{-j} r\right) V(2^{\lfloor j/2 \rfloor}\omega)$$

and

$$\gamma_{(j,\ell,k)}(\cdot) := \gamma_{(j,0,0)}(R_{\theta_{(j,\ell)}}(\cdot - x_{(j,\ell,k)}))$$

with $\theta_{(j,\ell)} = \pi\ell/2^{j/2}$ and $x_{(j,\ell,k)} = R_{\theta_{(j,\ell)}} A_{2^{-j}} m$ for $j \geq j_0$, $\ell = 0, \ldots, 2^{\lfloor j/2 \rfloor} - 1$, and $m \in \mathbb{Z}^2$.

It was shown in [3] that this system constitutes a Parseval frame for $L^2(\mathbb{R}^2)$ provided that appropriate functions to address the low frequency part are included.

The following result from 2004 is the first result providing a representation system which (almost) meets the benchmark from Theorem 3.1.

Theorem 5.1 ([3]). *The curvelet system provides (almost) optimally sparse approximations of cartoon-like functions $f \in \mathcal{E}^2(\mathbb{R}^2)$ in the sense that*

$$\|f - f_N\|_2^2 \leq C \cdot N^{-2} \cdot (\log N)^3 \quad \text{as } N \to \infty,$$

where f_N is the N-term approximation consisting of the N largest curvelet coefficients.

6 Parabolic Molecules

As can be seen from Sections 4 and 5, curvelets differ significantly from shearlets, since they do not form affine systems; they are based on rotation rather than shearing causing problems with faithful implementations, and no compactly supported version is available causing problems with high spatial localization. But there are also striking similarities, since both systems (almost) optimally sparsify cartoon-like functions. Thus the sparsity properties of curvelets and shearlets are similar, and even more, the results for the band-limited version of both systems are proven with resembling proofs [3, 10]. This observation raises the question whether there exists a general framework for such directional systems and what the fundamental concept behind sparse approximation results really is.

The properties we require of a general framework are to cover all systems known to provide optimally sparse approximations of cartoon-like functions, to enable

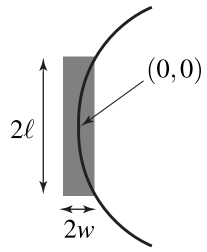

Figure 8: Anisotropic function approximating a curvilinear singularity.

an easy transfer of (sparsity) results between systems, to allow a categorization of systems with respect to sparsity behaviors, and to also be general enough to allow constructions of novel systems.

The common bracket between, in particular, curvelets and shearlets is *parabolic scaling*. This is due to the fact that parabolic scaling is perfectly adapted to the C^2 regularity of the discontinuity curve employed in the cartoon-like model as can be seen heuristically. For this, let $(E(x_2), x_2), x_2 \in I$ be a parametrization of the singularity curve in Figure 8 and, by noting that $E(0) = E'(0) = 0$, consider the approximation by the Taylor series

$$E(x_2) \approx \frac{1}{2}\kappa x_2^2.$$

For a fixed length 2ℓ, let $2w$ be the width of the smallest rectangle centered at $(0,0)$ containing the entire edge curve E (cf. Figure 8). Then, since $E(\ell) = w$, we obtain

$$w \approx \frac{\kappa}{2}\ell^2,$$

which can be interpreted as *'width* $\approx$ *length*^{2}*'*, which in turn is parabolic scaling.

After agreeing on the type of scaling, a general framework requires a common parameter space, which will be chosen as

$$\mathbb{P} := \mathbb{R}_+ \times \mathbb{T} \times \mathbb{R}^2,$$

where $(s, \theta, x) \in \mathbb{P}$ describes scale 2^s, orientation θ, and location x.

Definition 6.1. A *parametrization* is a pair (Λ, Φ_Λ), where Λ is a discrete index set and Φ_Λ is a mapping

$$\Phi_\Lambda : \begin{cases} \Lambda & \to & \mathbb{P}, \\ \lambda & \mapsto & (s_\lambda, \theta_\lambda, x_\lambda). \end{cases}$$

We notice that for now, no properties of the map Φ_Λ are required.

Since curvelets are very adapted to the parameter space, a possible associated parametrization can be easily derived as follows.

Example 6.1. The *canonical parametrization* (Λ^0, Φ^0) with Λ^0 being the index set associated with curvelets from (5.1) is defined by

$$\Phi^0(j,\ell,k) = (s_\lambda, \theta_\lambda, x_\lambda) = (j, \ell 2^{-\lfloor j/2 \rfloor}\pi, R_{-\theta_\lambda} A_{2^{-s_\lambda}} k).$$

The key idea of the definition of parabolic molecules from [9] which now follows is to define maximally flexible systems based on parabolic scaling, rotation, and translation with parameter space $\mathbb{P}$ which allow a different generating function for each index. Decay and smoothness properties of those functions are then governed by the parameters (L,M,N_1,N_2), where L measures spatial localization, M the number of directional (almost) vanishing moments, and N_1, N_2 smoothness.

Definition 6.2. Let (Λ, Φ_Λ) be a parametrization. Then $(m_\lambda)_{\lambda \in \Lambda}$ is a *system of parabolic molecules of order* $(L,M,N_1,N_2) \in (\mathbb{Z}_+ \cup \{\infty\}) \times \mathbb{Z}_+^3$, if, for all $\lambda \in \Lambda$,

$$m_\lambda(x) = 2^{3s_\lambda/4} a^{(\lambda)} \left(A_{2^{s_\lambda}} R_{\theta_\lambda} (x - x_\lambda) \right), \quad \Phi_\Lambda(\lambda) = (s_\lambda, \theta_\lambda, x_\lambda),$$

such that, for all $|\beta| \leq L$,

$$\left| \partial^\beta \hat{a}^{(\lambda)}(\xi) \right| \lesssim \min\left(1, 2^{-s_\lambda} + |\xi_1| + 2^{-s_\lambda/2}|\xi_2|\right)^M \langle |\xi| \rangle^{-N_1} \langle \xi_2 \rangle^{-N_2},$$

where $\langle x \rangle := (1+x^2)^{1/2}$.

This framework can be shown to, in particular, include parabolic frames [17], curvelets [3], band-limited shearlets [10], frame decompositions [1], and compactly supported shearlets [12].

Heading towards a general result which poses conditions on the parameters (L,M,N_1,N_2) for a system of parabolic molecules to deliver (almost) optimally sparse approximations of cartoon-like functions, we first state a result which analyzes the decay of the cross-Gramian of two systems of parabolic molecules. This will allow us to transfer the optimal sparse approximation result from curvelets to other systems of parabolic molecules. For this decay result though, we require a distance function between two indices from the common parameter space.

Definition 6.3. Let (Λ, Φ_Λ) and $(\tilde{\Lambda}, \Phi_{\tilde{\Lambda}})$ be parametrizations. For $\lambda \in \Lambda$ and $\mu \in \tilde{\Lambda}$, we define the *index distance* by

$$\omega(\lambda,\mu) := \omega(\Phi_\Lambda(\lambda), \Phi_{\tilde{\Lambda}}(\mu)) := 2^{|s_\lambda - s_\mu|} \left(1 + 2^{\min(s_\lambda, s_\mu)} d(\lambda,\mu)\right),$$

where

$$d(\lambda,\mu) := |\theta_\lambda - \theta_\mu|^2 + |x_\lambda - x_\mu|^2 + |\langle (\cos(\theta_\lambda), \sin(\theta_\lambda))^\top, x_\lambda - x_\mu \rangle|.$$

We remark that d is nothing else than the Hart Smith's phase space metric on $\mathbb{T} \times \mathbb{R}^2$ (cf. [17]). Using the index distance, we can next state the result on the decay of the cross-Gramian of two systems of parabolic molecules.

Theorem 6.1 ([9]). *Let $N > 0$, and let $(m_\lambda)_{\lambda \in \Lambda}$, $(p_\mu)_{\mu \in \tilde{\Lambda}}$ be systems of parabolic molecules of order (L, M, N_1, N_2) with*

$$L \geq 2N, \quad M > 3N - \frac{5}{4}, \quad N_1 \geq N + \frac{3}{4}, \quad N_2 \geq 2N.$$

Then, for all $\lambda \in \Lambda$ and $\mu \in \tilde{\Lambda}$,

$$\left| \langle m_\lambda, p_\mu \rangle \right| \lesssim \omega(\lambda, \mu)^{-N}.$$

As already mentioned, this result will be key to transfer the (almost) optimal sparse approximation result from curvelets to various other systems of parabolic molecules. This is however only possible provided that the parametrization of this other system is in some sense 'consistent' with the (canonical) parametrization of curvelets introduced in Example 6.1.

Definition 6.4. A parametrization (Λ, Φ_Λ) is *k-admissible*, if

$$\sup_{\lambda \in \Lambda} \sum_{\mu \in \Lambda^0} \omega(\lambda, \mu)^{-k} < \infty \quad \text{and} \quad \sup_{\lambda \in \Lambda^0} \sum_{\mu \in \Lambda} \omega(\lambda, \mu)^{-k} < \infty.$$

As expected, the curvelet parametrization is, for instance, consistent with itself for all $k > 2$, as was shown in [9].

The next main result now reveals a very large class of representation systems parametrized as systems of parabolic molecules which all provide (almost) optimally sparse approximations of cartoon-like functions. It in fact proves that this class consists of all frames of parabolic molecules whose parametrization is consistent with the canonical one and whose associated parameters (L, M, N_1, N_2) are sufficiently large.

Theorem 6.2 ([9]). *Let $(m_\lambda)_{\lambda \in \Lambda}$ be a system of parabolic molecules of order (L, M, N_1, N_2) such that*

(i) *$(m_\lambda)_{\lambda \in \Lambda}$ constitutes a frame for $L_2(\mathbb{R}^2)$,*
(ii) *Λ is k-admissible for all $k > 2$,*
(iii) *it holds that*

$$L \geq 6, \quad M > 9 - \frac{5}{4}, \quad N_1 \geq 3 + \frac{3}{4}, \quad N_2 \geq 6.$$

Then, for any $\varepsilon > 0$ *and for any* $f \in \mathcal{E}^2(\mathbb{R}^2)$*,* $(m_\lambda)_{\lambda\in\Lambda}$ *satisfies*

$$\|f - f_N\|_2^2 \leq C \cdot N^{-2+\varepsilon} \quad \textit{as } N \to \infty,$$

It should be emphasized that this result does not only provide a higher level view of the properties which are required of systems to deliver (almost) optimal sparse approximations of cartoon-like functions, but it also allows the construction of a variety of novel systems which automatically exhibit such approximation properties.

7 ... α-Molecules?

Finally, the question arises whether it might be possible to extend the framework of parabolic molecules to include wavelets; maybe even ridgelets. In fact, very recently a much more elaborate framework coined α*-molecules* was introduced in [8], the key idea being to incorporate a parameter $\alpha \in [0,1]$ to measure the amount of anisotropy by considering the scaling matrix

$$A_{\alpha,a} = \begin{pmatrix} a & 0 \\ 0 & a^\alpha \end{pmatrix}, \quad a > 0.$$

In this setting, $\alpha = 1$ corresponds to the scaling associated with wavelets, $\alpha = \frac{1}{2}$ to curvelets or shearlets, and $\alpha = 0$ to ridgelets. In [8] in the spirit of parabolic molecules, an elaborate framework is introduced which then allows to derive sparse approximation results for large classes of systems, thereby on a higher level linking approximation properties to structural properties.

A yet different extension, which should also be mentioned are so-called *universal shearlets*, introduced in [7]. Those systems allow a different scaling matrix for each scale j with $\alpha_j \in [\frac{1}{2}, 1]$, and are specifically designed to parametrize a path from wavelets to shearlets. They were introduced for the purpose of analyzing the ability of wavelets versus shearlets for compressed sensing based inpainting algorithms. Still it can be envisioned that the idea of varying scaling could be incorporated in the framework of α*-molecules* allowing even more flexibility.

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Interview with Abel Laureate Pierre Deligne

Martin Raussen and Christian Skau
Aalborg, Denmark and Trondheim, Norway

The Abel Prize

Dear Professor Deligne, first of all we would like to congratulate you as the eleventh recipient of the Abel Prize. It is not only a great honour to be selected as recipient of this prestigious prize, the Abel Prize also carries a cash-amount of 6 million NOK, that is around 1 million US$. We are curious to hear what you are planning to do with this money...

I feel that this money is not really mine, but it belongs to mathematics. I have a responsibility to use it wisely and not in a wasteful way. The details are not clear yet, but I plan to give part of the money to the two institutions that have been most important to me: the Institut des Hautes Études Scientifiques (IHÉS) in Paris and to the Institute for Advanced Study (IAS) in Princeton.

I would also like to give some money to support mathematics in Russia. First to the Department of Mathematics of the Higher School of Economics (HSE). In my opinion, it is one of the best places in Moscow. It is much smaller than the Faculty of Mechanics and Mathematics at the University, but has better people. The student body is small; only fifty new students are accepted each year. But they are among the best students. The HSE has been created by economists. They have done their best under difficult circumstances. The department of mathematics has been created five years ago, with the help of the Independent University of Moscow. It is giving prestige to the whole HSE. There I think some money could be well used.

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Another Russian institution I would like to donate some money to is the Dynasty Foundation, created by the Russian philanthropist Dmitry Zimin. For them, money is most likely not that important. It is rather a way for me to express my admiration for their work. It is one of the very few foundations in Russia that gives money to science; moreover, they do it in a very good way. They give money to mathematicians, to physicists and to biologists; especially to young people, and this is crucial in Russia! They also publish books to popularize science. I want to express my admiration for them in a tangible way.

The Abel Prize is certainly not the first important prize in mathematics that you have won. Let us just mention the Fields Medal that you received 35 years ago, the Swedish Crafoord Prize, the Italian Balzan Prize and the Israeli Wolf Prize. How important is it for you, as a mathematician, to win such prestigious prizes? And how important is it for the mathematical community that such prizes exist?

For me personally, it is nice to be told that mathematicians I respect find the work I have done interesting. The Fields Medal possibly helped me to be invited to the Institute for Advanced Study. To win prizes gives opportunities, but they have not changed my life.

I think prizes can be very useful when they can serve as a pretext for speaking about mathematics to the general public. I find it particularly nice that the Abel Prize is connected with other activities such as competitions directed towards children and the Holmboe Prize for high school teachers. In my experience, good high school teachers are very important for the development of mathematics. I think all these activities are marvellous.

Youth

You were born in 1944, at the end of the Second World War in Brussels. We are curious to hear about your first mathematical experiences: In what respect were they fostered by your own family or by school? Can you remember some of your first mathematical experiences?

I was lucky that my brother was seven years older than me. When I looked at the thermometer and realized that there were positive and negative numbers, he would try to explain to me that minus one times minus one is plus one. That was a big surprise. Later when he was in high school he told me about the second degree equation. When he was at the university he gave me some notes about the third degree equation, and there was a strange formula for solving it. I found it very interesting.

When I was a Boy Scout, I had a stroke of extraordinary good luck. I had a friend there whose father, Monsieur Nijs, was a high school teacher. He helped me in a

Pierre Deligne during his acceptance speech at the Abel award ceremony. (Photo: Heiko Junge/NTB scanpix)

From left to right: Pierre Deligne, Martin Raussen, Christian Skau. (Photo: Anne-Marie Astad)

number of ways; in particular, he gave me my first real mathematical book, namely *Set Theory* by Bourbaki, which is not an obvious choice to give to a young boy. I was 14 years old at the time. I spent at least a year digesting that book. I guess I had some other lectures on the side, too.

Having the chance to learn mathematics at one's own rhythm has the benefit that one revives surprises of past centuries. I had already read elsewhere how rational numbers, then real numbers, could be defined starting from the integers. But I remember wondering how integers could be defined from set theory, looking a little ahead in Bourbaki, and admiring how one could first define what it means for two sets to have the "same number of elements", and derive from this the notion of integers. I was also given a book on complex variables by a friend of the family. To see that the story of complex variables was so different from the story of real variables was a big surprise: once differentiable, it is analytic (has a power series expansion), and so on. All those things that you might have found boring at school were giving me a tremendous joy.

Then this teacher, Monsieur Nijs, put me in contact with professor Jacques Tits at the University of Brussels. I could follow some of his courses and seminars, though I still was in high school.

It is quite amazing to hear that you studied Bourbaki, which is usually considered quite difficult, already at that age.

Can you tell us a bit about your formal school education? Was that interesting for you, or were you rather bored?

I had an excellent elementary school teacher. I think I learned a lot more in elementary school than I did in high school: how to read, how to write, arithmetics and much more. I remember how this teacher made an experiment in mathematics which made me think about proofs, surfaces and lengths. The problem was to compare the surface of a half sphere with that of the disc with the same radius. To do so, he covered both surfaces with a spiralling rope. The half sphere required twice as much rope. This made me think a lot: how could one measure a surface with a length? How to be sure that the surface of the half sphere was indeed twice that of the disc?

When I was in high school, I liked problems in geometry. Proofs in geometry make sense at that age because surprising statements have not too difficult proofs. Once we were past the axioms, I enjoyed very much doing such exercises. I think that geometry is the only part of mathematics where proofs make sense at the high school level. Moreover, writing a proof is another excellent exercise. This does not only concern mathematics, you also have to write in correct French – in my case – in order to argue why things are true. There is a stronger connection between language and mathematics in geometry than for instance in algebra, where you have a set of equations. The logic and the power of language are not so apparent.

You went to the lectures of Jacques Tits when you were only 16 years old. There is a story that one week you could not attend because you participated in a school trip. . . ?

Yes. I was told this story much later. When Tits came to give his lecture he asked: Where is Deligne? When it was explained to him that I was on a school trip, the lecture was postponed to the next week.

He must already have recognised you as a brilliant student. Jacques Tits is also a recipient of the Abel Prize. He received it together with John Griggs Thompson five years ago for his great discoveries in group theory. He was surely an influential teacher for you?

Yes; especially in the early years. In teaching, the most important can be what you don't do. For instance, Tits had to explain that the centre of a group is an invariant subgroup. He started a proof, then stopped and said in essence: "An invariant subgroup is a subgroup stable by all inner automorphisms. I have been able to define the centre. It is hence stable by all symmetries of the data. So it is obvious that it is invariant."

For me, this was a revelation: the power of the idea of symmetry. That Tits did not need to go through a stepby-step proof, but instead could just say that symmetry makes the result obvious, has influenced me a lot. I have a very big respect for symmetry, and in almost every of my papers there is a symmetry-based argument.

Can you remember how Tits discovered your mathematical talent?

That I cannot tell, but I think it was Monsieur Nijs who told him to take good care of me. At that time, there were three really active mathematicians in Brussels: apart from Tits himself, professors Franz Bingen and Lucien Waelbroeck. They organised a seminar with a different subject each year. I attended these seminars and I learned about different topics such as Banach algebras, which were Waelbroeck's speciality, and algebraic geometry.

Then, I guess, the three of them decided it was time for me to go to Paris. Tits introduced me to Grothendieck and told me to attend his lectures as well as Serre's. That was an excellent advice.

This can be a little surprising to an outsider. Tits being interested in you as a mathematician, one might think that he would try to capture you for his own interests. But he didn't?

No. He saw what was best for me and acted accordingly.

Algebraic geometry

Before we proceed to your career in Paris, perhaps we should try to explain to the audience what your subject algebraic geometry is about.

When Fields medalist Tim Gowers had to explain your research subjects to the audience during the Abel Prize announcement earlier this year, he began by confessing that this was a difficult job for him. It is difficult to show pictures that illustrate the subject, and it is also difficult to explain some simple applications. Could you, nevertheless, try to give us an idea what algebraic geometry is about? Perhaps you can mention some specific problems that connect algebra and geometry with each other.

In mathematics, it is always very nice when two different frames of mind come together. Descartes wrote: "La géométrie est l'art de raisonner juste sur des figures fausses" (Geometry is the art of correct reasoning on false figures). "Figures" is plural: it is very important to have various perspectives and to know in which way each is wrong.

In algebraic geometry, you can use intuitions coming both from algebra – where you can manipulate equations – and from geometry, where you can draw pictures. If you picture a circle and consider the equation $x^2 + y^2 = 1$, different images are evoked in your mind, and you can try to play one against the other. For instance, a wheel is a circle and a wheel turns; it is interesting to see what the analogue is in algebra: an algebraic transformation of x and y maps any solution of $x^2 + y^2 = 1$ to another. This equation describing a circle is of the second degree. This implies that a circle will have no more than two intersections points with a line. This is a property you also see geometrically, but the algebra gives more. For instance, if the line has a rational equation and one of the intersection points with the circle $x^2 + y^2 = 1$ has rational coordinates, then the other intersection point will also have rational coordinates.

Algebraic geometry can have arithmetical applications. When you consider polynomial equations, you can use the same expressions in different number systems. For instance, on finite sets on which addition and multiplication are defined, these equations lead to combinatorial questions: you try to count the number of solutions. But you can continue to draw the same pictures, keeping in mind a new way in which the picture is false, and in this way you can use geometrical intuition while looking at combinatorial problems.

I have never really been working at the centre of algebraic geometry. I have mostly been interested in all sorts of questions that only touch the area. But algebraic geometry touches many subjects! As soon as a polynomial appears, one can try to think about it geometrically; for example in physics with Feynman integrals, or when you consider an integral of a radical of a polynomial expression. Algebraic geometry can also contribute to the understanding of integer solutions of polynomial equations. You have the old story of elliptic functions: to understand how elliptic integrals behave, the geometrical interpretation is crucial.

Algebraic geometry is one of the main areas in mathematics. Would you say that to learn algebraic geometry requires much more effort than other areas in mathematics, at least for a beginner?

I think it's hard to enter the subject because one has to master a number of different tools. To begin with, cohomology is now indispensable. Another reason is that Algebraic geometry developed in a succession of stages, each with its own language. First, the Italian school which was a little hazy, as shown by the infamous saying: "In Algebraic geometry, a counterexample to a theorem is a useful addition to it." Then Zariski and Weil put things on a better footing. Later Serre and Grothendieck gave it a new language which is very powerful. In this language of schemes one can express a lot; it covers both arithmetical applications and more geometrical aspects. But it requires time to understand the power of this language. Of course, one needs to know a number of basic theorems, but I don't think that this is the main stumbling block. The most difficult is to understand the power of the language created by Grothendieck and how it relates to our usual geometrical intuition.

Apprentice in Paris

When you came to Paris you came in contact with Alexander Grothendieck and Jean-Pierre Serre. Could you tell us about your first impression of these two mathematicians?

I was introduced to Grothendieck by Tits during the Bourbaki seminar of November 1964. I was really taken aback. He was a little strange, with his shaved head, a very tall man. We shook hands, but did nothing more until I went to Paris a few months later to attend his seminar.

That was really an extraordinary experience. In his way, he was very open and kind. I remember the first lecture I attended. In it, he used the expression "cohomology object" many times. I knew what cohomology was for abelian groups, but I did not know the meaning of "cohomology object". After the lecture I asked him what he meant by this expression. I think that many other mathematicians would have thought that if you didn't know the answer, there wouldn't be any point to speak to you. This was not his reaction at all. Very patiently he told me that if you have a long exact sequence in an abelian category and you look at the kernel of one map, you divide by the image of the previous one and so on.... I recognized quickly that I knew about this in a less general context. He was very open to people who were ignorant. I think that you should not ask him the same stupid question three times, but twice was all right.

I was not afraid to ask completely stupid questions, and I have kept this habit until now. When attending a lecture, I usually sit in front of the audience, and if there is something I don't understand, I ask questions even if I would be supposed to know what the answer was.

I was very lucky that Grothendieck asked me to write up talks he had given the previous year. He gave me his notes. I learned many things, both the content of the notes, and also a way of writing mathematics...This both in a prosaic way, namely that one should write only on one side of the paper and leave some blank space so he could make comments, but he also insisted that one was not allowed to make any false statement. This is extremely hard. Usually one takes shortcuts; for instance, not keeping track of signs. This would not pass muster with him. Things had to be correct and precise. He told me that my first version of the redaction was much too short, not enough details...It had to be completely redone. That was very good for me.

Serre had a completely different personality. Grothendieck liked to have things in their natural generality; to have an understanding of the whole story. Serre appreciates this, but he prefers beautiful special cases. He was giving a course at Collège de France on elliptic curves. Here, many different strands come together, including automorphic forms. Serre had a much wider mathematical culture than Grothendieck. In case of need, Grothendieck redid everything for himself, while Serre could tell people to look at this or that in the literature. Grothendieck read extremely little; his contact with classical Italian geometry came basically through Serre and Dieudonné. I think Serre must have explained him what the Weil conjectures were about and why they were interesting. Serre respected the big constructions Grothendieck worked with, but they were not in his taste. Serre preferred smaller objects with beautiful properties such as modular forms, to understand concrete questions, for instance congruences between coefficients.

Their personalities were very different, but I think that the collaboration between Serre and Grothendieck was very important and it enabled Grothendieck to do some of his work.

You told us that you needed to go to Serre's lectures in order to keep your feet on the ground?

Yes, because it was a danger in being swept away in generalities with Grothendieck. In my opinion, he never invented generalities that were fruitless, but Serre told me to look at different topics that all proved to be very important for me.

The Weil Conjectures

Your most famous result is the proof of the third – and the hardest – of the so-called Weil conjectures. But before talking about your achievement, can you try to explain why the Weil conjectures are so important?

There were some previous theorems of Weil about curves in the one-dimensional situation. There are many analogies between algebraic curves over finite fields and the rational numbers. Over the rational numbers, the central question is the Riemann hypothesis. Weil had proved the analogue of the Riemann hypothesis for curves over finite fields, and he had looked at some higher-dimensional situations as well. This was at the time where one started to understand the cohomology of simple algebraic varieties, like the Grassmannians. He saw that some point-counting for objects over finite fields reflected what happened over the complex numbers and the shape of the related space over the complex numbers.

As Weil looked at it, there are two stories hidden in the Weil conjectures. First, why should there be a relation between apparently combinatorial questions and geometric questions over the complex numbers. Second, what is the analogue of the Riemann hypothesis? Two kinds of applications came out of these analogies. The first started with Weil himself: estimates for some arithmetical functions. For me, they are not the most important. Grothendieck's construction of a formalism explaining why there should be a relation between the story over the complex numbers, where one can use topology, and the combinatorial story, is more important.

Secondly, algebraic varieties over finite fields admit a canonical endomorphism, the Frobenius. It can be viewed as a symmetry, and this symmetry makes the whole situation very rigid. Then one can transpose this information back into the geometric world over the complex numbers, it yields constraints on what will happen in classical algebraic geometry, and this is used in applications to representation theory and the theory of automorphic forms. It was not obvious at first that there would be such applications, but for me they are the reason why the Weil conjecture is important.

Grothendieck had a program on how to prove the last Weil conjecture, but it didn't work out. Your proof is different. Can you comment on this program? Did it have an influence on the way you proved it?

No. I think that the program of Grothendieck was, in a sense, an obstruction to finding the proof, because it made people think in just a certain direction. It would have been more satisfying if one had been able to do the proof following the program, because it would have explained a number of other interesting things as well. But the whole program relied on finding enough algebraic cycles on algebraic varieties; and on this question one has made essentially no progress since the 70's.

I used a completely different idea. It is inspired by the work of Rankin and his work on automorphic forms. It still has a number of applications, but it did not realize the dream of Grothendieck.

We heard that Grothendieck was glad that the Weil conjecture was proved, of course, but still he was a little disappointed?

Yes. And with very good reason. It would have been much nicer if his program had been realized. He did not think that there would be another way to do it. When he heard I had proved it, he felt I must have done this and that, which I hadn't. I think that's the reason for the disappointment.

You have to tell us about the reaction of Serre when he heard about the proof.

I wrote him a letter when I did not have a complete proof yet, but a test case was clear. I think he got it just before he had to go to the hospital for an operation of a torn tendon. He told me later that he went into the operation theatre in a euphoric state because he knew now that the proof was roughly done.

Several famous mathematicians have called your proof of the last Weil conjecture a marvel. Can you describe how you got the ideas that led to the proof?

I was lucky that I had all the tools needed at my disposal at the same time and that I understood that those tools would do it. Parts of the proof have since been simplified by Gérard Laumon, and a number of these tools are no more needed.

At the time, Grothendieck had ideas for putting into a purely algebraic framework the work of Solomon Lefschetz from the 20s about families of hyperplane sections of an algebraic variety. Of particular interest was a statement of Lefschetz, later proved by William Hodge, the so-called hard Lefschetz theorem. Lefschetz' approach was topological. In contrast to what one might think, if arguments are topological there is a better chance to translate them into abstract algebraic geometry than if they are analytic, such as the proof given by Hodge. Grothendieck asked me to look at the 1924 book *L'analysis situs et la géométrie algébrique* by Lefschetz. It is a beautiful and very intuitive book, and it contained some of the tools I needed.

I was also interested in automorphic forms. I think it is Serre who told me about an estimate due to Robert Rankin. I looked carefully at it. Rankin was getting some non-trivial estimates for coefficients of modular forms by proving for some related L-functions what was needed to apply results of Landau, in which the location of the poles of an L-function gave information on the poles of the local factors. I saw that the same tool, in a much less sophisticated way, just using that a sum of squares is positive, could be used here because of the control the work of Grothendieck gave on poles. This was enough. The poles were much easier to understand than the zeros and it was possible to apply Rankin's idea.

I had all these tools at my disposal, but I cannot tell how I put them together.

A little bit about subsequent work

What is a motive?

A surprising fact about algebraic varieties is that they give rise not to one, but to many cohomology theories. Among them the l-adic theories, one for each prime l different from the characteristic, and in characteristic zero, the algebraic de Rham cohomology. These theories seem to tell the same story, over and over again, each in a different language. The philosophy of motives is that there should exist a universal cohomology theory, with values in a category of motives to be defined, from which all these theories could be derived. For the first cohomology group of a projective non-singular variety, the Picard variety plays the role of a motivic H^1: the Picard variety is an abelian variety, and from it the H^1 in all available cohomology theories can be derived. In this way, abelian varieties (taken up to isogeny) are a prototype for motives.

A key idea of Grothendieck is that one should not try to define what a motive is. Rather, one should try to define the category of motives. It should be an abelian category with finite dimensional rational vector spaces as *Hom* groups. Crucially, it should admit a tensor product, needed to state a Künneth theorem for the universal cohomology theory, with values in the category of motives. If only the cohomology of projective non-singular varieties is considered, one speaks of pure motives. Grothendieck proposed a definition of a category of pure motives, and showed that if the category defined had a number of properties, modelled on those of Hodge structures, the Weil conjectures would follow.

For the proposed definition to be viable, one needs the existence of "enough" algebraic cycles. On this question almost no progress has been made.

What about your other results? Which of those that you worked on after the proof of the Weil conjecture are you particularly fond of?

I like my construction of a so-called mixed Hodge structure on the cohomology of complex algebraic varieties. In its genesis, the philosophy of motives has played a crucial role, even if motives don't appear in the end result. The philosophy suggests that whenever something can be done in one cohomology theory, it is worthwhile to look for a counterpart in other theories. For projective nonsingular varieties, the role played by the action of Galois is similar to the role played by the Hodge decomposition in the complex case. For instance, the Hodge conjecture, expressed using the Hodge decomposition, has as counterpart the Tate conjecture, expressed using the action of Galois. In the l-adic case, cohomology and action of Galois remain defined for singular or non-compact varieties. This forces us to ask: what is the analogue in the complex case? One clue is given by the existence, in l-adic cohomology, of an increasing filtration, the weight filtration W, for which the i-th quotient W_i/W_{i-1} is a subquotient of the cohomology of a projective nonsingular variety. We hence expect in the complex case a filtration W such that the i-th quotient has a Hodge decomposition of weight i. Another clue, coming from works of Griffiths and Grothendieck, is that the Hodge filtration is more important than the Hodge decomposition. Both clues force the definition of mixed Hodge structures, suggest that they form an abelian category, and suggest also how to construct them.

What about the Langlands program? Have you been involved in it?

I have been very interested in it, but I have contributed very little. I have only done some work on GL(2), the linear group in two variables. I tried to understand things. A somewhat remote application of the Weil conjecture has been used in Ngo's recent proof of what is called the fundamental lemma. I didn't do a lot of work myself, though I had a lot of interest in the Langlands program.

French, American and Russian mathematics

You have already told us about the two institutions you mainly have worked for, namely the IHÉS in Paris and then, since 1984, the IAS in Princeton. It would be interesting for us to hear what your motives were for leaving IHÉS and moving to Princeton. Moreover, we would like to hear what unites the two institutions and how they differ, in your opinion.

One of the reasons I left, was that I don't think it's good to spend all of one's life in the same place. Some variation is important. I was hoping to have some contact with Harish-Chandra who had done some beautiful work in representation theory and automorphic forms. That was a part of the Langlands program that I am very interested in, but unfortunately Harish-Chandra died shortly before I arrived at Princeton.

Another reason was that I had imposed on myself to give seminars, each year on a new subject, at the IHÉS in Bures. That became a little too much. I was not really able to both give the seminars and to write them down, so I did not impose the same obligation on myself after I came to Princeton. These are the main reasons why I left the IHÉS for IAS in Princeton.

Concerning the difference between the two institutions, I would say that the Institute for Advanced Study is older, bigger and more stable. Both are very similar in the way that there are many young visitors that come there. So they are not places where you can fall asleep since you will always be in contact with young people who will tell you that you are not as good as you think you are.

In both places there are physicists, but I think the contact with them was more fruitful for me in Princeton than it was in Bures. In Princeton, there have been common seminars. One year was very intense, with both mathematicians and physicists participating. This was due mainly to the presence of Edward Witten. He has received the Fields Medal even though he is a physicist. When Witten asks me questions, it's always very interesting to try to answer them, but it can be frustrating as well.

Princeton is also bigger in the sense that it has not only maths and physics, but also the School of Historical Studies and the School of Social Sciences. There is no real scientific interaction with these Schools but it is pleasant to be able to go and hear a lecture about, for instance, ancient China. One good feature about Bures which you do not have in Princeton is the following: In Bures, the cafeteria is too small. So you sit where you can and you don't get to choose the people you are sitting with. I was often sitting next to an analyst or a physicist and such random informal interactions are very useful. In Princeton, there is one table for the mathematicians, another for the astronomers, the ordinary physicists and so on. You will not be told to go away if you sit down at the wrong table, but still there is segregation.

The Institute for Advanced Study has a big endowment, while the IHÉS had none, at least when I was there. This didn't affect the scientific life. Sometimes it created instability, but the administration was usually able to hide the difficulties from us.

Apart from your connections with French and US mathematics, you have also had a very close contact with Russian mathematics for a long time, even from long before the fall of the iron curtain. In fact, your wife is the daughter of a Russian mathematician. How did your contact with Russian mathematics develop?

Grothendieck or Serre told Manin, who was in Moscow at the time, that I had done some interesting work. The Academy invited me to a conference for I. M. Vinogradov, a terribly anti-Semitic person, by the way. I came to Russia, and I found a beautiful culture for mathematics. At that time mathematics was one of the few subjects where the communist party could not meddle, as it did not understand it at all, and this turned it into a space of freedom.

We would go to somebody's home and sit by the kitchen table to discuss mathematics over a cup of tea. I fell in love with the atmosphere and this enthusiasm for mathematics. Moreover, Russian mathematics was one of the best in the world at that time. Today there are still good mathematicians in Russia, but there has been a catastrophic emigration. Furthermore, among those wanting to stay, many need to spend at least half of the time abroad, just to make a living.

You mentioned Vinogradov and his anti-Semitism. You talked to somebody and asked whether he was invited?

It was Piatetskii-Shapiro. I was completely ignorant. I had a long discussion with him. For me it was obvious that someone like him should be invited by Vinogradov, but I was explained that that was not the case. After this introduction to Russian mathematics, I still have some nostalgia for the beautiful memories of being in Moscow and speaking with Yuri Manin, Sergey Bernstein or being at the Gelfand seminar. There was a tradition, which still exists, of a strong connection between the university and the secondary education. People like Andrey Kolmogorov had a big interest in secondary education (perhaps not always for the best).

They have also the tradition of Olympiads and they are very good at detecting promising people in mathematics early on in order to help them. The culture of seminars is in danger because it's important that the head of the seminars is working full time in Moscow and that is not always the case. There is a whole culture which I think it's important to preserve. That is the reason why I used half of the Balzan Prize to try to help young Russian mathematicians.

That was by a contest that you arranged.

Yes. The system is falling apart at the top because there is no money to keep people, but the infrastructure was so good that the system continues to produce very good young mathematicians. One has to try to help them and make it possible for them to stay somewhat longer in Russia so that the tradition can continue.

Competition and collaboration in mathematics

Some scientists and mathematicians are very much driven by the aim to be the first to make major discoveries. That seems not to be your main driving force?

No. I don't care at all.

Do you have some comments on this culture in general?

For Grothendieck it was very clear: he once told me that mathematics is not a competition sport. Mathematicians are different and some will want to be the first, especially if they are working on very specific and difficult questions. For me it's more important to create tools and to understand the general picture. I think mathematics is much more a collective enterprise of long duration. In contrast to what happens in physics and biology, mathematical articles have long and useful lives. For instance, the automatic evaluation of people using bibliographic criteria is particularly perverse in mathematics, because those evaluation methods take only account of papers published during the last three or five years. This does not make sense in mathematics. In a typical paper of mine, I think at least half of the papers cited can be twenty to thirty years old. Some will even be two hundred years old.

You like to write letters to other mathematicians?

Yes. Writing a paper takes a lot of time. Writing it is very useful, to have everything put together in a correct way, and one learns a lot doing so, but it's also somewhat painful. So in the beginning of forming ideas, I find it very convenient to write a letter. I send it, but often it is really a letter to myself. Because I don't have to dwell on things the recipient knows about, some short-cuts will be all right. Sometimes the letter, or a copy of it, will stay in a drawer for some years, but it preserves ideas and when I eventually write a paper, it serves as a blue-print.

When you write a letter to someone and that person comes with additional ideas, will that result in a joint paper?

That can happen. Quite a lot of my papers are by me alone and some are joint work with people having the same ideas. It is better to make a joint paper than having to wonder who did what. There are a few cases of genuine collaborations where different people have brought different intuitions. This was the case with George Lusztig. Lusztig had the whole picture of how to use l-adic cohomology for group representations, but he did not know the techniques. I knew the technical aspect of l-adic cohomology and I could give him the tools he needed. That was real collaboration.

A joint paper with Morgan, Griffiths and Sullivan was also a genuine collaboration.

Also with Bernstein, Beilinson and Gabber: we put together our different understandings.

Work style, pictures, and even dreams

Your CV shows that you haven't taught big classes of students a lot. So, in a sense, you are one of the few fulltime researchers in mathematics.

Yes. And I find myself very lucky to have been in this position. I never had to teach. I like very much to speak with people. In the two institutions where I have worked young people come to speak with me. Sometimes I answer their questions, but more often I ask them counterquestions which sometimes are interesting, too. So this aspect of teaching with one-to-one contact, trying to give useful information and learning in the process, is important to me.

I suspect it must be very painful to teach people who are not interested, but are forced to learn math because they need the grade to do something else. I would find that repulsing.

What about your mathematical work style? Are you most often guided by examples, specific problems and computations, or are you rather surveying the landscape and looking for connections?

First I need to get some general picture of what should be true, what should be accessible and what tools can be used. When I read papers I will not usually remember the details of the proofs, but I will remember which tools were used. It is important to be able to guess what is true and what is false in order not to do completely useless work. I don't remember statements which are proved, but rather I try to keep a collection of pictures in my mind. More than one picture, all false but in different ways, and knowing in which way they are false. For a number of subjects, if a picture tells me that something should be true, I take it for granted and will come back to the question later on.

What kind of pictures do you have of these very abstract objects?

Sometimes very simple things! For instance, suppose I have an algebraic variety, and hyperplane sections, and I want to understand how they are related, by looking at a pencil of hyperplane sections. The picture is very simple. I draw it in my mind something like a circle in the plane and a moving line which sweeps it. Then I know how this picture is false: the variety is not one-dimensional, but higher dimensional and when the hyperplane section degenerates, it is not just two intersection points coming together. The local picture is more complicated, like a conic which becomes a quadratic cone. These are simple pictures put together.

When I have a map from some space to another I can study properties it has. Pictures can then convince me that it is a smooth map. Besides having a collection of pictures, I also have a collection of simple counter-examples, and statements that I hope to be true have to be checked against both the pictures and the counter-examples.

So you think more in geometric pictures than algebraically?

Yes.

Some mathematicians say that good conjectures, or even good dreams, are at least as important as good theorems. Would you agree?

Absolutely. The Weil conjectures, for instance, have created a lot of work. Part of the conjecture was the existence of a cohomology theory for algebraic systems, with some properties. This was a vague question, but that is all right. It took over twenty years of work, even a little more, in order to really get a handle on it.

Another example of a dream is the Langlands program which has involved many people over fifty years, and we have now only a slightly better grasp of what is happening. Another example is the philosophy of motives of Grothendieck about which very little is proved. There are a number of variants taking care of some of the ingredients. Sometimes, such a variant can be used to make actual proofs, but more often the philosophy is used to guess what happens, and then one tries to prove it in another way. These are examples of dreams or conjectures that are much more important than specific theorems.

Have you had a "Poincaré moment" at some time in your career where you, in a flash, saw the solution of a problem you had worked on in a long time?

The closest I have been to such a moment must have been while working on the Weil conjecture when I understood that perhaps there was a path using Rankin against Grothendieck. It took a few weeks after that before it really worked, so it was a rather slow development. Perhaps also for the definition of mixed Hodge structures, but also in this case, it was a progressive process. So it was not a complete solution in a flash.

When you look back on fifty years of doing mathematics, how have your work and your work style changed over the years? Do you work as persistently as you did in your early years?

I am not as strong as I was earlier, in the sense that I cannot work as long or as intensively as I did. I think I have lost some of my imagination but I have much more technique that can act as a substitute to some extent. Also the fact that I have contact with many people, gives me access to some of the imagination I am lacking myself. So when I bring my technique to bear, the work can be useful, but I'm not the same as when I was thirty.

You have retired from your professorship at IAS rather early…

Yes, but that's purely formal. It means I receive retirement money instead of a salary; and no School meetings for choosing next year's members. So that's all for the best, it gives me more time for doing mathematics.

Hopes for the future

When you look at the development of algebraic geometry, number theory and the fields that are close to your heart, are there any problems or areas where you would like to see progress soon? What would be particularly significant, in your opinion?

Whether or not it's within reach in ten years, I have absolutely no idea; as it should be... but I would very much like to see progress in our understanding of motives. Which path to take and what are the correct questions, is very much in the air. Grothendieck's program relied on proving the existence of algebraic cycles with some properties. To me this looks hopeless, but I may be wrong.

The other kind of question for which I would really like to see some progress is connected with the Langlands program, but that is a very long story...

In yet another direction, physicists regularly come up with unexpected conjectures, most often using completely illegal tools. But so far, whenever they have made a prediction, for instance a numerical prediction on the number of curves with certain properties on some surface – and these are big numbers, in the millions perhaps – they were right! Sometimes previous computations by mathematicians were not in accordance with what the physicists were predicting, but the physicists were right. They have put their fingers on something really interesting, but we are, so far, unable to capture their intuition. Sometimes they make a prediction and we work out a very clumsy proof without real understanding. That is not how it should be. In one of the seminar programs that we had with the physicists at IAS, my wish was not to have to rely on Ed Witten but instead to be able to make conjectures myself. I failed! I did not understand enough of their picture to be able to do that, so I still have to rely on Witten to tell me what should be interesting.

What about the Hodge conjecture?

For me, this is a part of the story of motives, and it is not crucial whether it is true or false. If it is true, that's very good and it solves a large part of the problem of constructing motives in a reasonable way. If one can find another purely algebraic notion of cycles for which the analogue of the Hodge conjecture holds, and there are a number of candidates, this will serve the same purpose, and I would be as happy as if the Hodge conjecture were proved. For me it is motives, not Hodge, that is crucial.

Private interests – and an old story

We have the habit of ending these interviews by asking questions that are outside of mathematics. Could you tell us a little bit about your private interests outside *your profession? We know about your interest in nature and in gardening, for example.*

These are my main interests. I find the earth and nature so beautiful. I don't like just to go and have a look at a scenery. If you really want to enjoy the view from a mountain, you have to climb it by feet. Similarly, to see the nature, you have to walk. As in mathematics, in order to take pleasure in nature – and the nature is a beautiful source of pleasure – one has to do some work.

I like to bicycle because that's also a way to look around. When distances are a little bigger than what is convenient by feet, this is another way of enjoying the nature.

We heard that you also build igloos?

Yes. Unfortunately, there's not enough snow every year and even when there is, snow can be tricky. If it's too powdery, it's impossible to do anything; likewise if it's too crusty and icy. So there is maybe just one day, or a few hours each year when building an igloo is possible, and one has to be willing to do the work of packing the ice and putting the construction together.

And then you sleep in it?

And then I sleep in the igloo, of course.

You have to tell us what happened when you were a little child.

Yes. I was in Belgium at the sea-side for Christmas, and there was much snow. My brother and sister, who are much older than me, had the nice idea to build an igloo. I was a little bit in the way. But then they decided I might be useful for one thing: if they grabbed me by my hands and feet, I could be used to pack the snow.

Thank you very much for granting us this interview. These thanks come also on behalf of the Norwegian, the Danish and the European mathematical societies that we represent. Thank you very much!

Thank you.

Martin Raussen is associate professor of mathematics at Aalborg Univesity, Denmark. Christian Skau is professor of at the Norwegian University of Science and Technology at Trondheim. They have together taken interviews with all Abel laureates since 2003.

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European Women in Mathematics

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Organisation. Die Vereinigung European Women in Mathematics (EWM) existiert seit 1986, um Mathematikerinnen in ganz Europa zu verbinden. Dabei ist der Begriff Europa nicht in einem engen Rahmen zu sehen; so hat EWM etwa auch zahlreiche Mitglieder aus Russland, Georgien und der Türkei.

EWM hat heute mehrere Hundert Mitglieder in über 30 Ländern im europäischen Raum. Neben den General Meetings und den Summer Schools, die alle zwei Jahre stattfinden, verschickt EWM regelmäßig einen Newsletter und unterhält Email-Netzwerke (allgemein und regional). Innerhalb von EWM gibt es einige regionale Gruppen, die zusätzliche Aktivitäten organisieren wie kleinere Workshops. Viele Informationen über EWM können über die Webpage gefunden werden, siehe *http://www.europeanwomeninmaths.org*. Unter anderem findet sich dort der dreiteilige Film *Women in Mathematics*, den EWM 2009 produziert hatte.

Ziele von EWM. Die Ziele von EWM umfassen die Ermutigung von Schülerinnen, Mathematik zu studieren sowie die Unterstützung von Mathematikerinnen in ihrer Laufbahn. EWM stellt eine Plattform bereit, wo sich Mathematikerinnen austauschen können über ihre Erfahrungen während Ausbildung und Karriere. Die Organisation fördert wissenschaftlichen Austausch durch die General Meetings alle zwei Jahre und kooperiert mit anderen Verbänden, die ähnliche Ziele haben. Ein weiteres Ziel von EWM ist, Informationen über Frauen in der Mathematik zu sammeln und zur Verfügung zu stellen.

Kurzum, EWM unterstreicht wichtige Errungenschaften und strebt die wissenschaftliche Anerkennung von Mathematikerinnen an. Wie EWM zum heutigen Stand kam, möchte ich im Folgenden darstellen.

Kurze Geschichte. Die Grundlegung zur EWM wurde 1986 am International Congress of Mathematicians Berkeley gelegt, anlässlich von Diskussionen unter europäischen Mitgliedern der AWM (Association for Women in Mathematics). Sie hatten die Idee, eine ähnliche Organisation wie AWM in Europa zu gründen.

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Zu der Zeit befanden sich unter diesen Mathematikerinnen einige Französinnen, die wegen der Fusionierung der Ecole Normale Supérieure de Jeunes Filles mit der Ecole Normale Supérieure besorgt waren: Diese ging mit einer signikanten Abnahme von zugelassenen Studentinnen einher. Also lag den französischen Mitgliedern von AWM insbesondere die Situation in Frankreich am Herzen, was wohl ein Grund dafür war, dass das erste Treffen der zu gründenden Organisation in Paris stattfand.

Damals stand das Internet noch nicht als Instrument zur Bewerbung solcher Veranstaltungen zur Verfügung. So waren einige der Gründerinnen überrascht, dass an dem ersten Treffen im Dezember 1986 auch viele europäische Mathematikerinnen aus andern Ländern teilnahmen, die davon über irgendwelche Wege erfahren hatten. Dies zeigt, dass die Idee einer Organisation, die sich für die Belange der Mathematikerinnen in Europa einsetzt, breiteren Rückhalt gewonnen hatte. Auf Initiative der damals Teilnehmenden entstanden die nächsten beiden Treffen, 1987 in Kopenhagen (Technical Institute in Lyngby), mit Unterstützung der Regierung Dänemarks, und dann in Warwick 1988. In Dänemark kamen 22 Teilnehmende aus 9 Ländern zusammen. Es war das erste Mal ein Hauptvortrag eingeplant. In Warwick war das Treffen bereits wieder um einiges gewachsen: Über 3 Tage dauerten die Vorlesungen. Außerdem hatten die Organisatorinnen einen Diskussionsnachmittag für Gymnasiastinnen aus der Region angeboten. Als im darauf folgenden Jahr das General Meeting aus personellen Gründen beinahe nicht zustandekam, wurde klar, dass sich EWM stärker strukturieren musste. Ein standing committee wurde eingerichtet, mit einer „Präsidentin" und „Vize-Präsidentin" (convenor, currently: Susanna Terracini, University of Torino, Italy, and vice-convenor, Angela Pistoia, University of Rome) an der Spitze. In 1993 wurde in Finnland offiziell ein Büro eingerichtet und EWM hat sich als rechtliche Organisation registrieren lassen. Seit 1994 existiert eine Email-Liste, und seit 1997 hat EWM eine Webpage mit Informationen über die Organisation, über Aktivitäten etc. Teilweise wird EWM über europäische Gelder finanziert, teilweise erhält die Organisation lokale Unterstützung (z.B. von Femmes et Mathématiques, siehe unten) und es werden auch Mitgliederbeiträge eingezogen.

Seit den Anfängen wurden die General Meetings in den folgenden Ländern abgehalten: Frankreich (dreimal), Deutschland, Spanien, UK (je zweimal), Dänemark, Portugal, Polen, Italien, Malta, Russland, Serbien. Wer weiß, vielleicht wird auch Österreich einmal Gastland für ein General Meeting von EWM.

Von Anfang an war es für EWM ein Anliegen, Informationen über die Stellung von Mathematikerinnen in verschiedenen europäischen Ländern zusammenzutragen. Es wurde schnell klar, dass die verschiedenen Kulturen in den verschiedenen Ländern zu großen Unterschieden in der Praxis führen. So etwa fiel schnell auf, dass der Anteil von Frauen an mathematischen Instituten viel höher im Süden (Italien, Spanien, Portugal) ist als im Norden (da gehören auch die deutschsprachigen Länder dazu) Europas.

Fast gleichzeitig wie EWM ist die Organisation Femmes et Mathématiques in Frankreich entstanden. Femmes et Mathématiques waren immer wieder sehr eng mit EWM verknüpft und haben die europäische Organisation sehr unterstützt, so etwa mit der Organisation einer ersten einwöchigen Konferenz in Luminy 1991. Dies half EWM, die eigenen Strukturen verstärkt auszubilden. Seit 1991 finden die Versammlungen von EWM jedes zweite Jahr statt. Die mathematischen Vorträge werden ergänzt durch Diskussionsrunden über die Organisation sowie über ihre Ziele. Neben den Vollversammlungen organisiert EWM regelmäßig Summer Schools zu verschiedenen aktuellen Themen, dazu kommen immer wieder regionale Workshops. Die Treffen sind immer für Mathematikerinnen und Mathematiker offen.

Aktuelles. 2012/13 war EWM bei der Gründung der African Women in Mathematics Organization (AWMA) beteiligt und hat Mitglieder der AWMA zum 16. General Meeting eingeladen. Eines der neueren Ziele von EWM ist es, die Verbindungen zu den mathematischen Verbänden der einzelnen europäischen Ländern zu verstärken.

Persönlicher Bezug. Ich wurde durch eine Mitstudentin auf EWM aufmerksam und nahm an den General Meetings 1995 in Madrid und 1997 in Triest teil. Zu dieser Zeit war ich Studentin und dann Diplomandin an der Universität Zürich. An meinem Institut gab es keine einzige Professorin, und auch unter den Doktorierenden nur wenig Frauen, obwohl das Verhältnis unter den Studierenden fast ausgeglichen war. Ich war daher umso mehr beeindruckt von den beiden Treffen. Es war das erste Mal, dass ich etablierte Mathematikerinnen getroffen hatte und dann gleich in einer großen Zahl. Mathematisch war es ebenso ein Gewinn: Die Übersichtsvorträge waren thematisch gruppiert und erlaubten Einblicke in neue Gebiete. In Triest präsentierte ich ein Poster über meine Diplomarbeit und konnte damit erstmals meine eigene Arbeit einem weiteren Publikum vorstellen. Ich fand beide Treffen sehr inspirierend, sowohl mathematisch als auch persönlich.

So habe ich mich sehr gefreut, im September 2013 als Hauptvortragende in Bonn am 16. General Meeting von EWM teilzunehmen. Dort hatte ich Gelegenheit, mit jungen Doktorandinnen über ihre Arbeit, über ihr Vorankommen und über die Situation in ihren Ländern zu diskutieren sowie mich mit diversen Mitgliedern auszutauschen und zu sehen, wie Mathematikerinnen andere Gebiete der Mathematik vorwärtsbringen.

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Buchbesprechungen

G. W. Anderson, A. Guionnet, O. Zeitouni: An Introduction to Random Matrices. (Cambridge Studies in Advanced Mathematics 118.) Cambridge University Press, 2010, xiv+492 S. ISBN 978-0-521-19452-5 H/b £ 40,–.

Zufallsmatrizen wurden lange Zeit in der Mathematik wenig beachtet, obwohl sie seit Jahrzehnten in der Statistik und noch mehr in der Physik seit den Arbeiten Fishers und Wigners eifrig studiert wurden. Das vielzitierte Buch von Mehta liegt mittlerweile in der dritten Auflage vor, während es noch keine umfassende mathematische Monographie zu dem Thema gibt, eine Lücke, die sich im letzten Vierteljahrhundert immer mehr bemerkbar macht, seit Zufallsmatrizen auch ein großes Thema in der Mathematik sind. Das vorliegende Buch will diese Lücke nicht schließen, sondern hat das Ziel, eine fundierte mathematische Grundlage für den Einstieg zu liefern, und zwar liegt der Schwerpunkt auf der analytischen Behandlung des Eigenwertproblems hermitischer Zufallsmatrizen.

Nach einer kurzen Einleitung ist das Buch in 4 weitere Kapitel gegliedert, deren Inhalt sich wie folgt gestaltet:

In Kapitel 2 werden die grundlegenden Tatsachen aus der Spektraltheorie der reellen und komplexen Wignerschen und Gaußschen Zufallsmatrizen sowohl vom kombinatorischen als auch vom analytischen Blickwinkel betrachtet und die Verteilung der Eigenwerte hergeleitet.

Auf dieser Grundlage werden im Kapitel 3 die asymptotischen feinen Eigenschaften des Spektrums dieser Matrizen analysiert: Verteilung des größten Eigenwerts, Verteilung des Abstände der Eigenwerte, etc., und die dazu nötigen analytischen Hilfsmittel bereitgestellt.

Während die vorhergehenden Kapitel sich im Wesentlichen auf reelle und komplexe Gaußsche Zufallsmatrizen beschränken, werden in Kapitel 4 wesentliche analytische Hilfsmittel für das Studium allgemeinerer Zufallsmatrizen bereitgestellt,

In Kapitel 5 wird der abstrakte Rahmen von Voiculescus freier Wahrscheinlichkeitstheorie eingeführt und in den Zusammenhang mit der Asymptotik von Zufallsmatrizen gestellt.

Zur Abrundung des Buchs reicht der abschließende Anhang grundlegende Definitionen und Tatsachen aus den zahlreichen Fachgebieten nach, die im Laufe des Buchs berührt werden: von Linearer Algebra und Topologie über Mannigfaltigkeiten bis hin zu Operatoralgebren.

Da es sich um ein einführendes Werk handelt, bleibt naturgemäß vieles ausgespart, darunter auch wichtige Themen, wie das Riemann-Hilbert-Problem, nichthermitische, insbesondere unitäre Zufallsmatrizen und freie Entropie, um ein paar zu nennen. Nichtsdestotrotz ist das Buch sehr empfehlenswert als Einführung in ein rasch wachsendes Teilgebiet der Mathematik.

F. Lehner (Graz)

J. Beck: Inevitable Randomness in Discrete Mathematics. (University Lecture Series, Vol. 49.) American Mathematical Society, Providence, Rhode Island, 2009, xi+250 S. ISBN 978-0-8218-4756-5 P/b $ 59,–.

Der Autor ist motiviert durch 2 vage Komplexitäts-Gesetze: (1) Diskrete Systeme sind entweder "simple" oder folgen "advanced pseudorandomness" und (2) a priori Wahrscheinlichkeiten existieren oft.

Der Text entstand aus einem fortgeschrittenen Graphentheoriekurs und kann auch als spin-off seines viel umfänglicheren Buchs *Combinatorial Games: Tic-Tac-Toe Theory* gesehen werden. Es gibt 3 Teile: A ist ein Essay, der das "big picture" entwirft, B ist teils neu, teils ein Überblick über Spieltheorie, und C handelt von Spielen auf Graphen, die des Autors Theorien stützen.

Vieles in dem Buch ist neu und berührt mannigfache Themen aus unterschiedlichen Gebieten. Man kann es sich als Unterlage für fortgeschrittene Seminare vorstellen. Wer dem Autor Beck seit vielen Jahren treu folgt, wird auch diesen Text gern lesen.

Beck hat offensichtlich ein erlesenes und vorgebildetes Publikum im Auge, welches an dem reichhaltigen Material seine Freude haben wird.

H. Prodinger (Stellenbosch)

E. Behrends: Elementare Stochastik. Ein Lernbuch – von Studierenden mitentwickelt (Springer Spektrum) Wiesbaden 2013, xiii+374 S. ISBN 978-3-8348-1939-0 S/b € 20,51.

Dieses Buch ist eine leicht lesbare und verständlich geschriebene Einführung in die wichtigsten Konzepte der Wahrscheinlichkeitstheorie und der Statistik, wobei der Statistikteil eher der Abrundung dient. Der Schwerpunkt liegt eindeutig auf dem ersteren Gebiet, dem ca. 3/4 des Buchs gewidmet sind.

Obwohl das Buch einige Begriffe und Methoden der Maß- und Integrationstheorie, wie etwa σ-Algebren, Dynkin-Systeme und das Prinzip der guten Menge verwendet, ja im Anhang sogar bewiesen wird, dass die Vereinigung einer strikt monoton steigenden Folge von σ-Algebren keine σ-Algebra sein kann (dabei folgt der Autor dem von Overdijk, Simons und Thiemann stammenden, eleganten Beweis eines etwas allgemeineren Resultats über Ringe), ist es in erster Linie für Nichtmathematiker geschrieben, die sich ohne allzu großen Zeitaufwand einen Überblick über die grundlegenden Ideen der Stochastik verschaffen wollen. Diese werden es auch ohne maßtheoretische Vorkenntnisse problemlos lesen können. Die Zielgruppe rechtfertigt zudem die Tatsache, dass viele Ergebnisse nicht in ihrer allgemeinsten Form, sondern unter Zusatzvoraussetzungen bewiesen werden. So wird etwa das starke Gesetz der großen Zahlen nur für iid-Folgen mit endlicher Varianz bewiesen. Hier finde ich allerdings den von Prochorov stammenden Ansatz intuitiver und anschaulicher als den im Buch beschriebenen Weg.

Eine Ausnahme von der Tendenz des Buchs, auf volle Allgemeingültigkeit der Resultate zugunsten der Verständlichkeit zu verzichten, bildet neben der Aussage über die Vereinigung von σ-Algebren die Erdős-Rényi-Verallgemeinerung des 2-ten Lemmas von Borel-Cantelli, die sonst nur in fortgeschrittenen Texten, wie Billingsley, Galambos oder Rényi zu finden ist.

Insgesamt halte ich das Werk für ein gut konzipiertes und für den oben beschriebenen Leserkreis empfehlenswertes Buch.

N. Kusolitsch (Wien)

V. Berinde: Exploring, Investigating and Discovering in Mathematics. Birkhäuser, Basel, Boston, Berlin, 2004, XIX+246 S. ISBN 3-7643-7019-X P/b € 36,38.

Das Buch umfasst 24 Kapitel, in dem verschiedene Problemstellungen diskutiert und Lösungswege aufgezeigt werden, wobei die Themen relativ breit gestreut sind. Die betrachteten Probleme sind weitgehend elementarer Natur, ohne dass sich ein unmittelbarer Lösungsweg direkt anbietet, etwa vergleichbar mit Aufgaben bei der Mathematik-Olympiade.

Der Titel ist programmatisch und erinnert ein bisschen an ein Zitat von Paul Halmos: *Mathematics is not a deductive science; that's a cliché. When you try to prove a theorem you don't just list the hypotheses, and then start to reason. What you do is trial and error, experimentation, guesswork.*

Dies wird, auf eher einem elementaren Niveau, in den einzelnen Kapiteln in die Tat umgesetzt, wobei die „Kunst des Entdeckens“ durch systematisches, auch experimentelles Herangehen an ein Problem betont wird. Die Themenbereiche umfassen Zahlentheorie, Geometrie, Analysis, Ungleichungen, Rekursionen, etc., bunt gemischt.

Wie üblich bei diesem Genre findet man einerseits didaktisch Brauchbares (z.B. zu den Themen Differenzengleichungen oder Stammfunktionen unstetiger Integranden), andererseits ist manches eher kurios, wie z.B. ein (tippfehlerbehaftetes) „Kriterium“ für Folgenkonvergenz, das im Wesentlichen zur Definition der Konvergenz äquivalent ist. Manches wiederum wirkt ein bisschen an den Haaren herbeigezogen, ähnlich so manchem, was man in den Quizecken von Sonntagszeitungen findet (damit soll jedoch nicht gesagt sein, dass eine derartige Beschäftigung mit Mathematik grundsätzlich abzulehnen ist).

Bei „exploring, investigating and discovering“ denkt man natürlich auch an den Einsatz des Computers; dies kommt jedoch nur ganz am Rande vor.

W. Auzinger (Wien)

J. Bruna, J. Cufí: Complex Analysis. Translated from the Catalan by I. Monreal. (EMS Textbooks in Mathematics) EMS, Zürich, 2013, xii+564 S. ISBN 978-3-03719-111-8 H/b € 58,–.

The first part of the book is devoted to the classical theory up to the residue theorem and its applications. The authors seek a maximum number of points of contact with other parts of analysis. This results in the inclusion of many aspects that are not common in other texts. The theorem of Cauchy-Goursat is derived from Green's formula of classical vector calculus, which allows to formulate an approach to the concept of a holomorphic function from a real variable point view. The concept of a harmonic function then naturally appears, and various aspects of mathematical physics are explained in detail. Harmonic functions and the Laplace operator in the context of real variables in $\mathbb{R}^n$ are studied systematically, with the emphasis on the special case of dimension 2 and the relation with holomorphic functions, including the Riesz potential of a measure and the solution of the Poisson equation and the the inhomogeneous Dirichlet and Neumann problems. The existence of the solutions to the Dirichlet problem is proved by Perron's method, which can be generalized to any dimension. The relationship between Green's function and conformal mappings allows to prove the Riemann mapping theorem using the solution of the Dirichlet problem. A general version of Runge's approximation theorem provides interesting applications to the inhomogeneous Cauchy-Riemann equations. The final chapters are devoted to the study of the distribution of zeros of holomorphic functions in terms of their growth, to the complex Fourier transform and to the decomposition of meromorphic functions in simple elements. Each chapter contains a number of interesting exercises.

The book presents complex analysis in some new approaches and can also serve as an introduction to potential theory.

F. Haslinger (Wien)

L. A. Caffarelli et al: Nonlinear Partial Differential Equations. (Advanced Courses in Mathematics CRM Barcelona.) Birkhäuser, Basel, 2012, viii+149 S. ISBN 978-3-0348-0190-4 P/b € 24,95.

The present book consists of the lecture notes for four courses given at the school *Topics in PDE's and Applications* held at the FisyMat-Universidas de Granada and at the Center de Recerca Mathemàtica in Barcelona in 2008.

The first lecture by L. Cafarelli and A. Vasseur introduce the classical De Giorgi truncation method and its applications to integral diffusions and the quasi-geostrophic equation. The second notes by F. Golse deal with the Boltzmann-Grad limit for Lorentz model for the the motion of electrons in a solid. The third lecture by Y. Guo is concerned with the Boltzmann equation in bounded domains in the near Mexwellian regime. Finally, the notes by C. Kenig introduce to a concentration-compactness/rigidity method to establish global well-posedness and scattering for the energy critical nonlinear Schrödinger (both focusing and defocusing) and wave equations.

All notes are a good source for getting an overview of these recent methods in nonlinear partial differential equations.

G. Teschl (Wien)

O. Deiser, C. Lasser, E. Vogt, D. Werner: 12×12 **Schlüsselkonzepte zur Mathematik.** Spektrum, Heidelberg, 2011, x+338 S. ISBN 978-3-8274-2297-2 P/b € 19,95.

Mit diesem Werk haben die Autoren den nicht gerade einfachen Versuch gewagt, ein Zwischending zwischen mathematischem Lexikon und mathematischem Lehrbuch zu schaffen. Ihre zugrundegelegte Intention ist es, zu den von ihnen ausgewählten „Schlüsselkonzepten" jeweils etwa zwei Seiten inhaltlichen Hintergrund anzubieten, wobei die wichtigsten Ergebnisse in den jeweiligen Teilbereichen ohne Beweis, aber mit den wichtigsten Grunddefinitionen angeführt werden. So soll es Studierenden ermöglicht werden, einen Überblick über die wichtigsten mathematischen Ergebnisse zu gewinnen, ohne in den Details der Einzelbeweise steckenzubleiben. Das Konzept ist auch sehr gelungen, und so kann auch ein geübter Mathematiker einiges zum Auffrischen der eigenen Kenntnisse finden, das nicht unbedingt zum eigenen Alltagsgeschäft gehört. Die 12 im Titel genannten Kapitel geben eine breite Streuung an, und von „Zahlentheorie" über „Höhere Analysis" und „Numerik" bis zur „Stochastik" findet man die meisten Kapitel, die man auch in einem Werk mit diesen Zielen erwarten würde.

Von meiner Warte als gelernter Geometer gibt es freilich eine auffallende Lücke: Leider ist die Bedeutung der klassischen Geometrie in der Meinung der Autoren so gering, dass sich Begriffe wie „euklidische Ebene" oder „projektive Ebene" nur in Randbemerkungen finden, und Hinweise auf so Dinge wie Polyeder, Kegelschnitte, Abbildungs- oder Dreiecksgeometrie völlig fehlen. Dies ist meines Erachtens sehr schade, ist doch die inhaltliche Zusammensetzung des Buchs ansonsten sehr breit und vor allem inklusiv gestreut. Der Inhalt des Kapitels mit dem Titel „Topologie und Geometrie" ist leider ein weiteres Beispiel der Geringschätzung der klassischen Geometrie, der man gelegentlich begegnet.

Bis auf diese leider wohl bewusste Weglassung ist das Buch allerdings durchaus empfehlenswert. Studierende werden damit einen guten Überblick über ihre Lerninhalte finden, und die „alten Hasen" unter den Mathematikern können damit ihre Kenntnisse über die wichtigsten Aspekte der mathematischen Teilbereiche, die sie nicht tagtäglich anwenden, leicht auffrischen.

R. Geretschläger (Graz)

J. Heitzer: Orthogonalität und Approximation. Vom Lotfällen bis zum JPEG-Format. Von der Schulmathematik zu modernen Anwendungen. Springer, Spektrum, Heidelberg, 2012, xii+329 S. ISBN 978-3-8348-1758-7 P/b € 34,95.

Das Buch bietet eine ausgezeichnet lesbare Einführung in die Theorie der Approximation durch Normalprojektion. Im Vordergrund steht dabei die mögliche Umsetzung im Schulunterricht auch im Hinblick auf das Verständnis bekannter Komprimierungsverfahren, die z.B. auf JPEG- und MP3-Formate führen. So werden nach einer Einführung in die Theorie der euklidischen Vektorräume Orthogonalsysteme vorgestellt und die beste Approximation als Normalprojektion auf entsprechende Unterräume gewonnen. Dies ermöglicht die Verarbeitung zunächst diskreter digitaler Signale (inklusive einer Erklärung des Haar-Algorithmus) und schließlich den Übergang auf stetige Signale. Den Abschluss dieses theoretischen Teils bilden Untersuchungen zu Haar-Wavelets und anderen Funktionenräumen sowie Fourier-Analysen akustischer Signale. Nach diesem theoretischen Teil folgen Hinweise zur Didaktik, der Lehrplananbindung und die Erfahrungen bei der Umsetzung in der Schule. Der dritte Teil des Buchs bietet eine Fülle von Unterrichtsmaterialien, die von der Autorin offenbar erfolgreich in Schulen getestet wurden.

Insgesamt bietet das Buch eine schöne und gut lesbare Darstellung des Stoffgebiets. Die dafür nötigen mathematischen Grundlagen stehen meiner Ansicht nach üblicherweise an den österreichischen höheren Schulen leider nicht zur Verfügung. Das Stoffgebiet kann ich mir vor allem als Angebot für eine Vertiefung im Mathematikunterricht vorstellen. Auch für Studierende der ersten Semester scheint mir das Buch gut geeignet zu sein, bietet es doch einen verständlich verfassten Einstieg in die interessante Theorie der Approximation.

O. Röschel (Graz)

N. Heldermann: Höhere Mathematik I. Lösungen und Aufgaben. (Berliner Studienreihe zur Mathematik, Band 22.) Heldermann Verlag, Lemgo, 2012, vii+272 S. ISBN 978-3-88538-122-1 H/b € 32,–.

Der Begleittext des gleichen Autors, Höhere Mathematik 1 (Band 21 der Studienreihe), wurde bereits besprochen. Es sei daran erinnert, dass das Buch sich thematisch an den Übergang zwischen Schule und Fachhochschule in einem Studiengang der Produktion und Wirtschaft wendet. Inhaltlich werden u.a. die folgenden Themen behandelt: Aufbau des Zahlensystems, Funktionen, lineare Gleichungssysteme, elementare Geometrie, Trigonometrie, Ableitungen (inklusive Newtonverfahren), Taylorreihen, Exponentialfunktionen und schließlich Funktionen in zwei Variablen. Der vorliegende (fest gebundene!) Band enthält auf 272 Seiten vollständige Lösungen der Aufgaben und stellt insofern für Studierende und Lehrende eine willkommene Hilfe dar.

C. Elsholtz (Graz)

J. Hilgert: Arbeitsbuch Mathematik für das erste Studienjahr. Beweise und Lösungen zum Lesebuch. Springer Spektrum, Springer Berlin, Heidelberg, 2013, 212 S. ISBN 978-3-642-37549-1 P/b € 18,68.

Dies ist das Begleitbuch zum *Lesebuch Mathematik für das erste Studienjahr* des gleichen Autors. Bemerkenswert ist, dass sowohl das Lese-, als auch dies Arbeitsbuch sowohl Analysis als auch Lineare Algebra umfassen. Damit ist sichergestellt, dass die entsprechende Notation verwendet wird, und diese häufig disjunkten Themen interagieren. Dies scheint mir ein erheblicher konzeptioneller Vorteil zu sein.

Das Lesebuch (328 Seiten) soll einen ersten Überblick verschaffen, das Arbeitsbuch (212 Seiten) aber zum aktiven Arbeiten anregen. Das Arbeitsbuch besteht aus drei Teilen: Ein erster Teil enthält einige Beweise, die im Lesebuch ausgelassen wurden. Ein zweiter (aber vergleichsweise kurzer) Teil enthält die Übungsaufgaben mit Lösungsvorschlägen. Dieser Teil kann auch unabhängig vom Lesebuch von Interesse sein. Der dritte (längste) Teil enthält ergänzende Beispiele und weitere (häufig die Beispiele motivierende) Übungsaufgaben. Durch die Auslagerung vieler Beispiele in dieses Arbeitsbuch gelingt es, auch das Lesebuch im Umfang zu beschränken.

Das Buch (zusammen mit dem Lesebuch) kann allen Studierenden des ersten Studienjahres, auch als Begleitlektüre, empfohlen werden, insbesondere allen, die Analysis und Lineare Algebra aus einem Guss lernen möchten.

C. Elsholtz (Graz)

I. Hilgert, J. Hilgert: Mathematik – ein Reiseführer. Springer, Spektrum, Heidelberg, 2012, v+273 S. ISBN 978-3-8274-2931-5 P/b € 24,95.

Dieses Buch möchte ein Begleiter in die Welt der Mathematik sein, insbesondere auch eine Brücke schlagen zwischen der Mathematik in der Schule und in der Wissenschaft. Vier Gebiete stehen im Zentrum: die Geschichte der Mathematik, Anwendungsbereiche, Nützlichkeit der Mathematik und Mathematik als Beruf.

Wie Ingrid und Joachim Hilgert am Ende des Vorworts schreiben, kann ein noch so guter Reiseführer die eigentliche Reise nicht ersetzen. Das Buch ist unersetzlich für alle, die bereit sind, sich auf diese Reise einzulassen.

G. Schranz-Kirlinger (Wien)

B. Kanitscheider: Natur und Zahl. Die Mathematisierbarkeit der Welt. Springer Spektrum, Springer, Berlin, Heidelberg, 2013, xiv+385 S. ISBN 978-3-642-37707-5 H/b € 19,90.

Dieses lesenswerte Buch bietet vielfältige Essays über die Natur der Mathematik. Im Stil einer Rhapsodie geschrieben, hat es den Vorteil, dass man die einzelnen Kapitel zumeist voneinander unabhängig lesen kann, aber den Nachteil, dass vieles offen bleibt – was aber wohl Absicht ist. Das beherrschende Thema ist die verstörend erstaunliche Anwendung mathematischer Theorien in den Naturwissenschaften (frei nach E. P. Wigner, The unreasonable effectiveness of mathematics in the natural sciences). Der Geschichte des Problems entsprechend findet auch eine Auseinandersetzung mit theologischen Positionen statt, die aber manchmal der Wesensart religiösen Glaubens nicht ganz gerecht wird (etwa bei Hinweisen auf die „Unsterblichkeit der Seele" auf S. 214). Der Leser sollte einiges über moderne Physik und Mathematik wissen. Die Abschnitte, in denen auf spezielle mengentheoretische Probleme eingegangen wird, kann man dabei (da wohl interessant, aber für die Diskussion nicht entscheidend) auch übergehen.

F. Schweiger (Salzburg)

G. Krauthausen: Digitale Medien im Mathematikunterricht der Grundschule. (Mathematik Primarstufe und Sekundarstufe I+II.) Springer, Spektrum, Heidelberg, 2012, vii+296 S. ISBN 978-3-8274-2276-7 P/b € 22,95.

Die umfassende Entwicklung der Informations- und Kommunikationstechnologie verlangt die Beantwortung der Frage nach ihrem optimalen Einsatz im Unterricht, und zwar prinzipiell auf allen Ebenen. Das vorliegende Werk stellt sich dieser Frage für den Mathematikunterricht in der Grundschule, wobei der Autor, als ausgewiesener Fachdidaktiker, bei der Beantwortung dieser Frage – berechtigterweise – natürlich die Mathematikdidaktik ins Zentrum der Betrachtung rückt. Der Autor macht auch nicht den Fehler, fertige Antworten zu liefern, sondern versucht den interessierten Leser zum eigenständigen Nachdenken anzuregen. Ein solcher Ansatz kann sich natürlich auch nicht am sturen Einsatz vorliegender Produkte orientieren. Dementsprechend ist nach einem einleitenden Kapitel, in dem der Autor seinen Standpunkt und den von ihm gewählten Zugang erläutert, das zweite Kapitel den mathematikdidaktischen Akzenten gewidmet, wobei immer wieder Anregungen zur (gemeinsamen) Bearbeitung herausgearbeitet werden, die für die praktische Anwendung sehr hilfreich sind. Im dritten Kapitel geht der Autor auf das Qualitätsdilemma digitaler Lernumgebungen ein, mit dem man in der Praxis immer wieder konfrontiert ist. Sehr wertvoll sind in diesem Kapitel die exemplarischen Diskussionen, insbesondere gezeigt anhand einer Stundenskizze „Visualisieren von Daten mit Excel". Auch dem doch recht schwierigen Thema der Wirksamkeit von Lernsoftware verschließt sich der Autor im Rahmen dieses Kapitels nicht. Ungefähr die Hälfte des Werks wird vom Kapitel 4 über Szenarios zu digitalen Medien im Mathematikunterricht eingenommen. Wobei der Autor versucht, die ganze Bandbreite zu erfassen, von der Textverarbeitung über die Tabellenkalkulation, Apps (zeitgemäß sehr umfangreich) und das Internet bis zu Foto und Film. Der Autor geht dabei auch der Frage nach, ob nicht oft inhaltlich wohlüberlegte Rahmungen mit dem Fokus auf medienspezifische Möglichkeiten die bessere Wahl sind. In einem sehr kurzen abschließenden Kapitel 5 widmet sich der Autor den Perspektiven der Zukunft. Ein Werk wie das vorliegende wäre ohne eine kurze Abhandlung über Mediendidaktik per se unvollständig; dies wird im Anhang durch einen ca. 20-seitigen Beitrag von Helmut Meschenmoser nachgeholt. Im Ganzen ist dies sicher ein recht lesenswertes Buch, insbesondere für jene, die nicht bereit sind, einfach eine fertige Software mehr oder weniger gedankenlos einzusetzen.

G. Haring (Wien)

W. Kühnel: Differentialgeometrie. Kurven – Flächen – Mannigfaltigkeiten. (Aufbaukurs Mathematik) Springer Spektrum, Springer Fachmedien, Wiesbaden, 2013, vii+384 S. ISBN 978-3-658-00614-3 P/b € 29,86.

Wenn ein im Jahr 1999 erschienenes Buch in der 6. Auflage vorliegt, so spricht dies allein schon für die Qualität eines Lehrwerks. Das Buch führt in verschiedene Gebiete der Differentialgeometrie mit Betonung Riemannscher Mannigfaltigkeiten ein, wobei auch zahlreiche jüngere Ergebnisse berücksichtigt werden. Für den theoretischen Physiker ist die Einbeziehung für die Relativitätstheorie wichtiger Ergebnisse interessant. Das Buch ist eine gute Grundlage für eine Jahresvorlesung, kann aber auch Studierenden empfohlen werden, die die Mühe nicht scheuen, sich in diese (manchmal etwas kompliziert dargestellte) Materie gründlich einzuarbeiten.

F. Schweiger (Salzburg)

D. Lau: Algebra und Diskrete Mathematik 1 und 2. Band 1: Grundbegriffe der Mathematik, Algebraische Strukturen 1, Lineare Algebra und Analytische Geometrie, Numerische Algebra. (Springer-Lehrbuch.) Springer, Berlin, Heidelberg, New York, 2004, xiv+478 S. ISBN 3-540-20397-4 P/b € 29,95; Band 2: Lineare Optimierung, Graphen und Algorithmen, Algebraische Strukturen und Allgemeine Algebra mit Anwendungen. (Springer-Lehrbuch.) Springer, Berlin, Heidelberg, New York, 2004, xiv+494 S. ISBN 3-540-20398-2 P/b € 29,95.

Dieses zweibändige Werk ist aus Vorlesungen hervorgegangen, die die Autorin im Rahmen der Mathematikausbildung für Studierende der Informatik an der Universität Rostock gehalten hat.

Inhalt Band 1: Grundbegriffe, algebraische Strukturen; lineare Algebra und analytische Geometrie; numerische [lineare] Algebra.

Inhalt Band 2: Lineare Optimierung; Graphen und Algorithmen; algebraische Strukturen und allgemeine Algebra.

Die Darstellung ist systematisch gegliedert, übersichtlich und ausführlich, sozusagen enzyklopädisch im Stil, jedoch didaktisch nicht herausragend. Sie lässt auch den Anwendungsbezug weitgehend vermissen, wobei Band 2 diesbezüglich etwas besser gelungen ist. Manches wird formelmäßig bzw. algorithmisch spezifiziert, ohne dass der mathematische Hintergrund hinreichend genauer beleuchtet wird (Beispiel: Verfahren der konjugierten Gradienten).

Schwerpunkt von Band 2 ist das Algebra-Kapitel, das sehr ausführlich geraten ist und den Rahmen eines einführenden Lehrbuchs sprengt.

Eine umfangreiche Sammlung von Übungsaufgaben (mit gelegentlichen Lösungshinweisen) ergänzt die Darstellung.

W. Auzinger (Wien)

Y. A. Melnikov, M. Y. Melnikov: Green's Functions. Construction and Applications. (De Gruyter Studies in Mathematics 42.) De Gruyter, Berlin, 2012, xii+435 S. ISBN 978-3-11-025302-3 H/b € 99,95.

This book provides a large list of Green's functions which are crucial for practical solution of partial differential equations. The main purpose of this text is to give easily computable or computer-friendly representations of Green's functions. The book covers the two-dimensional Laplace equation, the static Klein-Gordon, the biharmonic, the diffusion, and the Black-Scholes equations, a range from fluid and solid mechanics to financial engineering. The first chapter lays the methodical background for the construction procedure of Green's functions and for the special symmetry feature of Green's function. Each section of the book contains various interesting examples illustrating the text and a carefully worked out set of exercises with answers and hints to most of them in the Appendix.

F. Haslinger (Wien)

T. Napier, M. Ramachandran: An Introduction to Riemann Surfaces. (Cornerstones.) Birkhäuser, Basel, 2011, xvii+560 S. ISBN 978-0-8176-4392-9 H/b € 64,95.

The present book gives a solid introduction to the theory of both compact and noncompact Riemann surfaces. While modern introductions often take the view point of algebraic geometry, the present book tries to also cover the analytical aspects. Moreover, the present book also emphasizes the connection to several complex variables by using the L^2 $\bar{\partial}$-method as a central object. The first chapter collects some basic results in the complex plane. The following two chapters develop the $\bar{\partial}$-method and establish classical results like the Mittag-Leffler, the Runge approximation and the Weierstrass theorem for Riemann surfaces. The next three chapters cover advanced topics on compact Riemann surfaces, uniformization and embedding and holomorphic structures on topological surfaces. Finally, there are three chapters with background material on higher analysis and Hilbert spaces, (multi)linear algebra, manifolds, (co)homology, and Sobolev spaces.

The book is well written and constitutes a nice contribution to the existing literature on this topic.

G. Teschl (Wien)

A. Papadopoulos (ed.): Strasbourg Master Class on Geometry. (IRMA Lectures in Mathematics and Theoretical Pysics 18.) EMS, Zürich, 2012, vii+454 S. ISBN 978-3-03719-105-7 P/b € 48,–.

This book grew out of two weeks of intensive courses held in Strasbourg under the title "Geometry Master Class" in 2008 and 2009. The aim of these courses was to introduce advanced master students and PhD students to topics of current research in geometry and topology. The volume contains articles by various authors, which cover the topics of eight of the ten courses. Each of the articles offers an introduction to and a survey of the topic of the respective course. Together, they form a nice panorama of several areas of current interest in geometry.

The first article on non-Euclidean geometry sticks out a bit, since with about 180 pages, it has the size of a small book on its own. The other articles in the collection are shorter, between 20 and 50 pages each. The topics covered in these other articles are hyperbolic geometry and its relation to number theory, origamis in Teichmüller space, the topology of 3-manifolds, globally symmetric spaces, the geometry of representation spaces in $SU(2)$, hyperbolic 3-manifolds, and asymptotic geometry.

A. Čap (Wien)

P. Schneider: p-Adic Lie Groups. (Grundlehren der mathematischen Wissenschaften 344.) Springer, Berlin, Heidelberg, 2011, xi+254 S. ISBN 978-3-642-21146-1 H/b € 79,95.

This book offers a comprehensive and well readable introduction to the theory of p-adic Lie groups, or rather introductions to two different approaches to this topic. A specific feature of the book is that it focuses on the p-adic situation and does not discuss Lie groups over the reals.

The first part (about 150 pages) offers an introduction to the topological approach to p-adic Lie groups. Beginning with background on ultrametric spaces, non-Archimedean fields and analysis on them, the author develops the basic theory of manifolds over p-adic fields and topologies on spaces of analytic functions on such manifolds. Then Lie groups are introduced and the p-adic version of the correspondence between Lie groups and Lie algebras is developed.

The second part (about 100 pages) is much more algebraic in nature (and also requires more algebraic background). Here the author gives the first introduction in book form to M. Lazard's algebraic approach to p-adic Lie groups. This is based on the notion of a p-valuation on a pro-p-group. Such a valuation is available on an open subgroup of any p-adic Lie group. Conversely, Lazard has shown that under a small additional assumption any pro-p-group endowed with a p-valuation can be made into a p-adic Lie group. This is based on the study of a completed group ring that can in turn be related to the Lie algebra.

A. Čap (Wien)

J. Strom: Modern Classical Homotopy Theory. (Graduate Studies in Mathematics, Vol. 127.) American Mathematical Society, Providence, Rhode Island, 2011, xxi+835 S. ISBN 978-0-8218-5286-6 H/b \$ 95,–.

The phrase "classical homotopy theory" used in the title of the book refers to the circle of ideas in homotopy theory which originates from concepts like fibrations and cofibrations, loop spaces and suspensions, CW complexes, and long exact sequences induced by fibrations and cofibrations. Via the Brown representation theorem, these ideas can also be applied to the study of homology and cohomology theory. The concept of a model category, which captures the ingredients needed for this approach axiomatically is kept in mind as a background throughout the book, but the actual presentation focuses on categories of spaces. The style of presentation is modern in the sense that categorical language is used throughout the book and homotopy limits and colimits are a crucial ingredient.

An unusual feature of the presentation is that the book contains no complete proofs. Rather than that, proofs are provided in the form of sequences of problems. For experts, these can be viewed as condensed outlines of proofs, while the novice is invited to actively participate in the development of the theory. Another feature of the book is that the author tries to use topological rather than algebraic arguments as much as possible.

In view of the style described above and the size of the book (more than 800 pages) it is not surprising that an impressive amount of material is covered in the text. This ranges from the basics of category theory and homotopy theory to topics like cohomology operations and the Steenrod Algebra, the Leray-Serre spectral sequence, Bott periodicity and K-Theory, cohomology of Eilenberg-Mac Lane spaces, and stable homotopy groups of spheres.

A. Čap (Wien)

S. Tabachnikov: Geometrie und Billard. (Springer Lehrbuch) Springer Spektrum, Springer Berlin, Heidelberg, 2013, xi+165 S., ISBN 978-3-642-31924-2 P/b € 23,35.

Begeisterte Karambol-, Pool- oder Snookerspieler in der mathematischen Community machen sich zwangsläufig bei jedem Tischbesuch Gedanken über gewisse geometrische Eigenschaften ihrer Stöße. Dementsprechend haben sich im Laufe der Jahre eine Vielzahl von Autoren konkretere Gedanken zu diesen Dingen gemacht, und in der Literatur findet man eine ansprechende Anzahl an Arbeiten, die sich diesem Thema widmen.

Anlässlich eines Intensivkurses für Undergraduates an der Pennsylvania State University im Jahr 2005 sammelte der Autor verschiedene Ergebnisse aus diesem Themenbereich und baute seine Sammlung schließlich zum vorliegenden Werk aus (im original Englischen als AMS-Band veröffentlicht; jetzt in deutscher Übersetzung bei Springer Spektrum erschienen). Er versucht dabei die In-halte so anzuordnen, dass sie von Studentinnen und Studenten mit oder ohne Vorkenntnisse in Differentialgeometrie, Topologie oder Analysis begriffen werden können, wobei solche mit Kenntnissen in diesen Gebieten selbstverständlich tiefer in die Materie eintauchen werden können. Aufgelockert wird der Diskurs zwischendurch mit Ausflügen zu so disparaten Kapiteln wie der projektiven Geometrie, Brachistochronen, dem Vierscheitelsatz für glatte, einfach geschlossene ebene Kurven, dem Benfordschen Gesetz oder der mathematischen Theorie der Regenbögen.

Es ist also klar, dass es in diesem Werk nicht um den systematischen Aufbau eines mathematischen Teilgebiets geht, sondern vielmehr um die spielerisch-entdeckende Auseinandersetzung mit einem Sachverhalt, der Fäden in unerwartet viele mathematische Teilbereiche zieht. Die Lektüre scheint also für solche Leser prädestiniert, die sich mit Themen der (etwas) höheren Mathematik gern zum Vergnügen auseinandersetzen, aber auch z.B. für besonders interessierte Maturanten, die ein anregendes, wenn auch nicht gerade anspruchsloses Thema für eine vorwissenschaftliche Arbeit suchen. Jedenfalls darf ein potentieller Leser keine Scheu davor haben, plötzlich mit Inhalten aus einem mathematischen Teilgebiet konfrontiert zu sein, die etwas jenseits der eigenen *comfort zone* angesiedelt sind. Gefragt ist wendiges Denken und bringt man dies zur Lektüre mit, wird man durchaus mit Interessantem belohnt.

R. Geretschläger (Graz)

J. B. Walsh: Knowing the Odds. An Introduction to Probability. (Graduate Studies in Mathematics, Vol. 139.) American Mathematical Society, Providence, Rhode Island, 2012, xvi+421 S. ISBN 978-0-8218-8532-1 H/b $ 75,–.

Der Untertitel des Buchs *An Introduction to Probability* ist insofern gerechtfertigt, als es dem Inhalt und Stoffumfang nach etwa das enthält, was in einem zweisemestrigen Kurs vermittelt werden kann und nicht versucht, jedes Teilgebiet in enzyklopädischer Breite abzuhandeln. In diesem Sinne aber ist das Buch tatsächlich eine Einführung in die Wahrscheinlichkeitstheorie und die Theorie stochastischer Prozesse, denn während im ersten Teil Wahrscheinlichkeitsräume, Zufallsvariable, Erwartung, Konvergenzarten, Gesetze der großen Zahlen, Verteilungskonvergenz und der Zentrale Grenzverteilungssatz betrachtet werden, enthält die zweite Hälfte Kapitel über Markovketten, bedingte Erwartungen, Martingale und Brownsche Bewegung.

Man sollte sich aber durch den oben erwähnten Untertitel zu keinen voreiligen Schlüssen im Bezug auf das mathematische Niveau dieses Buchs verleiten lassen. Wie der Autor in seinem Vorwort schreibt, wendet es sich an eine Leserschaft mit soliden Kenntnissen aus Analysis und komplexer Analysis, und wenngleich maßtheoretisches Wissen nicht unbedingt erforderlich ist, so wird es doch das Verständnis mancher Abschnitte erheblich erleichtern.

Ein Beispiel möge das ein wenig konkretisieren: Durchaus im Sinne einer Einführung wird im Text nur Cantellis Gesetz der Großen Zahlen gebracht, aber Kolmogoroffs erstes Gesetz der Großen Zahlen findet sich im Wesentlichen in einer Reihe von Übungsbeispielen (mit Hinweisen), und Kolmogoroffs zweites Gesetz wird mithilfe von Rückwärtsmartingalen bewiesen.

Dieses Buch ist sehr gut konzipiert und kann dem oben beschriebenen Leserkreis, etwa Studierenden des Masterabschnitts, ohne Wenn und Aber empfohlen werden, zumal die einzelnen Kapitel auch noch durch zahlreiche Beispiele (ohne Beweise) der verschiedensten Schwierigkeitsgrade abgerundet werden.

N. Kusolitsch (Wien)

S. Weinberg: Lectures on Quantum Mechanics. Cambridge University Press, 2012, xiv+358 S. ISBN 978-0-521-89697-9 H/b £ 40,– $ 75,– € 57,–.

The present book by one of the most eminent theoretical physicists gives a graduate level introduction to non-relativistic quantum mechanics. It is well written in a clear and concise style with many historic details. Rather than trying to cover everything, the author has made a nice selection starting with a historical introduction and continuing with particles in a central field, general formalism, spin, basic approximation techniques, scattering theory, canonical formalism, particle in electromagnetic fields, radiation, and entanglement. Clearly the selection is also intended to lay the foundations for tackling his famous treatise "The Quantum Theory of Fields". Symmetry is used as one of the guiding principles throughout the text and I also enjoyed the fact that Dirac's bra-ket notation is not used, but some people might consider this a drawback. In any case, there is no doubt that it will establish itself as a classical reference. As a final remark, the apparent lack of illustrations should be fixed in the next edition.

G. Teschl (Wien)

Nachrichten der Österreichischen Mathematischen Gesellschaft

Schülerinnen- und Schülerpreis für herausragende Fachbereichsarbeiten in Mathematik oder Darstellender Geometrie 2013

Seit dem Jahr 2009 vergibt die Österreichische Mathematische Gesellschaft einen Preis für herausragende Fachbereichsarbeiten aus Mathematik und Darstellende Geometrie, der auch vom Bundesministerium für Unterricht, Kunst und Kultur unterstützt wird. Dieses Jahr wurden vier besonders herausragende Arbeiten im Rahmen des ÖMG-DMV-Kongresses in Innsbruck (27. September 2013) gewürdigt:

- Stefan Kartusch (BG/BRG Freistadt): GPS – Global Positioning System: Mathematische Hintergründe. Betreuer: Mag. Robert Schürz.
- Marlene Koch (BG/BRG Schwechat): Let's make money: Berechnung verschiedener Geldanlage- und Finanzierungsmöglichkeiten. Betreuer: Mag. Bernhard Diemer.
- Georg Maierhofer (BG/BRG Mürzzuschlag): Principal Component Analysis und Independent Component Analysis. Betreuerin: Mag. Ursula Brünner.

Von links nach rechts: M. Oberguggenberger, B. Diemer, M. Koch, T. Hollaus, S. Kartusch, R. Schürz, J. Siebert, U. Brünner, G. Maierhofer, H. Humenberger.

- Jannik Siebert (Franziskanergymnasium Hall in Tirol): Markov-Modelle: Grundlagen und Anwendungsgebiete in der Statistik. Betreuer: Mag. Thomas (Br. Pascal) Hollaus.

Die Preisträger waren eingeladen, ihre Arbeiten in einem kurzen Referat vorzustellen. Die Preisverleihung (Urkunde, Buchpreis, einjährige Mitgliedschaft bei der ÖMG) erfolgte durch Hans Humenberger und Michael Oberguggenberger. Beide betonten das außerordentlich hohe Niveau der eingereichten Arbeiten und die exzellente Qualität der Präsentationsvorträge. Die Themenwahl war vielseitig und anspruchsvoll. Wir gratulieren allen Teilnehmerinnen und Teilnehmen ganz herzlich zu diesen hervorragenden Leistungen.

Die ÖMG wird den Preis in den kommenden Jahren weiter regelmäßig ausschreiben, auch wenn ab dem Schuljahr 2014/15 anstelle von Fachbereichsarbeiten die *vorwissenschaftlichen Arbeiten* treten. Der Springer-Verlag hat zugesagt, in den kommenden Jahren den ÖMG-Schülerinnen- und Schülerpreis mit einem Buchgeschenk zu sponsern.

Michael Drmota (Vorsitzender der ÖMG und Vorsitzender der Schülerpreisjury) und Michael Oberguggenberger (Stellvertretender Vorsitzender der ÖMG)

Neue Mitglieder

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