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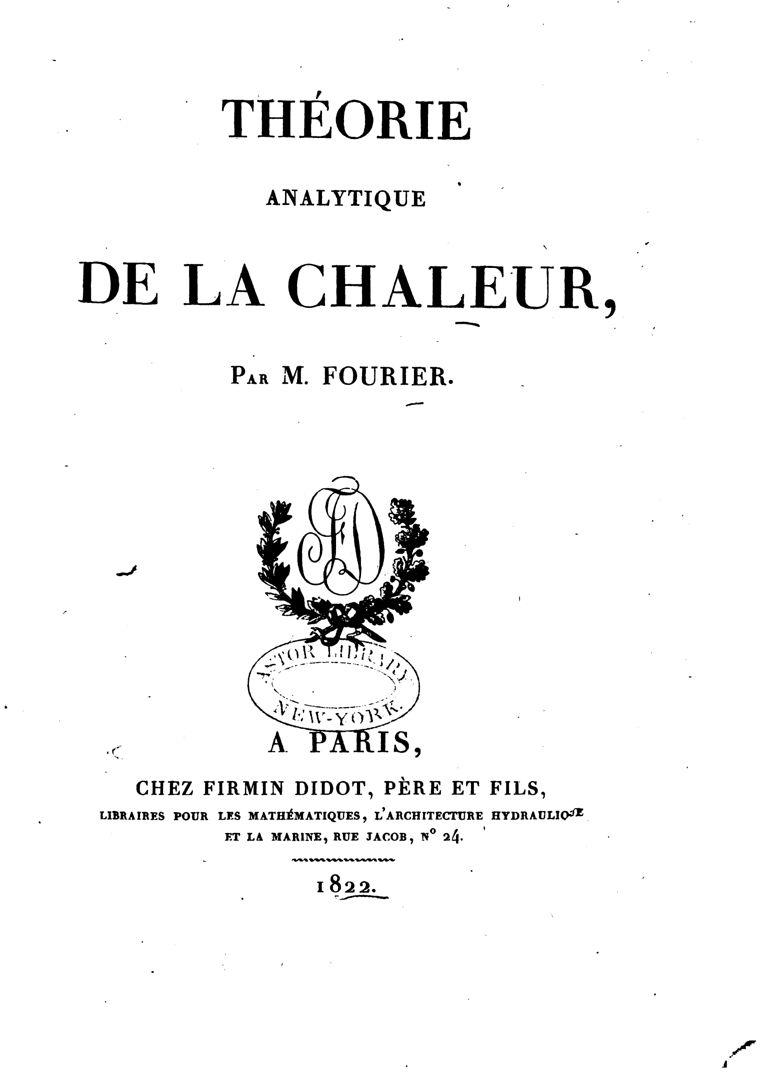

THÉORIE

ANALYTIQUE

DE LA CHALEUR,

PAR M. FOURIER.

A PARIS,

CHEZ FIRMIN DIDOT, PÈRE ET FILS,

LIBRAIRES POUR LES MATHÉMATIQUES, L'ARCHITECTURE HYDRAULIQUE ET LA MARINE, RUE JACOB, N° 24.

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Cover picture: Joseph Fourier's 'Théorie analytique de la chaleur', 1822

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Fourier, one man, several lives

by BERNARD MAUREY (Sorbonne, Paris)

Fourier was born 250 years ago, twenty-one years before the French Revolution in 1789. The events of those troubled times turned his life into an adventure novel: the Revolution with its mortal dangers; Bonaparte's expedition to Egypt with its discoveries; later a political career as prefect of Isère at Grenoble, where Fourier wrote the first versions of the *Théorie analytique de la chaleur*, when he was not busy with the construction of the road from Grenoble to Turin or the drainage of marshland at Bourgoin; and finally, his academic role in the very heart of the Parisian scientific community during the years 1820–1830. While relating a variety of aspects which are not all of scientific concern, we shall, of course, dedicate an important space to the theory of heat, Fourier's major work, as well as to the Fourier series which are a crucial element of his mathematics.

Some books about Fourier

Numerous authors have written about Fourier, especially from the second half of the 20th century on, when several new works were published. Jean Dhombres and Jean-Bernard Robert have done a colossal work [D–R] which I did not hesitate to exploit, although often at the price of regrettable simplifications. A new work, under the direction of Dhombres, is to appear this year [Fo-R]; it aims at a broader public, contains a great number of illustrations and as yet unpublished archive material. Ivor Grattan-Guinness was the first to publish, in his 1972 book [GraF], the content of the essay *Sur la propagation de la chaleur* presented by Fourier at the *Académie des Sciences* in December, 1807. Umberto Bottazzini [Bott] dedicated two sections[1] to the study of the heat problem in the years 1800–1830. Another book I am going to refer to is the one by Jean-Pierre Kahane and Pierre Gilles Lemarié-Rieusset [K–L], the first part of which, written by Kahane, presents a history of Fourier series. The treatise on harmonic analysis by Thomas Körner [Körn] masterly sets out the mathematics attached to the name of Fourier. It also contains two small chapters on Fourier's life[2], based mainly on the very informative book by John Herivel [Heri].

[1] [Bott, 2.3, 2.4]
[2] [Körn, part VI, ch. 92 and 93]

1 The Revolution, the Egypt campaign

Jean-Baptiste Joseph Fourier was born on 21 March 1768 in Auxerre. The baptism certificate bears *Jean-Joseph* as his first name. He was born into a family of craftsmen in social ascension: his father, a tailor, had about ten employees. At the age of ten, Jean-Joseph was orphaned and the clergy of Auxerre took care of him. There was a remote – and uncertain – family relationship with a beatified priest, so the abandonment of the boy was out of the question. He received a good education at the *École Royale Militaire d'Auxerre*, which was run by monks. After finishing school, at the age of 19, he applied for admittance to the entrance examination to the artillery, which was curtly refused. "Not being noble," it was impossible for Fourier to become an artillery officer. So he turned to the religious orders and became a novice at the Benedictine Fleury Abbey in Saint-Benoît-sur-Loire. He lived there for two years, from 1787 to 1789, and could have become *Father Fourier*; but the French Revolution broke out and the constituent *Assemblée nationale* issued decrees suspending the pronunciation of religious vows just before Fourier would pronounce his own in November 1789. Early in 1790, he returned to his former school in Auxerre, this time as a teacher. It was called "Collège National–École Militaire" at that time. He stayed there for four years, taught different subjects such as history, philosophy, eloquence and also mathematics, and became a "civil servant teacher".

Initially reserved towards the French Revolution, Fourier engaged in the *Comité de surveillance* of Auxerre in the beginning of '93 and even became its president in June '94. He witnessed violent scenes of desecration of churches during the wave of dechristianizing of '93–94, though we do not know what his feelings about it were. From September '93 on, the Comité d'Auxerre found itself in charge of executing the decisions of Maximilien de Robespierre and the *Comité de Salut Public*. Fourier, being rather moderate, might have been jeopardised by his lack of zeal in supporting the head cut- ters. Victor Cousin, his successor at the *Académie française* in 1831, reported in the *Notes additionnelles à l'éloge de M. Fourier* – years after the events – that Fourier had deliberately spoiled the arrest, in the town of Tonnerre, of a man sentenced to scaffold[3]. Nevertheless, Fourier signed a certain number of arrest warrants in the context of his competence in the *Comité d'Auxerre*. One event was to

[3] [D–R, ch. III, p. 94]

have led to his imprisonment: the "affaire d'Orléans,"which is reported in great detail by Herivel[4].

Early in October '93, Ichon, a member of the *Convention*, was dispatched to collect weapons, equipment and horses in the Yonne and six surrounding *départements*, preparatory to certain operations in the Vendée. With this aim, he named six citizens of Auxerre – among them Fourier – for a one month mission in Orléans from mid-October on. The city was troubled by the conflicts between sans-culottes and bourgeois. Laplanche, also a member of the *Convention*, had been sent there from Paris at the beginning of September. He first took revolutionary measures supposed to satisfy the sans-culottes, but then he did not resist the pressure from the richer classes and clashed with the leaders of the sans-culottes. Fourier opposed himself to Laplanche and, clearly exceeding the scope of his mission, supported the "left wing,"as we would call it today. As a result, Laplanche and the authorities of Orléans requested Fourier's recall to Auxerre and denounced his behavior. Their complaint was transmitted to Paris. A decree from Paris relieved Fourier from all his duties on 29 October '93: "The Commission conferred [. . .] to the citizen Fournier [*sic*] is revoked; he is no longer authorized to receive such Commissions,"and he was not permitted to carry out any more public functions. Ichon, who was responsible for Fourier's dispatch to Orléans, felt part of the blame reflecting on him; in his fury, he issued an arrest warrant against Fourier, who fortunately had not returned to town yet. As things had calmed down a bit by his return to Auxerre, he was left in peace. Meanwhile, the Orléans affair ended up reappearing in Paris. With Robespierre fighting to his left as well as to his right, the agitators from Orléans were targeted, and so was Fourier. On 19 June '94, the *Comité de Sûreté Générale* ordered his arrest (it was the very June the "Grande Terreur" law was adopted). We know today [Fo-R] that Fourier was not imprisoned; he benefited from privileged treatment and was put under house arrest on 4 July at his home in Auxerre. Robespierre fell at the end of July and Fourier was "freed" on 11 August.

At the end of '94, Fourier was selected as one of the young teachers to be trained at the newly established *École Normale*, the "École Normale de l'an III."The institution lasted only one semester, from January to June '95. Fourier was a distinguished student. Fourier was a distinguished student, but

[4] [Heri, 2.2]

because of political changes, his former participation in the Jacobin committee got him into trouble. In times of the Thermidorian Reaction, a hunt for "terrorists" was taking place. The new authorities in Auxerre wanted Fourier to be expelled from the *École Normale*; they reproached him with his past in an address to the *Convention Nationale*[5]:

> We, the Representatives, shudder when we consider that the pupils of the *Écoles Normales* have been chosen under the rule of Robespierre and by his protégés; it is only too true that Balme and Fourrier [*sic*], pupils from the Yonne department, have for a long time uttered the appalling principles and the infernal maxims of the tyrants.

At the beginning of June '95, Fourier was imprisoned. After a few days he obtained a conditional release order, but the order was not followed and he stayed in prison for a month or more. At the end of August he was freed, his judicial troubles finally settled and all his civil rights restored.

The first years of the Revolution were certainly dangerous, though undoubtedly exciting, too. Kahane[6] cites a passage from a letter Fourier wrote:

> As the natural ideas of equality were developing it became possible to conceive the sublime hope to establish among us a free government without kings nor priests and to take this double yoke away from the European soil that had been usurped for so long.

And yet it was thanks to the education he received from the Benedictines at the *École Royale Militaire* of Auxerre, that he was able to write beautiful sentences like the one we just cited, and the institution made a teacher out of him. The above extract is taken from a long letter written in the summer of '95 to Edmé-Pierre-Alexandre Villetard, deputy of the Yonne (reproduced by Dhombres and Robert[7]), under the circumstances mentioned above, when Fourier tried to justify his behaviour in the years '93–'94, his integrity being questioned.

As an outstanding student of the École Normale, he attracted the attention of Gaspard Monge, he attended lectures of Pierre-Simon Laplace and of the eminent Joseph-Louis Lagrange, "the first among Europe's scholars,"as Fourier

[5] [D–R, ch. IV, p. 150]
[6] [K–L, ch. 1, p. 8]
[7] [D–R, Annexe IV, p. 709]

wrote in his *Notes sur l'École Normale*[8]. Laplace, an acknowledged scientist under the Ancien Régime, had to seclude himself during the Terreur; in '95, he reappeared on the scene and quickly became very influential. Fourier mentioned in his *Notes* that he also attended lectures on physics by René-Just Haüy, on chemistry by Claude-Louis Berthollet as well as the lectures by the – very old – naturalist Louis Jean-Marie d'Aubenton (I am citing only the most well-known). When the *École Normale* was closed, Fourier became a teacher at the *École Polytechnique* (which we are going to refer to simply as "the École" in the following; before September '95 it was called *École Centrale des Travaux Publics*). Recommended by Monge, he became *substitut* at the *École* in the end of May '95 – his mission consisted of supervising the students' works –, then assistant teacher in October '95. For over two years, he deeply committed himself in his duties as a mathematics teacher.

One can get an impression of the lectures held by Fourier from 1796 to 1797 from the notes taken by students[9], which are kept at the *Institut de France* and at the *École des Ponts et Chaussées*.[10] These lectures were not based on the manuals from 18th century (like the treatise by Étienne Bézout), but rather inspired by Lagrange and Laplace's lectures given at the *École Normale*; also the geometric spirit of Monge is discernible. From November '95 onwards, Fourier was in charge of the lectures on *Algebraic Analysis*, which prepared for the lectures on differential calculus. In January '96 he took over part of Prony's Analysis lectures, including the calculus of variations. In March '96, he showed the students the existence of complex roots of polynomials by means of the method presented by Laplace at the *École Normale* [Éc-N]. Laplace's proof applies to the case of real coefficients; it puts the degree n of the polynomial into the form $n = 2^i s$ where s is odd, and proceeds by a subtle recurrence on i, the case $i = 0$ being determined by the intermediate-value property – taken as evident. Fourier simplified and generalized a bit: if we suppose that the polynomials with complex coefficients of odd degree have a complex root, we can factor them into complex factors of first degree. In May '96, he treated differential and integral calculus. In '97, he succeeded Lagrange in the chair of Analysis and Mechanics. He could have occupied it for many years, as several

[8] [D–R, Annexe II]
[9] [D–R, ch. IV, p. 158]
[10] [GraF, ch. 1, p. 6–7]

others did. However, political events were to divert the course of his existence.

The expedition to Egypt was a pivotal episode in Fourier's life. Early in '98, the authorities of the *Directoire exécutif* enjoined him to take part in an operation that was surrounded by secrets: by then, only few of its members knew the exact destination. At the end of March '98, Fourier left Paris, as did some forty current and former students of the *École*, out of the graduates '94 (the first year) to '97, whose teacher Fourier might have been. Among them were Jean-Baptiste Prosper Jollois and Édouard de Villiers du Terrage ("Devilliers" at the *École*, a tribute to the Revolution), engineer and future engineer of the *Ponts et Chaussées* corps, aged 22 and 18; they would write many pages of the monumental work *Description de l'Égypte*, which, published from 1809 on, shall record the discoveries of the expedition through the texts and illustrations from numerous contributors; Fourier would contribute with a long preface. Among the illustrators are Vivant Denon and Henri-Joseph Redouté (painter, brother of Pierre-Joseph Redouté, who is known for his watercolor paintings of roses). Scientists and engineers like Monge, Berthollet, Étienne Geoffroy Saint-Hilaire, Nicolas-Jacques Conté, and Pierre-Simon Girard, also took part in the journey. Back to France, Girard, chief engineer of the *Ponts et Chaussées* corps, would direct the construction of the Ourcq canal; under his orders in 1809: the young Augustin Louis Cauchy, 20 years old, aspiring engineer at *Ponts et Chaussées*.

Fourier boarded in Toulon in mid-May. An expeditionary corps of over 30,000 men set off from France and Italy. The Egypt campaign was not easy: among many other victims, 7 of 42[11] young polytechnicians would never come back. The expedition landed at Alexandria, early in July. At the beginning of August, in Rosetta (the place where the famous stone was found in July '99, at about 50 km from Alexandria), Fourier became responsible of the *Courrier de l'Égypte*, a newspaper with the mission to promote the engagement of the General-in-chief Napoléon Bonaparte. At the end of August '98, he was named Permanent Secretary of the *Institut d'Égypte* created in Cairo by Bonaparte. He played an administrative as well as a political role, especially when it came to negotiations with the local authorities. Dhombres and Robert point out that when Bonaparte engaged in Syria (February-June '99), Fourier found himself as the de facto governor of Lower Egypt, without officially holding

[11] [Mass, annexe]

the title. When Bonaparte (and Monge) returned to France in August ’99, he remained the principal civil authority, in particular after the death of General Jean-Baptiste Kléber who had been assassinated in Cairo in June 1800 and whose eulogy had been given by Fourier (he knew how to write speeches and was a good orator). He ensured the link between the civilians and the servicemen of the expedition. He negotiated again when the adventure came to an end when General Menou surrendered in September 1801, this time with the English, who held the Egyptian harbors, in order to obtain for the French scientists the right to leave under the best possible conditions, keeping the essential parts of their notes and discoveries. Nevertheless, the Rosetta stone would be sent to England, where it is kept until today.

Fourier’s activity in Egypt was not limited to administration and politics. In October ’98, he acted as examiner of the *École Polytechnique*: together with Monge, he questioned students who graduated in ’96 and came to Egypt. He participated in scientific and archeological expeditions, namely in Upper Egypt in September-October ’99. He led mathematical research, presented several communications at the *Institut d’Égypte* on algebraic subjects, rather minor works which were not published, and also a *Mémoire sur l’analyse indéterminée*, judged more convincing by Dhombres–Robert [D–R] and Grattan-Guinness [GraF] who understand it as a forerunner of what we call *linear programming*. Fourier would pick up this question again, much later, in communications at the *Académie des Sciences* in 1823 – in order to simplify, we use the trerm *Académie des Sciences* for the institution which has been also called *Académie Royale des Sciences* and *Classe des Sciences de l’Institut* – as well as, in an article from 1826 in the *Bulletin des Sciences*, *la Société philomathique*.

2 Grenoble, Paris, the work

On his return from Egypt, Fourier landed in Toulon in November, 1801, and returned to Paris in early January, 1802, where he briefly went back to the *École Polytechnique*. However, Napoléon then sent him to Grenoble as prefect of the Isère department in 1802 after the death of the previous prefect, Gabriel Ricard. Fourier accepted the position and arrived in mid-April. It is possible that this was partially an aggravation but there was also a need to fill the role with a capable and dependable person: qualities that Fourier demonstrated in

Egypt. In Grenoble, he began work on the theory of heat and in 1805, he wrote an unpublished essay that was a sort of first draft of the theory. At the end of 1807, he presented a first essay on the propagation of heat to the *Académie*. The four "examiners" recorded in the minutes of the meeting on 21 December were Lagrange, Laplace, Monge, and Sylvestre Lacroix. The text was not well received by Lagrange[12], [13], and had a slightly better reception with Laplace who, in a memoir of 1809–1810 [Lapl], attributed to Fourier the discovery of the heat equation.

Fourier's 1807 essay, still unpublished, was published and commented on by Grattan-Guinness in 1972 [GraF]; it was kept on at the *École nationale des Ponts et Chaussées*, where Claude Louis Marie Henri Navier, a friend of Fourier's, was a professor. Navier was the "executor" of Fourier's manuscripts. Gaston Darboux, the editor of Fourier's *Œuvres* (Works – published in 1888 and 1890), discovered the essay at the end of the 1880s but did not make use of it. Attached to the "Essay" were documents sent by Fourier to the *Académie* in 1808 and 1809: these showed that he had been made aware of the objections of the examiners and that he had responded. Included in these documents were an *Extrait* submitted in 1809 (only the first ten pages have been preserved) that is a short non-mathematical presentation of the essay's content, and a ten-page collection of *Notes* responding to the objections[14].

The *Académie* remained silent on the work presented by Fourier in 1807. A rather cold summary by Siméon Denis Poisson, published in the *Bulletin des Sciences* in March 1808, mentioned the heat equation, but not the processing by means of "Fourier series."In 1809 Fourier finished writing the *Préface Historique* to the *Description de l'Égypte* (Historic Introduction to the Description of Egypt). This composition hung over him at a time when his mind was occupied with heat and he wanted to see his 1807 essay recognized. The *Préface*, an impressive document of 90 pages, was checked over by Napoleon: Fourier traveled up to Paris to present his work. He had to be a historian to report on the history of Egypt, both ancient and contemporary, a stylist to deliver a text that he considered flawless, and a diplomat to know how he had to describe the actions taken in Egypt by the man who was now the Emperor. Körner states that an Egyptologist of his acquaintance considers this *Préface*

[12] [GraF, p. 24, end of ch. 1]
[13] [Bott, Note(^{5}) for ch. 2]
[14] [GraF, ch. 1, p. 24]

to be “a masterpiece and a turning point in the subject,”[15] and that this Egyptologist was surprised to learn that the author was also equally well known as a mathematician! In order to carry out his task, Fourier was assisted by Jacques-Joseph Champollion-Figeac, who was passionate about Egyptology. His younger brother, Jean-François, who was born in 1790 and was a pupil at the *lycée impérial* in Grenoble in 1804, the same year it was established, was an enthusiast of ancient languages and had a small part in preparing the *Préface*. Jean-François Champollion began deciphering hieroglyphics in 1822. After his death in 1832, he was buried – in accordance with his wishes – near to Fourier (who was also buried not far from Monge) in the Père-Lachaise Cemetery in Paris.

In 1811, Fourier significantly reworked his 1807 text and was finally awarded a prize by the *Académie* in January 1812. Lagrange continued to oppose him (he died the following year). The report awarding this prize was not without its reservations, “. . . the way in which the author reaches his equations is not without its difficulties and . . . his analysis to integrate them leaves something to be desired, as regards the level of generality or even on the side of rigo.r”Although honored by the prize, Fourier was offended. He protested to the permanent secretary for mathematical sciences, Jean-Baptiste Joseph Delambre, but there was not much to be done. The following years brought major political upheavals that occupied and affected the prefect of Isère: 1814 and 1815 saw Napoléon’s first exile, then his return from the island of Elba and his downfall.

After Napoléon’s defeat in Russia, it was French territory that was threatened from the end of 1813 by a coalition primarily made up of Britain, Austria, Prussia and Russia. Henri Beyle, 30 years old and not yet known as the writer Stendhal, was attached to the *Conseil d’État* (Council of State) during the war. He was sent to Dauphiné in November 1813 in order to assist the special commissioner responsible for the measures to be taken to protect the region. In January 1814, Grenoble feared the arrival of the Austrian forces that had taken Geneva. The prefect had to organise the defence with the help of the military and Stendhal. Stendhal did not like Fourier, who, in his opinion, delayed and hindered military action; he had particularly contemptuous words for the prefect: “One of the causes for my trouble in Grenoble was the little intellectual scientist with practically no character and the low manners of a decorated

[15] [Körn, end of ch. 92]

servant, named Fourier."[16] Paris fell on 31 March and Napoléon abdicated on 6 April. On 12 April, he signed the Treaty of Fontainebleau and departed for his new kingdom, the island of Elba. With Austrian troops in Grenoble, Fourier and the majority of his prefecture rallied behind the First Restoration on 16 April. Napoléon's route took him close to Grenoble, to the great discomfort of Fourier, who was to have almost another year in his role as prefect.

In 1815, on his return from Elba, Napoléon entered Grenoble and Fourier left to avoid him. After having suspended him and threatened him with arrest on 9 March, Napoléon reconsidered and named him prefect of the Rhône department on 11 March. Fourier began work again at his new post but it ended with his refusal to apply the purging measures set by Napoleon and his Ministry of the Interior – Lazare Carnot being the Minister of the Interior – and he was dismissed on 3 May 1815.

During the Second Restoration, Fourier's pension was taken away as he was too well known as having served in the Napoleonic regime, particularly for his participation in the Hundred Days. He then received welcome support from the prefect of the Seine, Gaspard Chabrol de Volvic. Chabrol was a former student of the *École* (class of 1794), had had Fourier as a teacher, and, furthermore, had been in Egypt. He was already the prefect of the Seine under Napoléon but did not participate in the Hundred Days and remained in the same role until 1830. Chabrol entrusted Fourier with managing the statistical office for the Seine department. Fourier dedicated himself to this task with great interest and published *Recherches statistiques sur la Ville de Paris et le département de la Seine* (Statistical research on the city of Paris and the Seine department) in four volumes between 1821 and 1829. These were far from the theoretical works on probabilities or statistics by Laplace, but Körner mentions that some demographers know Fourier only as the man who played a significant role in the development of government statistics in France[17].

In 1817, the political upheavals had abated and Fourier was elected a member of the *Académie* after an initial candidacy and an election in 1816 that was not approved by King Louis XVIII. He became the permanent secretary of the Académie des Sciences five years later, after the death of Delambre. As a leading member of the *Académie*, he had the opportunity to be in contact with

[16] [D–R, ch. VI, p. 347]
[17] [Körn, end of ch. 93]

Sophie Germain and they exchanged letters regularly between 1820 and 1827; he obtained spaces for her to attend the Institut's public meetings, he supported her against Poisson, who was also working on the theory of elastic surfaces, and she backed him for the post of permanent secretary in 1822. It is thought that Laplace, in his old age (he was 73 years old in 1822), became closer to Fourier and also supported him. Fourier gave a eulogy for Laplace (deceased in 1827), again a fine speech. In 1822, he edited the definitive version of the Analytical Theory of Heat, and his essay from 1811 was finally published in 1824! He was elected to the *Académie française* in 1826, although the decision was not unanimously appreciated, as it is true that his literary work was somewhat meagre.

The end of Fourier's life was difficult due to ill health. He suffered from chronic rheumatism (also whilst in Grenoble) and may have contracted a tropical disease in Egypt; he became extremely sensitive to the cold, as Grattan-Guinness[18] comments: "[illness] caused him to discourage the diffusion of heat in his quarters,"to the point where he wore thick woollen clothes and ran the heating in all seasons. Throughout these years, he was absent from many of the *Académie*'s meetings. His final months were especially difficult, and he spent his days in a special chair[19] from which he was still able to work. The disease may have also diminished his intellectual faculties when the permanent secretary should have taken better care of the famous essay by Évariste Galois, presented in 1829 and then in 1830. Fourier died on 16 May 1830 in Paris at the age of 62.

For us, Fourier is primarily the man of a unique work, the theory of heat. He published several lesser-known works, including essays on statics in 1798 (an article on rational mechanics, including three proofs of the principle of virtual work using the concept of moment) and on statistics between 1821 and 1829. He left a mass of manuscripts, many of which can be found in the National Library of France. One particular topic must be mentioned: for a very long period of time, Fourier conducted research on determining the number of real roots of a polynomial that are in a given interval, and on the methods of calculating values close to these roots. The question had already interested him in 1787 and even throughout his earlier years [Fo-R]. On 9 December 1789, he

[18] [GraF, end of ch. 22]
[19] [GraF, end of ch. 22]

presented a statement to the *Académie* on this subject, which he also focused on in his lessons at the *École* in 1796 and 1797 and which he worked on in Egypt and then in Grenoble in 1804. These clarifications were given by Navier; see below. Fourier published several articles in the same vein from 1818 and submitted communications to the *Académie* between 1820 and 1830. His research led to a higher limit for the number of roots. In 1829, Jacques Charles-François Sturm discovered the theorem that is now named after him (his essay was published in 1835) and that allowed him to find the *exact* number of roots. Sturm stated that “the theorem that is developed throughout this essay is greatly similar to that of Fourier.”

In his final years, Fourier started a work titled “Analyse des équations déterminées” (Analysis of determinate equations) that he was unable to finish; it was meant to bring together in two volumes the algebraic works mentioned above. Navier went on to publish the existing parts in 1831 and he wrote a “Foreword by the editor” of 24 pages that aimed to confirm Fourier’s precedence over results older than 40 years. Navier cited the documents in his possession: he paid a particular attention to a pre-1789 manuscript *Recherches sur l’algèbre* (Research on algebra) attributed to Fourier (but not by his hand and incomplete, with only the first 28 pages remaining), and mentioned notes taken by a student during Fourier’s lessons at the *École* in 1797, then a text written in Grenoble in 1804. He also concentrated on the existence of accounts that made it possible to date each of these documents. Precedence was contested by François Budan de Boislaurent, who became a doctor of medicine in 1803 and General Inspector of Public Instruction in 1808. He was a skilled mathematician, although an amateur: he submitted an essay to the *Académie* in 1803, published an article in 1807 and a book in 1822 on the same question of the number of roots[20]. The dispute was very heated, even if it is not as important today. If Fourier’s analytical method led to Sturm’s result, it was that of Budan, which is combinatorial and of an algorithmic nature, that has consequences in algebraic computation nowadays.

[20] see Jacques Borowczyk [Boro]

3 Trigonometric series

It was, of course, not Fourier who invented the trigonometric series: Leonhard Euler, Daniel Bernoulli and many others had used them before him; we may need to go back to Brook Taylor, the man of the *Taylor formula*, one of the first to link, around 1715, the vibration of cords to sinusoidal curves, which at the time were called "companions of the cycloid." But Fourier gave some beautiful examples of such series, and above all, systematized the relation between "function" and "Fourier series." By doing so, he helped to modify and specify the conception of functions in mathematics, a task to be completed about twenty years later by Dirichlet. Fourier calculated a large number of trigonometric series expansions of 2π-periodic, not necessarily continuous functions, some of which already figured in his essay from 1805: he rediscovered the expansion of the function equal to x when $|x| < \pi$, mentioning of course that it was Euler's due, and clearly stating[21] the need to limit its validity to $|x| < \pi$; he expanded in a sine series the odd function which equals $\cos x$ for $0 < x < \pi$ (a fact that shocked Lagrange and even Laplace), also the function which equals $\sinh x$ for $|x| < \pi$, and many others. Reading the book by Grattan-Guiness [GraF] one realizes the vastness of the mathematical content in Fourier's works on heat. In the following, I would like to dwell on an example which is undoubtedly the most famous one.

After having explained the physical principles needed to understand the temperature evolution in bodies and having established the *heat equation* inside a solid:

$$\frac{\partial v}{\partial t} = \kappa \Delta v, \qquad \kappa > 0,$$

Fourier proposes[22] to explicitly determine the equilibrium temperature $v(x, y, z)$ in an infinite solid limited by two parallel planes and a third one perpendicular to the two others, supposing a fixed temperature at the edge. The solid is put into an equation so that the geometry and the temperature do not depend on the coordinate z: in art. 165, it is restricted to a problem in x, y, namely a rectangular blade which is modeled by the set $\{(x, y) : x \geqslant 0, |y| \leqslant \pi/2\}$. At the edge, the temperature v equals 1 when $x = 0$ and $|y| < \pi/2$, or 0 when $x \geqslant 0$ and $|y| = \pi/2$. The equilibrium equation in the blade is $\Delta v = 0$. The condition at

[21] for example, [Fo-C, art. 184]

[22] [Fo-C, ch. III, art. 163, p. 159 and next.]

the edge being even in y, Fourier searches for solutions that are even in y: he considers a solution that combines functions $\mathrm{e}^{-kx}\cos(ky)$, where the fact that v is zero in the case $|y| = \pi/2$ imposes that k is an odd integer, and where we have $k > 0$, for reasons of physical likelihood[23]. The method of separated variables had already been used by Jean d'Alembert and Euler, the superposition (even of an infinity) of solutions by Bernoulli. So Fourier searches a v of the following form:

$$v(x, y) = a\,\mathrm{e}^{-x}\cos y + b\,\mathrm{e}^{-3x}\cos 3y + c\,\mathrm{e}^{-5x}\cos 5y + \cdots$$

The condition $v = 1$ for $x = 0$ makes him try to find an expansion which satisfies

$$1 = a\cos y + b\cos 3y + c\cos 5y + \cdots \tag{3.1}$$

when $|y| < \pi/2$. He first determinates the coefficient a of $\cos y$, then finds analogously the following coefficients $b, c, \ldots$ To achieve this, he takes derivatives of equation (3.1) an even number of times and writes for any integer $j > 0$ the identity

$$0 = a\cos y + b3^{2j}\cos 3y + c5^{2j}\cos 5y + \cdots\,. \tag{3.2}$$

To calculate the coefficients, Fourier supposes, as a first step, a limited number of m unknowns $a, b, \ldots, r$, and considers a system of m equations, the first one resulting from (3.1) while the $m-1$ other ones, i.e., for $j = 1, \ldots, m-1$, are

$$0 = a\cos y + b3^{2j}\cos 3y + c5^{2j}\cos 5y + \cdots + r(2m-1)^{2j}\cos(2m-1)y.$$

Putting $y = 0$, he obtains a Vandermonde system, which he solves in order to find an approximate value $a^{(m)}$ to the solution a, and takes the limit with m using the Wallis product formula that provides $a = 4/\pi$.

Going a bit more into detail, using $x_1, \ldots, x_m$ instead of $a, b, \ldots, r$, and setting $k_i = (2i-1)^2$, $i = 1, \ldots, m$, the m equations considered by Fourier are

$$k_1^j x_1 + k_2^j x_2 + \cdots + k_m^j x_m = \delta_{j,0}, \qquad j = 0, \ldots, m-1,$$

where $\delta_{j,0}$ is the Kronecker symbol. Fourier calculates by hand, filling four pages, but we can make use of Cramer's rule that expresses x_1, the approximate value $a^{(m)}$ at step m, with the help of a quotient of two Vandermonde determinants,

[23] [Fo-P, art. 33], to be found in [GraF, p. 138]

$$x_1 = \frac{k_2 \dots k_m \prod_{1<i<\ell\leqslant m}(k_\ell - k_i)}{\prod_{1\leqslant i<\ell\leqslant m}(k_\ell - k_i)} = \frac{k_2 \dots k_m}{\prod_{1<\ell\leqslant m}(k_\ell - 1)}$$
$$= \frac{3.3.5.5 \dots (2m-1)(2m-1)}{2.4.4.6 \dots (2m-2) \quad (2m)},$$

what leads us to Wallis. The calculation for $x_2, x_3, \dots$ is analogous.

Fourier remarks further that the value 1 on the left of equation (3.1) will change into -1 if we add π to y. This essential remark makes him understand which are the values of the 2π-periodic extension of the sum of his series, constant on the interval $(-\pi/2, \pi/2)$: he has obtained [24] the trigonometric series development of a "crenel function,"

$$\frac{\pi}{4}\operatorname{sign}(\cos y) = \cos y - \frac{1}{3}\cos 3y + \frac{1}{5}\cos 5y - \frac{1}{7}\cos 7y + \cdots . \tag{3.3}$$

The formula already figures in the manuscript from 1805 and the study from 1822 of this problem can also be found in the dissertation from 1807[25]. Further on in the text, Fourier comes to the "Fourier" integral formulas for the calculation of coefficients. He had not used them in the previous example, where he applied the computational method described above. Then, using again the same lines of arguments, he establishes the integral formulas, at least initially. Considered a flaw by some, a quality by others, Fourier is not concise: he sets about a long proof, beginning at art. 207, first article of Section VI, *Développement d'une fonction arbitraire en séries trigonométriques* [Fo-C]; he starts from an odd periodic function and writes its development in Taylor series at 0, which is supposed to exist. Equating the Taylor series to the trigonometric (sine) series found for this same function, he calculates the "Fourier" coefficients with the help of equations that look like the ones he gave in the case of the crenel function. This leads to art. 218, the integrals appearing in art. 219. Fourier does not restrict himself to only one proof: in art. 221, he finally proposes to multiply the sum of the trigonometric series by $\sin nx$ and integrate term by term from 0 to π, using the orthogonality which will play such a fundamental role in analysis. He uses this method at least from 1807 on;[26] the

[24] [Fo-C, ch. III, art. 177–180]
[25] [Fo-P, art. 32–43]
[26] [Fo-P, art. 63]

issue, though, is not yet the justification of the integration term by term. In his progressive and "pedagogical" approach, he started from a regular function to apply the first proof (which, to a small extent, could prove the *existence* of the development), and he notes in the end that he is now, with the help of the integral formulas, able to analyze "general" functions.

Back to physics, Fourier gives many examples "limited" in space, one of them being the case of the *armilla*, a metal ring (ch. IV). The study of heat in a cylinder of infinite length leads to Bessel functions: they were presented by Friedrich Bessel in 1816–1817 at the Berlin Academy and published in 1819; but Fourier had studied this example since 1807 [GraF, ch. 15 et 16] and written the power series of J_0 long before Bessel's publication (although after Euler, in 1766 and 1784[27]). Fourier solves by power series the differential equation $u'' + u'/x + \kappa u = 0$ ($\kappa > 0$, u is linked to the Bessel function J_0 by $u(x) = cJ_0(\sqrt{\kappa}x)$), and uses this to produce, for the cylinder, eigenmodes – he called them "modes propres" – that are orthogonal. The constant κ is determined by the condition (7.2) on the surface of the cylinder (given further on), which provides a series of possible values, linked to the solutions $\kappa_i > 0$ of an equation of the form $J_0(\sqrt{\kappa_i}r) + \sqrt{\kappa_i}J_0'(\sqrt{\kappa_i}r) = 0$, $r > 0$ being the radius of the cylinder. Finally, the case of unlimited space, omitted in 1807, reveals the Fourier transformation on the real line: it appears in the awarded dissertation from 1812 (art. 71) and in article 346 of the last chapter of the book from 1822, with its inverse transformation. That chapter IX is simply entitled *De la diffusion de la chaleur* (On the diffusion of heat).

It seems difficult for the amateur historian to evaluate the proof that lead to the equation (3.3) giving the *crenel function* and which used arguments that might be considered totally wrong according to rigorous criteria: the derived series (3.2) given by Fourier are grossly divergent; it is comprehensible that mathematicians from the mid-19$^{\text{th}}$ century may have not taken his mathematics seriously. Today one can say that those series converge in the sense of distributions, but Fourier used the pointwise value of partial sums; Kahane[28] sees it as the search for trigonometric polynomials that become more and more "flat" at 0 and which converge towards the solution. One is tempted to state that Fourier has been lucky in that matter. And even then! As he liked to accumulate concor-

[27] [GraC, 3.4.4, 9.2.8]
[28] [K–L, 2.4]

dant evidence, he calculated explicitly the derivative of the partial sum of (3.1), when one replaces the undetermined coefficients $a, b, c, \ldots$ by the obtained values; this derivative is equal to $(-1)^m \sin(2mx)/(2\cos x)$, and he deduced that the antiderivatives, partial sums of (3.1), were more and more close to constant functions on $(-\pi/2, \pi/2)$[29].

Niels Henrik Abel [AbeU] and Peter Gustav Lejeune Dirichlet [DirC] soon came to bring more rigor into the processing of function series. Abel had not, strictly speaking, considered trigonometric series in his paper from 1826 (which he wrote in French[30]; it was translated into German by August Leopold Crelle, the “chief” of the *Journal für die reine und angewandte Mathematik*, see [AbeO, préface p. III]; see also Bottazzini [Bott, 3.1] for a review of the article). Abel, on the occasion of studying Newton’s binomial series, which he considered not to be sufficiently justified by Cauchy (although he praised the treatise on Analysis by the latter), established principles for the study of function series, in particular for the power series of a complex variable, the continuity of which he proved in the open disc of convergence. Moreover, Abel also wrote the complex number $z = a + \mathrm{i}b$ as $z = r(\cos\varphi + \mathrm{i}\sin\varphi)$ and then obtained Fourier series. Unfortunately, his good principles did not prevent him from making too optimistic statements that turned out to be false[31].

4 Competition for heat, enmities

The study of heat was a serious subject around 1800, especially with the rise of the steam engine. Jean-Baptiste Biot, a student of the first graduation class in the *École polytechnique* in 1794, and later close to Laplace, was a member of the *Institut* from 1803. He is still known mainly for the Biot–Savart Law (1820), as well as for the law on the rotation of polarized light passing through a liquid (1835). In 1804, he published an essay on the propagation of heat (*Mémoire sur la propagation de la chaleur*) [BioM][32]. In this essay, which is very reverential with regard to “Mr Laplace,” he deals with the temperature equilibrium in a bar that is heated at the end, a subject that had already been studied extensively in several European countries, both through experimenta-

[29] [Fo-P, art. 43]
[30] [AbeO, XIV, p. 219]
[31] [Bott, 3.5]
[32] [Bott, 2.3.a]

tion and with attempts at mathematization. We can cite the book by mathematician and physicist Johann Heinrich Lambert *Pyrometrie oder vom Maaße des Feuers und der Wärme* (Pyrometry, or the measurement of fire and heat), which was published in 1779, two years after his death. This book was printed in a Gothic script, which does not help us, and was therefore little read in France and had a correspondingly weak impact. It is also necessary to refer back to an (anonymous) article by Isaac Newton in 1701 that sets an initial principle that one can summarize as follows: the temperature of a warm body, cooled in a constant and low-temperature air current, is a decreasing exponential function of time.

Biot described his experiment, stating that it is not possible to noticeably heat the end of an iron bar that is 2 m long by 3 cm in the cross-section if the other end is placed in an intense fire. In the temperature equilibrium, he found an exponential decay in the temperature of points of the bar when one moves away from the source, putting forward a verbal mathematical proof, but he did not write an equation. He explained the equilibrium that occurs at each point of the bar between the heat received from the source, the heat transferred to the further points of the bar and the heat lost at the surface, but without writing a formula. He also did not cite Lambert, even if these considerations were practically identical to those in art. 326 of the latter[33].

Biot mentioned that the results depend on a second order differential equation (one can think that it takes the form $u'' = \kappa u$, $\kappa > 0$), where appears the quotient of the *radiance* and *conductivity* of the bar, two coefficients that he differentiates between, measuring loss towards the surface and internal conduction. He indicates, without a formula, that the usual solution to the differential equation (in $a\,\mathrm{e}^{\sqrt{\kappa}x} + b\,\mathrm{e}^{-\sqrt{\kappa}x}$) includes only one term here as it must stay bounded when x becomes large (positive). In addition, he highlights using only words that the mathematical process leads, outside the equilibrium state, to a second order partial differential equation involving time. In order to evaluate the temperature of a very hot source, Biot also suggested applying the exponential law discovered: using a bar that has one end touching the source, too hot for a thermometer, it is possible to measure the temperature of a point of the bar that is suitably far from this end and to therefore deduce the temperature of the heat source.

[33] [Heri, 8.1, p. 163]

Fourier's first essay from 1805 already included general equations for the propagation of heat but it was not published. Rather, these were personal notes totalling some 80 pages. Fourier went much further than Biot: he dealt with equilibrium temperatures $v(x, y)$ or $v(x, y, z)$ that depend on several space variables and also looked at the variation with time. However, he wrote the differential equation (4.1) below, simply in x, for the temperature equilibrium of a bar heated at one end and he politely mentioned[34] Biot's work from 1804. In this essay, the heat equation was not yet in its correct form as Fourier included in the equation inside a solid the $h(v - v_e)$ term of the equation (7.2) given below. This term should appear only on the surface[35]; even so, it can be seen in a note added to the margin that he was not sure that this term should be present[36]. The formalization of the physical phenomenon was still not satisfactory[37]: Biot and Fourier struggled with *differential homogeneity* in the infinitesimal analysis of the problem, an "analytical difficulty" that Fourier circumvented then with an artificial contortion. On the other hand, the essay includes accomplished mathematical sections. There are several developments in trigonometric series[38] that Fourier will present again later on, including the crenel function and the sawtooth function.

In his essay submitted to the Académie [Fo-P] in 1807, Fourier gave for the temperature equilibrium v of the "Biot bar" the equation

$$\frac{\partial^2 v}{\partial x^2} = \frac{2h}{K\ell} v \tag{4.1}$$

which involved the width $\ell/2$ of the bar. And Biot's name disappeared, for reasons I was unable to discover. At that time, Fourier applied a physical analysis that he (almost) did not change later, by presenting his concept of *heat flow* (which resolved his problem of homogeneity). Biot's analysis took into account conductivities h, K – in the *quotient* mentioned by Biot and referenced above –, but it did not account for ℓ. Later, Fourier, feeling mistreated by Biot, took pleasure in insisting on several occasions in his correspondence on "the mistake" of the latter: it was incorrect to claim that a 2-metre-long iron rod heated at one end could not be heated at the other end if it has a small cross-section.

[34][GraF, end of ch. 8, p. 186]
[35][Bott, end of 2.3.a, p. 65–66]
[36][GraF, ch. 5, p. 111]
[37][Heri, 8.1, p. 164–165]
[38][GraF, end of ch. 8, p. 184]

Biot was an excellent scientist but Fourier often treated him with disdain. They were not on the same side, either politically or ideologically – Biot was a conservative Catholic –, and on several occasions, Biot disparaged Fourier's work. This opposition could have started with this essay of 1807, copies of which Fourier sent to Biot and Poisson. Rightly or wrongly, Biot believed that Fourier took from his 1804 article, without now citing him, and was insulted. Poisson also attacked Fourier's mathematics. Biot and Poisson were both ambitious and talented young men who were influenced by Laplace; it seems that the "patron" stayed above this clash.

For his part, Laplace wrote on the propagation of heat in 1809 in an essay that dealt with plenty of other subjects in physics, as the title indicates [Lapl]; for heat, he adopted[39] the principle of transmission through action at a short distance. He discovered the heat equations, though he accepted Fourier's priority: "I must remark that Mr. Fourier already got to these equations." He added however, "of which the real foundations seem to me to be those that I have presented." In October 1809, Biot published in *Mercure de France*, a literary magazine, an article [BioC] summarizing *Du calorique rayonnant* (Of radiating heat) by Pierre Prévost. In this article, he cited a number of scientists, such as Laplace, Lavoisier (1784), Pictet, Rumford and Leslie and explained Prévost's perspective on radiation, describing specific examples to grasp the phenomenon. Until this point, there was not much here to anger Fourier, who, at the time, was not particularly concerned with radiation. But Biot continued:

> This is what led a major geometrician (2) [*this (2) refers to a footnote of Biot's article, see below*] to extend radiation even to the interior of solid bodies . . . These considerations immediately provided the mathematical laws of transmitting heat in accordance with phenomena and they have the advantage of removing an analytical difficulty that, until this point, has stopped all those who wanted to calculate the propagation of heat through bodies.

Fourier's name appeared not once in the dozen pages of Biot's article. The note (2) was phrased as follows: "(2) Mr Laplace. What has been related here has been gathered from his conversations and form the subject of a work on heat that he has not yet published." Actually, Laplace had already "read" a text

[39] [Lapl, Note, p. 290 in *Œuvres de Laplace*, t. XII]

at the *Académie* during the meeting of Monday 30 January 1809, which was the prelude to the 1810 essay [Lapl]. Biot actually credited Laplace with all the recent discoveries on the theory of heat and he implicitly contested the validity of Fourier's results, without citing him: "an analytical difficulty [...] that has [...] stopped *all* who [...]." This passage in particular shocked and nettled Fourier. He responded and compiled very sharp criticisms of Biot in letters to several correspondents[40], [41]. Even if he was loath to cause controversy in scientific reviews, Fourier was also a politician with his supporters: to advance his cause, he knew to write to those with influence (he also learned that silence is the most effective in certain circumstances). He also communicated with Laplace in highly civil terms, although he still held a grudge that led him to forget to cite Laplace throughout the entirety of his major work [Fo-C][42].

Biot opposed Fourier, but he was quite quick to leave his research on the theory of heat, unlike Poisson. Nevertheless, Biot discussed heat in his large, four-volume work *Traité de physique expérimentale et mathématique* (Treaty on experimental and mathematical physics) in 1816[43]. In a lengthy footnote on page 669 in volume 4, he claimed to have been the first to establish the correct equation for the stationary state in his 1804 essay. Fourier had no difficulty in contradicting this claim of precedence[44]. In the same footnote, Biot also cited Laplace as having discovered the general heat equations, whereas Fourier only "rediscovered" them: omitting that of 1807, he mentioned Fourier's award-winning essay of 1812, which followed Laplace's essay. To conclude, Biot highlighted the works by Poisson, in which he praised the handling of the problem of heat as being superior to that of Fourier's, which used trigonometric series. No trace of the controversy can be found after 1816, at least in Fourier's lifetime. However, at 68 years old, Biot still had some venom to let out: in an article in the *Revue des Savants* in 1842, which was dedicated, according to the title, to the publication started in 1836 of the *Comptes Rendus Hebdomadaires de l'Académie*, he lashed out at the leadership of permanent secretary Fourier, the quality of his eulogy for Laplace, etc.

40 [D–R, ch. VI, p. 340]
41 [Heri, Appendix, letters XVII and XVIII]
42 [D–R, ch. VIII, p. 479])
43 [GraC, 7.7, 9.4.2]
44 [Heri, ch. 7, p. 158]

5 Parisian Life

Kahane[45] talks about competitors of Fourier, namely Cauchy, and especially Poisson, whose mathematics is rehabilitated by him (if this were necessary); he may want to balance Grattan-Guinness, who said very negative things about Poisson in one of his books [GraF]. Poisson was a competitor, if not an opponent of Fourier. In a seminar, in 2018, I heard Gilles Lebeau talking about Poisson as a great man. It is funny to have a look on how he was seen by a future great man, the young Abel, 24 years old, living some times in Paris (to be precise, from 10 July to the 29 December, 1826). Fortunately for Frenchmen like me, a collection of Abel's letters in French translation appeared in [AbeM], published in 1902 to the centenary of his birth (part of these letters already appeared in 1881, in French with slightly different translations, see [AbeO]). Abel was hoping to enter in contact with French mathematicians, but summer time was not the best period to do so. He writes:

> I have only seen Poisson on a promenade; he looked quite self-satisfied. It is said, though, that he is not. (Lettre XVI, to Hansteen, 12th of August 1826).

and later:

> Poisson is a short man with a nice little stomach. He carries himself with dignity. Like Fourier. (Lettre XVIII, to Holmboe, 24th of october 1826).

As regards physical aspects, Abel certainly preferred young girls of Paris, who are mentioned in the same letter of the 24 October to his Norvegian friend. Abel's letters contain several expressions in French, reproduced below in italics. By their private nature, these letters heavily contrast with Fourier's severity[46], whose emotional life is not really known (although we know that he was jovial by nature). After having said he likes to see Miss Mars in the theatre, and talked about the funeral of the great actor Talma, Abel adds the following:

> I go sometimes to the Palais Royal which is named by people of Paris as *un lieu de perdition*. As a large number, there are *des femmes de*

45 [K–L, 3.5]

46 [D–R, épilogue, p. 683]

> *bonne volonté*. They are absolutely not indiscreet. All we hear is *Voulez-vous monter avec moi mon petit ami; petit méchant*. …Lots of them are quite beautiful.

Abel assures meanwhile that, being engaged in Norway, he stays very reasonable. He also notices the following meeting:

> …Herr Le-jeune Dirichlet, a Prussian who went to talk to me, considering me as a compatriot..

This "Prussian" was born in 1805 in Düren, located at that time in Napoléon's France, between Cologne and Aix-la-Chapelle, but Düren came back to Prussia after 1815; his grandfather was born in Verviers.[47] In May 1822, the young German came to Paris in order to study there. He showed in 1825 one case – among two – of "Fermat's great theorem" for $n = 5$, and presented his results to the *Académie*; the other case was rapidly completed by Adrien-Marie Legendre (and later, by Dirichlet himself, in a paper published in 1828 in Crelle's journal. At the end of 1825, General Foy, who gave him a confortable position as preceptor since the summer of 1823, dieD and Dirichlet considers leaving France. Dirichlet belonged to a circle of Fourier's "supporters," including Sturm, Sophie Germain, Navier and, a little bit later, Joseph Liouville, about 20 yearS old. From the editors' comments on Abel's letters, Fourier recommended Dirichlet for his first position at Breslau (named today Wrocław in Poland) in 1827. It is probably under his influence that the arithmetician Dirichlet turned out to study trigonometric series. In his celebrated article [DirC] published in 1829 in French on that subject, he reproduces identically, without explicitly mentioning Fourier's name but by citing "*Théorie de la chaleur*, No. 232 et suiv.," the equation for the coefficients which can be found at the end of the article 233 of Fourier's book [Fo-C],

$$\frac{1}{2\pi}\int \varphi(\alpha)\,\partial\alpha + \frac{1}{\pi}\left\{\begin{matrix}\cos x\int\varphi(\alpha)\cos\alpha\,\partial\alpha + \cos 2x\int\varphi(\alpha)\cos 2\alpha\,\partial\alpha\ldots. \\ \sin x\int\varphi(\alpha)\sin\alpha\,\partial\alpha + \sin 2x\int\varphi(\alpha)\sin 2\alpha\,\partial\alpha\ldots.\end{matrix}\right\}.$$

Of course, Fourier's work has come close to Dirichlet's kernel D_n and to its use: he wrote[48] indeed – using the outdated function *sinus verse* – the sum

[47] see Jürgen Elstrodt [Elst]
[48] [Fo-C, ch. IX, art. 423, p. 562]

$D_n(x) = \sum_{j=-n}^{n} \cos(jx)$ in the equivalent form $\cos(nx) + \sin(nx)\,\mathrm{cotan}(x/2)$ and he additionally had an heuristic reasoning to a "Riemann lemma," and also to the convergence towards $\varphi(x_0)$ of integrals of φ multiplied by the translation by x_0 of these kernels. Dirichlet made this "reasoning" into proofs.

Does the absence of the name "Fourier" in the paper [DirC] mean that Fourier was such a great man for Dirichlet that naming him was useless? Having this in mind, one can nowadays understand his first paragraph:

> ... This property had not escaped the attention of the celebrated geometer who has opened a new field of applications of analysis, by introducing ways of expressing arbitrary functions; they are given in the Memoir that contains all his first researches on heat.

Dirichlet's article in Crelle's journal, after the title, was introduced in this way:

(By Mr. *Lejeune - Dirichlet*, prof. de mathém.)

and dated on the last page: "Berlin, Janvier 1829," one month before his 24th birthday. Later, in German, in an article [DirD] published in 1837 to the mathematical physicists, Dirichlet mentions Fourier and makes explicit his high esteem of him. On the other hand, Dirichlet [DirC] criticizes Cauchy, who proposed proofs concerning Fourier series (*Mémoire sur le développement des fonctions en séries périodiques*, 1827). Bad memories of Paris, from May, 1822, to 1826? Coming back to the letter of 24 October, 1826, Abel wrote:

> Legendre is a very nice man but unfortunately "old as stones" [*steinalt, in original German*]. Cauchy is *fou*, and one would have nothing to do with him,

but he also adds the following: "although he is nowadays the mathematician who knows how to tackle mathematics." Later, about a memoir entitled *Sur une certaine classe d'équations transcendantes*, which he had just finished and wanted to present to the *Académie*, Abel confides:

> « I have shown it to Cauchy; but he barely had a look at it. And without undue immodesty I dare say that it is good. I am curious to know the opinion of the *Institut*. »

It was precisely the permanent secretary Fourier who would deal with that manuscript, but not as good as it could have been; Legendre (who was, albeit an expert, already 74-year-old) and Cauchy were appointed as referees, in the meeting of 30 October, 1826. Then the process got stuck. Two years later, Carl Jacobi wrote to Legendre, from Königsberg[49], on 14 March, 1829, in order to obtain news from this memoir, one month before Abel's death. Legendre answered on 8 April, from Paris. He explained that "the memoir was almost not readable, being written in very white ink with badly done characters," and Cauchy and he agreed on asking the author to hand in a more readable copy, something that Abel did not, and the matter did not move forward. According to Legendre, Cauchy bears the greatest responsibility for that:

> Mr. Cauchy kept the manuscript without taking care of it .... However, I asked Mr. Cauchy to give me the manuscript which I never have had and I will check what I can do to right, if possible, the lack of care that he gave to a work which would have certainly deserved better.

Kahane[50], speaking of Fourier, tells that Cauchy "was not his friend," which is a nice understatement. It is often written though that Cauchy acknowledged Fourier's authorship of the notation $\int_a^b$ for the definite integral, taken between a and b: It is as if Georg Cantor, or Karl Weierstrass after 1885, insisted to acknowledge Leopold Kronecker's authorship of his δ symbol...

6 Reception of His Work: Riemann

One can read in several documents that Fourier stayed unknown, badly discussed in France – even Victor Hugo had his opinion about that![51] – in spite of his (slow) recognition by the *Académie*. His collected works ("ses Œuvres") were belatedly published, in 1888 and 1890. Nevertheless, Dirichlet celebrated him, in French, only seven years after the publication of the book of 1822, and he certainly passed on his high assessment of Fourier's works to Bernhard Riemann. The historical part of Riemann's habilitation thesis [Riem], written

[49] [JacW, p. 436]
[50] [K–L, end of ch. 1]
[51] [K–L, end of ch. 1]

in 1854 and published in 1867 after his death, is given by Kahane[52] in both German and French (translation by L. Laugel [R–L], 1873); from the first page, Riemann states the following:

> The trigonometric series, as named by Fourier . . . have a pivotal role in the part of Mathematics dealing with arbitrary functions.

Later, after having recalled d'Alembert, Euler, Bernoulli and Lagrange:

> Even after almost fifty years, no decisive progress on the problem of the possibility of the analytic representation of arbitrary functions had been done until Fourier's remark, which gave a new viewpoint on this problem. This has marked the coming of a new era for this part of Mathematics, which soon came to light in a brilliant way via the great developments of the Mathematical Physics. Fourier noted that, in the trigonometric series . . . the coefficients are given by the formulas
>
> $$a_n = \frac{1}{\pi}\int_{-\pi}^{\pi} f(x)\sin nx\,dx, \qquad b_n = \frac{1}{\pi}\int_{-\pi}^{\pi} f(x)\cos nx\,dx.$$
>
> [*Riemann writes* $a_n \sin nx + b_n \cos nx$, *in contrast with what is done nowadays*] He saw that these equations can also be used when the function $f(x)$ is arbitrary.

Riemann then refutes Poisson's viewpoint, who, each time he cited these formulas (Riemann takes, as an example, *Traité de mécanique* from 1833, art. 323, p. 638), referred to a publication by Lagrange in *Miscellanea Taurinensia* (t. III, 1762–1765). In this long manuscript, Lagrange solves a certain number of equations and differential systems and comes back in the art. 38 on his solution to the problem of vibrating strings (wave equations), where his reasoning is based on N identical masses situated at equidistant points of the string, letting N going to infinity afterwards. The formula cited by Poisson appears in the article 41. Lagrange rises there a question of interpolation on the interval $[0, 1]$ by a trigonometric polynomial that is a sum of sine functions.

Given a "curve" $Y(x)$ such that $Y(0) = Y(1) = 0$, Lagrange looks for another curve $y(x) = \alpha\sin(x\pi) + \beta\sin(2x\pi) + \cdots + \omega\sin(nx\pi)$, for large

[52] [K–L, 5.9]

but fixed n, which equals the initial curve Y at the points $x_k = k/(n+1)$, $k = 1, \ldots, n$. He writes his solution (up to some change of notation) as

$$y(x) = \sum_{j=1}^{n} 2Z_j \sin(jx\pi), \quad \text{where } Z_j = \frac{1}{n+1} \sum_{k=1}^{n} Y(x_k) \sin(jx_k\pi), \quad j = 1, \ldots, n;$$

Lagrange's reasonings in the previous pages yield the "inverse" equation $y(x_k) = Y(x_k)$, for any $k = 1, \ldots, n$. One recognizes the direct and inverse transformations of "Fourier," on the group $\mathbb{Z}/(2n+2)\mathbb{Z}$, restricted to "odd" functions (one could extend the function Y as an odd function on $[-1, 1]$). Then, Lagrange *decides to set* $n + 1 = 1/(dX)$ and $x_k = k/(n+1) = X$. He thus rewrites the equation for Z_j as an "integral from $X = 0$ to $X = 1$"; doing this replacement in $y(x)$, he gets a kind of *Fourier integral equation* (for odd functions, and restricted to a finite degree n), which, in modern notation, reads as

$$y(x) = 2 \sum_{j=1}^{n} \Big(\int_0^1 Y(X) \sin(jX\pi)\, dX \Big) \sin(x\pi). \tag{6.1}$$

Lagrange emphasizes that he has found in this way a function $y(x)$ which *equals* $Y(x)$ at the points $x_k = k/(n+1)$, $k = 1, \ldots, n$ (and also $k = 0, n+1$).

There is still one issue: To agree with Poisson's viewpoint against the precedence of Fourier, one has to read a true *integral*. However, in order to succeed in the above interpolation, Lagrange *must* keep a finite sum. To Poisson's expected bias with respect to Fourier, even after Fourier's death, Riemann replies with a little lack of sincerity, by refusing to acknowledge at least, in these "Riemann sums," the partition mesh of which tends to 0, the beginnings of Fourier's integral equations! He writes:

> This formula has the same form as Fourier's series, in a such way that, at first glance, a confusion can easily be possible; but, this perception only results from the fact that Lagrange used the symbol $\int dX$, where he would have used today the notation $\sum \Delta X$. ...If Lagrange would have taken the limit with n going to infinity in this equation, he would have arrived at Fourier's result ....

Although introduced by Euler in 1755, the notation $\sum$ for finite sums (without bounds, like for the integral at that time) was not common before 1800; Lagrange needed a notation in order to write on only two lines the double sum in the formula (6.1) for $y(x)$, the sum in $X = x_k$ (thus expressed in terms of integrals from 0 to 1), and the one in j which is written as $s_1 + \cdots + s_n$; to this end, he would have used the notation $\int$. Riemann adds that Lagrange *did not believe* in the possibility of representing arbitrary functions by trigonometric series and therefore, he did not arrive at a derivation of Fourier's formulas: "Of course, nowadays, it seems to be scarcely conceivable that Lagrange did not obtain Fourier's series from his sum formula." He goes on:

> It is Fourier who has first understood in a complete and exact way the nature of trigonometric series.

He then proceeds with the first general proofs of Fourier's theorem, i.e., with Dirichlet's article [DirC].

7 Mathematical Physics or Pure Mathematics?

It is beyond my expertise to comment on the obvious seminal character, affirmed in the title of Dhombres and Robert's book [D–R], of Fourier's work with respect to mathematical physics. It is clear that Fourier wanted to develop the understanding of the world and derive equations for an extraordinarily important natural phenomenon, as Newton did for the gravitational attraction. His ambitions are high, and the mathematical-physics viewpoint is already affirmed in the first lines of the preliminary discussion (*Discours préliminaire*) of the *Théorie analytique* [Fo-C][53]:

> Like gravity, the heat penetrates all substances of the universe . . . The aim of our manuscript is to state the mathematical laws of such a phenomenon. This theory will be one of the most important field of general physics.

and in the middle of the preliminary discussion:

> The thorough study of nature is the most fertile source of mathematical discoveries.

[53] cited in [D–R, Annexe V, p. 717] and [K–L, 2.5]

Fourier stresses at the beginning of the *Discours* that he himself had taken numerous measurements in support of his theory, with the most precise instruments. It was not his intention to take into account the particular aspects that can characterize heat; he avoided having to distinguish between the different forms of propagation – by contact, diffusion or radiation. Biot shared this point of view in 1804 [BioM]:

> I will not examine here whether heat is a body or if it is nothing but the result of the internal motion of material's particles, but rather, assuming that its effects are measurable by the thermometer, once they become noticeable, I will seek the laws of its propagation.

In his essay of 1807, and more definitely since the award-winning essay of 1812,[54] [55] Fourier based his approach on the notion of *heat flux*, which may seem natural today but is in fact his invention. Let there be a point P inside a homogeneous solid, a time t and a direction given by a unitary vector u. Consider an infinitesimal circle $d\sigma$ with centre P and contained in an affine plane that is orthogonal to u. Let dS be the area of $d\sigma$ and dq the quantity of heat that crosses $d\sigma$, in the direction of u and in a duration dt after the date t. The heat flux at the point P, at the time t and in the direction u is the limit ϕ_u of the quotient $dq/(dS\,dt)$. In modern terms, Fourier's fundamental law indicates that this flux is expressed as a scalar product $\phi_u = -\kappa\nabla v \cdot u$, where v is the temperature and where the coefficient $\kappa > 0$ depends on the solid. He put it in other words[56] in the articles 96 and 97 of the section *Mesure du mouvement de la chaleur en un point donné d'une masse solide* (Measuring the movement of heat at a given point of a solid mass). In fact, there are no vectors in Fourier's text, only rectangular coordinates. The flux in the general case is determined by three values, the fluxes in the directions of increasing x, y and z. In his book, he gets there very progressively, starting with uniform movements of heat, and at first even uniform in the direction of a coordinate axis (ch. I, sec. 4 and sec. 7). Fourier returns to the flux in art. 140, before deducing from it the heat equation in art. 142.

Of course, Fourier did not write the heat equation without including the characteristic physical constants of the given body. So, for the equation that

[54] [Heri, ch. 9]
[55] [Fo-P, art. 18 and next]
[56] [Fo-C, ch. I, sec. VIII, p. 89]

governs the temperature v inside a solid, he writes

$$\frac{\partial v}{\partial t} = \frac{K}{CD}\Big(\frac{\partial^2 v}{\partial x^2} + \frac{\partial^2 v}{\partial y^2} + \frac{\partial^2 v}{\partial z^2}\Big), \tag{7.1}$$

where D is the density, K the inner conductivity and C the specific heat. Furthermore, he was among the first to pay attention to the *dimension equations* involving positive or negative powers of physical dimensions, the length, the time and, for him, the temperature[57]. Today we would have the mass instead of the temperature, expressing heat by a mechanical equivalent.

Fourier defines the boundary condition for his partial differential equation (7.1): the equation at the border of the solid is, in modern notation,

$$\nabla v \cdot n = -\frac{h}{K}(v - v_e), \tag{7.2}$$

where n is the outgoing normal vector, h the exterior conductivity and v_e the temperature outside of the solid[58] (Fourier supposed $v_e = 0$).

Dhombres and Robert[59] point out that still at the present time, the teaching of heat propagation follows Fourier's approach. They remark:

> ...the practically unchanged manner in which we formulate, present, and demonstrate today the fundamental results that Fourier articulated ...,

stating that in the major manuals of physics from the middle of the 20$^{\text{th}}$ century (Georges Bruhat, Richard Feynman and others) the calculations given for a metal plate or a ring, for example, are essentially similar to Fourier's. They complement that today, we do not demonstrate the law of heat diffusion in solids any more, partly because we do not know how to do it from the first principles of atomic physics, while Fourier's reasoning seems not to be atomistic enough nowadays.

After the consecration, Fourier published between 1817 and 1825 his "contributions à l'étude de la chaleur rayonnante" (contributions to the study of radiating heat), the phenomenon of radiation by which heat (or cold) can propagate over a distance, without any contact. But this subject had to wait for certain

[57] [D–R, ch. VIII, p. 515–518]
[58] [Fo-C, art. 146 p. 138 and art. 147]
[59] [D–R, ch. IX, p. 626]

progress in physics, to take place at the end of the 19th century (Stefan's law in 1860, rediscovered by Boltzmann in 1879), before more complete answers could be obtained. In 1824, Sadi Carnot, son of Lazare Carnot, published his *Réflexions sur la puissance motrice du feu* (Reflections on the driving power of fire), but Fourier did not get familiar with this research – and he was not the only one in the 1825–1830 years. Carnot's publication, however, contributed to the birth of thermodynamics.

Kahane has written several articles on Fourier. He mentions the opposed viewpoints of Fourier and certain "pure" mathematicians. He cites[60] a famous extract from a letter that Jacobi wrote to Legendre, sent on 2 July 1830, little after Fourier's death in mid-May 1830. Jacobi addressed Legendre in French, excusing himself here and there for the possible incorrectness of his language use. Jacobi's letters have been transcribed by Joseph Bertrand [JacL]; we have to trust Bertrand and his editor for the exactitude of the transcription: the letters have burned during the Paris Commune in 1871, as did Bertrand's house in the Rue de Rivoli.

Jacobi writes[61]: "I was delighted to read Mr. Poisson's report on my work, and I think I can be very pleased with it; he seems to have presented [*my work*] very well. But Mr. Poisson should not have reproduced in his report the not very suitable statement of the deceased Mr. Fourier, reproaching Abel and me for not having paid prime attention to the movement of heat." He added:

> It is true that Mr. Fourier was of the opinion that the main aim of mathematics was its public utility and the explanation of natural phenomena; but a philosopher like him should have known that the sole purpose of science is the honor of the human mind, and that in this regard, a question about numbers is as worthy as a question about the system of the world.

Jacobi continued by expressing his regret at Fourier's death: "Such men are rare today, even in France, they cannot be replaced that easily." He closed by asking Legendre to give his "regards to Miss Sophie Germain whose acquaintance I had the good fortune to make, and let me know about her condition." Sophie Germain suffered from a "long disease," she died the following year.

60 [K–L, 4.6]
61 [JacW, vol. 1, p. 454]

Four years earlier, Abel had written to Holmboe (24 October 1826) that he regretted Fourier's and other French mathematicians' commitment to applied sciences:

> [Cauchy] is by the way the only one to work on pure mathematics at present. Poisson, Fourier, Ampère etc. focus on nothing else but magnetism and other subjects of physics.

Poisson, Fourier, André-Marie Ampère: three professors at the École Polytechnique. We could ask ourselves if the scientific preeminence of the *École* in France during the first half of the 19$^{\text{th}}$ century, with its mission to educate mainly engineers, could be one of the reasons for the decline of French mathematics in the middle of the same century, when it was surpassed by the German university. Joseph Ben-David [B-Da] rather incriminated the teaching practice at the *École*, which did not keep up with the progress of science and forgot one of the institutional missions fixed by the founding fathers – the second term of the grandiose maxim of 1804: "Pour la Patrie, les Sciences et la Gloire" (For Country, the Sciences, and Glory).

Fourier's mathematical fame suffered an eclipse in France in the second half of the 19$^{\text{th}}$ century, but Harmonic analysis, Fourier series and the Fourier transform have found their place in the "very pure" French mathematics of the 20$^{\text{th}}$ century. Kahane has contributed to this by his articles and books dealing with most specialized subjects regarding *thin sets*, that result from an exclusively mathematical study of Fourier series. A little paradoxically, the same Kahane turned himself into a defender of Fourier's mathematical physics. Regarding the temporary "eclipse," he observes, in his article [KahQ] of 2014, a forceful return of Fourier's standpoints on the occasion of a mathematics-physics convergence in our days:

> This underestimation of Fourier does now belong to the past. It could only maintain itself in France thanks to a divorce between mathematics and physics, which is today completely overcome. One of the biggest French universities, in Grenoble of course, carries the name of Joseph Fourier.

We conclude with Kahane, who writes in the same text:

> When I was young, and it is still the same among the young people, "the honor of the human mind" sounded more glorious than "the thorough study of nature." However, Fourier's philosophy seems to me to be closer than ever to the actual evolution of mathematics and their – sometimes termed "unreasonable" – impact on the natural sciences.

Chronology

Joseph-Louis Lagrange	1736–1813	Gaspard Monge	1746–1818
Pierre-Simon Laplace	1749–1827	Adrien-Marie Legendre	1752–1833
Lazare Carnot	1753–1823	François Budan de Boislaurent	1761–1840
Sylvestre-François Lacroix	1765–1843	Joseph Fourier	1768–1830
Napoléon Bonaparte	1769–1821	Jean-Baptiste Biot	1774–1862
Marie-Sophie Germain	1776–1831	Jacques-Joseph Champollion-Figeac	1778–1867
Siméon Denis Poisson	1781–1840	Henri Beyle (Stendhal)	1783–1842
Friedrich Wilhelm Bessel	1784–1846	Claude Louis Marie Henri Navier	1785–1836
Augustin Louis Cauchy	1789–1857	Jean-François Champollion	1790–1832
Nicolas Sadi Carnot	1796–1832	Niels Henrik Abel	1802–1829
Jacques Charles Sturm	1803–1855	Carl Gustav Jacobi	1804–1851
Gustav Lejeune-Dirichlet	1805–1859	Joseph Liouville	1809–1882
Évariste Galois	1811–1832	Bernhard Riemann	1826–1866

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Most of the "historical" references below, like Fourier's *Théorie analytique de la chaleur* or the *Mémorial* compiling Abel's letters, are nowadays easy to access, thanks to websites like EuDML, Gallica, archive.org and many others.

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Nonacademic careers

In 2019 the *News Bulletin* shared the experiences of some people in our community who had pursued non-academic careers, as well as resources such as The BIG Math Network (https://bigmathnetwork.org/). To balance those mostly mathematical perspectives, in this issue we hear from the physics community, with a first-person account and an article pointing to career resources from the APS and IOP.

A whole new world

by Erika Harnett (Amazon Web Services)

When I was growing up my two loves were space science and math. I used to carry around *National Geographic* books about astronomy or results from the Voyager missions, and look through them over and over. For summer breaks my mother would buy as many math workbooks at Kmart as we could afford, for me to work through. She would have to parcel them out, however, so I wouldn't work through all of them in the first two weeks of summer break. One of the best summers, between 6th and 7th grade, was when she found a high school geometry book for me, and I spent the whole summer teaching myself geometry.

This might make it seem like my path to academia was a natural one, but being a first-generation college student who grew up in a very blue-collar neighborhood, I actually kind of bumbled my way into it. I am very grateful for accidentally overhearing classmates' conversation in one of my undergraduate physics classes, where I learned that I could actually get paid to go to graduate school in a STEM field, and not have to end up horrendously in debt. I am appreciative that I ended up working on a problem for my PhD that some initially wrote off as a dead end. It ended up being a really emergent field and I became a world expert in it, as a second-year grad student, partially because I was one of the first ones willing to investigate it.

I managed to find a post-PhD career path that suited my family requirements. It was not the elusive tenure-track path, but it worked for me. I had the opportunity to work on many interesting research projects, and work with lots of passionate scientists along the way. After about 8 years as both a Research Assistant Professor and a Research Associate Professor, doing research in space physics and planetary science, however, I realized it was time for me to move on from academia. Many factors contributed to this decision.

I went into being research (soft money) faculty, with the full intention for it to not be my lifetime career, but rather for it to be a stepping stone to something else. When I began on the research faculty track, I fully expected that the next step would be a mid-level administrative position (e.g an Associate Dean of Research or Diversity for a college within a University; an Associate Director for a STEM-related campus program). I had seen others, people I considered mentors, follow that path, and I considered it one that I would like to follow. My struggles as a first-generation college student meant that I wanted to be able to be in a position with the power to help others avoid the difficulties I encountered.

Unfortunately, the landscape began to completely change from underneath me as I started the process. I began my faculty career right before the 2008 Great Recession began. This had impacts that are still being felt today [1, 2, 3, 4]. The first and most immediate one was the dramatic decrease in funding for state-funded higher education institutions, in the US. In many places, most of that funding has still not returned to pre-2008 recession levels. That translated into a significant decline in the number of tenure track positions being supported by departments, and, important for me, a near collapse in the pathway into administrative positions for non-tenured faculty. When tenured faculty transition into an administrative position, it primarily involves a shell game of moving pots of money to pay their salary, while soft-money faculty moving into administrative positions require an injection of new, and permanent, funds to pay their salary. Those new funds no longer exist.

Sequestration, which was enacted in late 2012, and began taking effect in 2013, only made matters worse. The initial 8% across the board budget cuts where heavily concentrated in the grant funding portions of the budgets of grant-funding institutions, like NASA and NSF. In my field of research, prior to sequestration, approximately 30-35% of submitted proposals were funded. After sequestration, that percentage dropped to 5%. Five years after sequestration took effect, that percentage had only climbed back up to approximately 10%. I was able to supplement my salary by teaching, but the ripple effects had a major impact on my desire to stay in the field. The scarce resources meant that too many people began seeing any success by others as a threat to themselves. A lot of the collegial environment had disappeared. E.g., research has shown [5, 6] that even a small percentage of "selfish" paper referees (ones that react negatively when they perceive the work they are reviewing to be a threat to their own) can dramatically impact the number of papers accepted for publication.

I did look at academic options beyond my current institutions at the time, and applied for many, but I began seeing that in order to successfully switch to those institutions, I would have needed to take so many steps backwards in my career, the penalty was not worth the eventual pay-off. Just as important, I started realizing, through my own experiences and hearing the stories of others, that the much-claimed valuing of diversity and zero tolerance of harassment in academia is rarely, if ever, acted upon in a substantive way. All of this caused my enthusiasm for staying in academia and basic research to disappear.

Once I realized that the academic administrative track no long had the appeal it once had, I decided to look at the private sector instead. I took stock of my skills and knowledge base, and looked to how I might transfer those skills into the private sector. I am a space physicist and planetary scientist by training – not a lot of call for those in the private sector. But to do my space physics research, I wrote complex computer models to run simulations that numerically solved coupled differential equations. These simulations produced large amounts of complex data that then needed to be compared with satellite data, coming from multiple data sources, in multiple formats. I then needed to figure out how to visualize and communicate the results from my research to a broad type of scientists, with diverse backgrounds. These are all skills required for working in the data science/research science realm in the private sector.

I feel it is important to note, at this point, that I do not have a computer-science background. I took exactly one computer programming class, as an undergraduate. Nearly all of my computer programming skills are self-taught, with a lot of help from my math degree for understanding and optimizing logic. The self-taught nature of my computer programming skills is something that is common among most of the scientist, and even some of the engineers, I work with in the private sector.

It was a 2-year process from when I started looking to transition away from my academic position, to accepting the offer for my current position. There were multiple paths I started down, only to realize they were dead ends, and a lot of learning I had to do, regarding what it takes to be considered for private-sector jobs. Most academics do not have a solid knowledge base in this [1, 2], and that is really something that needs to change if they want to make their degrees attractive to current and future students. Those students understand the current employment landscape, and are very savvy when it comes to considering the long-term career options.

These are some of the key things I learned along the way:

What I did that helped:

1) Learning that vocabulary can be different.

Like most people with PhDs, I am primarily self-taught when it comes to many of the skills I used to do the research that earned me my PhD, and I use for my post-PhD research career. One of the key skills for being successful in a PhD program is being able to go out and learn what you need in order to do your research. It is important to note that this is a key skill for being successful in the private sector as well.

Being self-taught, I began to wonder if part of the reason I was not initially getting interviews for private sector positions was because I had noticeable (to employers) gaps in my knowledge. To try to address this, I began taking online data science and big-data courses. It didn't take me long to realize that the problem wasn't my knowledge and skills, it was how I communicated that knowledge and those skills. The vocabulary I used, and others around me in academia used, to communicate about those skills and techniques was completely different from the vocabulary used to communicate them in the private sector. While I had all the skills and know-how to be a research or data scientist, I didn't sound like one to the people reviewing my resume. One broad example, most academics think of 'deliverables' as peer reviewed papers accepted for publication. In the private sector, 'deliverables' mean codes developed and shared for others to use, or major conclusions that others can then use to make decisions from (or use as a key component in their own research).

2) Having my resume professionally rewritten.

This was something I did on a whim, and it turned out to be one of the most important things I did. I knew academic CVs are different from a private sector resume, but I didn't understand just how much so until I saw the difference between my own CV and the rewritten resume. I had most of the important information in the resume I was originally sending out, but both the presentation and (again!) terminology were all wrong. In academia, academic pedigree, publication record, and funding record tend to be the most highly valued. In the private sector, it is about deliverables and responsibility progression.

First and foremost, my educational preparation went from being the first thing listed on my CV to the last thing listed on my resume. Second, my list of publications completely disappeared from my resume. Instead the focus became on my skills developed and used, and the impact of my research for my research group and the people in my research community. How did others, particularly those outside my immediate research group, use the conclusions from my research? Did anything I do in the academic research setting make a research/lab process more efficient for others? Did I work to eliminate/streamline a costly (either in terms of actual dollars or people-effort) process, developing a replacement that was less costly? And yes, did I successfully acquire grant funding? The actual dollar amount was less important, rather the success of acquiring and managing the project(s) is what was highlighted.

Demonstrating responsibility progression can be tricky for those coming from an academic setting, because official job titles change infrequently, even when a person's actual responsibilities are increasing. It is OK to give yourself unofficial job titles that accurately demonstrate what your real responsibilities were/are. As an example, for a newly minted PhD, you may have had only one official job title while working on your PhD (e.g. 'Research Assistant') but during that time you may have gone from research assistant only learning the ropes, to project leader (you were advising an undergrads and/or master's student on the daily aspects of their projects), to lab manager. In my case, I used my official job titles, but I highlighted when I took on key new responsibilities, within those job titles. Also, make sure you succinctly describe what the job title means. Many official job titles in academia have no meaning to those outside of academia, and even have different names within academia.

What I wish I had known to do:

1) Practice white-boarding.

Those of us who have received some sort of advanced degree are likely to have experienced an oral exam, be it master's defense, PhD general exam, or

PhD defense. The interview process in academia for those with PhDs tends to focus entirely on the expertise of the candidate in their field. They almost exclusively ignore questions about foundational topics. It is assumed that a person's knowledge of foundational topics is sound, as they would not have been able to earn a PhD, if they were not. While that is true, it means it is easy to get rusty on being able to discuss those topics.

The skills that enable you to be successful in the oral exam setting are also important for the inevitable white-boarding session of an initial phone screen and/or in-person interview for a tech job. During a white boarding session, the interviewer(s) will ask you to code up some logic to solve a particular type of problem, explain the pros and cons, mathematically, of multiple analysis algorithms, etc. Like all skills, this is something one needs to practice. I forgot about that, which was doubly important for me as I tend to suffer from test anxiety. I was also ignorant of how prevalent and important the white-boarding session is for jobs at all levels. I would have learned this if I had asked more people I know in tech about the general interview process.

My first tech phone screen started off well, and then crashed and burned once we switched to the virtual white-boarding section. I got so focused on trying to remember the exact python syntax for one command call that I spiraled into a complete mental lock-up. I will forever appreciate the patience my interviewer showed in not ending the interview right there. Instead, she began giving me alternative questions to answer. Nonetheless, I barely limped along, and finished the interview feeling very dejected. I didn't let myself stay there. After a few days of licking my wounds and lamenting my terrible performance to very sympathetic friends, I started practicing. I looked up databases with common white-boarding questions, and I practiced those (just like I was prepping for my qualifying exam). I realized that with each interview, I would get better.

2) Reach out to people in the industry/in the type of position you would like to pivot into as soon as you think you want to make a career change.

I started much too late, looking for people to in industry to connect with, but once I did, their insight proved invaluable. I learned what types of roles I was the most qualified for and had better insight into what skills the different job types really require. I also learned which companies have a track record for hiring people from academia and which do not.

You will likely have to move well out of your comfort zone in order to do this. One of the most informative interactions I had required me to attend a networking session in a completely different department (where I knew no one), and where I was the only non-PhD student in attendance, other than the featured speaker. It would have been easy for me to feel self-conscious and embarrassed about being a mid-career faculty member amongst all the students, but I didn't care. I was focused on my goal of learning as much as possible from the speaker. I only worried about hogging the conversation and making sure I let others have a chance to ask questions, too.

What I have learned since making the transition to private sector:

1) Scientists can have a large amount of autonomy in directing their research. One of my biggest worries about switching to working in the private sector was my perception that I would be told I must work on specific projects and have no say on what I worked on. I have been very pleasantly surprised that is not the case. Yes, my team (which is made up of other scientists) has a prioritized roadmap of projects and we need to work on the highest-priority projects first, but we are all very much empowered to pick the high-priority projects we are the most interested in. We are also encouraged to look for other high-priority projects, not already identified, by speaking with our colleagues on other teams. Yes, we have to make a business case for working on the project, but this is no more onerous than a grant writer having to demonstrate their proposed research is relevant to a grant agency's funding roadmap.

2) The science culture can have a lot of similarities, and some important differences, with academic science.

One thing that I have come to appreciate very much about working at a larger company is the breadth and diversity of the other scientist that I have the opportunity to work with. I have more opportunities than I can take advantage of to attend seminars and learn about the cutting-edge research my colleagues are doing and others outside of my company are doing. I also have opportunities on a daily basis to learn from my team-mates, and for them to learn from me.

I have also found that siloing is much less likely to occur at my current organization than when I was in academia. I regularly have meaningful and engaging interactions with engineers on sister teams, which I never seemed able to have in academia. Some of that may be because I now have the time to.

I am not spending so much of my time writing grant proposals that have little chance of being funded, freeing me to actually do science research. Most of it, however, is due to the expectation that I engage all stakeholders in the projects I work on, and everyone understands that in order to solve the problems, they have to participate in the solution.

Summary.

Making the leap from academia to private sector was definitely a very scary jump (I really had no idea if I would like working in the private sector), but it has turned into one of the best decisions I have ever made. I went from seeing my career options rapidly narrowing, to being on a path in which my career options are vast and exciting. I now have more, incredibly interesting options than I could ever take advantage of. My enthusiasm and excitement for the research I am doing has returned. I am learning so much from my incredibly bright, and equally enthusiastic teammates.

I have had people ask me, since making the jump, do I miss doing planetary science research and the answer is No. Not because I now hate the topic. I still enjoy it, I just enjoy it differently. I still read about current results from satellite missions, but now it is with the freedom of knowing I don't have to figure out a way to propose some research project regarding the results. I still discuss the topics, but now it is primarily with the general public, as a (volunteer) Science Fellow at my local science center. And now I am getting to work on interesting, cutting-edge problems that engage my love of math. A love that remained from when I enthusiastically worked through math workbooks on my summer breaks, but just took a bit of a backseat for a while.

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A new guide for job seekers

by LEAH POFFENBERGER (American Institute of Physics)

For some early-career physicists, the way forward after receiving a degree is clear – heading towards academia or a field that grabbed their interest in school. But others may find themselves wondering what to do with a physics degree or where to go next in their careers.

APS and the Institute of Physics (IOP) in the UK have teamed up to alert early-career physicists to the many possibilities in the field with a new publication: the *APS Careers 2020*. Modeled after *Physics World Careers* published by the IOP, which focused on career information for UK and EU physicists, the APS guide provides invaluable information for job seekers in the United States. The guide is available online.

Crystal Bailey, Head of Career Programs at APS, has been working closely with Tushna Commissariat and Edward Jost, both of IOP, to launch the new publication and provide a valuable resource to APS members.

"*APS Careers 2020* is targeted to all members, with a special focus on early career physicists," says Bailey, who managed the project from the APS side. "We made an effort to make sure this guide has something beneficial to members at all levels of their careers."

And serving members, especially those just starting out, is a key component of the recently implemented APS Strategic Plan, which states that "... to attract and retain members and to fully serve the current and next generation of physicists, we will grow and broaden the APS membership to include more physicists in industry and the private sector and provide additional tangible member benefits."

The guidebook contains a wealth of information focused on how someone with a physics degree can get a job outside of academia–and what some of those jobs are. More standard career-hunting advice, such as how to tailor a résumé for industrial job applications, is coupled with profiles of physicists with unconventional jobs. The guide also features a directory of companies, both industry and national labs, that are looking to hire people with physics degrees.

"Even though most physics graduates will pursue career paths outside of academia, many physics students will have only been exposed to academic research careers by the time they graduate," Bailey noted.

"A physics degree is really a platform to take on genuinely almost any job anywhere in the world, and we want to make sure they know that so they don't ever feel limited," said Commissariat, reviews and careers editor for *Physics World*, IOP's membership magazine.

For the past three years, IOP has been producing their own guide as an expansion of the careers information that was originally published in the member magazine.

"It was a lot of timeless stuff that we wanted people to see more regularly, so we put together the *Physics World Careers* guide with some of our best content coupled up with some profiles," says Jost, Head of Media Business Development at IOP. "It's gone down really well with our membership here at the Institute–one member claimed it was one of the best things he'd had as a member benefit in years of being a member at the institute."

In October 2018, APS was approached by IOP with a proposal to partner in creating a version of the *Physics World Careers* guide that was appropriate for US audiences. Together, APS and IOP produced a collection of over 20 pieces of content for the APS Careers 2020, with the needs of APS members in mind.

Bailey worked closely with the IOP team to ensure that all parts of the guide came together in time for launch in October 2019. The APS communications group ensured that the publication is consistent with the APS brand and have created several promotional ads for APS programs to be included. Bailey also curated a number of professional development articles and profiles from a library of existing content to be included in the guide, as well as provided information to create new articles focusing on APS programs (such as the PIPELINE program, the IMPact program, and others).

As part of these contributions, APS has also helped connect the IOP sales team with US companies and national labs that could be featured in the employer directory, which was the most difficult aspect they encountered in expanding into US markets.

The directory, for example, includes the same type of information from each company that participated to ensure an easy comparison for job seekers. An important addition to this section, which keeps the international membership of APS in mind, is a question on whether companies require their employees to have a US visa. Articles are also included in the guide profiling some of the careers programs offered through APS that provide additional support for early-career physicists.

In addition, a popular section from the *Physics World* guide called "Once a Physicist" will be reprised in the APS guide, profiling people who have physics degrees who have used them to do something completely different.

"We've selected articles that feature careers in industry, high school physics teaching, and entrepreneurship; there is also a multi-page feature on careers in the field of medical physics," Bailey said. "*APS Careers 2020* is a tangible benefit that we are glad to be making available to our members and the broader physics community."

For more on career resources at APS, visit aps.org/careers.

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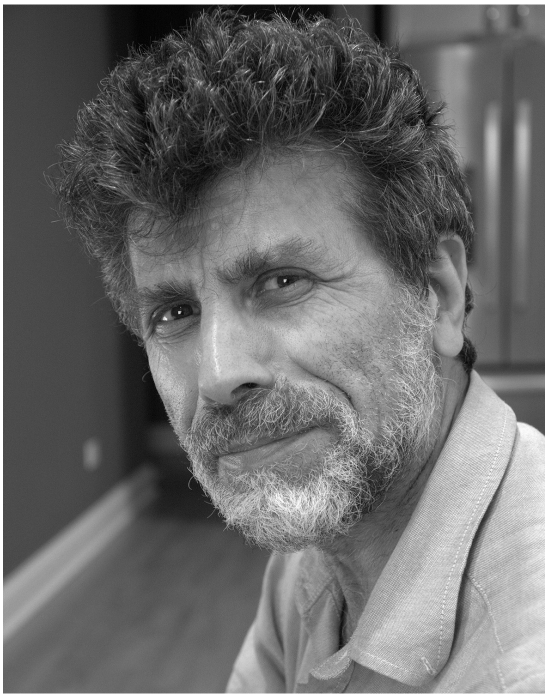
Alexander Gordon

(May 10, 1947 – May 13, 2019)



Alexander (Sasha) Gordon, died in Chicago on May 13, 2019, after a long illness. Sasha was a brilliant mathematician, author of a number of beautiful and original results in diverse fields of spectral theory and other areas of analysis. His insights were behind some of the foundations of almost periodic operators as well as the theme of genericity of singular continuous spectrum developed by B. Simon and collaborators. Born in Kharkov, Ukraine, on May 10, 1947, Sasha has had an unusual path in mathematics. At the time of his death, he was an associate professor of mathematics at UNCC. His life story can be viewed as a triumph of human and mathematical spirit over the circumstances.

Sasha is best known for what is called *Gordon's lemma,* a statement about absence of decaying solutions of 1D equations close to periodic. It has led to the first example of an almost periodic operator with singular-continuous spectrum, to the understanding of importance of arithmetics in this area, to ultimately sharp arithmetic spectral transitions (e.g. [1, 17, 18]) as well as powerful consequences in the field of 1d quasicrystals (e.g. [4]).

Before Sasha's work it was not even known whether a condition on the frequency, imposed in various KAM arguments to prove point spectrum, is necessary (it was sometime before the singular continuous revolution of the 90s which allowed for an easy soft argument). His proof is based on a beautiful in its simplicity lemma on $\mathrm{SL}(2,\mathbb{R})$ matrices, formulated and proved in his two-page "Uspehi"[62] paper [5]. Later E. A. Gorin (1935–2018) [3] found a sleeker proof that easily generalized to a $\mathrm{GL}(n,\mathbb{R})$ version, using the Cayley–Hamilton theorem, a result that remained unpublished. Gordon's paper appeared, in Russian, in 1975, and was not known in the west until B. Simon's Moscow visit in 1981, when S. Molchanov communicated to him, on the blackboard, Sasha's lemma with Gorin's proof. Avron and Simon at the time were trying to implement Sarnak's suggestion that spectral properties of quasiperiodic operators might depend on arithmetic properties of the frequencies, and this lemma then led to their first example of an operator with singular-continuous spectrum: the super-critical almost Mathieu operator with Liouville frequency [2]. This lemma was prominently featured in many of Simon's articles and textbooks under the name "Gordon's lemma," which Sasha made several futile attempts to change to Gordon–Gorin's, see, for instance, Footnote 2 in [12]. With the 1D result so natural and so simple, Sasha went on a life-long quest to find a multi-dimensional version. There have been attempts by various authors, not leading to anything significant. Sasha finally succeeded recently, in a highly original joint work with his friend from the undergraduate days A. Nemirovsky [15]. They provided the first quantitative condition for absence of point spectrum of multidimensional quasiperiodic operators, a result he was still not fully satisfied with and thriving to make stronger till his last days.

Another fundamental work of comparable importance is Gordon's theorem that 1D Schrödinger operators with interval spectrum have no point spectrum for generic boundary values [10, 11]. This indirectly influenced B. Simon's discovery of the Wonderland theorem [21] and other results on generic nature of singular continuous spectrum, a big theme in the 90s.

[62] The top Soviet journal at the time, publishing mainly announcements with a two-page limit.

A certain prelude to that was a remarkable paper [9] where Gordon constructed an explicit potential with Green's function decay outside a set of energies of measure zero (leading to localization for a.e. boundary value by spectral averaging). It motivated powerful results by Kirsch, Molchanov, Pastur and others, as well as the work described above because the example, being so explicit, led to a natural question whether the "a.e." in the boundary values is necessary – something that Sasha answered in a surprising and very general way in [10, 11].

Another fundamental contribution was the proof of measurability of eigenelements of continuous operator families, first in a joint work with Jitomirskaya, Last, and Simon [13] and then in the work with Kechris [14]. The issue is that for ergodic families, say, the entire collection of eigenvalues is not a measurable object, because it is invariant, yet not a.e. constant. Gordon's work showed, for example, that, in a very general setting, there exists a measurable enumeration. This was fundamental for several further important advances, particularly recent results on localization as a corollary of dual reducibility for quasiperiodic operators, and first results on arithmetic localization in the multi-dimensional setting.

Finally, we mention Sasha's proof [6] of the Hartman–Putnam conjecture that the lengths of spectral gaps of 1-D Schrödinger operators with bounded potentials tend to zero at infinity, a long open problem. Hartman and Putnam in their 1950 paper [16] proved this under an additional condition of uniform continuity and asked whether it could be removed.

Sasha produced several gems also outside the spectral theory. The highlights include

- a counterexample to the long-standing conjecture of A. Kolmogorov (1953) about the impossibility of mixed spectrum for an analytic flow with an integral invariant on a 2-D torus [7];
- an effective sufficient condition for the cohomological equation to have no measurable solutions [8].

It should be noted that most of the beautiful results described above were obtained when Sasha was doing math in his spare time. His story is in a way characteristic of time and circumstances. Sasha got his undergraduate degree from Moscow State University in 1970, with high honors. As an undergraduate, he was an active participant of the Banach algebras seminar led by E. Gorin and V. Lin, published two independent papers in top Russian journals, and wrote an excellent MS thesis. Should he have finished in 1968 or earlier, such a performance would have more than guaranteed him a recommendation to continue as a PhD student, requiring only to pass several formal examinations. Things had changed however in 1969, when 39 Jewish students recommended for the PhD by their advisors, got Cs on the "History of the Communist Party" exam and were not allowed in. This marked the beginning of an era of significantly increased antisemitism at the School of Mathematics at MSU and in Soviet math in general, an era that lasted till the end of the 80s and of which Sasha was one of the victims. From 1970 on, Jews were largely not even recommended to be admitted to the graduate admission exams, although the problem soon almost disappeared because they mostly stopped being admitted as undergrads already in 1968.[63] Michael Brin, Svetlana Katok, Yakov Pesin are some other names of mathematicians who finished their undergraduate studies in 1969–70 and were treated similarly. For an excellent account of related issues see [19].

For the next almost 25 years Sasha's work in math was done purely for fun. He took a day job but was able to stay in Moscow which made it possible for him to attend research seminars at MSU. The MSU math department was then at the beginning of the end of its "golden age," and the quality of seminars was still outstanding (see again [19] for more detail). Sasha regularly attended the one on mathematical physics led by S. Molchanov, A. Ruzmaikin, and D. Sokoloff. There, he was exposed to problems in spectral theory that led to his papers.

His advisor was S. Molchanov, whose questions proposed at the seminar indeed stimulated much of Sasha's research in spectral theory, but who views his own role rather as that of a friend and benefactor than an advisor. Sasha's thesis featuring, among other things, Hartman–Putnam and absence of point spectrum, was ready by about 1980, but to get a PhD, it was necessary for the advisor to find a university which would agree to schedule a defense for an outsider. It usually involved also finding a fake advisor, thus providing no benefit to the academic record of the true advisor. Most Jewish PhDs in the 70s–80s were obtained this way. Yet it also took time and financial resources to travel

[63] See, for instance, [20] for an account of the techniques used to achieve this in the society that proclaimed its ideals of equality and internationalism, and for a fascinating story of an underground school, Jewish People's University, created in 1978 so that "Jewish children can learn math."

and be away from the day job, something that Sasha didn't have, making the task especially complicated in his case. Molchanov's multiple attempts to organize the defense at various universities failed. The university in Sasha's native Kharkov that was successfully used for this purpose in the 70s, say for the defense of M. Brin, by the 80s would no longer allow their own Jews, with, for example, M. Lyubich having to defend in Uzbekistan. Things changed around 1987, and in 1988 Molchanov finally succeeded in organizing Sasha's defense at the Moscow Institute of Electronic Machine Building where V. Maslov (of Maslov's index) was a department chair and thought it would be easy, but at the end, it still took Maslov threatening to quit his job to make it happen. Finally, in 1993 Sasha got a job at a research laboratory that would utilize his PhD and pay for him doing mathematics. At about the same time, the Russian government essentially stopped paying not just living wages, but any wages at all, to scientists.

As the Iron Curtain fell, Sasha got emboldened by the realization that his earlier work, popularized by Molchanov and Simon, has found interest and acclaim, and started trying to realize the dream of his youth to become a professional mathematician. He had several short visiting positions in Europe, and when invited by Molchanov to visit UNCC for a year in 1995–96, he decided to try to make it in the US. Yet, despite the fame of Gordon Lemma, for a 48 year old with a heavy accent and a short list of short publications full of 3–5 year gaps, this seemed an almost impossible quest. There was not much he could do about the age or the accent, but, remarkably, in the next ten years he managed to change the publication list around, producing a steady record of publications in computational bio-statistics, other aspects of applied math (true random number generators), and squeezing in a few in his beloved spectral theory. His work in bio-statistics was related to his job as a programmer at the Department of Computational Biology, University of Rochester, 2001–2006. It has been published in some of the leading journals in the field, including *Annals of Applied Statistics* and *SIAM Journal on Applied Mathematics,* so we believe he has brought his remarkable originality there as well. In 2006, Sasha's UNCC friends managed to achieve what seemed to be impossible, and, aged 59, Sasha became a tenure-track assistant professor there. He was happy and grateful for an opportunity to live a normal academic life. He continued working in both spectral theory and computational bio-statistics.

Sasha's papers were usually short and always struck with their simplicity, clarity and mathematical beauty. While his list of early publications is much shorter than it could have been had he not had so many obstacles, it is fair to say that most of his works from that period, those that found lots of resonance as well as those that went almost unnoticed outside Russia, are true mathematical masterpieces.

Outside mathematics, Sasha was a connoisseur of Russian literature, especially poetry. He was active at UNCC's Russian literature club, where he often led the readings. He knew thousands of poems by heart and also wrote beautiful poems himself.

Sasha was battling a grave illness for the last six years, yet he continued working and creating until the very end. He finished grading his students' exams from his hospital bed two days before his death. His last papers, one each in computational bio-statistics and in spectral theory are being published posthumously, the latter one in the present issue [of JST]. He will be greatly missed.

Svetlana Jitomirskaya,
Stanislav A. Molchanov,
Barry Simon,
Boris R. Vainberg

October 10, 2019

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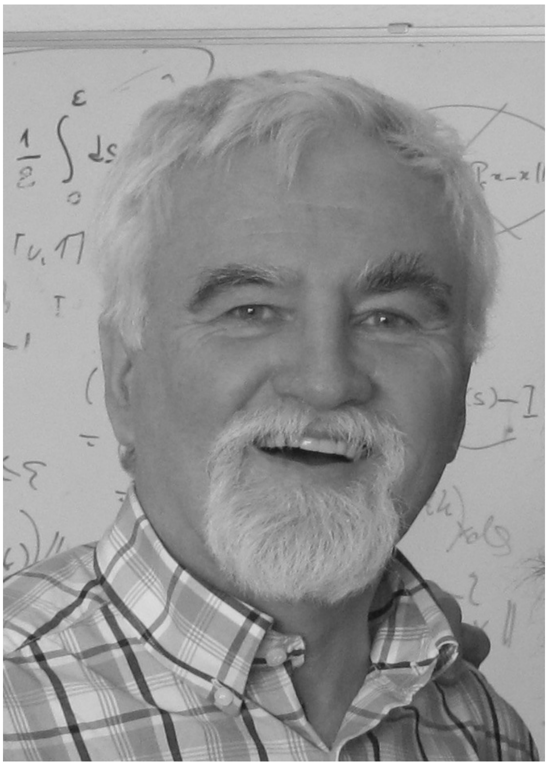
Hagen Neidhardt

(November 20, 1950 – March 23, 2019)



Our friend and co-author, brilliant mathematician Hagen Neidhardt passed away on 23 March 2019 at the age of 68. Author of more than 150 research papers, Hagen was a world-renowned expert in the area of functional analysis, operator theory, and mathematical physics, where he made a number of highly original contributions.

Hagen Ernst Neidhardt was born on 20 November 1950 in the provincial town of Gefell (Thuringia) in the German Democratic Republic (GDR). His father, Hubertus Neidhardt, was director at the Engineering School for Textile Technology, Reichenbach/Vogtland. His mother Ruth Neidhardt (Löffler) was a clerk at Vogtlandstoffe VEB Kombinat wool and silk Meerane.

Hagen was passionate about mathematics from a very young age. On one occasion he came across a mathematics enyclopedia his parents had been planning to give him as a Christmas present that year, and by the time Christmas came around, it was clear to his parents that he had already worked through the entire book. His whole life beat to the rhythm of mathematics.

School, University, Karl Weierstraß Institute

Neidhardt's special talent and inclination towards mathematics were recognized early on by parents and teachers. Hagen attended a local primary school in Gefell from 1957 to 1967, after which he moved on to advanced high schools in Schleiz and Reichenbach (Vogtland).

At only 17 years of age, Hagen left his childhood home for good. In 1967, he moved to the Faculty of Workers and Peasants (ABF) at Martin Luther University Halle-Wittenberg for two years preparation for studying in the Soviet Union. The Institute for Preparation for Studying Abroad in Halle (Saale) prepared delegated students from all over the GDR for studying abroad. Hagen was preparing for his studies in the USSR at the Leningrad State University (LSU).

Six years later, on 28 February 1975, Hagen graduated with distinction from the Faculty of Physics of the LSU and was awarded the diploma in mathematical physics. His tutor and promotor of his diploma thesis on the scattering theory was Professor Mikhail Shlemovich Birman. This background and his time with Professor Birman, who was then the head of the Leningrad Mathematical Physics Seminar, were decisive for the mathematical orientation of the young Hagen Neidhardt. This became the spectral theory of operators and, in particular, the scattering theory, which were the main topics of the seminar at that time, with participation of Ludwig D. Faddeev, Olga A. Ladyzhenskaya and Boris S. Pavlov.

Almost immediately after his return to the GDR, on 4 March, 1975, Hagen took up a junior position in the Karl Weierstraß Institute of Mathematics in Berlin. There he wrote his first research paper in 1976: "Zwei-Raum-Verallgemeinerung des Theorems von Rosenblum und Kato" on spectral analysis of the scattering theory, motivated by one of Birman's publications. This paper appeared in *Mathematische Nachrichten* 84(1978) 195–211 and signified one of the preferred directions of Hagen's scientific interests. He returned to this question in the paper "A nuclear dissipative scattering theory" (*J. Operator Theory* 1985), and then in "A Converse of the Kato-Rosenblum Theorem" in the same journal (1991). The last paper, where he revisited this problem, was published in 2017.

In the meantime, Hagen started to work on his Ph.D. thesis. His supervisor was Professor Hellmut Baumgärtel, a leading expert in the mathematical scattering theory. Although this topic offered enough scope to be worth continuing, Hagen was attracted to another project related to the solution of the non-autonomous Cauchy problem with the help of extension to evolution semigroups. This method was advocated by Howland (1974) and Evans (1976). In "Integration von Evolutionsgleichungen mit Hilfe von Evolutionshalbgruppen" (Dissertation, AdW der DDR, Berlin 1979, defended on 5 April 1979) Hagen generalized this approach to an arbitrary Banach space. The main result, published in "On abstract linear evolution equations. I" (*Mathematische Nachrichten* 1981), proved the one-to-one correspondence between a set of evolution semigroups and strongly continuous solution operators (propagators) for non-autonomous Cauchy problem.

The Howland-Evans-Neidhardt approach is now well known for both parabolic and hyperbolic cases. They were scrutinized by Hagen in two papers, "On abstract linear evolution equations. II" and III, in 1981-82. He returned to the elucidation of the difficult hyperbolic case versus Schrödinger evolution in "Linear non-autonomous Cauchy problems and evolution semigroups", *Adv. Diff. Equations* 2009. Hagen liked the evolution semigroups approach to non-autonomous Cauchy problem and was returning to applications of this method many times, in particular, in the framework of product formula approximations for propagators. One of the very last of his papers on this subject: "Convergence rate estimates for Trotter product approximations of solution operators for non-autonomous Cauchy problems", appeared only recently in *Publ. RIMS Kyoto Univ.* 2020.

Joint Institute for Nuclear Research, Dubna

Hagen Neidhardt, Dubna (1989)

A new chapter in Hagen's scientific evolution opened in the second half of the eighties, when he arrived with his family for a scientific stay (15 September, 1986 - 14 September, 1990) at the Joint Institute for Nuclear Research (JINR) in Dubna, USSR.There he first returned to the "spectral shift" problem, that was studied in Leningrad by L.S.Koplienko, one of Birman's students. Note that this problem can be traced back to M.G. Krein (1953,1962), who introduced the terms "spectral shift function" and "trace formula". Let $\{H, H_0\}$ be a pair of self-adjoint operators on a separable Hilbert space $\mathfrak{H}$ which differ by a nuclear operator. M. G. Krein proved the existence of a summable real function $\xi(\cdot)$ defined on $\mathbb{R}^1$ such that for a certain class of functions $\psi(\cdot)$ the relation

$$\mathrm{Tr}(\psi(H) - \psi(H_0)) = \int_{\mathbb{R}^1} d\lambda\, \xi(\lambda)\, \partial_\lambda \psi(\lambda)$$

holds. The function $\xi(\cdot)$ is called the *spectral shift function* of the pair $\{H, H_0\}$ and the relation itself, the *trace formula*. In his paper: "Spectral Shift Function and Hilbert-Schmidt Perturbation: Extensions of Some Work of L.S. Koplienko" *Mathematische Nachrichten* 138(1):7 - 25(1988), Hagen made an important step forward in this problematic. Then (1987 - 1990) he generalized the trace formula for non-unitary and non-self-adjoint operators and showed that a summable real spectral shift function can be introduced for a pair of contractions, or dissipative operators, such that the trace formula holds if they differ by an operator which is *slightly* more compact than a trace-class operator.

These results constituted a part of Hagen's Dissertation of *Doctor scientiarum naturalium* awarded on 30 June, 1987 by the Akademie der Wissenschaften der DDR. The formula proved by him is known now as the *Koplienko-Neidhardt Trace Formula*.

Hagen Neidhardt was very friendly with colleagues and always open to new ideas. In the Laboratory of Theoretical Physics of JINR he was a member of the Mathematical Physics Group headed by Werner Timmermann and then by

Pavel Exner. The problems discussed at the group seminar inspired Hagen to new projects.

One of those was motivated by the question from quantum statistical mechanics and brought a new object to his attention: the Gibbs semigroup, i.e., strongly continuous semigroups $\{e^{-tA}\}_{t\geq 0}$ with values in the $*$-ideal of trace-class operators $\mathcal{C}_1(\mathfrak{H})$ on a separable Hilbert space $\mathfrak{H}$ for $t > 0$. The question was whether the well-known strongly convergent *Trotter product formula*:

$$\lim_{n\to\infty} \left(e^{-tA/n}e^{-tB/n}\right)^n = e^{-tH}, \qquad t > 0,$$

converges in the *trace-norm* topology to semigroup with some generator H if B is the generator of a strongly continuous semigroup. In the paper ”The Trotter-Kato product formula for Gibbs semigroups” with V. Zagrebnov (*Commun.Math.Phys.*1990) this question was answered affirmatively in a general framework of non-exponential Kato functions.

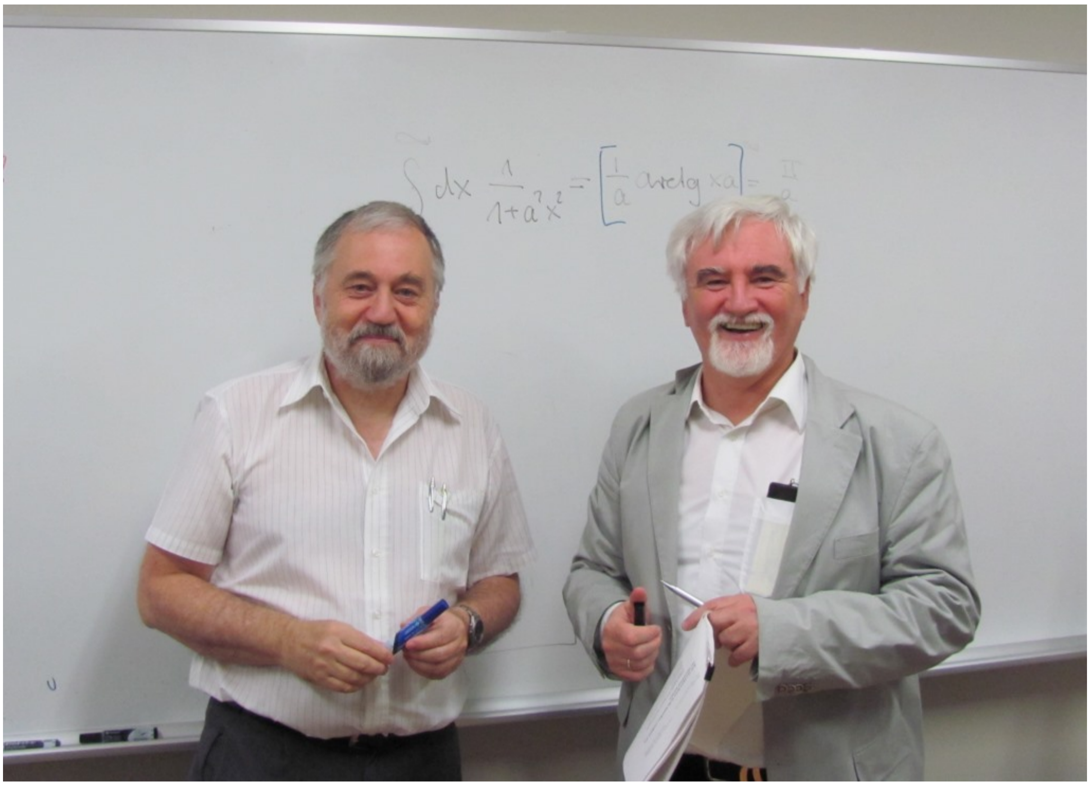
This paper triggered an important long-term research project on the *product formulae* approximations in the trace-norm and the operator-norm topologies for semigroups, unitary groups and for propagators that involved Hagen Neidhardt and his co-authors: P. Exner, T. Ichinose, V. Zagrebnov.

In addition to his research work, Hagen actively participated in the life of the community concentrated around the mathematical physics group of the Laboratory of Theoretical Physics. He attended conferences that were the beginning of what was later known as the ”Mathematical Results in Quantum Physics” (or QMath) series and helped to organize the third one in 1989, dedicated to the memory of M.G. Krein. He also co-edited this proceedings conference volume of QMath3, which appeared under the title ”Order, Disorder and Chaos in Quantum Systems” as Volume 46 in the Birkhäuser ”Operator Theory: Advances and Applications” (Basel 1990).

Back to Berlin

In September 1990 Hagen returned with his family to Berlin. It was not an easy time for them. One of the the results of the German ”reunification” was the demise of the Karl Weierstraß Institute of Mathematics with all of its employees having been made redundant. For two years in 1992-1993 he worked at the Technical University of Berlin, and from the 1st of January, 1994, to

31 December, 1999, he was a research associate at the University of Potsdam. These difficulties neither discouraged him, nor did they reduce his enthusiasm for doing mathematics.



Pavel Exner and Hagen Neidhardt, Kanazawa (2010)

At that time he often visited the Mediterranean University of Marseille-Luminy to continue the collaboration with V. Zagrebnov on the Trotter–Kato product formula and operator-norm convergence. They also started a new project on singular perturbations, regularization and extension theory; let us quote a few principal papers in this connection: "Towards the right Hamiltonian for singular perturbations via regularization and extension theory" (1996), "Does each symmetric operator have a stability domain?"(1998), "On semibounded restrictions of self-adjoint operators"(1998). These results motivated an important article with P. Exner and V. Zagrebnov, "Potential approximations to δ': an inverse Klauder phenomenon with norm-resolvent convergence"(2001).

During the nineties Hagen also collaborated closely with J. Brasche on Krein's extension theory and singularly continuous spectrum of self-adjoint extensions, as well as on the inverse spectral theory for self-adjoint extensions. Once more, the research did not consume all of his energy. During his stay at Potsdam

Valentin Zagrebnov, Hagen Neidhardt, Jürgen Voigt, QMath 7, Prague (1998)

University he organised, in collaboration with M. Demuth, P. Exner and V. Zagrebnov, the fifth issue of the QMath conference series in Blossin in the Berlin suburbs, giving effectively the series a new life; he also coedited the conference proceeds appearing as Volume 70 of the indicated Birkhäuser edition.

Back to the Weierstraß Institute

In January 2000 Hagen Neidhardt succeeded in returning to his mathematical *alma mater*, reborn under the name Weierstraß Institute for Applied Analysis and Stochastic (WIAS). As usual he was full of plans and enthusiasm.

The Trotter–Kato product formulae activity for semigroups progressed successfully in collaboration with Valentin Zagrebnov leading to the operator-norm convergence with the rate estimate subsequently extended to symmetrically-normed ideals, and with T. Ichinose, V. Zagrebnov to fractional conditions ”Trotter–Kato product formula and fractional powers of self-adjoint generators” (*J. Funct. Anal.* 2004). A few interesting results together with P. Exner, T. Ichinose, and V. Zagrebnov were also established for the unitary case in ”Zeno product formula revisited”, (*Integral Equations and Operator Theory* 2007) and in ”Remarks on the Trotter–Kato product formula for unitary groups” (*Integral Equations and Operator Theory* 2011).

During one of his visits to Marseille-Luminy, Hagen came across the activity concerning the non-equilibrium steady states (NESS) in quantum many-body systems, popular there at that time. He quickly realized that there was room here for the application of his expertise in the scattering theory. This was the beginning of his fruitful collaboration with J. Rehberg, H. Kaiser, M. Baro, see e.g. ”Macroscopic current induced boundary conditions for Schrödinger-type operators”, (*Integral Eq. Oper. Theory* 2003), ”Dissipative Schrödinger–Poisson systems” (*J. Math. Phys.* 2004), ”A quantum transmitting Schrödinger–Poisson system”, (*Rev. Math. Phys.* 2004), ”Classical solutions of drift-diffusion equations for semiconductor devices: The two-dimensional case” (*Nonlinear Analysis* 2009).

At the same time, Hagen never ceased to pay attention to the ”purely” mathematical aspect of the NESS and its possible applications, as seen in the papers (with H. Cornean and V. Zagrebnov) ”The effect of time-dependent coupling on non-equilibrium steady states” *Annales Henri Poincaré* 2009), ”The Cayley transform applied to non-interacting quantum transport” (*J. Funct. Anal.* 2014), and ”A new model for quantum dot light emitting-absorbing devices: proofs and supplements” (*Nanosystems* 2015). Note that two last papers were a part of the thesis of his PhD student Lukas Wilhelm (WIAS Berlin).

A similar inclination was shown by Hagen in the papers: ”Non-equilibrium current via geometric scatterers” (*J. Phys. A* 2014) with P. Exner, M. Tater, and V. Zagrebnov and ”A model of electron transport through a boson cavity” Nanosystems (2018), with A. Boitsev, J. Brasche, and I. Popov. The mathematical background of this model was developed within Hagen’s important project on *boundary triplet* technique in the paper ”Boundary triplets, tensor products and point contacts to reservoirs” (*Annales Henri Poincaré* 2018) by the same authors including M. Malamud. There the boundary triplet technique was employed. In this paper, a model of electron transport through a quantum dot assisted by a cavity of photons is proposed. In this model, the boundary operator is chosen to be the well-known Jaynes–Cummings operator, which is regarded as the Hamiltonian of the quantum dot.

The beginning of the collaboration between Hagen and Mark Malamud dates back to the end of the nineties. Originally it was influenced by Hagen’s inter-

Shigetoshi Kuroda and Hagen Neidhardt, Prague (2006)

est in extensions of a symmetric operator with a gap and his joint results with S. Albeverio and J. Brasche. This collaboration started with an attempt to apply the technique of boundary triplets and the corresponding Weyl functions to the problem of existence (and description) of self-adjoint extensions with prescribed spectrum within a gap. Their result, obtained together with S. Albeverio and J. Brasche, was published in ”Inverse spectral theory for symmetric operators with several gaps: scalar type Weyl functions” (*J. Funct. Anal.* 2005).

Later on Hagen (together with J.Behrndt and M.Malamud) applied the Weyl function technique to investigate scattering matrices of two resolvent comparable self-adjoint operators, i.e., operators with a trace-class resolvent difference. In this direction they published two papers: ”Scattering matrices and Weyl functions” (*Proc. Lond. Math. Soc.* 2008) and ”Scattering matrices and Dirichlet-to-Neumann maps” (*J. Funct. Anal.* 2017). There the scattering matrix of two resolvent comparable self-adjoint extensions of a symmetric (not necessarily densely defined) operator with equal deficiency indices has been expressed by means of the limit values of the Weyl function on the real axis and a boundary operator. The abstract result was then applied to various differ-

Mark Malamud and Hagen Neidhardt, Prague (2009)

ent realisations of the Schrödinger operator, where the Weyl function is closely related to the classical Dirichlet-to-Neumann map.

In 2007 the assertion of the first paper for the scattering matrix was generalized to the case of a pair of self-adjoint and maximal dissipative extensions of a symmetric operator with finite deficiency indices in "Scattering theory for open quantum systems with finite rank coupling" (*Math. Phys. Anal. Geom.* 2007).

Using the formula for the scattering matrix the authors recovered a connection, first discovered by V. M. Adamyan and D. Z. Arov, between the Lax–Phillips scattering matrix and the characteristic function of the maximal dissipative operator. Moreover, it was shown there that the Lax–Phillips scattering matrix coincides with the lower diagonal entry of the scattering matrix of the pair of two self-adjoint extensions, with one of them being a minimal self-adjoint dilation of the dissipative operator under consideration.

Let us next mention another Hagen's paper, joint with M. Malamud, "Perturbation determinants for singular perturbations" (*Russian J. of Math. Phys.* 2014). Here the boundary-triplets technique was applied to perturbation determinants (PD) for pairs of resolvent-comparable operators. Treating both operators as proper extensions of a certain symmetric (not necessarily densely defined) operator and choosing a boundary triplet, a PD is expressed via the Weyl function and boundary operators. In applications, it allows one to express a PD of two boundary value problems (BVPs) directly in terms of boundary conditions and the Weyl function. In particular, a PD for two BVPs for Schrödinger operators in a domain with smooth compact boundary via the Dirichlet-to-Neumann map is explicitly computed.

Finally, the series of Hagen's publications in collaboration with M. Malamud and V. Peller deserve a mention: "Trace formulas for additive and non-additive perturbations" (*Advances in Math.* 2015), "Analytic operator Lipschitz functions in the disc and trace formulas for functions of contractions" (*Func. Anal. and Appl.* 2017), and "Absolute continuity of spectral shift" (*J.Funct.Analysis* 2019).

As is clear from their titles, the papers are devoted to the Krein-type trace formulae for pairs of resolvent-comparable operators. Here Hagen returned to the subject of his Dr.Sc. dissertation (1987). In particular, it was shown that a pair of contractions (maximal dissipative operators) admits a (non-unique) complex-valued summable spectral-shift function (SSF), i.e. their resolvent difference outside the unit disc admits a Krein-type representation. Besides, the SSF can be selected to have non-negative (or non-positive) imaginary part whenever the first (second) operator is unitary. A particular case of the later result, where the resolvent difference belongs to the ideal which is slightly narrower than the trace class one and defect operators are of the trace class, was analysed by Hagen (jointly with V. M. Adamyan) in "On the summability of the spectral shift function for pair of contractions and dissipative operators" (*J.Oper.Theory* 1990).

In the two last papers it was also shown that the maximal class of functions for which the Krein-type Trace Formula holds is the class of operator Lipschitz functions, which are analytic in the unit disc.

Note that the problem of description of the maximal class of functions for which the trace formula holds for any pair of self-adjoint operators with trace-class difference, has been posed by M. G. Krein in 1964, and was then solved by V. Peller in 2016. Thus, Hagen jointly with M. Malamud and V. Peller obtained a solution of a version of the M. G. Krein problem for pairs of contractions (maximal dissipative operators).

Takashi Ichinose, Hagen Neidhardt, Valentin Zagrebnov, Prague (2009)

Note that Hagen constructed the first example of a pair of contractions which does not admit a real-valued locally summable SSF in the paper: ”Scattering matrix and spectral shift of the nuclear dissipative scattering theory” (*Operators in indefinite metric spaces, scattering theory and other topics*, Birkhäuser Verlag, Basel 1987). Conversely, in the last paper of the series (*J. Funct. Analysis* 2019) it was proved that a real valued SSF, which is A-integrable (in the sense of A. N. Kolmogorov), always exists.

Hagen's role as a QMath conference co-organizer was not exhausted by the two events mentioned above. In 2012 he came to rescue when the original plan ran into trouble, and it was his work which made the meeting at the Humboldt University in Berlin possible. As in the previous cases, he co-edited the proceedings volume of QMath12, which was published this time by World Scientific in 2014.

Another of Hagen's projects starting in the same year was the book *Trotter–Kato product formulae*. It was planned for the Lecture Notes in Mathematics series and actually is still in progress.

Farewell in 2016

After retirement from WIAS in 2016, Hagen was still active and kept a ”corner” in the institute to invite his visitors and collaborators.

With his student Artur Stephan and V. Zagrebnov, Hagen returned to the *evolution semigroup* approach to construction of a *solution operator* (propagator) for the non-autonomous Cauchy problem in Hilbert and Banach spaces. In fact, this idea goes back to his PhD thesis from 1979 about the Howland-Evans-Neidhardt approach to solution of abstract non-autonomous Cauchy problems. A new aspect was to use the full power the Trotter product formula approximations for evolution semigroups and their one-to-one correspondence with propagators to produce product-formula approximations for the latter.

The one-to-one correspondence allowed control over the rate of convergence of approximants for propagators: ”Convergence rate estimates for Trotter product approximations of solution operators for non-autonomous Cauchy problems” first in archive: arXiv:1612.06147 (2016), and finally in the *Publications of Research Institute for Mathematical Sciences, Kyoto* 2020).

The main results appeared in the series of papers: ”On convergence rate estimates for approximations of solution operators for linear non-autonomous evolution equations” (*Nanosystems: mathematics* 2017), ”Remarks on the operator-norm convergence of the Trotter product formula” (*Integral Equations and Operator Theory* 2018), and ”Trotter Product Formula and Linear Evolution Equations on Hilbert Spaces” (*Analysis and Operator Theory*, vol.146, 2019).

Beyond Mathematics

Hagen was extraordinary person.

He loved mathematics as his the first priority. When visiting any new place on earth for scientific collaboration, he did not seem all that interested in taking time out to do sightseeing, etc.; what he wanted to do was mathematics. So the colleague who invited him would have to preach at him to take a break and do something else, otherwise he would just keep sitting in front of his PC and doing mathematics for the duration of his visit.

Second, he loved nature: he loved hiking, he loved mountains, preferably as high as possible. He enjoyed setting himself a challenge, be it fitness training, moun- taineering, or swimming through a dam or very far from the coast of the Mediterranean Sea in Marseille.

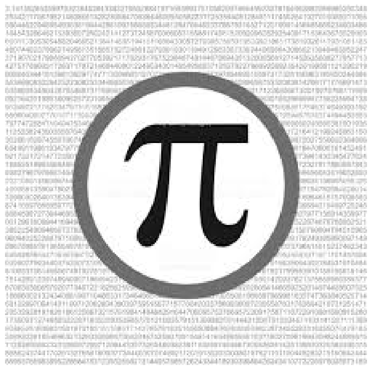
Hagen read several daily newspapers every day, and he declared: ”a high level of general education is to be interested in politics and sports, but also in the small things of everyday life.” Hagen had an extraordinary memory for time - for example, he had absolutely no problem recalling key sporting events and the winners. He also liked to watch live football matches at the Berlin stadium.

Hagen in Kanazawa (2010)

Hagen was rational and objective, but could be very funny as well. He had the knack of being able to reverse the mood in ”unpleasant” situations by his good, amusing comments, but he could also become emotional and philosophical. He was impressed by the way that time progresses. Hagen enjoyed music, for example classical music by Tchaikovsky, but also modern interpreters such as ABBA. He loved space and everything about space travel and technology; sometimes he had tears in his eyes while watching a rocket launch. But he also found joy in the simple things like the view from the terrace into the greenery. Hagen was a family man. When Hagen and Hiltrud celebrated their silver wedding anniversary, he declared: ”Dear Hille, I love you more than my mathematics.” Hagen loved his children and always stood by them with support and advice, without ever making empty promises. He very much admired his first granddaughter Sophie. He had started teaching her the digits of π.

Now you can see π on the gravestone of Hagen Neidhardt (20.11.1950 - 23.03.2019) in the Städtischen Friedhof Pankow-Buch.

We were greatly privileged to be friends and collaborators of Hagen.
We miss him so much.



Jussi Behrndt (Institute for Applied Mathematics, TU Graz, Austria)
Pavel Exner (Doppler Institute, Prague, Czechia)
Takashi Ichinose (Kanazawa University, Kanazawa, Japan)
Mark M. Malamud (People’s Friendship University of Russia, Moscow, RF)
Valentin A. Zagrebnov (Institut de Mathématiques de Marseille, France)

Call for Nominations for the 2021 Henri Poincaré Prize

The Henri Poincaré Prize, sponsored by the Daniel Iagolnitzer Foundation was created in 1997 to recognize outstanding contributions in mathematical physics, and contributions which lay the groundwork for novel developments in this broad field. The prize was also created to recognize and support young people of exceptional promise who have already made outstanding contributions to the field of mathematical physics. The prize is awarded every three years at the International Congress of Mathematical Physics (ICMP), and in each case is awarded usually to three individuals.

The prize winners are chosen by the Executive Committee of the IAMP upon recommendations given by a special Prize Committee. The Executive Committee has made every effort to appoint to the Prize Committee prominent members of our community that are representative of the various fields it contains. However, to be able to do its job properly the Prize Committee needs input from the members of IAMP. For this purpose the Executive Committee calls IAMP members to provide nominations for the Henri Poincaré Prize to be awarded at ICMP 2021 in Geneva, Switzerland.

A proper nomination should include the following:

- A description of the scientific work of the nominee emphasizing the key contributions
- A recent C.V. of the nominee
- A proposed citation should the nominee be selected for an award

Please keep the length of your nomination within a page and submit it to the IAMP President (president@iamp.org) or the Secretary (secretary@iamp.org). A list of previous winners can be found at: http://www.iamp.org.

To ensure full consideration please submit your nominations by **September 30, 2020**.

Call for Nominations for the 2021 Dannie Heineman Prize for Mathematical Physics

This prize recognizes outstanding publications in the field of mathematical physics. The prize consists of $10,000 and a certificate citing the contributions made by the recipient plus travel expenses to attend the meeting at which the prize is bestowed. It will be presented annually.

Establishment & Support. – This prize was established in 1959 by the Heineman Foundation for Research, Educational, Charitable, and Scientific Purposes, Inc., and is administered jointly by the American Physical Society and the American Institute of Physics. Biographical information on Dannie Heineman: https://www.aps.org/programs/honors/prizes/heineman-bio.cfm.

Rules & Eligibility. – This prize is awarded solely for valuable published contributions made in the field of mathematical physics with no restrictions placed on a candidate's citizenship or country of residence. "Publication" is defined as either a single paper, a series of papers, a book, or any other communication which can be considered a publication. The prize may be awarded to more than one person on a shared basis when all recipients have contributed to the same accomplishments Nominations are active for three years.

Nomination & Selection Process.

Deadline: Monday, June 1, 2020
The nomination package must include:
A letter of not more than 5,000 characters evaluating the qualifications of the nominee(s). In addition, the nomination should include:

- A biographical sketch.
- A list of the most important publications.
- At least two, but not more than four, seconding letters.
- Up to five reprints or preprints.

To start a new or update a continuing nomination, please see the Prize & Award Nomination Guidelines at https://www.aps.org/programs/honors/nomination.cfm.

2020 Selection Committee Members: Sylvia Serfaty (Chair), Jan Solovej (Vice-Chair), Francesco Calogero ('19 Recipient), Alexander Its, Michael Loss
Serving a diverse and inclusive community of physicists worldwide is a primary goal for APS. Nominations of qualified women, members of underrepresented minority groups, and scientists from outside the United States are especially encouraged.
Source: aps.org

News from the IAMP Executive Committee

New individual members

IAMP welcomes the following new members

1. PROF. ALBERTO CATTANEO, University of Zurich, Switzerland.
2. DR. TORBEN KRÜGER, University of Bonn, Germany.
3. DR. HERMANN TCHOKOUANSI TAKOUGOUM, University of Yaounde, Cameroon.
4. DR. MARYAM ASHRAFI, University of California at Davis, USA.

Proceedings of XIX ICMP

The Proceedings of the XIX International Congress on Mathematical Physics, held in Montreal between July 23 and July 28, 2018, are now available as a "Special Collection" of the *Journal of Mathematical Physics*, edited by Vojkan Jaksic and Robert Seiringer. They can be found online at

https://aip.scitation.org/toc/jmp/ICMP19/1.

All articles are freely accessible for a year. Moreover, a printed copy of the whole volume can be ordered from AIP at https://printorders.aip.org/JMP.

Open positions

Fellowship in Milan

One year fellowship (renewable) under the supervision of Prof. Vieri Mastropietro within the PRIN research programme "Mathematical Quamtum Matter" regarding Universality properties in transport coefficients or Many body localization. For information, see

https://www.unimi.it/it/ricerca/ricerca-lastatale/fare-ricerca-da-noi/assegni-e-borse/bandi-assegni-di-ricerca

Post-doc position available in Grenoble

Principal Investigator : Nicolas Rougerie
Project : ERC Starting grant CORFRONMAT
Duration: 2 years, starting in the fall of 2020

Description: Applications are invited for a CNRS Post-doc position in mathematical physics. The position is based in Grenoble, at the "Laboratoire de Physique et Modélisation des Milieux Condensés" and is funded by the ERC strating grant "Correlated frontiers of many-body quantum mathematics and condensed matter physics".

We search for candidates with a strong research potential, preferably with a background in one or several of the following fields

- Functional analysis
- Spectral theory
- Partial differential equations
- Many-body quantum mechanics
- Condensed matter physics

Suggested research topics include but are not limited to:

- The many-body problem for quantum particles with fractional statistics (anyons)
- Rigorous studies of fractional quantum Hall states
- Mean-field limits of bosonic equilibrium states

Applications should be submitted by email to nicolas.rougerie@lpmmc.cnrs.fr.

For more information on these positions and for an updated list of academic job announcements in mathematical physics and related fields visit

http://www.iamp.org/page.php?page=page_positions

Benjamin Schlein (IAMP Secretary)

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